New Deal Public Housing and Racial Segregation in U.S. Cities

Hector Blanco* Luca Perdoni‡

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Abstract

Racial residential segregation in U.S. cities rose sharply during the first half of the twentieth century. We study whether early federal public housing contributed to this rise by examining the first projects built by the Public Works Administration in the mid-1930s, most of which were racially designated. Using newly assembled data on project locations linked to full-count Census records from 1910 to 1950 and comparing built to planned-but-not-built projects, we find that public housing reinforced the racial composition of project sites but had little effect on surrounding neighborhoods. The results suggest that early public housing played a limited role in shaping neighborhood-level segregation.

*Rutgers University, CESifo, and NYU Furman Center. Contact: hector.blanco@rutgers.edu.
‡ifo Institut - LMU. Contact: perdoni@ifo.de

1. Introduction

Racial residential segregation in the United States rose sharply over the first half of the twentieth century, becoming a defining feature of American cities (Logan and Parman, 2025). By 1940, the average Black urban resident lived in a neighborhood that was almost majority Black, a dramatic rise from the more integrated residential patterns that prevailed at the turn of the century (Cutler et al., 1999). Economists have attributed this rise in segregation to two broad forces (Boustan, 2013). The first is residential sorting, which has been extensively studied in prior literature, particularly "white flight" from central cities in response to Black in-migration (Boustan, 2010; Shertzer and Walsh, 2019). The second emphasizes collective action through institutions, both private and public, that actively structured residential patterns along racial lines, such as restrictive covenants, mortgage insurance practices, or zoning regulations.

While the effects of many of these institutions are now well documented, less is known about the role of early federal public housing, which was explicitly racially designated, in shaping racial residential segregation. Public housing emerged in the 1930s, during a period of time when segregation was rapidly intensifying and Southern Black migration to Northern and Midwestern cities reached historic levels. These early public housing projects could have influenced segregation through several channels. First, racially designated projects may have mechanically reinforced segregation by placing same-race developments in previously integrated neighborhoods or by replacing racially homogeneous slums with new housing reserved for the same racial group. This mechanism aligns with theories emphasizing the institutional reinforcement of segregation. Second, public housing may have altered neighborhood trajectories if, for example, White-designated projects were constructed in central-city areas experiencing rapid Black in-migration, potentially slowing or redirecting contemporaneous segregation dynamics. Both of these channels operate through direct, mechanical effects arising from the racial composition of project residents. Finally, beyond these direct effects, public housing may have itself induced endogenous residential sorting, triggering migration responses such as white flight that further reshaped neighborhood

composition.

In this paper, we fill this gap in the literature by studying the effects of the first federally built public housing projects in the United States on neighborhood racial composition, demographic change, and economic outcomes. We focus on forty-seven projects constructed by the Public Works Administration (PWA) in the mid-1930s, most of which were explicitly racially designated. We compare these projects to a newly assembled dataset of eighty-eight PWA housing projects that were planned but not built, largely due to funding restrictions and court decisions. The PWA housing program operated at a moment when housing markets were exceptionally tight following the Great Depression, and when public housing was viewed not only as a slum clearance tool but also as a way to provide stable, decent housing to working families facing high rents and limited housing supply. Using full-count Census data from 1910 to 1950, we show that racially designated public housing projects substantially reshaped the racial and demographic composition of the project sites themselves, while having limited effects on surrounding neighborhoods relative to planned-but-not-built locations. Black-designated projects reinforced Black presence primarily by inducing modest white out-migration in the broader area, whereas White-designated projects generated some displacement of nearby Black residents. In neither case, however, did these dynamics translate into large changes in the racial composition of the broader neighborhood.

These PWA public housing projects, far removed from the institutional features and reputation of public housing in later decades, provide an ideal setting to study the interaction between public housing and residential segregation. First, PWA projects were sited, designed, and developed by the federal government, making the program a single, centralized intervention implemented nationwide under a common set of rules and design standards. In contrast, later projects were developed under the Wagner-Steagall Act of 1937, which transferred construction and management authority to local Public Housing Authorities (PHAs), making it more likely that they were shaped by local conditions. Moreover, PWA projects, which often replaced slums, were designed as high-quality, low- and medium-density housing intended for working families, rather than the high-rise structures commonly associated with the negative consequences of later public housing. As a result, any reinforcement of racial segregation

patterns we observe is more likely driven by the characteristics of incoming residents rather than by negative amenities from the new projects. Finally, explicit racial designation shuts down self-selection by race among project tenants.

A key contribution of this paper is the construction of a new, comprehensive dataset on the locations of PWA public housing projects and their plausible counterfactuals, together with a detailed description of the neighborhoods in which they were sited and the households they housed. Using archival records, PWA reports, and historical maps, we recover precise street-level locations, project characteristics, and racial restrictions. We link these data to full-count U.S. Census records from 1910 to 1950, which we also geocode at the address level. This allows us to track neighborhood composition and population changes before and after project implementation. We document that both built and planned projects were, on average, located in dense and relatively disadvantaged neighborhoods characterized by high Black population shares. Black-designated and White-designated projects were systematically sited in different areas: Black-designated projects tended to be located in denser neighborhoods with higher rents, higher Black population shares, and residents more likely to have migrated from the South, while White-designated projects were located in neighborhoods with lower Black shares and higher foreign-born populations. Households moving into PWA housing were positively selected along observable dimensions such as education and employment, consistent with the strict tenant selection criteria used in early public housing programs.

To identify the causal effects of early public housing on neighborhood outcomes, we estimate several difference-in-differences designs that leverage both hyperlocal spatial variation in proximity to projects and comparisons between built PWA projects and projects that were planned but never constructed ("control projects"). Our first approach uses the ring method, comparing project sites and nearby areas within a given distance to areas slightly farther away, which allows us to track how neighborhoods with projects evolved relative to the rest of the city. A second approach compares the evolution of project sites and surrounding areas near built projects to those near control projects. Because planned projects progressed far enough to have identified locations but were halted for reasons largely unrelated to local neighborhood trends, these sites provide a credible counterfactual to actual construction. This strategy

helps isolate the effects of public housing from contemporaneous forces such as the Great Migration, the Great Depression, and preexisting segregation trends. Our third approach combines both dimensions in a triple difference-in-differences design, which is our preferred specification due to improved precision and weaker identifying assumptions. While the ring method requires parallel trends between near and farther areas, and the built-versus-control comparison requires parallel trends between treated and control sites, the triple-difference framework relaxes these assumptions by differencing across both dimensions.

On average, PWA public housing projects had limited impact on project sites and their surrounding neighborhoods when compared to control projects. We start by documenting strong pre-trends in the ring method: before PWA construction, Black shares in project sites had already increased and stayed at the same level after construction, compared to those farther away areas. What we do observe is that project sites considerably increase their population as a result of PWA projects. However, the pre-trends in racial composition and the population effects fade out when using control projects as the comparison group: which suggests that, compared to the relevant counterfactual, these neighborhoods would have produced housing anyway and attracted the same racial composition of individuals. In this strategy, we also observe that families are less likely to live overcrowded, have better employment outcomes, and pay higher housing prices–consistent with the high quality of the projects. Beyond the project sites, there are no effects within this strategy. However, the ring method tells us that the broader area kept attracting more Black, Southern migrant populations over time.

The effects of public housing differed sharply by the racial designation of the project. To show this, we first address the limitation that, while racial designation is observed for all treated projects, it is missing for 39 percent of control projects. Therefore, we predict the racial designation of control projects by fitting a logistic regression with LASSO regularization using treated projects, drawing on a rich set of pre-treatment demographic and socioeconomic Census characteristics as predictors. This procedure correctly classifies over 94 percent of control projects for which racial designation is observed.

Using our preferred triple difference-in-differences specification and restricting comparisons

to same-race projects, we find that Black-only projects increased the Black population share at project sites and their immediate surroundings by roughly 15 percentage points, primarily through modest out-migration of White residents. These effects reinforced preexisting patterns of racial concentration: the affected areas were already predominantly Black prior to construction and became nearly entirely Black thereafter. White-only projects, by contrast, displaced preexisting Black residents. Beyond the immediate vicinity of the projects, we find little evidence of significant neighborhood change for either project type, with two exceptions. First, White-only projects modestly increased the Black population within a 750-meter radius. Second, home values increase only near White-only projects, even though both types of projects were similar in quality, suggesting that the broader neighborhood response depended critically on the racial designation of the project.

We then explicitly examine how racial designation interacts with outmigration responses by the race of incumbent residents. To do so, we focus on a group of residents that lived near the projects in 1930 that we are able to track in 1940 and 1950. This analysis provides a direct test of the residential sorting hypothesis: whether any observed demographic changes reflect residential sorting induced by public housing, rather than purely mechanical effects of its construction. We find that Black-designated projects did not induce any substantial out-migration compared to control projects. However, White-designated projects, were more likely to displace Black residents in the broader area, suggesting that the increase in Black population observed in the broader area should come from net inflows of Black households rather than within-neighborhood compositional change.

Taken together, our findings show that early federal public housing did not lead to large-scale neighborhood transformation. By focusing on the first generation of federally built, racially designated public housing, this paper clarifies how place-based housing policy interacted with demographic change during a critical period in the rise of racial residential segregation in U.S. cities. More broadly, the results highlight the limited capacity of public housing construction alone to alter entrenched segregation patterns when public policy assigns housing access along explicit racial lines.

This paper builds on a large literature examining the mechanisms through which racial

residential segregation intensified in the United States during the twentieth century (Cutler et al., 1999; Boustan, 2013; Logan and Parman, 2017, 2025). Prior work documents how residential sorting—particularly "white flight," defined as the out-migration of white households from central cities in response to Southern Black migration to Northern cities—explains a substantial share of rising segregation (Boustan, 2010; Shertzer and Walsh, 2019). A related literature emphasizes the role of collective action and institutional arrangements that structured residential patterns along racial lines, including restrictive covenants (Sood and E-Solberg, 2024), redlining and mortgage insurance practices (Fishback et al., 2020; Aaronson et al., 2021; Ali, 2024; Fishback et al., 2024; Hynsjö and Perdoni, 2024), and zoning (Cui, 2024). We contribute to this literature by examining a federal housing policy, public housing, that expanded rapidly at precisely the moment when urban racial segregation was rapidly increasing, yet has received comparatively little attention despite its explicit racial design.

We also contribute to the literature on the neighborhood effects of public housing. Much of the U.S. evidence to date focuses on the demolition of large, deteriorated public housing projects in the late 1990s and early 2000s, documenting substantial changes in neighborhood composition, including large declines in Black and low-income populations, sharp increases in housing prices, and sizable reductions in crime following demolition (Aliprantis and Hartley, 2015; Sandler, 2017; Tach and Emory, 2017; Blanco, 2023; Almagro et al., 2023; Staiger et al., 2025). More recent work has examined earlier waves of public housing construction, with mixed findings. Using matching approaches, Harris (2025) finds that post-1940 public housing attracted somewhat higher-income tenants without substantially altering neighborhood racial composition, while Bressler (2025) documents large increases in Black population shares and declines in rents and median income, with spillovers to adjacent areas. Our findings are more closely aligned with Harris (2025), likely because we focus on an earlier period when tenant composition had not yet shifted toward lower-income households, while Bressler (2025) includes that later period. Guennewig-Moenert (2025) similarly studies earlier public housing in New York City using slightly distant neighborhoods as controls and finds strong segregation effects. In contrast, we show that conclusions drawn from proximity-based comparisons can differ substantially from those obtained using sites that were actively considered but ultimately rejected for public housing construction.

We advance this literature in several ways. First, we introduce a novel and plausibly more comparable set of control sites based on planned-but-not-built projects. Second, we focus on PWA public housing, which has been largely excluded from prior studies and represents a distinctive treatment due to its federal nature and homogeneous siting and design. Third, we explicitly exploit the racial designation of projects to study heterogeneous effects and sorting mechanisms. Finally, while prior work relies primarily on census-tract-level analysis, we use precise address-level Census data, which enable us to cleanly distinguish effects at project sites from spillovers beyond project boundaries, and we also use pre-1930 Census data.

2. Background

In the early 1930s, U.S. cities faced an acute housing crisis marked by a collapse in new construction, housing instability, and persistently poor living conditions for low-income households. Following the 1929 stock market crash, housing supply declined sharply from its peak in 1925, when nonfarm housing starts had reached 937,000 units. By 1933, new housing starts had fallen to an absolute low of 93,000 units, down from 134,000 in the previous year and 254,000 in 1931.[1] The drop in construction was largely the result of a credit crunch: developers lost access to financing as banks became unwilling or unable to lend for new housing projects. The resulting lending freeze also contributed to a surge in foreclosures, further undermining housing stability for working- and low-income households. These pressures were particularly severe in central cities, where overcrowding and deteriorating housing stock were already widespread and where Northern cities were absorbing large inflows of Southern Black migrants during the Great Migration (Collins, 2021).

While the housing crisis reached its peak in the early 1930s, the intellectual foundations for federal housing intervention had begun to take shape earlier. Throughout the 1920s, European governments—most notably in Germany and Vienna—developed large-scale public housing programs that directly influenced the nascent American Housing Movement (Radford,

1. U.S. Bureau of the Census, Historical Statistics of the United States, Bicentennial Edition, vol. 1 (Washington, DC: Government Printing Office, 1975), N156, p. 640.

1997). Led by Catherine Bauer, an intellectual network of East-Coast progressives started to advocate for a new approach to housing: a universalistic policy providing *"Modern Housing for America"*. Crucially, these ideas gained political traction because they aligned with a widely shared concern: slum clearance. America's poorest neighborhoods were increasingly viewed not only as sites of extreme deprivation with high rates of disease, crime, and fire, but also as active threats to surrounding property values.[2] Since private enterprises appeared unable to provide proper low-income housing, planned neighborhoods built by the federal government became the only solution to the "*moral menace of the slum*" (Public Works Administration, 1939).

Usually, the U.S. real estate industry would have easily stopped such radical proposals for publicly-funded, broadly-targeted housing. However, the Great Depression and the 1932 elections created a unique political opening. The elected congress had a majority of non-southern Democrats, and the Roosevelt administration wanted to consolidate new, urban Democratic support into a permanent coalition (Radford, 1997). The National Industrial Recovery Act of June 1933 allowed the new Public Works Administration (PWA) to engage in the "*construction, reconstruction, alteration or repair under public regulation or control of low-rent housing and slum clearance projects.*"[3]

The task of developing the first federal housing interventions was assigned to the Housing Division of the PWA. Bulletins from that time describe four objectives (Federal Emergency Administration of Public Works, 1936). The first goal was to create jobs in the building sector, the second was to provide healthy living conditions to low-income Americans. Slum clearance was the third goal, while the last aim was to demonstrate the feasibility of large-scale community planning. While public intervention in the housing market was novel, the first policy adopted by the housing division was rather traditional, focusing on private initiative.[4]

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2. Public Works Administration (1939) reports: *"It was a blight on the city and the blight was not static, it was moving glacier-like toward high-priced business property, carrying depression of values along with it steadily and surely."*

3. National Industrial Recovery Act of 1933, Title II, Sec. 202

4. Direct government intervention in residential real estate was an extraordinary step and one of the most radical innovations of the New Deal reforms (Radford, 1997; Federal Emergency Administration of Public Works, 1936). While fifteen years earlier, federal housing programs were characterized in the Senate

The government would loan the necessary capital to private "limited-dividend" corporations with convenient terms, capping the potential profits.[5] The PWA received more than 500 applications, but its staff approved only 7 of them, judging most of them as ill-conceived, financially unsustainable, or "last attempts to save failed commercial ventures" (Public Works Administration, 1939).

The failure of the limited dividend program was denounced by Harold L. Ickes, the Secretary of the Interior and PWA administrator, as a failure of U.S. private enterprises.[6] The need for direct government intervention was presented as unavoidable, and in February 1934, Secretary Ickes announced that the Housing Division would start planning direct construction of public housing.

Since March 1934, the division's staff started to travel across the country to identify possible sites and develop preliminary projects. The acquisition of land titles proved to be the most daunting task, given that sites were often divided into hundreds of smaller parcels. As a solution, the PWA often took advantage of eminent domain to obtain the necessary land by condemnation. Unexpectedly, in January 1935, a judge ruled that PWA's use of eminent domain in Louisville, KY was unconstitutional. The Sixth Circuit Court of Appeals in Cincinnati, OH dismissed the government's appeal on July 15, 1935. Fearing wider consequences for projects across the country, the housing division decided not to fight the ruling at the U.S. Supreme Court.[7] Hence, in the later planning phase, PWA prioritized

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as *"insidious concerted efforts to overturn the entire government of the United States"*, this provision was approved without any congressional opposition at this time of crisis (Radford, 1997).

5. The loan covered 85% of the capital necessary, with a 4% interest rate and a 30-year maturity. Profits of such "limited-dividend" corporations were capped at 6% of their investment.

6. *"It quickly became obvious that our much vaunted private initiative, as so often happens when the goal is a social good instead of private profit, was unable or unwilling to undertake the job"*, as found in (Radford, 1997)

7. The PWA project files at the National Archives often contain communications between the PWA housing director and local housing officials. An example of a court-related suspension can be found in the material for project H4401 in Dayton OH. On July 23, 1935, the PWA director wrote: "*In view of a recent decision rendered by the Sixth Circuit Court of Appeals at Cincinnati, Ohio, I find it advisable to alter our procedure relating to housing projects in slum areas. It will be necessary, therefore to suspend the proposed [. . .] projects until such time as a satisfactory method for acquiring property can be effected*". Examples of similar letters can be found in Figure B.2.

vacant land sites to finalize its list of projects.[8]

By September 1935, the housing division had prepared plans for more than a hundred public housing developments. However, the President ordered to proceed only with projects that could be completed with minimum delay. At the same time, the program's budget was abruptly reduced. The complex interaction of financial, planning, and legal determinants that led to PWA's plan curtailment can be traced by the letters sent by the division director, A. R. Clas, to local housing officials, which we retrieved at the National Archives.[9] Ultimately, the federal program proceeded with the construction of 53 projects, 47 of which were built in the continental United States.[10] Construction works began in December 1935 and were completed by the end of 1937. PWA developments often mirrored pre-existing segregation patterns, targeting neighborhoods with higher baseline shares of the designated race. Accordingly, 17 Blacks-only projects were completed in historically African-American areas, while only white Americans could live in 24 developments. The remaining public housing projects allowed tenants of any race. While slum clearance had been the principal political selling point of the division's plan, this task was gradually abandoned after the judicial obstacles to condemnations: as a result, 21 projects were built on vacant land, and 25 projects replaced existing slums.

In September 1937, months before the completion of the last PWA buildings, the Wagner-Steagall Act restructured the U.S. public housing administration for good.[11] The PWA

8. The judge based the sentence on the argument that the land acquired to build public housing projects would not be "devoted to a public use". In the judge's view, public goods were limited to non-rival and non-excludable ones. The Circuit Court of Appeals in Cincinnati upheld the decision.

9. From the material related to project H1802 in Cincinnati, OH: "*Within the last week the allotment to this Division has been reduced to $100,000,000. [...] Priority of application was a major factor in the choice of those to be developed. The other lay in the stages of development attained. I deeply regret that the [...] Project was not among those on the restricted list approved. [...] It implies no lack of energy, ability, and integrity on the part of your Committee and those other public spirited citizens of Cincinnati who have given so generously of their intelligence and industry to obtain a project for their city. [...] The work which you and your associates have so ably accomplished in Cincinnati has been duplicated in a hundred cities.*" Examples of similar letters can be found in Figure B.2.

10. Nationwide, 53 projects were constructed. Forty-eight were built in the contiguous United States, and five were built in Puerto Rico and the U.S. Virgin Islands. We merge two geographically adjacent projects in Chicago—"Jane Addams Houses" and "Addition to Jane Addams Houses"—and therefore analyze 47 distinct sites.

11. Although the bill was framed at the time as a progressive measure, the conservative coalition managed

housing division was replaced with the United States Housing Authority (USHA). Since then, local Public Housing Authorities (PHA) have constructed and managed public housing units employing federal loans from USHA (Shester, 2013). The Housing Acts of 1949 and 1956 increased the American public housing stock and targeted assistance to the elderly and households most in need (Vale and Freemark, 2012). Federal housing policies permanently shifted from direct production to housing vouchers and subsidies to private investors with the Housing and Community Development Act of 1974. In recent years, the number of recipients of Section 8 tenant-based vouchers and units financed by the Low-Income Housing Tax Credit (LIHTC) vastly exceeds the availability of traditional public housing (Goetz, 2011; Vale and Freemark, 2012).

2.1 The First Public Housing Projects: Distinctive Features

Most of the public and academic debate about U.S. public housing has focused on recent times. The classic images of run-down public dwellings from the 1970s or later do not represent early PWA projects. The latter buildings had high design standards and were in high demand by the public, remaining "*for many decades the finest urban housing in America*" (Radford, 1997).[12]

There are four main ways in which PWA projects differed from later public housing projects. First, PWA projects were segregated by race. The PWA buildings replicated preexisting racial patterns of residential sorting. Accordingly, 17 Blacks-only projects were completed in historically African-American areas, while six public housing developments allowed tenants of any race. In the remaining 24 projects, only white Americans could live. There were no substantial quality differences between whites-only developments and the African-American ones (Radford, 1997). The PWA approach to race mirrors the one of other federal agencies dealing with the housing market at the time, such as the Federal Housing Authority (FHA)

to restrict housing assistance with several amendments. These included construction cost limits, a local-level approval process, and the requirement to demolish as many slum units as those that were built. These constraints made public housing construction more difficult and lowered the quality of the resulting buildings (Radford, 1997).

12. Pictures from three PWA housing projects can be found in Figure I

and the Homeowners Loan Corporation (HOLC). Since ethnic discrimination and racial segregation were endemic in 1930s American cities, these federal agencies accepted and replicated common racist norms, transforming them into official government policies (Hynsjö and Perdoni, 2024; Ali, 2024; Aaronson et al., 2021; Fishback et al., 2020, 2022).[13]

Second, PWA dwellings were a desirable housing option in high demand. The buildings were not meant to house only welfare recipients but were open to anybody who would apply. The first federally-funded projects aimed at accommodating the upwardly mobile working class and targeted admission to two-parent households with stable employment (Vale and Freemark, 2012). The rents necessary to financially sustain the projects excluded the poorest Americans and targeted the mid-third of the income distribution.[14] This fact, together with the replacement of existing slums, indicates that public housing did not carry the stigma associated with its later stages. These facts motivate two hypotheses. First, neighborhood composition must have considerably changed after PWA construction since eligible households were plausibly higher-income than incumbent slum-dwellers. Second, PWA projects, which were of higher quality than preexisting housing, potentially contributed to positive spillovers and higher values for nearby properties (Rossi Hansberg et al., 2010). These two hypotheses contrast with the prior literature in Economics focusing on public housing demolitions under the HOPE VI program during the 1990s and 2000s. The documented negative effects of public housing on neighborhood socioeconomic status (Tach and Emory, 2017) and house prices (Brown, 2009; Zielenbach and Voith, 2010; Blanco, 2023; Almagro et al., 2023) may be due to programs' specific characteristics and as consequences of a specific government program concentrating socioeconomic disadvantage at a time of welfare cuts. We argue that an alternative type of public housing at a different time can then have an opposite effect compared with the existing literature.

13. Scholars in Sociology and Political Science often label this standardization process as *institutional discrimination* (Small and Pager, 2020)

14. The Housing Division set up a selective, standardized process to review applicants. The process considered several aspects of applicant households: need for housing, general desirability (verified with home visits to revise cleanliness and care of the property, among others), and financial security (using rent records, other bills, and continuity of income). *Source*: United States Federal Emergency Administration of Public Works Housing Division (1938), Supplement A.

Third, PWA projects consisted of low- and medium-density housing structures. Since most of the staff of the division was drawn by members of the housing movement, the completed projects reflected their idea of public housing. Their objective was to plan 'complete neighborhoods' by "*remodeling existing street patterns and reduce the necessary street area as much as 30 percent*"(United States Public Works Administration, 1939), as well as to create a physically integrated environment that would encourage a livelier and more solidary local community.[15] As a result, PWA projects consisted of either one- to two-story rowhouses or two- to four-story apartments and flats, which have been considered "*better designed and built than later public housing*" (Radford, 1997). Later, public housing concentrated very low-income households in high-rise buildings. This type of design is associated with higher crime rates (Ellen and Turner, 1997; Glaeser and Sacerdote, 1999; Heller et al., 2010) and is consistent with large crime decreases after the demolition of public housing high-rises (Aliprantis and Hartley, 2015; Sandler, 2017).

Fourth, PWA projects were built and managed by the federal government. Although the Housing Division used local architects and contractors, they closely supervised all phases of work, acquired and maintained sites in their name, and retained title to land and structures after completion (Radford, 1997).[16] Hence, PWA projects shared the same management and likely resulted in similar 'treatments' across metropolitan areas, which makes our results less likely to be confounded by local idiosyncrasies. Furthermore, this context provides a unique opportunity to study the role of centralization in public housing, which contrasts with the management by local authorities of later public housing, accused of the deliberate neglect and lack of funding that would contribute to their decay (Popkin et al., 2004; Shester, 2013).

15. " See Radford (1997): *Many of those affiliated with the program also believed that a more physically integrated residential environment would encourage a livelier and more solidary local community. [...] many design professionals and others believed these concepts could be used to create something better than what the private real estate market offered even affluent families: neighborhoods that combined the lower densities, trees, and green spaces characteristic of the suburban fringe with the convenience of living near the metropolitan core.*".

16. The Management Branch of the Housing Division was in charge of the removal of incumbent tenants and assisting in relocating them. Such was the control of the Housing Division that "*even local housing authorities, formed originally at the urging of the Housing Division, had little impact on decisions in their cities. [...] Harold E. Ickes insisted on making the final decision on every project and signed every contract the Housing Division made*" (Radford, 1997).

In the second half of the 20th century, systemic underfunding and revised tenant selection policies permanently changed the character of American public housing. The buildings became shelters of last resort for "households on welfare, the otherwise homeless, and the disabled" (Shester, 2013). As political support for welfare programs eroded, public housing became the battlefield hospital to the "War on Poverty" (Vale and Freemark, 2012). Twenty years after championing a visionary federal housing program, Catherine Bauer lamented that "*Life in the usual public housing project just is not the way most American families want to live. Nor does it reflect our accepted values as to the way people should live*" (Bauer, 1957).

3. Data and Descriptive Statistics

We assemble address-level data on public housing projects from a variety of historical data sources and link these records to full-count Census data between 1910 and 1950. We show that the siting of PWA projects varied systematically by racial designation: Black-only projects were disproportionately located in dense, economically disadvantaged neighborhoods with high preexisting Black population shares, relative to both White-only projects and other areas within the same cities. We also document that tenants admitted to PWA projects were of substantially higher socioeconomic status than incumbent residents in the surrounding area.

3.1 Public Housing Projects

We identify public housing projects built by the Public Works Administration using historical documents issued by its Housing Division between May 1935 and September 1937 and reported in Radford (1997). The documentation includes the project code, city, state, project name, number of units, rooms, funding amount, lot size, lot status (vacant or slum), demolition of previous structures, and type of housing being built, among others. Importantly, we observe the race to which each project was targeted. There are a total of 47 projects that we can employ in our treated group. We list details about these developments in Appendix Table A.1. We obtain addresses by manually merging this dataset with publicly available

data from the US Department of Housing and Urban Development (HUD). Specifically, we link PWA projects to two HUD files: Subsidized Housing Projects' Geographic Codes from Form HUD-951, a publicly available file with detailed public housing addresses as of 1996; and geolocated public housing buildings as of December 2022.We turn the point-address information for the completed projects into development shapes drawing a perimeter following contemporaneous street grids.

We define the control group as projects that were proposed and planned but not constructed by the PWA. The construction of a reliable control group required the collection of archival data about plans for developments that the PWA ultimately did not undertake.[17] The records of the PWA housing division stored at the National Archives contain a rich set of information about any project ever planned by the agency. We located and scanned archival evidence for 132 unrealized developments. For 95 of these projects, we are able to recover precise addresses.[18] The type of evidence is not standardized across the different projects. Nevertheless, in most cases, we can recover details about the buildings' intended location, race restrictions, nature of the construction site, and the reason for curtailment. Appendix Figure B.3 shows two examples of sources for the location of the canceled projects, which can be either textual or visual.

We then build a control counterpart to the shapes of the completed housing projects, drawing the perimeters of these planned constructions.[19] Figure III shows examples of the digitized project shapes for completed constructions, in blue, and canceled ones, in orange. Our final control sample includes 88 projects that satisfy at least one of the following criteria: (i) projects that we can explicitly identify as curtailed by budget cuts in September 1935 (29

17. The task proved challenging since a unique, authoritative list detailing canceled projects could not be retrieved. In fact, Appendix Figure B.1 shows the final report of the National Emergency Council for the housing division from September 1935: all the final numbers were crossed out with red pencil, suggesting a last-minute overhaul of the program.

18. For the remaining 37 cases, seven correspond to projects whose plans were never fully developed. There is no evidence about the intended location of the other 30 projects, suggesting the planning never reached an advanced stage.

19. In most cases, we can find historical locations in contemporary street locators. When that proved impossible, we relied on historical fire-insurance Sanborn Maps to retrieve the intended location for the developments.

projects); (ii) projects for whom we have proof that land acquisition was blocked by courts through eminent domain challenges, mostly around July 1935 (13 additional projects); or (iii) projects for which the last available archival update before September 1935 dates up to May 1, 1935, with no subsequent evidence of construction (46 additional projects). This choice is grounded on the argument that, absent funding constraints and court challenges faced by the PWA, these projects were sufficiently advanced in planning to have plausibly received PWA financing and been constructed, making them credible counterfactual sites for built PWA projects.

Because the race of the project is central to our analysis, we predict the racial designation of control projects for which this information is missing. While race is observed for all 47 PWA projects, it is unavailable for 34 of the 88 control projects (39 percent). We focus on predicting Black-only versus White-only projects and exclude mixed-race projects, which are rare in our sample. As predictors, we use 21 demographic and socioeconomic Census variables—including population levels, family structure, race, education, employment, and housing values—measured in the pre-treatment periods of 1920 and 1930. We compute these variables both for the project footprint and for the area within 500 meters of its boundaries, and we additionally include changes in these variables between 1920 and 1930 when data are available in both decades. This yields a total of 135 predictors.

We predict project race using penalized logistic regression. Given the relatively small number of observed PWA projects available for training (41 non–mixed-race projects) and the high dimensionality of the predictor set, we adopt penalized methods to avoid overfitting. We estimate both LASSO and Ridge models on the built PWA projects using standardized predictors and apply the fitted models to the control projects. The LASSO model performs particularly well in distinguishing between Black-only and White-only projects. Panel (a) of Appendix Figure B.5 plots the predicted probability that a project is Black under both methods, along with their actual race for control projects with this information. Visual inspection shows that projects with a LASSO-predicted probability above 0.35 are overwhelmingly Black: 92 percent of control projects classified as Black above this threshold with non-missing race information are actually Black, while 97 percent of projects classified as White with

non-missing race information are actually White. We therefore adopt this LASSO threshold to impute race for control projects. Panel (b) of Appendix Figure B.5 further shows that the strongest predictors of a Black project designation are a high Black population share in 1930, both within the project site and within 500 meters, as well as high baseline rents in the project site.

3.2 Historical Census Data and Geographies

We use full-count U.S. Census data spanning 1910–1950. Household heads are geocoded by exploiting street addresses contained in the restricted Ancestry.com version of the Census and harmonized through IPUMS (Ruggles et al., 2020). Address strings are standardized using procedures commonly adopted in the spatial history literature (Logan and Zhang, 2018), and coordinates are assigned with ESRI StreetMap Premium 2024, a modern geocoding system that integrates parcel-level information with street network data. To study short-run residential mobility, we can also build person-level links across Census waves using the crosswalks available in the IPUMS Multigenerational Longitudinal Panel (MLP) (Ruggles et al., 2025).

These geocoded records allow us to construct neighborhood-level outcomes capturing demographic and socioeconomic composition, together with housing market conditions. Sociodemographic outcomes are available throughout our sample (1910–1950) and include population counts and shares by race, nativity, and further individual and household characteristics, including labor-market outcomes. By contrast, local housing market conditions are only available between 1930 and 1940. The georeferenced version of these variables allows us to measure racial residential segregation flexibly and to summarize local conditions by aggregating individuals onto a regular grid of hexagons.[20] We rely on this grid for multiple purposes. First, it allows us to report spatial measures such as area and population density in the descriptive statistics. Moreover, we use hexagons to define a consistent "small-area"

20. We treat these hexagons as the fundamental spatial unit, which facilitates combining datasets recorded at heterogeneous geographic scales. Each hexagon covers roughly 0.025 square kilometers (7.3 acres) and has a side length of about 100 meters (328 ft), a size comparable to a typical city block in grid-plan cities such as Chicago or New York.

geography when classifying whether an individual moved out in the final section. We paid attention to defining the "catchment" area of each development, taking into account geographical features such as bodies of water. We use a raster of U.S. waterbodies to ensure that that we are not treating any hexagon based on a development located on the other side of a river or a lake. Appendix Figure B.4 shows two examples of the surrounding areas for projects included in the analysis.

3.3 Descriptive Statistics

3.3.1 Project Characteristics and Siting

Figure II shows the spatial distribution of public housing projects, distinguishing between PWA projects that were built (treated, shown in blue) and projects that were planned but never built by the PWA (controls, shown in gray). The figure shows that nearly all projects were located in the Northeast, Midwest, and South of the continental United States. Within each of these regions, both treated and control projects are present. Several of the largest cities at the time—including Chicago, Detroit, New York, and Philadelphia—hosted both treated and control projects, while most other cities had either treated or control projects, sometimes with multiple projects located in the same city. Among treated projects, Appendix Figure B.6 shows no clear geographic pattern in the spatial distribution of Black-only and White-only projects, with some cities hosting only one type and others containing both.

Table I summarizes characteristics of treated (columns 1–3) and control projects (column 5), along with descriptive statistics for the remaining areas of the counties in which these projects are located (columns 4 and 6, respectively). For PWA projects, we further distinguish between Black-only and White-only projects (columns 2 and 3).

Panel A shows that PWA projects contained, on average, 456 housing units and spanned approximately 11 hexagons, with a majority replacing pre-existing slums (54 percent). Of the 47 PWA projects in our sample, 17 were Black-only and 24 were White-only. Black-only projects were slightly smaller in number of units but substantially more likely to replace slums (71 percent). While we observe fewer characteristics for control projects, these projects occupied a similar amount of land and exhibited comparable probabilities of being Black-only

and of replacing a slum.

Panels B and C describe neighborhood characteristics within 250 meters of the project sites in 1930, prior to construction. Relative to the rest of their respective cities, PWA projects were sited in neighborhoods with much larger Black populations, which were also more likely to be native-born or to have migrated from the South. These neighborhoods further exhibited lower educational attainment and labor market outcomes, lower homeownership rates, and significantly lower housing values. The latter highlights an important factor shaping project siting under the PWA: low land acquisition costs. Notably, nearly all of these differences disappear when comparing treated sites to control sites, with the exception of rents, which are higher in control areas. Moreover, the differences between control sites and the rest of their cities closely mirror those between treated sites and the rest of their cities, which reinforces the credibility of control projects as being the relevant counterfactual for PWA locations.

Finally, Black-only and White-only projects were located in sharply different neighborhoods. Black-only projects were built in denser areas with substantially higher Black population shares (69 versus 9 percent). These neighborhoods were predominantly native-born, with a sizable share of residents having migrated from the South, while White-only projects were typically located in largely non-Black neighborhoods with higher foreign-born populations. Residents near Black-only projects also exhibited lower educational attainment and occupational scores. While homeownership rates and housing values were similar across the two types of neighborhoods, baseline rents differed substantially: households living near sites that would later host Black-only projects paid rents roughly 45 percent higher than those near White-only projects, despite being more likely to reside in slums.

3.3.2 Project Tenants: Who Moved into the Projects?

In Figure IV, we study the characteristics of individuals who moved into the projects. In particular, we compare the 1930 (panel a) and 1940 (panel b) characteristics of individuals newly arrived to the projects or to nearby inner rings in 1940 to those arriving to the outermost ring, using the historical Census panel dataset.

In 1930, future project tenants closely resembled individuals who would later relocate to areas near the projects. They were similarly likely to be native-born or to have migrated

from the South and exhibited comparable levels of education and employment. They also faced comparable housing prices. While future project tenants were substantially more likely to be Black than individuals residing in the outermost ring, this difference reflects a broader spatial pattern: Black residents were disproportionately settling closer to the project sites, with this concentration attenuating with distance. A similar pattern appears in the education score and migration from the South. Similar spatial patterns are evident for educational attainment and Southern migration, consistent with the fact that a fraction of the projects replaced existing slums.

By 1940, however, project tenants displayed considerably better socioeconomic outcomes. They were significantly more likely to have teenage children enrolled in school and substantially less likely to be unemployed or out of the labor force. These patterns are consistent with the presence of strict tenant selection criteria in early public housing programs and suggest that access to public housing functioned as a reward for working families seeking decent quality and safe housing. Consistent with program design, project tenants were also much less likely to be homeowners and paid significantly higher rents.

4. Empirical Strategy

We estimate the impact of the first public housing projects on neighborhood outcomes and segregation using two sources of variation: proximity to the projects and the quasi-exogenous curtailment of projects that were planned but not built by the PWA. Based on these, we lay out three complementary stacked difference-in-differences (DID) specifications with alternative comparison groups and identifying assumptions. The first approach is the traditional ring methodology that compares areas immediately surrounding the projects to nearby areas that are sufficiently farther away. In the second approach, we compare areas near PWA projects to areas near planned-but-not-built projects. Finally, we combine these two designs in a triple difference-in-differences.

4.1 Ring DID

Our first approach compares outcomes in areas near PWA projects to areas farther away from these projects. The identifying assumption is that, in the absence of their construction, outcomes in close areas would have evolved in parallel to outcomes in farther away areas. Intuitively, the exact location of a PWA project within a neighborhood is quasi-random and depends on idiosyncratic circumstances, e.g., land availability, ability to claim eminent domain, or negotiations between the government and landowners, among others. The Status of Projects documents issued by the Housing Division provide several examples indicating that these considerations influence the final location of the projects.[21]

We implement this strategy as follows. We first identify all individuals living within one kilometer of each PWA project. Next, we construct 250 meter-wide mutually exclusive, concentric around the outline of each project ('inner rings'). Together with the project site, we consider these the treated areas in this specification. We use individuals living within 750-1,000m from the projects ('outer ring') as the comparison area. Finally, we stack all these project-specific datasets.

Given that an individual may be within one kilometer of more than one project within the same city, we estimate the following equation at the individual i, project p, year t level:

$$Y_{ipt} = \alpha_{p,h(i)} + \kappa_{pt} + \sum_{\tau} \sum_{r \in R} \beta_{r,\tau} \ \mathbb{1}(t = \tau, \text{Ring}_{ip} = r) + \varepsilon_{ipt} \tag{1}$$

The coefficient of interest is $\beta_{r,t}$, which measures changes in the outcome variable in the project site and the inner rings $r \in R = \{\text{Project}, 0-250\text{m}, 250-500\text{m}, 500-750\text{m}\}$ relative to the outermost ring for each year t. The inclusion of project-by-hexagon ($\alpha_{p,h(i)}$) and

21. For example, opponents to the policy took a Louisville project to court and won a decision that "*the Federal Government had no right to make use of eminent domain to acquire land for local housing projects, on the ground that housing projects did not serve a public purpose in its fullest sense*" (United States Public Works Administration, 1939). Another example: "*Many times it was a challenge just to locate owners, and even then titles were often defective. In Birmingham, Alabama, the division found that a key piece of property belonged to a branch of the Sons-and-Daughters-of-I-Will-Arise Lodge, a 200-member organization that had never been incorporated. No sale was possible without the approval of every member, and many members had moved away or simply disappeared*" (Radford, 1997).

project-by-year (κ_{pt}) fixed effects, respectively, controls for baseline levels of the outcome variable and time patterns across all rings around each project. We use corrective regression weights following Wing et al. (2024), which allow us to interpret our estimates of $\beta_{r,t}$ as the simple average treatment effect on the treated across projects, and cluster standard errors at the project level.

4.2 Control DID: Planned-But-Not-Built Projects as a Comparison Group

Our second approach compares outcomes in areas near PWA projects to areas at the same distance of other public housing projects planned but that were never built by the PWA. The identifying assumption is that areas surrounding projects built by PWA (treated) and planned-but-not-built projects (control) would have evolved similarly in the absence of any public housing construction. This assumption is grounded on the argument that the curtailment of PWA projects described in Section 3 is exogenous. Thus, control projects should provide a plausible counterfactual trajectory of the outcomes for treated projects in the post-treatment period since they were likely undergoing similar circumstances (e.g., they were either slums or in vacant land).

Using all individuals within one kilometer of treated and control projects, and the same ring definitions as before, we estimate the following stacked DID specification *separately* for the project site and each 250 meter-wide ring up to 1,000m away:

$$Y_{ipt} = \alpha_{p,h(i)} + \kappa_t + \sum_{\tau} \gamma_\tau \ \mathbb{1}(t = \tau) \times \text{Treated}_p + \varepsilon_{ipt} \tag{2}$$

where Treated_p is an indicator variable for whether the PWA-planned project was built. The coefficient γ_t measures changes in the outcome variable in the ring associated with the treated project relative to the ring associated with the control project. We include project-by-hexagon ($\alpha_{p,h(i)}$) and year (κ_t) fixed effects to control for baseline levels of the outcome variable and common time patterns in PWA and comparison projects. We also use corrective regression weights à la Wing et al. (2024) and cluster standard errors at the project level.

There are two main differences with the estimating equation in subsection 4.1. One is that Equation (2) does not control for time patterns around each project. In contrast, it relies on the assumption that areas in rings around treated and control projects are similar and follow parallel trends. While this may be true for near rings, this assumption is less likely to hold as we examine rings farther away from the projects. Another distinction is that, when there are spillovers in the outermost ring, effects in the inner rings in Equation (1) can only be interpreted as the change in the outcome variable *relative* to that outermost ring. Equation (2), however, allows us to recover the *absolute* effect of PWA construction, since comparison projects were not treated by definition.

4.3 Triple DID

The third approach combines both identification strategies above in a triple difference-in-differences design. In particular, we compare the evolution of outcomes in the inner rings to that in the outer ring for treated projects to the evolution of outcomes in the inner rings to that in the outer ring for control projects.

The identifying assumption is that, had PWA construction not taken place, the difference in outcomes between the inner and outer ring of the treated projects would have followed a parallel trend to the corresponding difference for control projects. Note that this assumption is less strict than the assumptions required for the two previous approaches. The ring DID requires parallel trends between inner and outer rings, which can be violated if the inner rings were on a different trajectory, and the control DID requires it between treated and control project, which breaks down if selection into treatment is not exactly random. The triple DID relaxes these two assumptions by differencing across the two dimensions.

Using the same dataset in the previous section, we estimate the following equation:

$$Y_{ipt} = \alpha_{p,h(i)} + \kappa_{pt} + \sum_{\tau} \sum_{r \in R} \beta_{r,\tau} \ \mathbb{1}(t = \tau, \text{Ring}_{ip}) + \sum_{\tau} \sum_{r \in R} \theta_{r,\tau} \ \mathbb{1}(t = \tau, \text{Ring}_{ip}) \times \text{Treated}_p + \varepsilon_{ipt} \tag{3}$$

The coefficient $\beta_{r,\tau}$ gives us an estimate of the common evolution of outcomes before and after 1930 for project sites and inner rings compared to the outermost ring among treated and control projects, while $\theta_{r,\tau}$ captures the additional difference in outcomes in this comparison for treated projects and thus this is the main estimate we will report below. The regression includes project-by-hexagon ($\alpha_{p,h(i)}$), project-by-year (κ_{pt}), and ring-by-year fixed effects. We use the regression weights in Section 4.1 and cluster standard errors at the project level.

5. The Short- and Medium-Run Impact of the Projects

5.1 The (Null) Average Effect of the Overall Program

We begin by examining the average effects of PWA projects on the sites where they were built, using our three identification strategies. Figure V presents the main event-study estimates for project sites under each specification described in Equations (1)–(3). Each panel corresponds to a different outcome: population, white share, Black share, and an indicator for whether the individual migrated from the South. The regressions are estimated with individual-level data. When population is the outcome, we aggregate the data to the project-by-ring-by-year level and estimate an analogous specification at this level.

The differences across strategies are stark. When the comparison group consists of the outermost ring within treated projects ("ring DID", in light gray), population in project sites increases sharply—by roughly 28 percent—immediately following construction and remains at this level thereafter. At the same time, there is clear evidence of pre-trends in racial composition: project sites had already displaced approximately 7 percentage points (11 percent) more White residents and absorbed 7 percentage points (20 percent) more Black residents by 1930, with these differences persisting in subsequent decades. In Appendix Figure B.8, which illustrates the evolution of Black and white populations in levels, we show that the increase in Black shares in 1930 comes both from the arrival of Black residents and the out-migration of white residents. Notably, a substantial fraction of the Black residents present in project sites by 1930 had migrated from the South, consistent with many cities with PWA projects being destinations of the Great Migration. The fact that Southern migration reverts

to zero after treatment suggests that many families involved in this earlier wave of migration, many of whom likely lived in the preexisting slums, did not benefit from the new projects.

These conclusions change when we instead use control projects as the relevant counterfactual (in dark gray). This comparison eliminates the pre-trends in racial composition observed under the ring DID and is consistent with the identifying assumption of quasi-random assignment of sites to treatment. Under this specification, population increases are limited to 1940 and disappear by 1950, suggesting that housing construction would have occurred at treated sites even in the absence of public housing construction by the PWA. Moreover, any potential effects on neighborhood racial composition vanish: relative to control projects, PWA projects neither reduced the White share nor increased the Black share. The absence of pre-trends in these outcomes reassures us that control projects capture areas that were a plausible counterfactual to treated sites prior to construction. In fact, Appendix Figure B.7 further reinforces this argument by showing that the evolution of Southern migration in treated sites followed parallel trends compared to control sites, particularly when separated by racial designation of the projects. Finally, since the triple DID (in blue) yields very similar results compared to the pure control DID while also further eliminating pre-trends in population and yielding smaller standard errors, we treat it as our preferred specification between the two going forward.

Based on these results, we argue that the ring DID and the triple DID provide two complementary perspectives on the impacts of PWA projects. The ring DID offers a descriptive account of how areas near the projects evolved relative to other, more distant neighborhoods within the same city. In contrast, the triple DID delivers a causal estimate of what these areas would have looked like absent PWA construction. Furthermore, this causal effect is not driven by the same type of construction in the counterfactual: we observe that only 22.7 of control sites had built public housing by 1950, after the decentralization of public housing program. The differences between the two approaches thus offer a broader cautionary lesson on ring methodologies that exploit proximity to treated areas to construct the control group, especially in settings where treatment is correlated with large population changes in the broader area just before treatment occurs.

Table II reports estimates from the ring DID specification in Equation (1) (panel A) and the triple DID specification in Equation (3) (panel B) for project sites and all inner rings. In this table, we pool all pre-treatment periods while still distinguishing between the effects in the short run (1940) and the medium run (1950).

In the short run, project sites experienced significant changes across a range of demographic and socioeconomic outcomes, a pattern that holds under both specifications. Column 1 shows that the arrival of new tenants reduced the likelihood that a household consisted of three or more families in 1940, which we interpret as a decline in overcrowding. Project sites also exhibited lower rates of male unemployment and male labor force nonparticipation (columns 4 and 5). These results are consistent with working families with stable employment replacing lower–socioeconomic-status slum dwellers. In addition, new tenants were nearly 50 percent less likely to be homeowners and paid roughly 50 percent higher rents, which aligns with the rental nature and higher quality of public housing. By 1950, however, these effects persist only when using distant areas as the comparison group; under the triple DID, all estimates shrink toward zero and become statistically insignificant—an exception is the increase in labor force nonparticipation, pointing to a potential shift in tenant selection. These findings suggest that, absent PWA construction, treated sites would have attracted populations similar to those who ultimately occupied public housing in the medium run.

In contrast, the projects generated limited effects beyond their boundaries. Consistent with patterns in project sites, the ring DID results show that neighborhoods surrounding treated sites had already attracted more Black residents migrating from the South prior to treatment. This spatial gradient mimics panel (c) of Figure V, with the largest effects within the project site and a gradual fade-out up to 750 meters away (see Appendix Figures B.9 and B.10 for the event studies of both ring and triple DID specifications). Beyond this pre-treatment pattern, the projects had few spillover effects. When using more distant neighborhoods as the comparison group, we observe a modest increase in male unemployment within 250 meters in 1940, potentially reflecting displacement of former slum residents or new incoming residents into nearby areas. Rents also increased by roughly 10 percent within the same radius, suggesting that project construction raised local demand by removing a

negative externality. As with other outcomes, these effects disappear under the triple DID, indicating that nearby areas would likely have experienced similar demographic shifts and amenity improvements even in the absence of PWA construction.

5.2 Heterogeneous Effects by the Project's Racial Designation

The limited average effects of the projects may mask substantial heterogeneity. In practice, nearly all PWA projects were racially segregated: roughly half were designated for White residents only and most of the remainder for Black residents only. As described in Section 3.3, Black-only projects were substantially more likely to replace existing slums and to be located in predominantly Black neighborhoods with high rates of Southern migration and lower socioeconomic status. These differences suggest that the impacts of public housing may have varied systematically by the race of the project.

Figure VI presents event-study estimates from the triple DID specification for project sites, estimated separately by project race and analogously to Figure V. For each project race, we restrict the control group to projects predicted to have been assigned the same race designation. Table III reports the regression results for a wider range of outcomes, reproducing Table II separately by project race and pooling all post-treatment periods for simplicity.

Focusing first on the areas where the projects were built, we find that Black-only projects led to significant changes in racial composition. As shown in Figure VI, Black-only projects did not increase population relative to control areas by 1940, suggesting that they largely replaced the population living in preexisting slums. By 1950, population slightly increases, though the estimates are imprecise and not statistically significant, indicating that Black-only projects may have helped maintain residential density in treated sites relative to control sites. Overall, Black-only projects reduced the white share by roughly 15 percentage points and increased the Black share by a similar amount. The confidence intervals are consistent with these areas becoming almost completely Black, relative to baseline levels. Notably, the share of residents migrating from the South remained stable, implying that at least some individuals from the Great Migration who already lived in those areas benefited from access to new public

housing. Table III further shows that Black residents in these projects were significantly less likely to be unemployed (column 4). While estimates for other outcomes are noisy due to the smaller sample of Black-only projects, they suggest lower levels of overcrowding (column 1) and higher rents (column 8), consistent with the PWA's public housing program.

White-only projects led to opposite effects on racial composition and slightly different demographic dynamics. Figure VI shows that White-only projects increased population by approximately 80 percent ($e^{0.6} - 1$) in 1940 relative to control sites, an effect that dissipates by 1950. This pattern aligns with White-only projects being more frequently constructed in previously vacant or underdeveloped areas than Black-only projects: treated sites were developed in the 1930s under the PWA, while the control sites for these White-only projects mostly remained vacant during the decade and experienced housing construction outside the program by 1950. Mirroring the results for Black-only projects, White-only projects increased the white share of residents by about 10 percentage points and a similar decline in the Black share, again consistent with these areas becoming fully white. In addition, these projects significantly reduced the share of Southern migrants, suggesting displacement of Black households that had arrived to these areas during the Great Migration. As shown in Table III, residents of White-only projects were significantly less likely to be homeowners and faced rents approximately twice as high as those in the control group.

In the broader neighborhoods surrounding the projects, we do not detect statistically significant effects for Black-only projects, though White-only projects seem to increase Black shares in the broader area (column 3). We further explore this result in the next section.

Several housing market responses are also suggestive. Home values rise within 250 meters of White-only projects, and rents increase within the same distance of both project types. Although these estimates are imprecise, they are consistent with nearby residents valuing the amenity improvements associated with new public housing construction. Notably, these improvements translate into statistically significant increases in home values only in the case of White-only projects, where construction displaced Black residents and turned these neighborhoods exclusively white (column 7). This pattern could motivate differential residential sorting by race around White-only projects. More broadly, it suggests that early public

housing generated positive local amenity effects, in sharp contrast to the negative spillover effects documented for later public housing (Blanco, 2023; Almagro et al., 2023).

5.3 Robustness

To assess the robustness of our findings, we study the sensitivity of our results to alternative spatial definitions and identification strategies. In particular, we test whether our conclusions depend on specific choices related to distance rings or the identification strategy.

First, we expand the spatial scope of the analysis by redefining distance rings to be 500 meters wide and extending them up to two kilometers from each project, using the area between 1.5 and 2 kilometers as the comparison group. Appendix Figures B.11 and B.12 replicate the event-study estimates for demographic outcomes in project areas, while Appendix Tables C.1 and C.2 report the corresponding estimates for effects on surrounding areas, both on average and separately by project race. The results are qualitatively similar to our baseline findings, particularly for project sites. Any estimated effects in the broader surrounding areas attenuate under this specification due to the use of wider rings.

Second, we report results based on the control DID specification in Appendix Tables C.3 and C.4. These estimates closely mirror those from the triple DID design, though they are somewhat noisier likely because of the smaller effective sample size in the control DID approach.

6. Do Racially Designated Projects Trigger Racial Sorting?

Beyond the mechanical effects of racially designated projects on neighborhood racial composition through the direct placement of same-race tenants, we next examine whether these projects triggered endogenous residential sorting responses. An established body of work documents how the Great Migration reshaped racial composition in the central core of Northern cities and contributed to residential sorting and suburbanization, particularly through "white flight" from central cities in response to the arrival of large Black populations (e.g., Boustan, 2010; Shertzer and Walsh, 2019). Within this context, PWA projects provide

a sudden, highly localized policy intervention that permanently placed large numbers of same-race households in specific neighborhoods, making them a natural setting in which to study neighborhood-level responses to the arrival of same- versus opposite-race residents.

We hypothesize that racially designated PWA projects may have triggered two distinct types of sorting responses. On the one hand, Black-only projects, by enabling Black residents of preexisting slums to remain in central-city neighborhoods, may have led to higher out-migration of white residents around these sites. On the other hand, White-only projects may have altered neighborhood trajectories by displacing the remaining Black residents and discouraging their subsequent in-migration. Understanding how nearby residents responded to nearby public housing construction helps clarify whether these projects amplified or mitigated contemporaneous patterns of racial sorting.

To study this question, we focus on the mobility rates of incumbent residents living near PWA projects prior to their construction. We construct a panel of residents who lived within one kilometer of a project site in 1930 and who we successfully link to the 1940 (71% match rate) or to 1950 (40% match rate) Censuses. For each decade, we construct an indicator variable for whether an incumbent resident moved out of their original 1930 hexagon to a location farther from the project site. Move-out rates are high among this population of incumbent residents: between 1930 and 1940, 68 percent of incumbent residents had moved out from their original hexagon.

Table IV reports estimates from pooled versions of the triple DID specification with move-out rates as the outcome, separately pooling pre- and post-treatment periods. We augment Equation (3) by interacting the main coefficient of interest with an indicator for whether the project is White-only, excluding project sites from the analysis. All fixed effects are similarly interacted with the White-only indicator. Columns (1)–(3) estimate the specification for the sample of all White residents, native-born White residents, and immigrant White residents, respectively; Columns (4)–(6) do so for all Black residents, non-Southern Black residents, and Southern Black migrants. This design allows us to test whether an individual's out-migration response depends on whether their race concords with the project's racial designation.

We find little evidence that race concordance with the project affected out-migration

patterns relative to control projects. White incumbent residents were no more likely to leave when the nearby project was designated for the opposite race (Column 1), and this null holds across nativity groups—if anything, immigrant White incumbents were less likely to leave neighborhoods near Black-only projects (Column 3). Similarly, Black residents did not exhibit significantly higher out-migration near White-only projects (Column 4). The point estimates in Column (5) suggest that non-Southern Black incumbents may have been displaced by White-only projects, but these estimates are imprecise given the small number of Black residents living near White-only project sites and should be interpreted with caution. Importantly, even this suggestive displacement does not translate into a corresponding increase in the White population nearby. As shown in Panel B of Table III, areas within 250 meters of White-only projects experience no significant change in racial composition, while areas between 250 and 750 meters exhibit an increase in the Black population of roughly 2.5 percentage points—about one-third relative to baseline.

Overall, we find no evidence that PWA projects causally contributed to white flight in the medium run, and limited evidence of Black displacement due to the construction of White-only projects. While Black-only projects had no effects on sorting patterns among incumbent residents, White-only projects displaced a small number of nearby Black residents without increasing retention among white incumbent residents. Together, these findings suggest that racially designated public housing interacted did not fundamentally alter neighborhood racial trajectories.

7. Conclusion

This paper studies the effects of the first federally built public housing projects on racial residential segregation during a period when segregation was rapidly intensifying in U.S. cities. Using newly assembled data on racially designated PWA projects and a novel comparison group of planned-but-not-built sites, combined with address-level Census data from 1910 to 1950, we estimate difference-in-differences designs that isolate the causal effects of early public housing on neighborhood composition and housing outcomes.

We find that PWA projects reshaped the racial composition of the project sites themselves but had limited effects on surrounding neighborhoods relative to control projects. Black-designated projects reinforced the concentration of Black population in the project sites themselves, primarily through modest white out-migration, while White-designated projects also displaced Black residents previously living in these areas. However, these effects did not translate into large or persistent changes in neighborhood racial composition beyond the immediate vicinity of the projects. When compared to planned-but-not-built sites, neighborhoods near PWA projects followed similar racial trajectories. One exception is housing values, which increased near White-designated projects but not near Black-designated projects, despite similar project quality.

Taken together, these results suggest that the explicit of early federal public housing did not play a substantial role in reinforcing racial residential segregation—except in the project sites themselves—beyond what would have occurred in its absence. More broadly, our findings imply that public housing construction alone may not be a sufficiently powerful policy tool to redirect neighborhood trajectories, especially at a time when segregation was being driven by strong residential sorting forces and discriminatory institutions operating at much larger scales.

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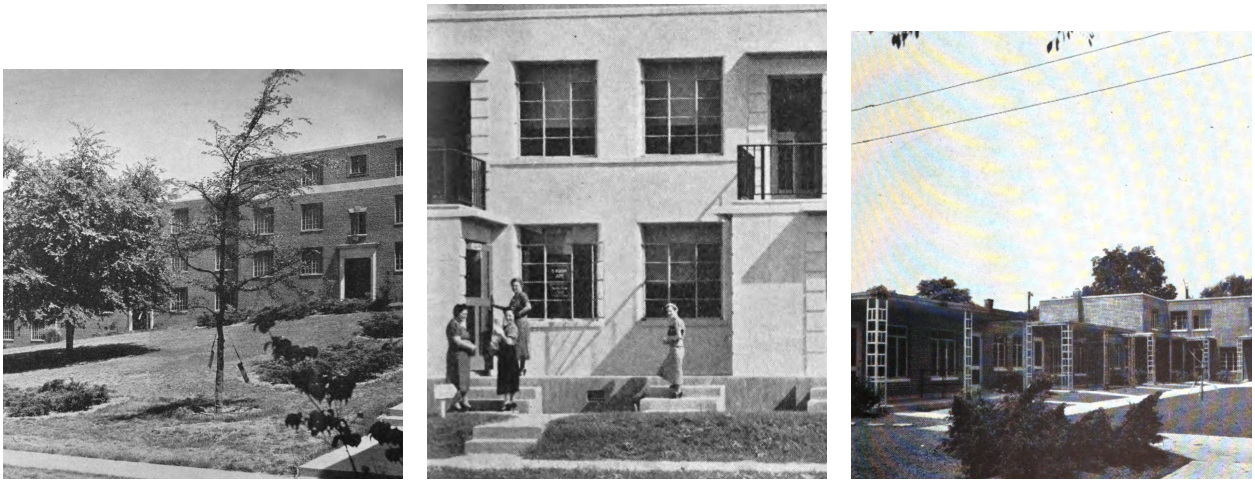
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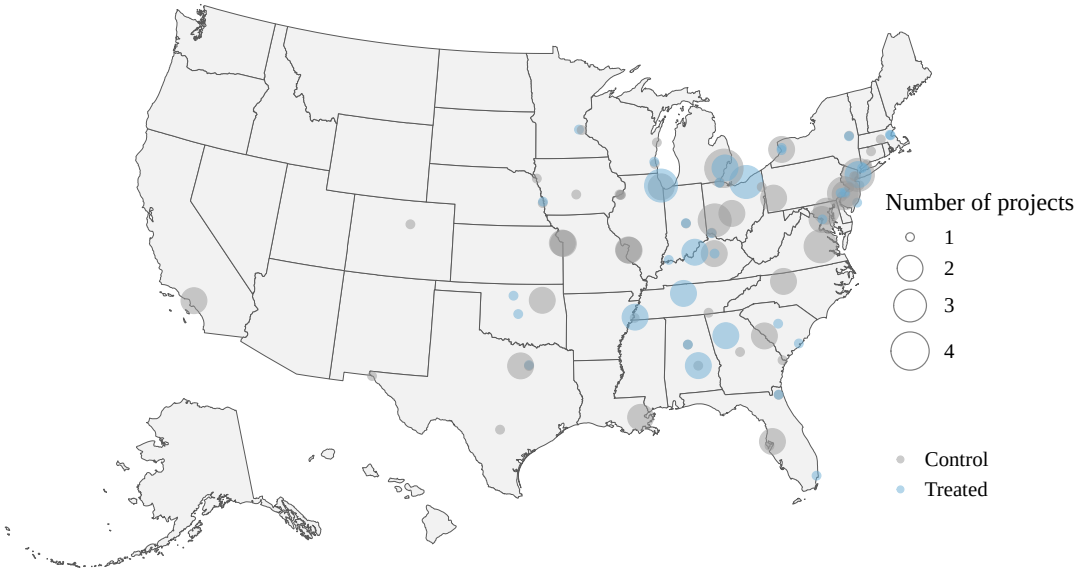
Figures and Tables

Figure I: Examples of realized PWA Public Housing Projects



(a) Atlanta GA **(b)** Dallas TX **(c)** Montgomery AL

Figure II: Map of built and planned-but-not-built projects by the PWA

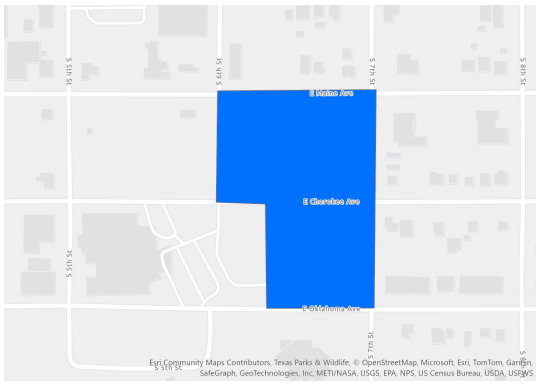


Note: This map illustrates the spatial distribution of the 47 public housing projects constructed by the Public Works Administration (PWA, shown in blue) and the 88 projects that were planned by the PWA but never built for which we obtained precise addresses (shown in gray). Bubble size indicates the number of projects in each city. This information was collected from archival sources (PWA - Housing Division, 1933 -), as indicated in Section 3.

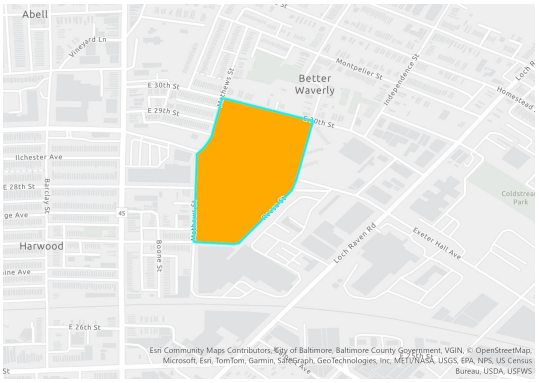
Figure III: Examples of Digitized PWA Projects' Shapes



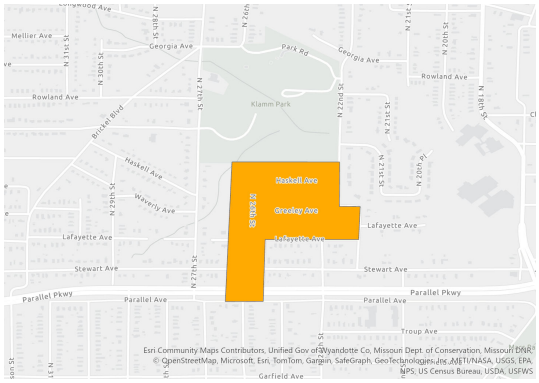
(a) Trumbull Park, Chicago IL



(b) Cherokee Terrace, Enid OK



(c) Baltimore, MD



(d) Kansas City, KS

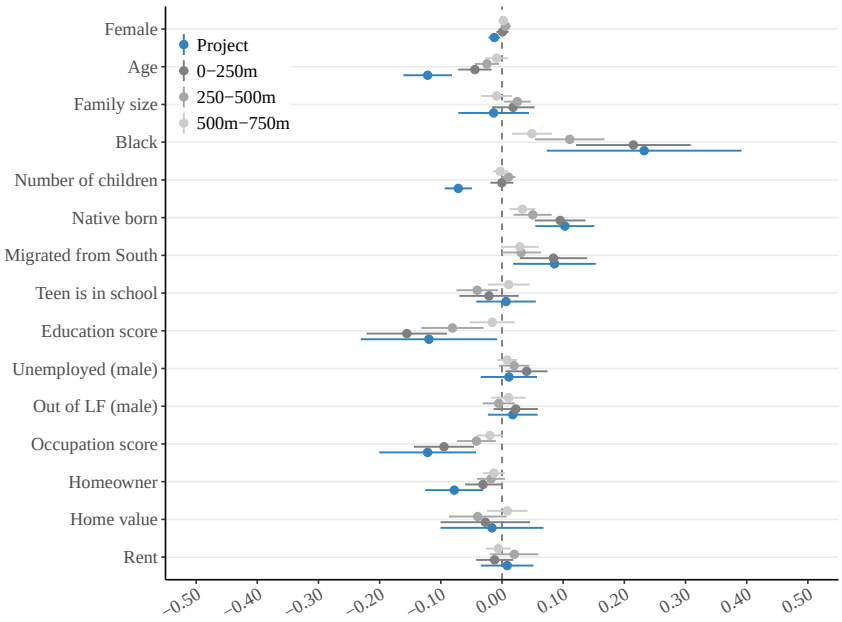
Note: The figure shows four examples of digitized shapes for PWA public-housing project sites. Blue shapes represent completed developments, while orange shapes indicate planned-but-not-built sites. Geographic information for control sites was collected from archival sources (PWA - Housing Division, 1933 -), as described in Section 3.

Table I: Summary statistics of PWA projects and control projects

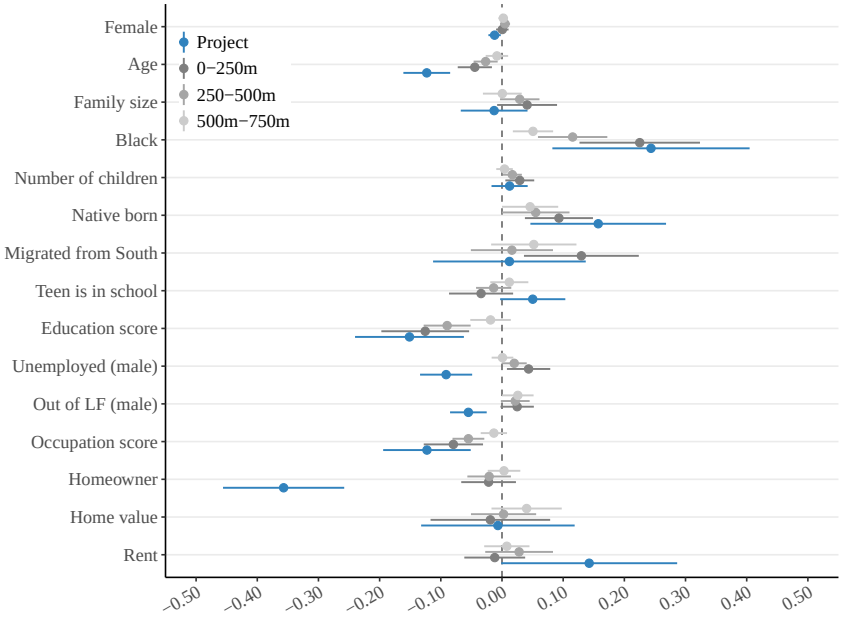
	Treated				Control	
	All projects	Black	White	Rest of county	All projects	Rest of county
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Project variables						
Units	456.1	390.9	516.1			
Number of hexagons	11.0	10.1	10.7		10.1	
Land area, acres	31.4	27.9	30.4		27.1	
Black	0.41	1.00	0.00		0.46	
Slum	0.54	0.71	0.39		0.44	
Funding, millions	2.83	2.15	3.34			
Panel B: Demographic variables 1930						
Population, per hexagon	138	175	111	117	123	115
Female	0.500	0.517	0.485	0.511	0.505	0.509
Family size	4.4	4.1	4.6	4.3	4.5	4.3
Age	28.2	28.7	27.9	29.5	28.6	29.6
White	0.64	0.31	0.90	0.85	0.65	0.86
Black	0.35	0.69	0.09	0.14	0.32	0.13
Hispanic	0.010	0.003	0.017	0.004	0.031	0.022
Native born	0.67	0.85	0.53	0.61	0.64	0.62
Migrated from South	0.14	0.25	0.05	0.05	0.11	0.05
Panel C: Socioeconomic variables 1930						
Education score	10.7	10.2	11.2	15.5	10.4	15.3
Occupation score	22.4	19.3	25.0	25.0	22.5	25.1
Male unemployment	0.12	0.12	0.12	0.10	0.11	0.10
Homeowner	0.29	0.26	0.33	0.42	0.36	0.44
Rent	33	42	29	54	61	53
Home value	5,463	5,788	5,759	6,968	5,313	6,557
Number of projects/counties	47	17	24	36	88	58

Note: The table reports sample means for several variables across the following samples, in order: all PWA projects; Black-only PWA projects; White-only PWA projects; the remainder of the county in which PWA projects are located; all control projects that were planned by the PWA but not constructed; and the remainder of the county in which control projects are located. Panel A reports project characteristics, with data obtained from PWA - Housing Division (1933 -). For control projects, the reported averages for Black-only and slum designations are computed using projects with non-missing information. Panels B and C report Census characteristics in 1930. In columns corresponding to projects, values are computed as the mean across all households residing within 250 meters of the project site; in columns corresponding to the rest of the county, values are computed as the mean across hexagons within the relevant county.

Figure IV: Characteristics of residents moving to project sites in 1940



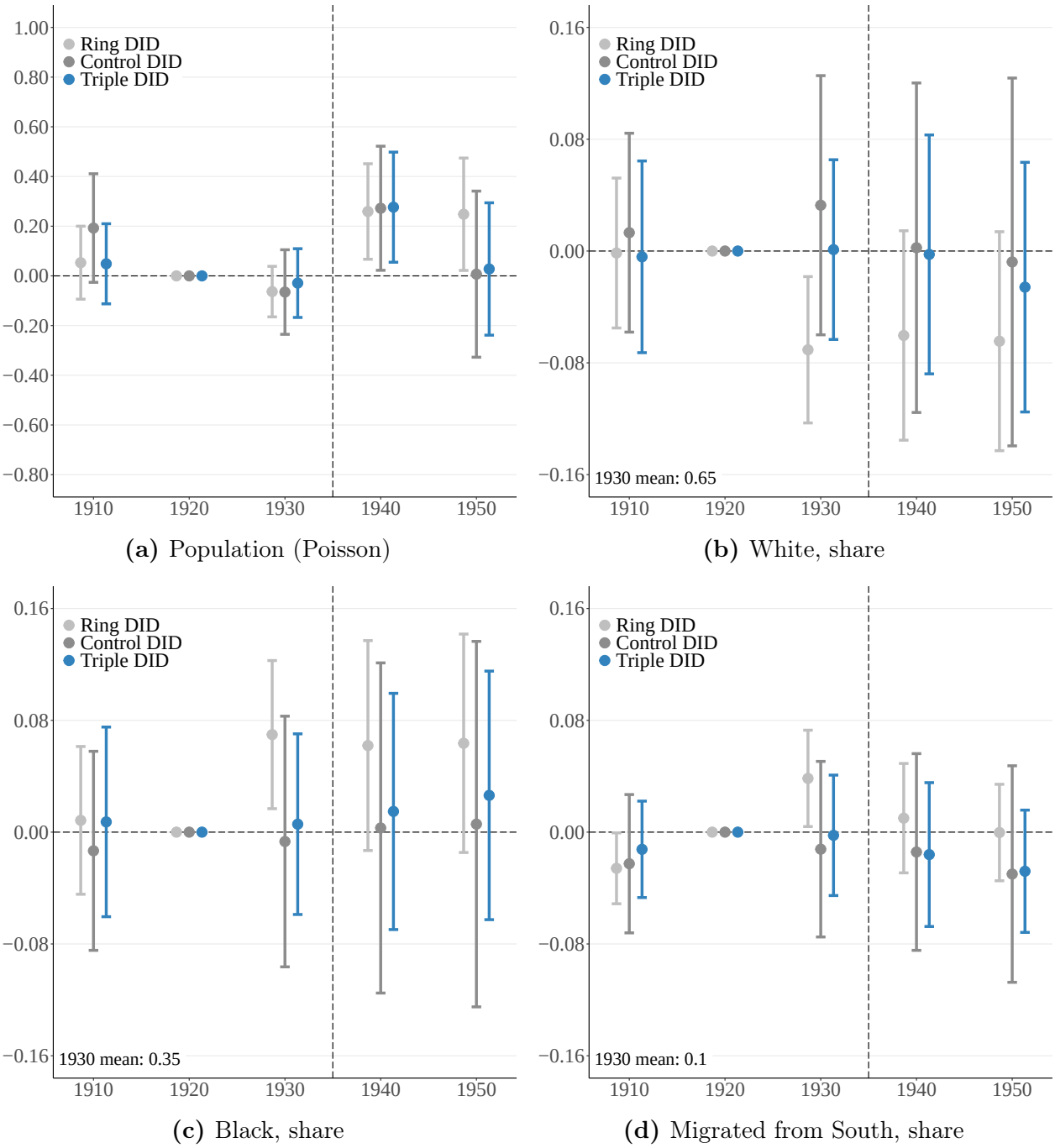
(a) 1930 characteristics



(b) 1940 characteristics

Note: Each panel reports regression estimates for multiple outcomes comparing residents who newly moved into areas near PWA projects with residents who newly moved into areas located 750–1,000 meters from the projects in 1940. We regress each outcome on a set of indicator variables for each distance ring (project site, 0–250 m, 250–500 m, and 500–750 m) and include project fixed effects. Dots denote point estimates and horizontal bars indicate 95 percent confidence intervals. Standard errors are clustered at the project level. Panel (a) uses residents' characteristics measured in 1930, prior to moving, while panel (b) uses characteristics measured in 1940.

Figure V: The impact of the projects on the project site by identification strategy



(a) Population (Poisson)

(b) White, share

(c) Black, share

(d) Migrated from South, share

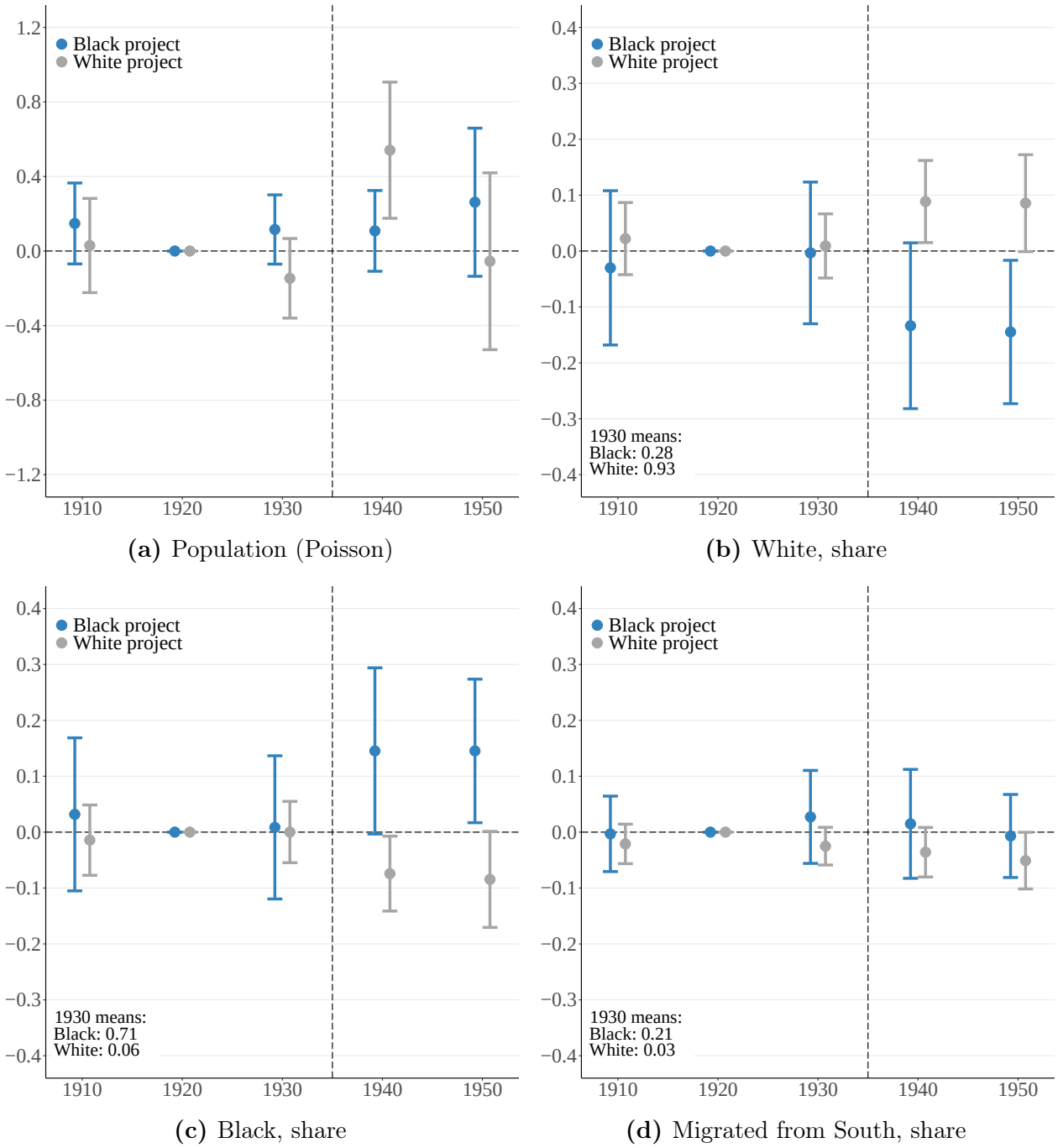
Note: Each panel reports estimates of $\beta_{r,\tau}$, γ_τ, and $\theta_{r,\tau}$ from Equations (1), (2), and (3), respectively, along with 95 percent confidence intervals. All estimates correspond to the project site (r = Project site), and each color denotes a different identification strategy. Standard errors are clustered at the project level. Panel (a) reports estimates from Poisson specifications using population as the outcome and a modified dataset collapsed to the project–ring–year level. Panels (b), (c), and (d) use indicators for White race, Black race, and migration from the South as outcomes. In the bottom-left corner of each plot, we report the 1930 sample mean of the outcome in the project site.

Table II: The impact of the projects on the surrounding area by identification strategy

	Multi-family	Migrated from South	Black	Unemployed, male	Out of labor force, male	Home-owner	Home value, log	Rent, log
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Ring DID								
1940: Project	-0.027*	0.0004	0.038	-0.037***	-0.018***	-0.105***	0.160	0.509***
	(0.014)	(0.016)	(0.035)	(0.010)	(0.006)	(0.022)	(0.100)	(0.124)
1940: 0-250m	0.011	0.033***	0.054***	0.013**	0.004	0.012	0.011	0.101**
	(0.011)	(0.011)	(0.019)	(0.005)	(0.006)	(0.010)	(0.039)	(0.040)
1940: 250-500m	-0.004	0.016**	0.019*	0.004	0.003	0.006	0.006	0.042
	(0.009)	(0.006)	(0.011)	(0.004)	(0.003)	(0.007)	(0.030)	(0.033)
1940: 500-750m	0.005	0.011**	0.008	-0.001	0.007*	0.007	-0.007	-0.015
	(0.005)	(0.004)	(0.006)	(0.004)	(0.004)	(0.007)	(0.023)	(0.018)
1950: Project	-0.068	-0.009	0.040	-0.015**	0.040***			
	(0.045)	(0.015)	(0.036)	(0.007)	(0.014)			
1950: 0-250m	-0.037*	0.022**	0.070***	0.0003	-0.001			
	(0.020)	(0.009)	(0.020)	(0.004)	(0.007)			
1950: 250-500m	-0.012	0.014**	0.038***	-0.005	0.005			
	(0.016)	(0.006)	(0.012)	(0.004)	(0.006)			
1950: 500-750m	-0.012	0.011**	0.021**	-0.002	-0.001			
	(0.018)	(0.004)	(0.008)	(0.003)	(0.003)			
Y mean (Project)	0.140	0.172	0.297	0.115	0.094	0.183	5,749.1	28.7
Y mean (0-750m)	0.146	0.116	0.203	0.090	0.102	0.216	6,425.0	46.3
Observations	3,808,695	2,487,635	4,444,181	1,155,839	1,445,289	3,532,221	410,406	1,406,313
R^2	0.214	0.399	0.610	0.067	0.113	0.234	0.522	0.438
Panel B: Triple DID								
1940: Project	-0.043**	-0.013	0.014	-0.032**	-0.033***	-0.109***	0.151	0.454***
	(0.017)	(0.022)	(0.038)	(0.013)	(0.010)	(0.029)	(0.115)	(0.139)
1940: 0-250m	0.007	0.014	0.013	0.004	0.0010	0.001	0.028	0.022
	(0.012)	(0.013)	(0.022)	(0.007)	(0.007)	(0.012)	(0.049)	(0.063)
1940: 250-500m	-0.015	0.005	0.002	0.008	0.005	0.006	0.024	-0.024
	(0.010)	(0.009)	(0.015)	(0.006)	(0.005)	(0.009)	(0.038)	(0.042)
1940: 500-750m	0.003	0.008	0.009	0.0004	0.006	0.014*	-0.010	-0.040*
	(0.006)	(0.005)	(0.010)	(0.005)	(0.004)	(0.008)	(0.029)	(0.024)
1950: Project	0.021	-0.025	0.026	-0.005	0.041**			
	(0.058)	(0.019)	(0.041)	(0.011)	(0.018)			
1950: 0-250m	-0.025	0.003	0.026	-0.003	0.006			
	(0.026)	(0.013)	(0.026)	(0.006)	(0.009)			
1950: 250-500m	0.023	0.004	0.008	-0.005	0.018**			
	(0.023)	(0.009)	(0.018)	(0.005)	(0.008)			
1950: 500-750m	-0.016	0.007	0.013	0.001	0.002			
	(0.021)	(0.005)	(0.011)	(0.004)	(0.006)			
Y mean (Project)	0.140	0.172	0.297	0.115	0.094	0.183	5,749.1	28.7
Y mean (0-750m)	0.146	0.116	0.203	0.090	0.102	0.216	6,425.0	46.3
Observations	10,029,390	6,517,843	11,807,838	3,021,686	3,781,417	9,366,360	1,240,306	3,676,741
R^2	0.211	0.362	0.645	0.067	0.117	0.237	0.543	0.441

Note: The table reports estimates of $\beta_{r,\tau}$ (Panel A) and $\theta_{r,\tau}$ (Panel B) from a version of Equations (1) and (3), respectively, that pools all pre-treatment periods. Each column uses a different outcome, in order: an indicator for whether three or more families live in the same unit, migration from the South, Black, male unemployment, male being out of the labor force, homeownership, the logarithm of the home value, and the logarithm of rent. At the bottom of each panel, we report the mean of the outcome variable in the pre-treatment period in the project site and across inner rings. Standard errors, clustered at the project level, are reported in parenthesis. Significance codes: ***: 0.01, **: 0.05, *: 0.1.

Figure VI: The impact of the projects on the project site by race under the triple DID



(a) Population (Poisson)

(b) White, share

(c) Black, share

(d) Migrated from South, share

Note: Each panel reports estimates of $\theta_{r,\tau}$ from Equation (3), along with 95 percent confidence intervals. All estimates correspond to the project site (r = Project site), and colors distinguish between Black-designated (blue) and White-designated (gray) projects. Standard errors are clustered at the project level. Panel (a) reports estimates from Poisson specifications using population as the outcome and a modified dataset collapsed to the project–ring–year level. Panels (b), (c), and (d) use indicators for White race, Black race, and migration from the South as outcomes. In the bottom-left corner of each plot, we report the 1930 sample mean of the outcome in the project site by race designation.

Table III: Triple DID: The impact of the projects on the surrounding area by project race

	Multi-family	Migrated from South	Black	Unemployed, male	Out of labor force, male	Home-owner	Home value, log	Rent, log
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Black-only projects								
Project	-0.049	-0.013	0.144**	-0.036**	0.005	-0.052	-0.067	0.174
	(0.032)	(0.030)	(0.058)	(0.016)	(0.017)	(0.036)	(0.202)	(0.129)
0-250m	0.026	-0.009	-0.008	-0.008	-0.0002	0.006	-0.020	0.054
	(0.021)	(0.021)	(0.040)	(0.009)	(0.009)	(0.022)	(0.078)	(0.107)
250-500m	-0.007	-0.002	-0.023	0.003	0.003	0.018	0.079	0.018
	(0.014)	(0.013)	(0.026)	(0.006)	(0.008)	(0.013)	(0.069)	(0.046)
500-750m	-0.0003	-0.009	-0.011	-0.0009	0.003	0.009	-0.0009	-0.017
	(0.012)	(0.007)	(0.016)	(0.006)	(0.006)	(0.011)	(0.047)	(0.029)
Y mean (Project)	0.183	0.268	0.451	0.134	0.087	0.206	5,617.2	33.1
Y mean (0-750m)	0.183	0.187	0.354	0.096	0.094	0.192	6,601.3	47.4
Observations	4,716,414	3,073,601	5,450,710	1,396,108	1,763,446	4,320,751	470,907	1,780,066
R^2	0.197	0.413	0.605	0.065	0.103	0.205	0.559	0.468
Panel B: White-only projects								
Project	-0.032	-0.041*	-0.080**	-0.007	0.007	-0.163***	0.375**	0.702***
	(0.021)	(0.023)	(0.034)	(0.015)	(0.017)	(0.048)	(0.147)	(0.250)
0-250m	-0.014	0.002	0.018	0.007	0.002	-0.0007	0.119*	0.058
	(0.015)	(0.011)	(0.016)	(0.008)	(0.008)	(0.017)	(0.069)	(0.081)
250-500m	-0.020	0.002	0.028**	0.002	0.016**	-0.007	0.027	-0.076
	(0.015)	(0.008)	(0.012)	(0.007)	(0.008)	(0.014)	(0.047)	(0.068)
500-750m	-0.003	0.016**	0.025***	0.002	0.007	0.018	0.007	-0.051
	(0.009)	(0.007)	(0.009)	(0.005)	(0.005)	(0.014)	(0.041)	(0.038)
Y mean (Project)	0.124	0.057	0.177	0.107	0.104	0.150	7,437.7	25.5
Y mean (0-750m)	0.125	0.040	0.073	0.088	0.110	0.242	6,775.3	45.1
Observations	4,668,413	3,009,811	5,594,833	1,437,896	1,774,783	4,438,618	710,776	1,656,338
R^2	0.217	0.226	0.463	0.069	0.132	0.246	0.520	0.407

Note: The table reports estimates of $\theta_{r,\tau}$ from a version of Equation (3) that pools all pre-treatment periods and all post-treatment periods. Panel A reports estimates for Black-only projects, and Panel B for White-only projects; in each case, the control group includes only control projects predicted to have the same racial designation. Each column uses a different outcome, in order: an indicator for whether three or more families live in the same unit, migration from the South, Black, male unemployment, male being out of the labor force, homeownership, the logarithm of the home value, and the logarithm of rent. At the bottom of each panel, we report the mean of the outcome variable in the pre-treatment period in the project site and across inner rings. Standard errors, clustered at the project level, are reported in parenthesis. Significance codes: ***: 0.01, **: 0.05, *: 0.1.

Table IV: Triple DID: The impact of the projects on move-out rates by project race concordance

	White residents			Black residents		
	All	Native	Immigrant	All	Non-South	From South
	(1)	(2)	(3)	(4)	(5)	(6)
0-250m	0.003	-0.010	-0.012	-0.024	-0.044	0.064
	(0.025)	(0.025)	(0.038)	(0.031)	(0.034)	(0.052)
250-500m	-0.022	-0.013	-0.048*	-0.010	-0.038	0.006
	(0.021)	(0.021)	(0.028)	(0.027)	(0.029)	(0.051)
500-750m	-0.015	-0.011	-0.034	-0.009	-0.024	0.015
	(0.015)	(0.015)	(0.024)	(0.023)	(0.025)	(0.034)
0-250m×White-only	0.0010	0.015	0.031	0.089	0.120*	-0.022
	(0.033)	(0.034)	(0.047)	(0.067)	(0.070)	(0.097)
250-500m×White-only	0.023	0.019	0.042	0.078	0.114*	-0.006
	(0.026)	(0.027)	(0.036)	(0.066)	(0.065)	(0.103)
500-750m×White-only	-0.010	0.010	-0.003	0.047	0.099*	0.013
	(0.021)	(0.024)	(0.032)	(0.055)	(0.055)	(0.080)
Y mean (1940, control)	0.724	0.745	0.701	0.671	0.646	0.691
Observations	1,866,481	766,200	1,068,514	381,181	209,603	166,298
R^2	0.589	0.636	0.546	0.602	0.593	0.650

Note: The table reports estimates from a pooled version of Equation (3) using move-out rates as the outcome. Pre-treatment and post-treatment periods are separately pooled, and project sites are excluded from the sample. Each specification augments the baseline equation by interacting the main coefficient of interest with an indicator for whether the project is White-only; all fixed effects are similarly interacted. The first three rows report coefficients on the interaction of ring, post-treatment, and treated indicators; the next three rows report the additional interaction with the White-only project indicator. Columns (1)–(3) restrict the sample to all White, native-born White, and immigrant White residents, respectively; Columns (4)–(6) restrict to all Black, non-Southern Black, and Southern Black migrant residents. At the bottom of the table, we report mean move-out rates in 1940 among incumbent residents in the control group. Standard errors, clustered at the project level, are reported in parentheses. Significance codes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A. Data and Sample Construction

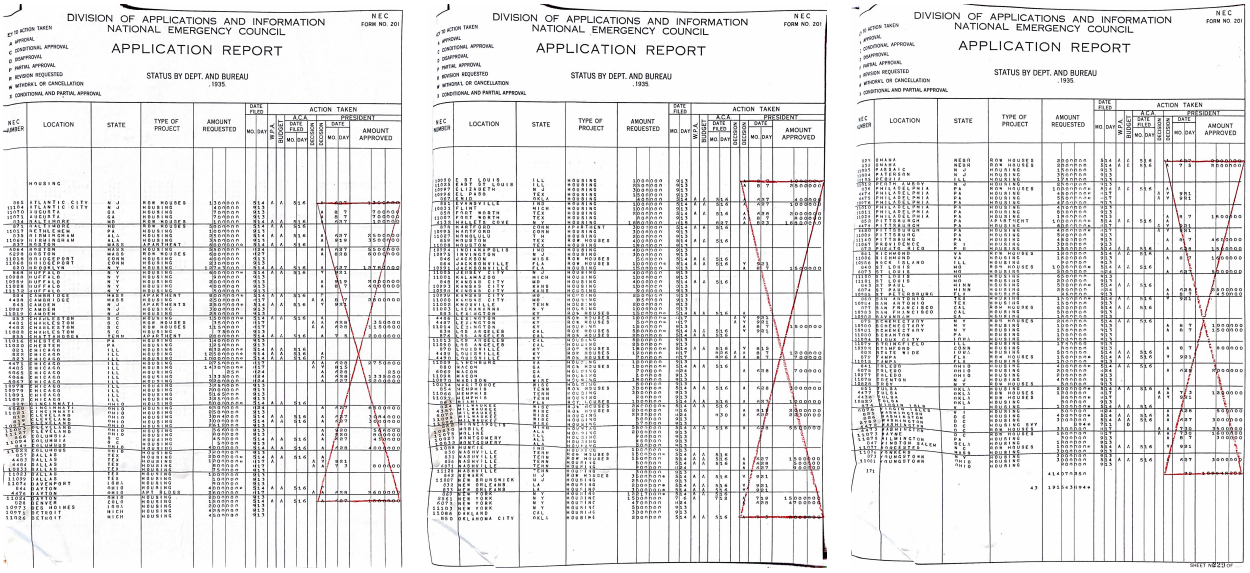
Table A.1: PWA Direct-Built Projects.

City	Name	Race	Site
Atlanta, GA	Techwood Homes	White	Slum
Atlanta, GA	University Homes	Black	Slum
Atlantic City, NJ	Smithfield Court	Black	Slum
Birmingham, AL	Stanley S. Holmes Village	Black	Slum
Boston, MA	Old Harbor Village	White	Vacant
Buffalo, NY	Kenfield	White	Vacant
Cambridge, MA	New Towne Court	White	Slum
Camden, NJ	Westfield Acres	White	Vacant
Charleston, SC	Meeting St. Manor/Cooper River Court	Mixed	Vacant
Chicago, IL	Jane Addams Houses	White	Slum
Chicago, IL	Julia C. Lathrop Homes	White	Vacant
Chicago, IL	Trumbull Park Homes	White	Vacant
Cincinnati, OH	Laurel Homes	Mixed	Slum
Cleveland, OH	Cedar-Central Apartments	White	Slum
Cleveland, OH	Outhwaite Homes	Black	Slum
Cleveland, OH	Lakeview Terrace	White	Slum
Columbia, SC	University Terrace	Mixed	Slum
Dallas, TX	Cedar Springs Place	White	Vacant
Detroit, MI	Brewster	Black	Slum
Detroit, MI	Parkside	White	Vacant
Enid, OK	Cherokee Terrace	White	Slum
Evansville, IN	Lincoln Gardens	Black	Slum
Indianapolis, IN	Lockfield Garden Apartments	Black	Slum
Jacksonville, FL	Durkeeville	Black	Vacant
Lackawanna, NY	Baker Homes	White	Vacant
Lexington, KY	Blue Grass Park/Aspen Dale	Mixed	Vacant
Louisville, KY	LaSalle Place	White	Vacant
Louisville, KY	College Court	Black	Vacant
Memphis, TN	Dixie Homes	Black	Slum
Memphis, TN	Lauderdale Courts	White	Slum

City	Name	Race	Site
Miami, FL	Liberty Square	Black	Vacant
Milwaukee, WI	Parklawn	White	Vacant
Minneapolis, MN	Sumner Field Homes	Mixed	Slum
Montgomery, AL	Riverside Heights	White	Vacant
Montgomery, AL	William B. Patterson Courts	Black	Slum
Nashville, TN	Cheatham Place	White	Slum
Nashville, TN	Andrew Jackson Courts	Black	Slum
New York, NY	Williamsburg Houses	White	Slum
New York, NY	Harlem River Houses	Black	Vacant
Oklah. City, OK	Will Rogers Courts	White	Vacant
Omaha, NE	Logan Fontenelle Homes	Mixed	Slum
Philadelphia, PA	Hill Creek	White	Vacant
Schenectady, NY	Schonowee Village	White	Slum
Stamford, CT	Fairfield Court	White	Vacant
Toledo, OH	Brand Whitlock Homes	Black	Slum
Washington, DC	Langston	Black	Vacant
Wayne, PA	Highland Homes	Black	Slum

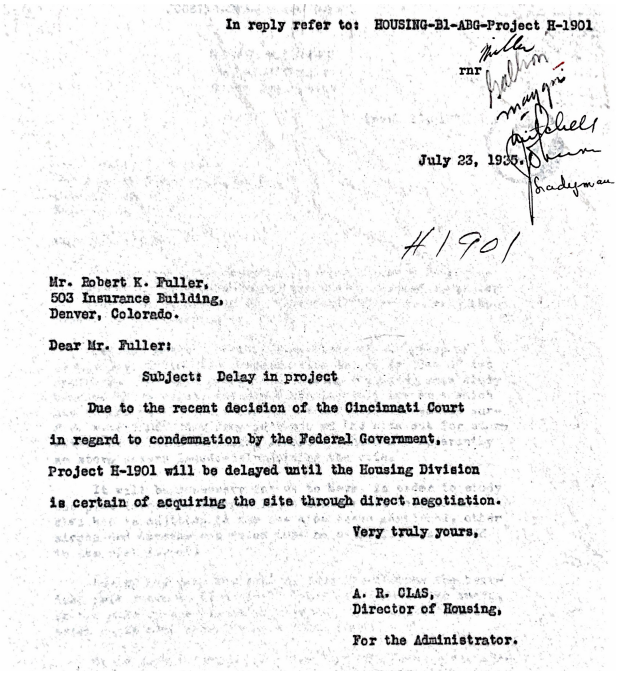
B. Appendix Figures

Figure B.1: Examples of realized PWA Public Housing Projects



Note: The figure contains the National Emergency Council report on applications submitted by the Housing Division of the Public Works Administration. This is the last report, labelled as "*Complete and Final*" in a series available at the national archives. Any number in the report relating to the housing program has been crossed out with red pencil. The documents were collected at the National Archives (National Emergency Council, 1935).

Figure B.2: Examples of Curtailment Letters from the PWA Housing Division Director.



In reply refer to: HOUSING-B1-ABG-Project H-1901

rnr

July 23, 1935.

H1901

Mr. Robert K. Fuller,
503 Insurance Building,
Denver, Colorado.

Dear Mr. Fuller:

Subject: Delay in project

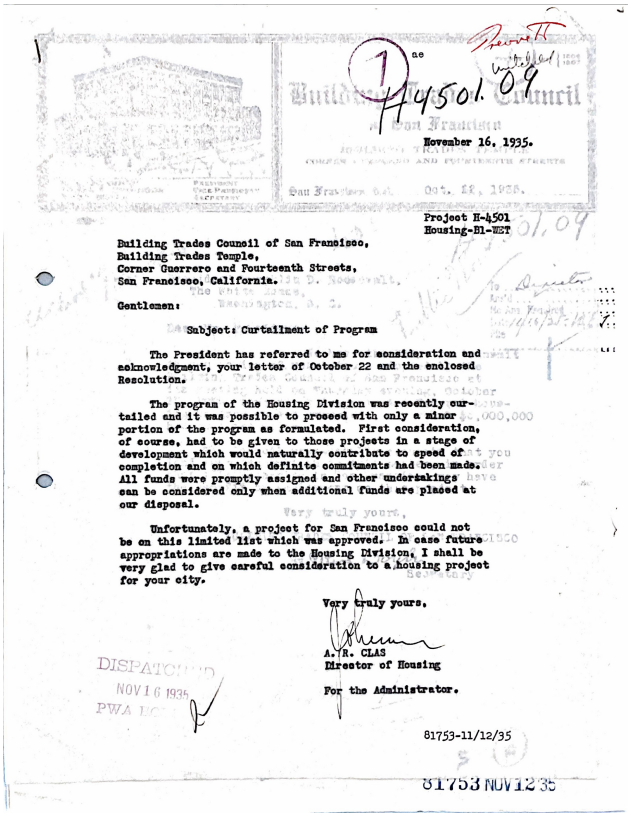
Due to the recent decision of the Cincinnati Court in regard to condemnation by the Federal Government, Project H-1901 will be delayed until the Housing Division is certain of acquiring the site through direct negotiation.

Very truly yours,

A. R. CLAS,
Director of Housing,

For the Administrator.

(a) Project H1901, Denver, CO



November 16, 1935.

Project H-4501
Housing-B1-MET

Building Trades Council of San Francisco,
Building Trades Temple,
Corner Guerrero and Fourteenth Streets,
San Francisco, California.

Gentlemen:

Subject: Curtailment of Program

The President has referred to me for consideration and acknowledgment, your letter of October 22 and the enclosed Resolution.

The program of the Housing Division was recently curtailed and it was possible to proceed with only a minor portion of the program as formulated. First consideration, of course, had to be given to those projects in a stage of development which would naturally contribute to speed of completion and on which definite commitments had been made. All funds were promptly assigned and other undertakings can be considered only when additional funds are placed at our disposal.

Unfortunately, a project for San Francisco could not be on this limited list which was approved. In case future appropriations are made to the Housing Division, I shall be very glad to give careful consideration to a housing project for your city.

Very truly yours,

A. R. CLAS
Director of Housing

For the Administrator.

DISPATCHED NOV 16 1935 PWA

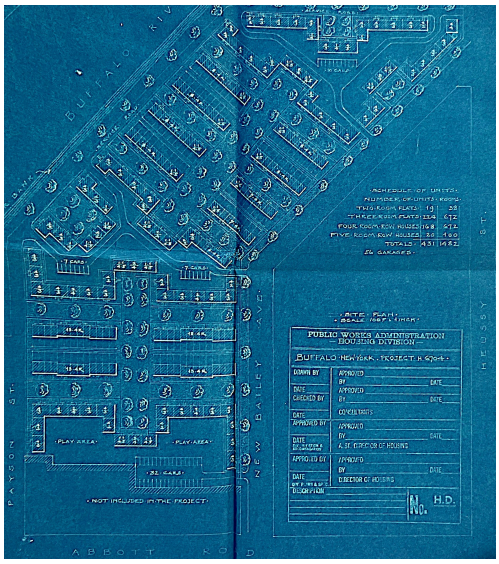
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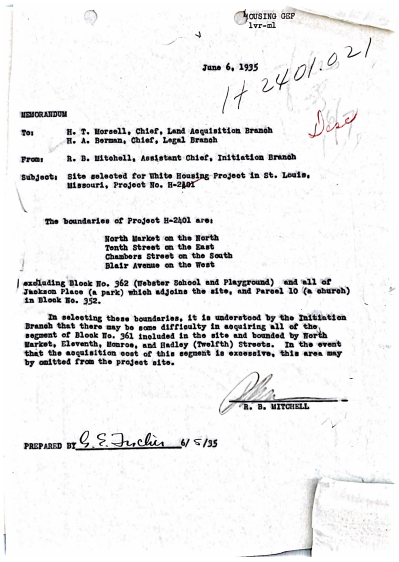
(b) Project H4501, San Francisco, CA

Note: The figure shows two examples of letters sent by the director of the PWA Housing Division, A.R. Clas, to local committees involved in planning federal public housing. Panel (a) provides an example of a curtailment prompted by judicial decisions restricting the use of eminent domain. Panel (b) shows the template used to announce curtailments due to budget cuts; we found letters using this template for several projects in our control group. The documents were collected by the authors at the National Archives (PWA - Housing Division, 1933 -).

Figure B.3: Examples of Archival Sources for the Locations of Cancelled PWA Projects.



(a) Project H6704, Buffalo, NY



HOUSING OGF
1vr-m1

June 6, 1935

1t 2401.021

MEMORANDUM

To: H. T. Morsell, Chief, Land Acquisition Branch
H. A. Berman, Chief, Legal Branch

From: R. B. Mitchell, Assistant Chief, Initiation Branch

Subject: Site selected for White Housing Project in St. Louis, Missouri, Project No. H-2401

The boundaries of Project H-2401 are:

North Market on the North
Tenth Street on the East
Chambers Street on the South
Blair Avenue on the West

excluding Block No. 362 (Webster School and Playground) and all of Jackson Place (a park) which adjoins the site, and Parcel 10 (a church) in Block No. 352.

In selecting these boundaries, it is understood by the Initiation Branch that there may be some difficulty in acquiring all of the segment of Block No. 361 included in the site and bounded by North Market, Eleventh, Monroe, and Hadley (Twelfth) Streets. In the event that the acquisition cost of this segment is excessive, this area may be omitted from the project site.

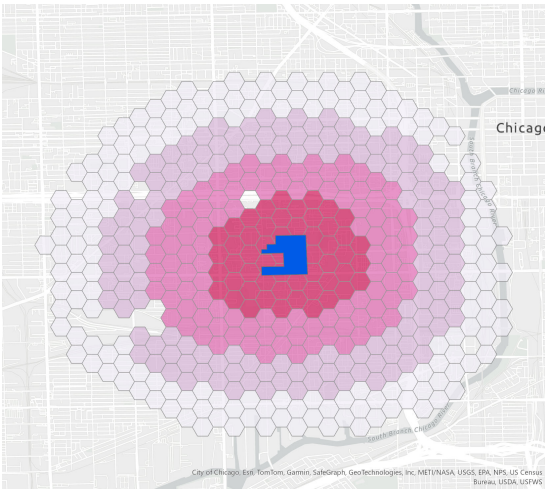
R. B. MITCHELL

PREPARED BY C. E. Jr. Chis 6/5/35

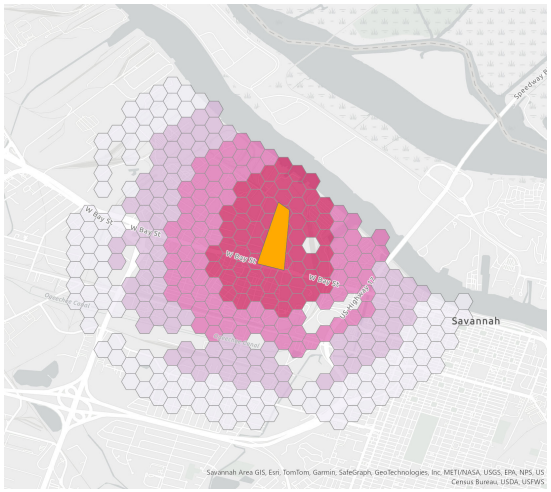
(b) Project H2401, St. Louis, MO

Note: The figure shows two examples of archival sources used to locate the intended sites of planned developments. Panel (a) shows a map for a project under development in Buffalo, New York. Panel (b) shows an internal PWA memo detailing the street boundaries of the site selected for a development in St. Louis that was ultimately not built. The documents were collected by the authors at the National Archives (PWA - Housing Division, 1933 -).

Figure B.4: Geography-Dependent Definition of Catchment Areas



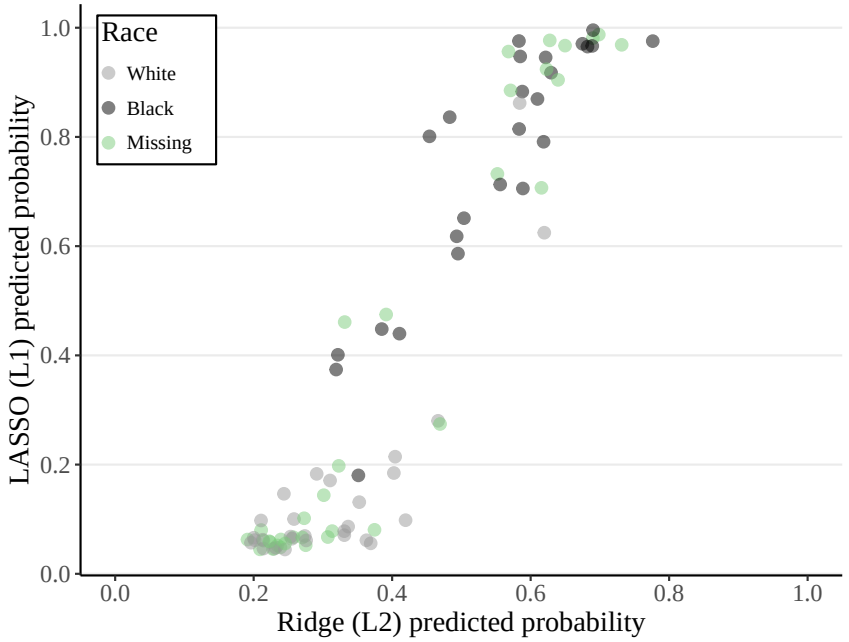
(a) Chicago, IL



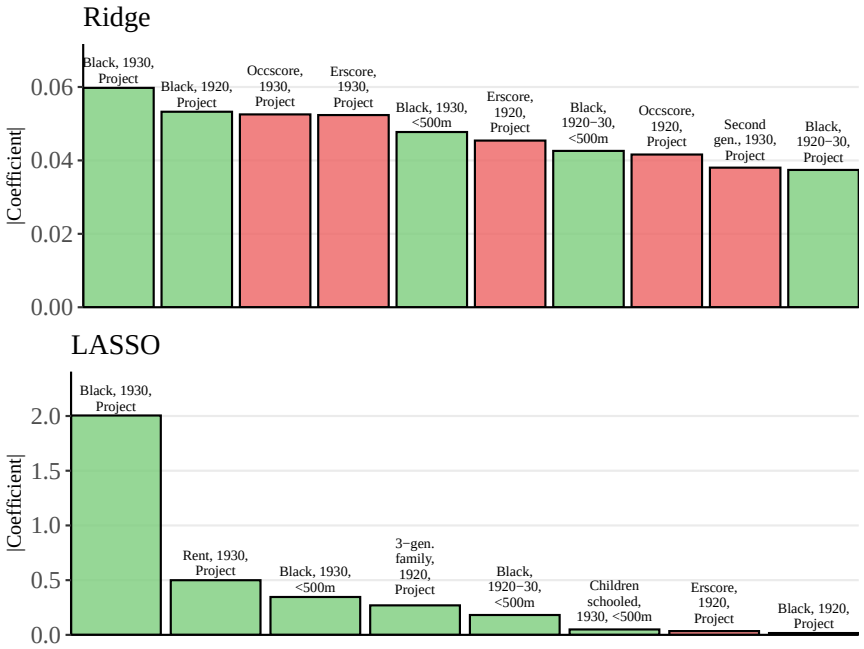
(b) Savannah, GA

Note: The figure shows two examples of the surrounding areas used to construct neighborhood measures around public housing projects in our analysis. The area of interest extends to a 1-kilometer radius and is typically divided into four concentric 250-meter rings. Panel (b) illustrates that when the radius includes a substantial body of water, the area of interest is truncated accordingly.

Figure B.5: Predicted racial designation for control projects



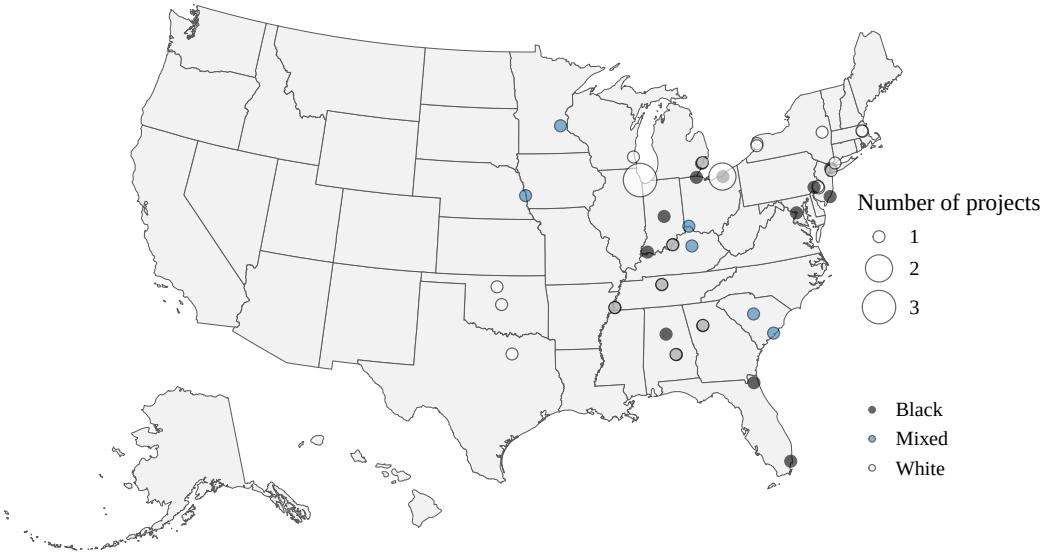
(a) Predicted "Black-only" project probability



(b) Top "Black-only" project predictors by method

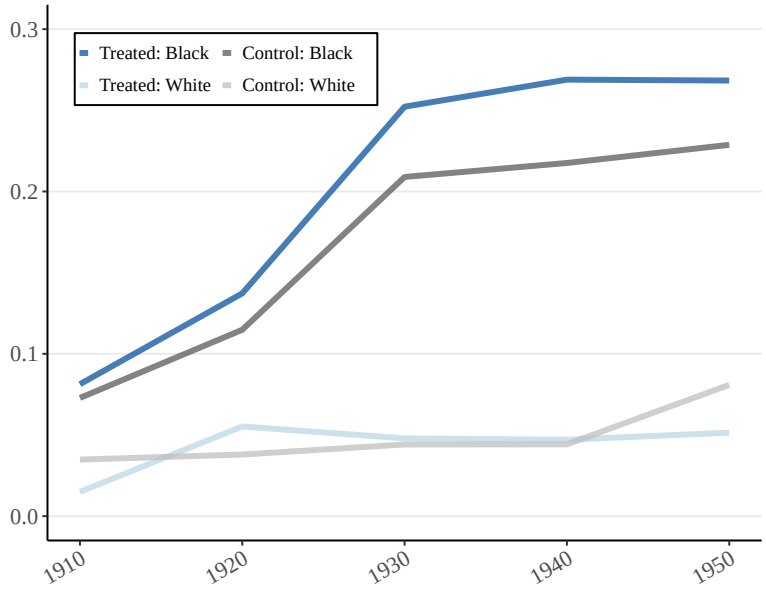
Note: Panel A plots the predicted probability that a control project was designated as Black-only using two penalized logistic regression methods. The x-axis reports predictions from the Ridge method, and the y-axis reports predictions from the LASSO method, as described in Section 3. Each dot represents a control project; dot color indicates whether the project was planned by the PWA as Black-only (black), White-only (gray), or has missing racial designation (green). Panel B reports, for each method, up to the ten most important predictors of Black designation. Green bars indicate predictors with a positive association, and red bars indicate predictors with a negative association.

Figure B.6: Map of PWA projects by racial designation



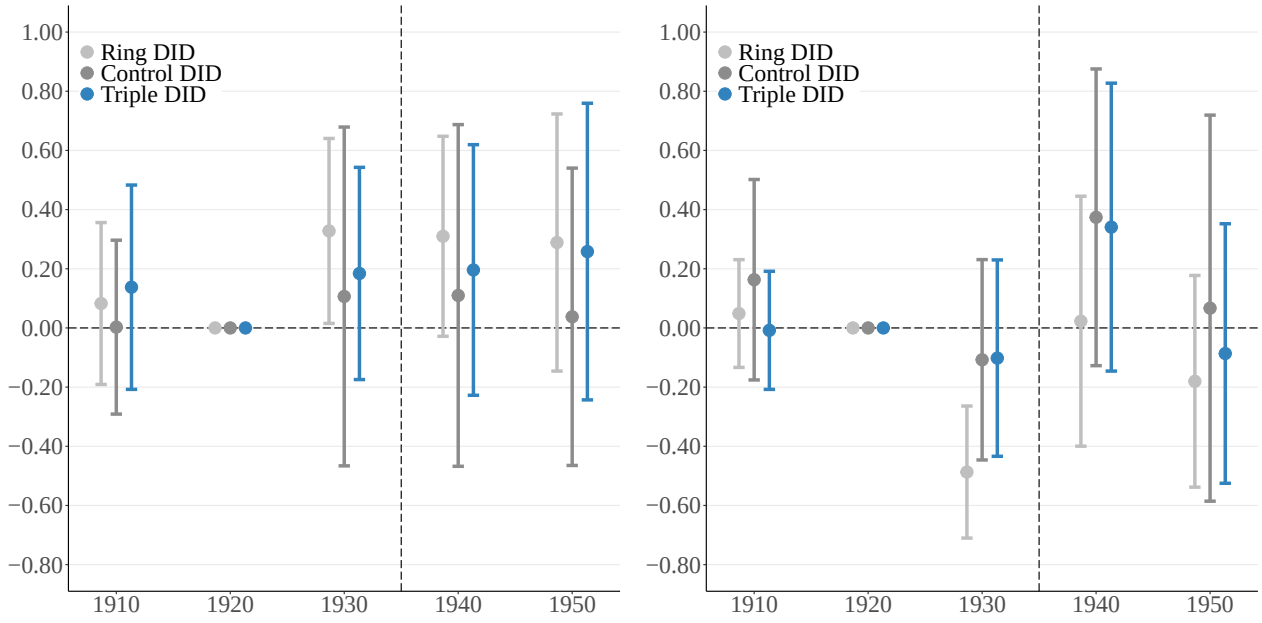
Note: This map illustrates the spatial distribution of the 47 public housing projects constructed by the Public Works Administration by racial designation: Black-only (black), White-only (white), and mixed (blue). Color gray indicates that the city had both Black-only and White-only projects. Bubble size shows the number of projects in each city. This information was collected from archival sources (PWA - Housing Division, 1933 -), as indicated in Section 3.

Figure B.7: Evolution of Southern Migration in treated and control projects, by racial designation



Note: The plot reports the evolution of the share of individuals who migrated from the South by year, separately for treated (blue) and control (gray) projects. Color intensity distinguishes projects by racial designation. For control projects, we use the predicted designation as described in Section 3.

Figure B.8: The impact of the projects on the Black and white populations of project sites by identification strategy

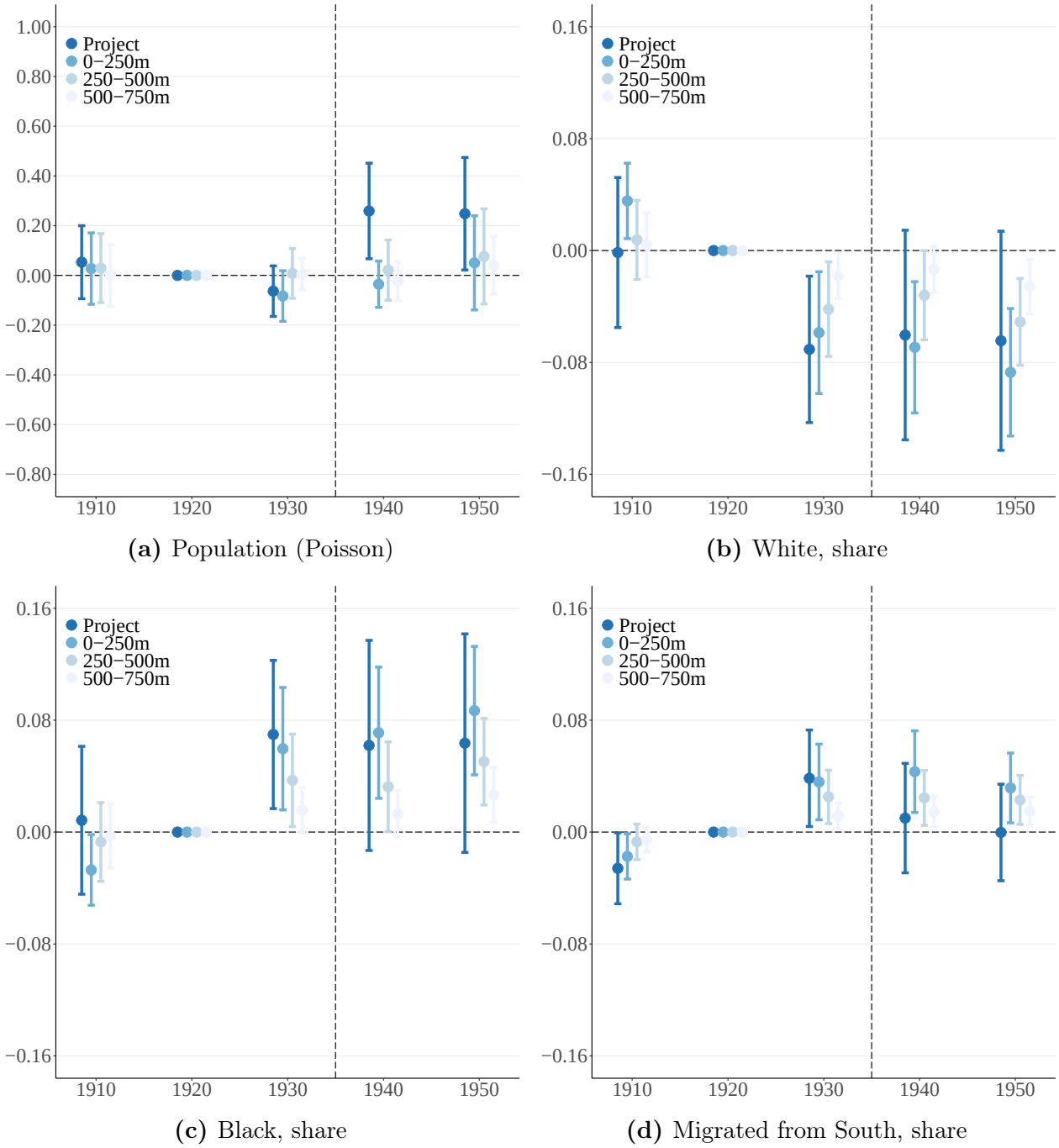


(a) Black population (Poisson)

(b) White population (Poisson)

Note: Each panel reports estimates of $\beta_{r,\tau}$, γ_τ, and $\theta_{r,\tau}$ from Equations (1), (2), and (3), respectively, along with 95 percent confidence intervals. All estimates correspond to the project site (r = Project site), and each color denotes a different identification strategy. Standard errors are clustered at the project level. Each panel reports estimates from Poisson specifications using Black (panel a) and white (panel b) population as the outcome and a modified dataset collapsed to the project–ring–year level.

Figure B.9: The impact of the projects on the neighborhood using the ring DID



(a) Population (Poisson)

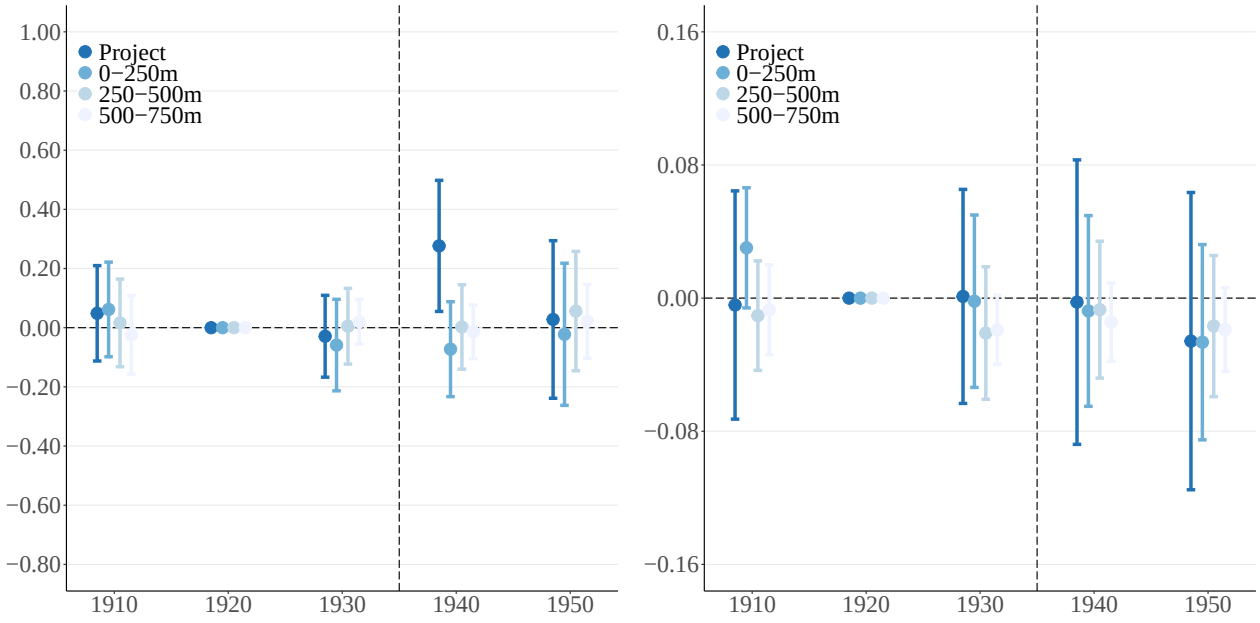
(b) White, share

(c) Black, share

(d) Migrated from South, share

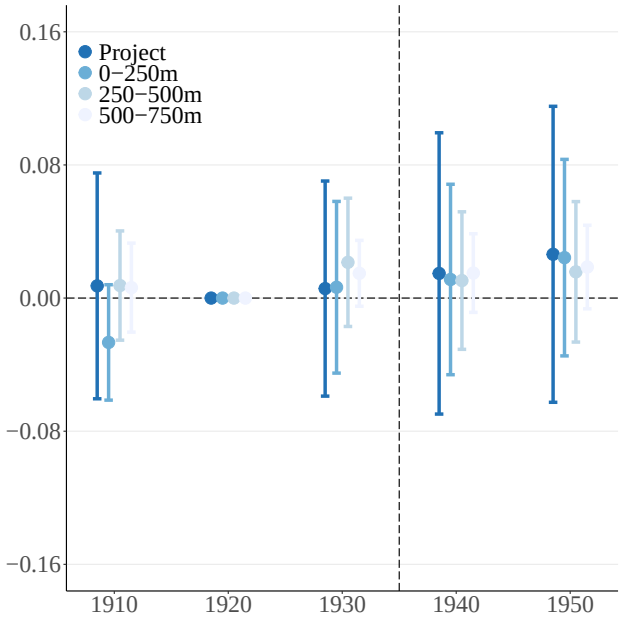
Note: Each panel reports estimates of $\beta_{r,\tau}$ from Equations (1), along with 95 percent confidence intervals. Each color denotes a different area/ring. Standard errors are clustered at the project level. Panel (a) reports estimates from Poisson specifications using population as the outcome and a modified dataset collapsed to the project–ring–year level. Panels (b), (c), and (d) use indicators for white, Black, and migration from the South as outcomes.

Figure B.10: The impact of the projects on the neighborhood using the triple DID

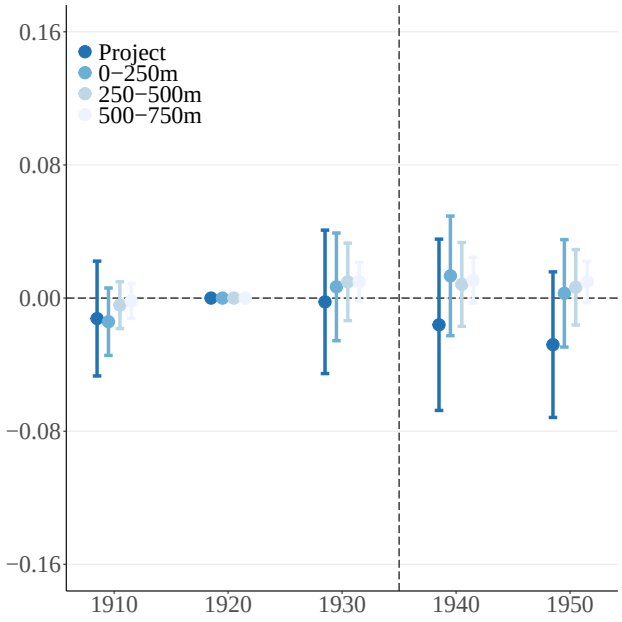


(a) Population (Poisson)

(b) White, share



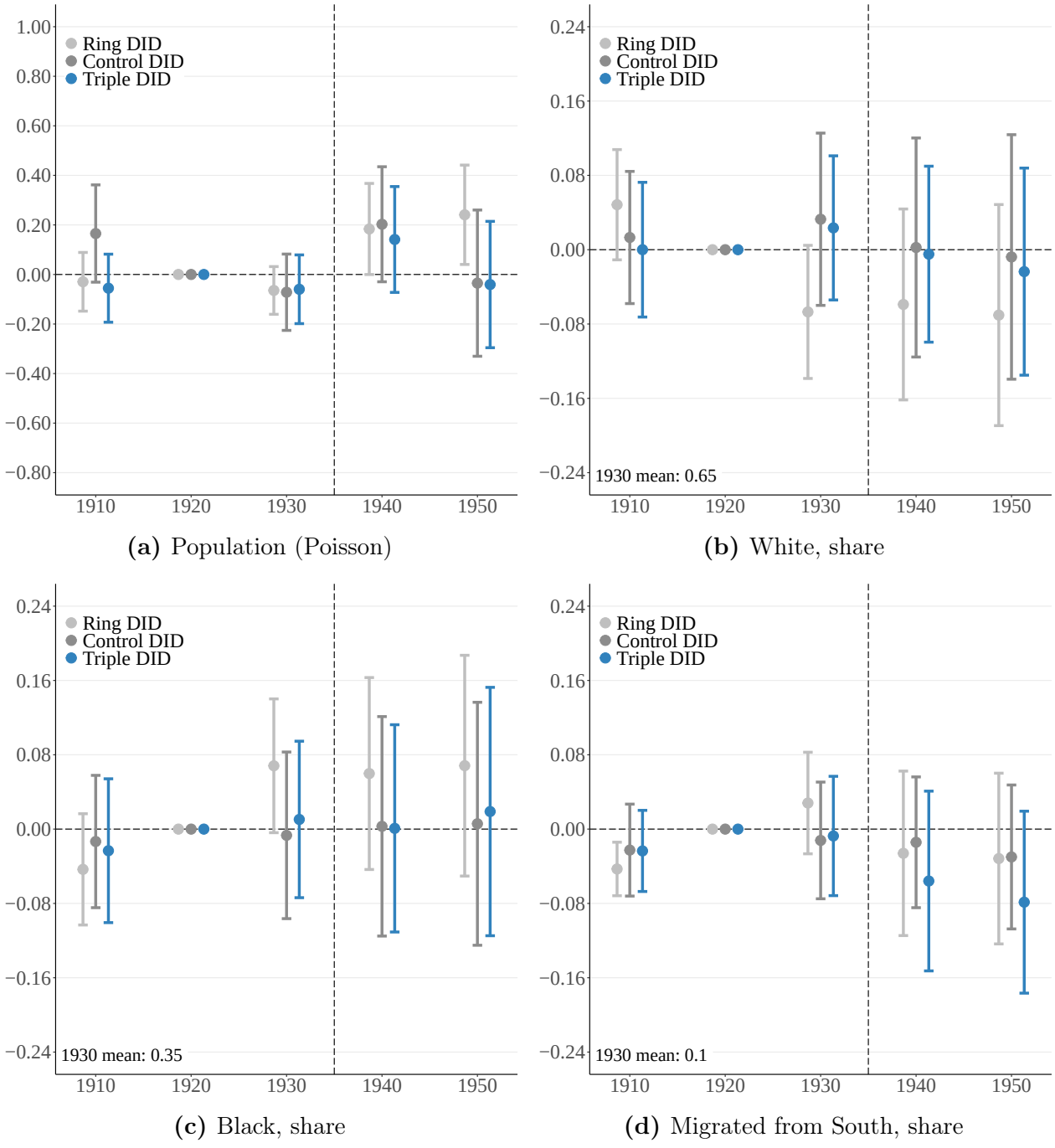
(c) Black, share



(d) Migrated from South, share

Note: Each panel reports estimates of $\theta_{r,\tau}$ from Equations (3), along with 95 percent confidence intervals. Each color denotes a different area/ring. Standard errors are clustered at the project level. Panel (a) reports estimates from Poisson specifications using population as the outcome and a modified dataset collapsed to the project–ring–year level. Panels (b), (c), and (d) use indicators for white, Black, and migration from the South as outcomes.

Figure B.11: Robustness: The impact on the project site by identification strategy with 500m-wide rings



(a) Population (Poisson)

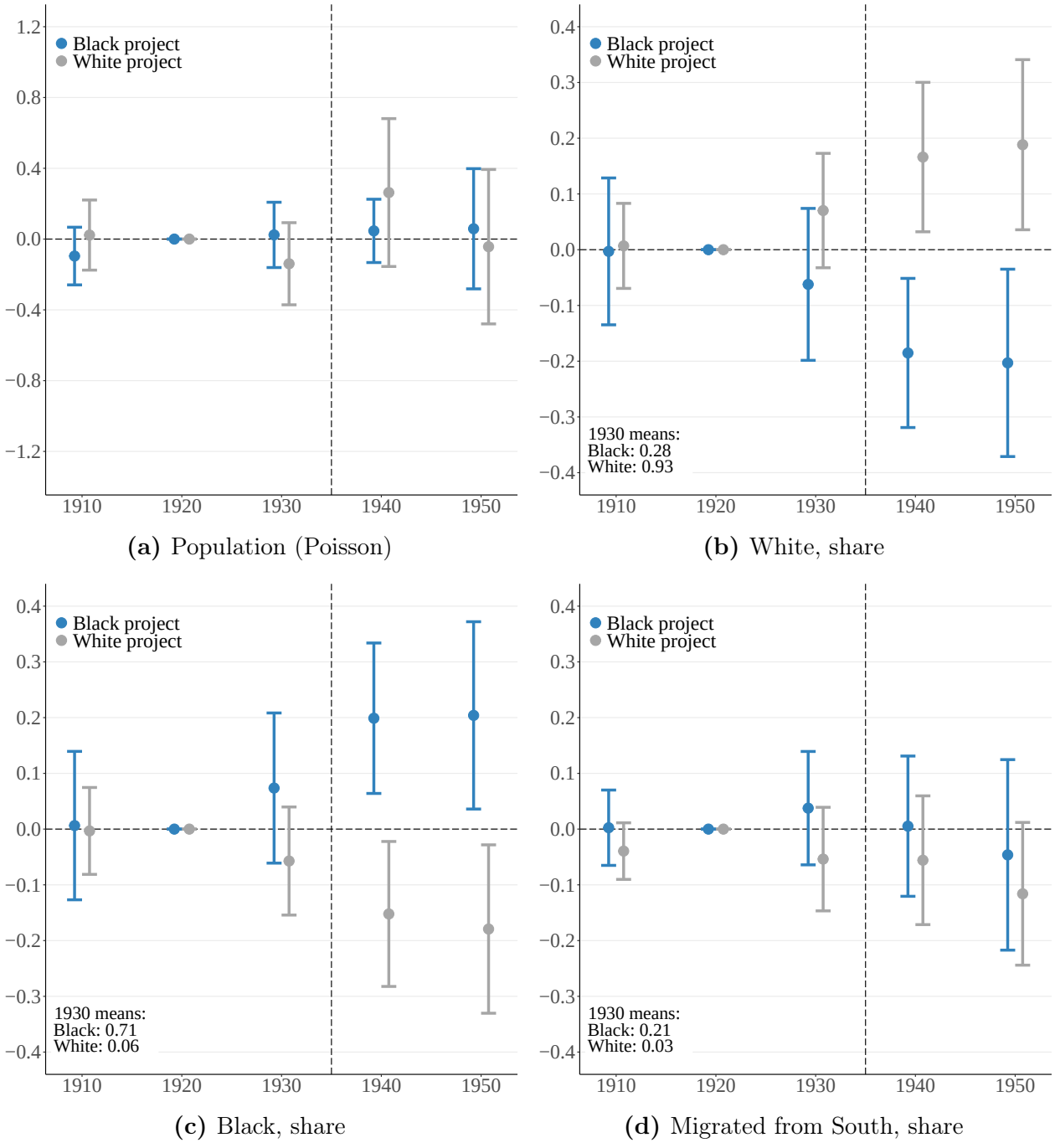
(b) White, share

(c) Black, share

(d) Migrated from South, share

Note: This figure reproduces Figure V using 500 meter-wide rings up to 2km away. The comparison group is the ring within 1.5-2km from the projects.

Figure B.12: Robustness: The impact on the project site by project race with 500m-wide rings and using triple DID



(a) Population (Poisson)

(b) White, share

(c) Black, share

(d) Migrated from South, share

Note: This figure reproduces Figure VI using 500 meter-wide rings up to 2km away. The comparison group is the ring within 1.5-2km from the projects.

C. Appendix Tables

Table C.1: Robustness: The impact of the projects on the surrounding area by identification strategy with 500m-wide rings

	Multi-family	Migrated from South	Black	Unemployed, male	Out of labor force, male	Home-owner	Home value, log	Rent, log
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Ring DID								
1940: Project	-0.027*	-0.027	0.047	-0.048***	-0.017	-0.091***	0.168	0.436***
	(0.016)	(0.038)	(0.045)	(0.013)	(0.011)	(0.031)	(0.112)	(0.141)
1940: 0-0.5km	0.001	-0.006	0.045	-0.001	0.004	0.026	0.025	0.0002
	(0.012)	(0.032)	(0.029)	(0.009)	(0.009)	(0.026)	(0.038)	(0.083)
1940: 0.5-1km	0.003	-0.023	0.016	-0.011	0.003	0.016	0.009	-0.075
	(0.010)	(0.031)	(0.022)	(0.008)	(0.008)	(0.025)	(0.044)	(0.079)
1940: 1-1.5km	0.001	-0.034	-0.001	-0.011	-0.0004	0.018	0.032	-0.022
	(0.009)	(0.029)	(0.015)	(0.008)	(0.008)	(0.023)	(0.040)	(0.072)
1950: Project	-0.064	-0.032	0.056	-0.010	0.051***			
	(0.050)	(0.040)	(0.053)	(0.008)	(0.018)			
1950: 0-0.5km	-0.017	-0.006	0.070**	0.004	0.013			
	(0.032)	(0.032)	(0.034)	(0.006)	(0.010)			
1950: 0.5-1km	0.003	-0.018	0.030	0.005	0.008			
	(0.031)	(0.031)	(0.028)	(0.006)	(0.009)			
1950: 1-1.5km	-0.004	-0.031	0.015	0.002	0.011			
	(0.029)	(0.029)	(0.020)	(0.006)	(0.008)			
Y mean (Project)	0.140	0.172	0.297	0.115	0.094	0.183	5,749.1	28.7
Y mean (0-1.5km)	0.151	0.088	0.171	0.090	0.098	0.235	6,647.3	48.5
Observations	6,994,143	4,584,285	8,172,707	2,143,383	2,665,800	6,473,932	824,439	2,551,756
R^2	0.223	0.368	0.603	0.062	0.120	0.227	0.521	0.435
Panel B: Triple DID								
1940: Project	-0.054***	-0.050	-0.0004	-0.050***	-0.043***	-0.117***	0.118	0.469***
	(0.019)	(0.042)	(0.048)	(0.017)	(0.016)	(0.042)	(0.134)	(0.166)
1940: 0-0.5km	-0.018	-0.025	-0.004	-0.011	-0.007	0.0008	-0.004	0.017
	(0.014)	(0.033)	(0.033)	(0.013)	(0.012)	(0.032)	(0.068)	(0.109)
1940: 0.5-1km	-0.009	-0.031	-0.007	-0.019	-0.007	-0.0004	-0.036	-0.010
	(0.012)	(0.031)	(0.025)	(0.012)	(0.011)	(0.031)	(0.069)	(0.103)
1940: 1-1.5km	-0.007	-0.038	-0.014	-0.016	-0.005	0.007	-0.002	0.056
	(0.010)	(0.029)	(0.018)	(0.011)	(0.010)	(0.029)	(0.062)	(0.095)
1950: Project	-0.058	-0.072*	0.018	0.022	0.030			
	(0.066)	(0.043)	(0.059)	(0.016)	(0.023)			
1950: 0-0.5km	-0.058	-0.043	0.011	0.021	0.001			
	(0.042)	(0.035)	(0.041)	(0.013)	(0.013)			
1950: 0.5-1km	-0.065	-0.042	0.002	0.026**	-0.013			
	(0.040)	(0.033)	(0.033)	(0.013)	(0.012)			
1950: 1-1.5km	-0.083**	-0.044	0.0008	0.018	-0.005			
	(0.037)	(0.031)	(0.027)	(0.012)	(0.011)			
Y mean (Project)	0.140	0.172	0.297	0.115	0.094	0.183	5,749.1	28.7
Y mean (0-1.5km)	0.151	0.088	0.171	0.090	0.098	0.235	6,647.3	48.5
Observations	18,924,584	12,441,409	22,239,161	5,749,254	7,202,828	17,546,751	2,441,275	6,821,210
R^2	0.216	0.337	0.632	0.065	0.117	0.234	0.548	0.438

Note: This table reproduces Table II using 500 meter-wide rings up to 2km away. The comparison group is the ring within 1.5-2km from the projects.

Table C.2: Robustness: The impact of the projects on the surrounding area by project race with 500m-wide rings and using triple DID

	Multi-family	Migrated from South	Black	Unemployed, male	Out of labor force, male	Home-owner	Home value, log	Rent, log
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Black-only projects								
Project	-0.035	-0.005	0.279***	-0.026*	0.025	-0.086**	0.083	0.104
	(0.031)	(0.072)	(0.052)	(0.015)	(0.021)	(0.031)	(0.205)	(0.200)
0-0.5km	0.020	0.003	0.148***	0.006	0.014	-0.013	0.071	-0.065
	(0.017)	(0.059)	(0.043)	(0.011)	(0.011)	(0.025)	(0.055)	(0.162)
0.5-1km	0.007	-0.023	0.078**	0.003	0.010	-0.027	0.027	-0.153
	(0.014)	(0.059)	(0.034)	(0.010)	(0.011)	(0.022)	(0.070)	(0.152)
1-1.5km	0.004	-0.044	0.040**	-0.002	0.0003	-0.030	0.024	-0.109
	(0.013)	(0.054)	(0.018)	(0.010)	(0.011)	(0.022)	(0.058)	(0.138)
Y mean (Project)	0.183	0.268	0.451	0.134	0.087	0.206	5,617.2	33.1
Y mean (0-1.5km)	0.191	0.138	0.278	0.092	0.092	0.212	6,822.8	52.2
Observations	2,965,017	1,950,379	3,384,863	903,344	1,134,685	2,683,969	285,177	1,152,234
R^2	0.211	0.375	0.584	0.059	0.103	0.217	0.547	0.451
Panel B: White-only projects								
Project	-0.060**	-0.089*	-0.145**	-0.022	0.006	-0.091	0.335*	0.830***
	(0.025)	(0.047)	(0.056)	(0.019)	(0.024)	(0.076)	(0.181)	(0.252)
0-0.5km	-0.044**	-0.045	-0.038	-0.010	0.010	0.073	0.045	0.124
	(0.019)	(0.032)	(0.036)	(0.015)	(0.016)	(0.063)	(0.093)	(0.141)
0.5-1km	-0.027*	-0.036	-0.047	-0.014	0.001	0.080	-0.020	0.112
	(0.016)	(0.028)	(0.031)	(0.013)	(0.015)	(0.061)	(0.097)	(0.142)
1-1.5km	-0.029*	-0.038	-0.040	-0.012	0.005	0.083	0.024	0.177
	(0.017)	(0.027)	(0.029)	(0.013)	(0.013)	(0.057)	(0.093)	(0.133)
Y mean (Project)	0.124	0.057	0.177	0.107	0.104	0.150	7,437.7	25.5
Y mean (0-1.5km)	0.121	0.033	0.071	0.092	0.105	0.256	6,822.8	47.5
Observations	8,665,621	5,640,906	10,361,053	2,684,138	3,314,997	8,186,290	1,348,326	3,036,614
R^2	0.215	0.263	0.541	0.066	0.135	0.239	0.529	0.422

Note: This table reproduces Table III using 500 meter-wide rings up to 2km away. The comparison group is the ring within 1.5-2km from the projects.

Table C.3: Robustness: The impact of the projects on the surrounding area using the control DID

	Multi-family	Migrated from South	Black	Unemployed, male	Out of labor force, male	Home-owner	Home value, log	Rent, log
1940: Project	-0.043**	-0.003	0.009	-0.039**	-0.038***	-0.108***	0.193	0.481***
	(0.018)	(0.027)	(0.048)	(0.016)	(0.013)	(0.030)	(0.126)	(0.163)
1940: 0-250m	0.002	0.019	0.020	0.007	-0.002	-0.016	0.002	-0.021
	(0.010)	(0.020)	(0.029)	(0.010)	(0.008)	(0.014)	(0.062)	(0.077)
1940: 250-500m	-0.017*	0.010	-0.001	0.009	0.001	-0.009	-0.022	-0.053
	(0.010)	(0.017)	(0.023)	(0.009)	(0.006)	(0.011)	(0.051)	(0.056)
1940: 500-750m	-0.001	0.012	0.007	0.002	0.002	-0.003	-0.035	-0.069*
	(0.008)	(0.013)	(0.018)	(0.007)	(0.006)	(0.010)	(0.053)	(0.037)
1940: 750-1,000m	-0.003	0.005	-0.001	-0.003	-0.005	-0.013	-0.017	-0.011
	(0.009)	(0.010)	(0.016)	(0.007)	(0.005)	(0.011)	(0.049)	(0.037)
1950: Project	0.020	-0.019	0.012	-0.002	0.039**			
	(0.060)	(0.031)	(0.056)	(0.013)	(0.020)			
1950: 0-250m	-0.023	0.010	0.037	0.006	0.005			
	(0.026)	(0.028)	(0.037)	(0.007)	(0.010)			
1950: 250-500m	0.023	0.009	0.003	0.002	0.014*			
	(0.022)	(0.025)	(0.032)	(0.007)	(0.009)			
1950: 500-750m	-0.006	0.008	0.009	0.007	0.000			
	(0.023)	(0.022)	(0.027)	(0.006)	(0.008)			
1950: 750-1,000m	0.005	0.002	-0.004	0.003	-0.004			
	(0.017)	(0.018)	(0.023)	(0.006)	(0.007)			
Y mean	0.13	0.09	0.17	0.09	0.09	0.24	6388.18	47.64

Note: The table reports estimates of γ_τ from a version of Equation (2) that pools all pre-treatment periods. Each column uses a different outcome, in order: an indicator for whether three or more families live in the same unit, migration from the South, Black, male unemployment, male being out of the labor force, homeownership, the logarithm of the home value, and the logarithm of rent. At the bottom of each panel, we report the mean of the outcome variable in the pre-treatment period across all rings. Standard errors, clustered at the project level, are reported in parenthesis. Significance codes: ***: 0.01, **: 0.05, *: 0.1.

Table C.4: Robustness: The impact of the projects on the surrounding area by project race using the control DID

	Multi-family	Migrated from South	Black	Unemployed, male	Out of labor force, male	Home-owner	Home value, log	Rent, log
Panel A: Black-only								
Project	-0.065*	0.003	0.147**	-0.048**	0.001	-0.066*	0.099	0.123
	(0.033)	(0.053)	(0.069)	(0.019)	(0.023)	(0.036)	(0.207)	(0.148)
0-250m	0.014	0.009	0.017	-0.003	-0.009	-0.009	-0.038	-0.040
	(0.017)	(0.051)	(0.053)	(0.012)	(0.013)	(0.022)	(0.098)	(0.137)
250-500m	-0.019	0.019	-0.010	0.013	-0.007	-0.005	0.044	-0.085
	(0.013)	(0.047)	(0.049)	(0.010)	(0.010)	(0.015)	(0.093)	(0.075)
500-750m	-0.010	0.006	0.007	0.006	-0.005	-0.001	0.004	-0.105*
	(0.015)	(0.040)	(0.042)	(0.010)	(0.009)	(0.015)	(0.081)	(0.061)
750-1,000m	-0.009	0.013	0.012	0.003	-0.008	-0.011	0.016	-0.065
	(0.017)	(0.035)	(0.032)	(0.010)	(0.008)	(0.014)	(0.086)	(0.062)
Y mean	0.19	0.17	0.32	0.09	0.09	0.19	6782.75	48.8
Panel B: White-only								
Project	-0.017	-0.041*	-0.093**	-0.003	-0.002	-0.156***	0.269	0.850***
	(0.020)	(0.021)	(0.044)	(0.020)	(0.020)	(0.053)	(0.183)	(0.307)
0-250m	-0.013	-0.004	0.018	0.012	0.001	-0.020	0.013	0.029
	(0.012)	(0.017)	(0.015)	(0.009)	(0.010)	(0.021)	(0.087)	(0.096)
250-500m	-0.012	-0.006	0.019	-0.000	0.013	-0.013	-0.071	-0.075
	(0.014)	(0.014)	(0.015)	(0.010)	(0.008)	(0.017)	(0.060)	(0.078)
500-750m	0.001	0.006	0.014	-0.001	0.005	-0.003	-0.094	-0.054
	(0.009)	(0.019)	(0.019)	(0.008)	(0.007)	(0.016)	(0.077)	(0.049)
750-1,000m	0.003	-0.008	-0.009	-0.006	-0.006	-0.012	-0.089	0.015
	(0.010)	(0.014)	(0.017)	(0.007)	(0.007)	(0.017)	(0.063)	(0.049)
Y mean	0.13	0.04	0.08	0.09	0.11	0.25	6806.14	47.38

Note: The table reports estimates of γ_τ from a version of Equation (2) that pools all pre-treatment periods and all post-treatment periods. Panel A reports estimates for Black-only projects, and Panel B for White-only projects; in each case, the control group includes only control projects predicted to have the same racial designation. Each column uses a different outcome, in order: an indicator for whether three or more families live in the same unit, migration from the South, Black, male unemployment, male being out of the labor force, homeownership, the logarithm of the home value, and the logarithm of rent. At the bottom of each panel, we report the mean of the outcome variable in the pre-treatment period across all rings. Standard errors, clustered at the project level, are reported in parenthesis. Significance codes: ***: 0.01, **: 0.05, *: 0.1.