Creating High-Opportunity Neighborhoods: Evidence from the HOPE VI Program*

Raj Chetty, Harvard University and Opportunity Insights
Rebecca Diamond, Harvard University and Cities, Housing, and Society Lab
Thomas B. Foster, U.S. Census Bureau
Lawrence Katz, Harvard University and Opportunity Insights
Sonya R. Porter, U.S. Census Bureau
Matthew Staiger, Harvard University and Opportunity Insights
Laura Tach, Cornell University

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Abstract

We study whether low-economic-mobility neighborhoods can be transformed into high-mobility areas by analyzing the HOPE VI program, which invested $17 billion to revitalize 262 distressed public housing developments. We estimate the program's impacts using a matched difference-in-differences design, comparing outcomes in revitalized developments to observably similar control developments using anonymized tax records. HOPE VI reduced neighborhood poverty rates by attracting higher-income families to revitalized neighborhoods, but had no causal impact on the earnings of adults living in public housing units. Children raised in revitalized public housing units earned more, were more likely to attend college, and were less likely to be incarcerated. Using a movers exposure design and sibling comparisons, we show that these improvements were driven by changes in neighborhoods' causal effects on children's outcomes. The improvements in neighborhood causal effects were driven in large part by changes in social interaction: HOPE VI increased interaction between public housing residents and peers in surrounding neighborhoods and increased earnings more for subgroups with higher-income peers. Many low-income families in the U.S. currently live in neighborhoods that are as socially isolated as the HOPE VI developments were prior to revitalization. We conclude that it is feasible to create high-opportunity neighborhoods and that connecting socially isolated areas to surrounding communities is a cost-effective approach to doing so.

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I Introduction

Recent work has shown that opportunities for upward income mobility vary significantly across neighborhoods (Chetty, Hendren and Katz, 2016; Chetty et al., 2026*a*; Chyn, 2018; Chyn, Collinson and Sandler, 2025). These findings have sparked renewed interest in policies that help low-income families "move to opportunity," for instance by using subsidized housing vouchers (Bergman et al., 2024) or building more affordable housing in high-opportunity neighborhoods (Sportiche et al., 2025). While the moving to opportunity approach has been widely studied, less is known about whether one can bring opportunity to low-income families where they currently live. Can low-opportunity neighborhoods be transformed into high-opportunity neighborhoods? What are the mechanisms through which one can increase economic mobility in low-opportunity areas?

In this paper, we study these questions in the context of the Housing Opportunities for People Everywhere (HOPE VI) program, one of the largest neighborhood revitalization programs implemented in the United States. Between 1993 and 2010, HOPE VI invested $17 billion (through a combination of public and private funds) to revitalize 262 large, highly distressed public housing projects that each had 1,355 residents at a given point in time on average.[1] The program demolished and replaced these housing projects with a mix of market-rate and subsidized units and created new community programs to support residents, such as childcare and job training services.

Prior work using data from repeated cross-sections has shown that neighborhoods improved significantly and became more desirable following HOPE VI revitalization. Poverty and crime fell significantly, while house prices increased (Tach and Emory, 2017; Zielenbach and Voith, 2010; Sandler, 2017; Bruhn, 2018; Blanco and Neri, 2025). However, it is unclear whether these changes in outcomes were driven by families re-sorting across neighborhoods (a selection effect) or whether the HOPE VI program actually improved neighborhoods' *causal effects* on their residents' outcomes. Would assigning individuals to a project post-revitalization lead to better outcomes than what they would have experienced had they lived in the same neighborhood pre-revitalization?

We analyze the effects of HOPE VI on neighborhoods' causal effects using an anonymized longitudinal dataset constructed by linking public housing records to information from tax returns spanning 1979–2019

[1] For consistency with prior work, we report total HOPE VI expenditures in nominal terms. All subsequent dollar values in this paper are expressed in real 2022 dollars, deflated using the CPI-U.

and decennial Census data. These data provide administrative information on address histories and a rich set of covariates from public housing records. They also allow us to study a broad range of outcomes for adults and children, including earnings, employment rates, college attendance, and incarceration.

We divide our empirical analysis into four parts. First, we analyze the effects of HOPE VI on adults' outcomes. We identify the program's causal effect on outcomes using a matched difference-in-differences design. Although HOPE VI targeted high-poverty, distressed public housing projects, many similarly distressed projects were not revitalized due to funding constraints. Exploiting this fact, we match each HOPE VI project to other control projects with similar levels of income, racial composition, and size prior to the intervention. Our research design assumes that absent the HOPE VI treatment, trends in outcomes in the HOPE VI neighborhoods would have been the same as those in the control neighborhoods. Consistent with this identification assumption, we find very similar trends in outcomes prior to revitalization in the two groups. In addition, we find very similar results when using alternative comparison groups, such as housing projects that applied for HOPE VI funding but were not selected by HUD.

We find that HOPE VI increased the mean household incomes of adults living in targeted neighborhoods by 45% relative to matched control neighborhoods in repeated cross-sections, consistent with the findings of Tach and Emory (2017). However, our longitudinal data reveal that the incomes of adults who lived in housing projects prior to its implementation did not change after the HOPE VI intervention. Instead, the increased incomes in treated neighborhoods were driven entirely by a change in the composition of residents in those neighborhoods, consistent with the findings of Staiger, Palloni and Voorheis (2024).

In the second part of our analysis, we examine the effects of HOPE VI on children's outcomes, measuring children's earnings and other outcomes in their late twenties and early thirties. Children who moved into the HOPE VI public housing units post-revitalization earned 16% more, were 17% more likely to attend college, and, among boys, were 20% less likely to be incarcerated than their counterparts in non-revitalized matched control projects.[2] We find similar results when comparing HOPE VI projects to projects that applied for but did not receive funding or projects that were partially demolished but not subsequently revitalized. We also find no change in outcomes for children living in nearby areas surrounding revitalized projects, supporting the parallel trends in potential outcomes assumption underlying our design.[3]

[2]On average, children who move into revitalized public housing units spend approximately 5 years of their childhood living in those units. We show below that these effects should therefore be interpreted as the impact of spending 5/18=28% of one's childhood in a revitalized project rather than an unrevitalized one.

[3]These findings also shed light on mechanisms, showing that the gains from revitalization cannot be driven by simply reducing the density of public housing or by broader community-level changes such as improvements in local schools.

The improvements in children's outcomes in revitalized neighborhoods could be the result of an improvement in the causal effects of those neighborhoods or a selection effect arising from changes in the types of families living in public housing units after revitalization. To distinguish between these hypotheses, we build on the movers research design developed in Chetty and Hendren (2018) to estimate the causal effect of HOPE VI on childhood exposure effects. Focusing on children who moved into a given project in a given calendar year, we first estimate each project-year cell's childhood exposure effect as the relationship between adult earnings and the number of years that children grew up in the project. We then apply our matching design to these childhood exposure effects, comparing the project-by-year childhood exposure effects in revitalized neighborhoods to control neighborhoods.

Using this movers research design, we find that each year of childhood exposure to a revitalized project instead of a control public housing project increased children's household income in adulthood by 2.77% (s.e. = 0.80%) on average. The impacts of exposure to a revitalized project are twice as large in adolescence than before age 12, consistent with studies that analyze the outcomes of children who move across neighborhoods (Chetty et al., 2026*a*; Deutscher, 2020). These estimates imply that spending 5 years of childhood in a revitalized public housing unit (the average duration of exposure) instead of a non-revitalized one increases children's household income at age 30 by 14%, consistent with the estimates from our baseline repeated cross-section estimator. Extrapolating back to birth, the estimates imply that growing up in a revitalized public housing project throughout childhood increases a child's housing income at age 30 by 50%.

The identification assumption underlying the movers estimator is that the relationship between potential outcomes and years of exposure to public housing is the same in HOPE VI and matched control projects (conditional on project-by-move-in-year fixed effects). This assumption would be violated if, for example, families with more resources (and thus children with better potential outcomes) move out of unrevitalized, distressed housing projects more quickly. Two pieces of evidence suggest that such dynamic selection effects are not prevalent in practice. First, children's predicted outcomes based on the rich set of individual-level observables in the public housing data do not covary differentially with childhood exposure across treated and control projects. Second, the estimates are similar when we include family fixed effects, comparing siblings who are exposed to projects for different durations purely because of differences in their ages when they moved in.

Our estimates imply that the present-discounted-value lifetime earnings gain from living in a revitalized project for one additional year is $24,900 for the average family (with 2.4 children) under the assumption that the treatment effect on earnings at age 30 remains constant in percentage terms over children's lifetimes. Over the ten years post-revitalization—the window we can analyze given currently available data—this translates to a total lifetime earnings gain of $218,800 in present value; if the gains from revitalization persist for 30 years, we predict total earnings gains of $502,700. These earnings gains are considerably larger than the program's upfront costs of $168,200 in public funds per revitalized unit. Moreover, the increased income tax revenues over children's lives offset much of the upfront cost to taxpayers.

In the third part of the paper, we analyze the mechanisms through which HOPE VI improved children's outcomes. Recent work has identified social interaction with higher-income peers as a central driver of economic mobility (e.g., Damm and Dustmann (2014); Chetty et al. (2022*a*, 2026*c*); Cattan, Salvanes and Tominey (2025); Mallah (2025)). Motivated by this evidence, we analyze the role of peers in explaining HOPE VI's treatment effects using two approaches.

We first analyze whether the treatment effects of HOPE VI on children's outcomes vary with the incomes of their nearby peers. We define potential peer groups for each child in the HOPE VI neighborhoods as the set of children residing outside the project itself but who live within a one-mile radius of the project and who are of the same age, sex, and race (given evidence of strong homophily in friendships by age, sex, and race in prior work). We find that HOPE VI had significantly larger treatment effects for subgroups with more affluent peers, as measured based on peers' ex-post incomes in adulthood or their parents' (pre-treatment) incomes. Strikingly, HOPE VI had zero treatment effect on children with the lowest-income peers, suggesting that improving the physical stock of housing by itself is insufficient to increase economic mobility and that social interaction is central.

Children's outcomes are much more strongly related to those of peers in their *own* cohort—with whom they are likely to interact most in school—than with peers who are a year older or younger. More generally, children with higher-income peers gained more from HOPE VI even when comparing children in different demographic subgroups *within* the same project. These within-project comparisons rule out the possibility that the heterogeneity in treatment effects is driven by other project-level differences that are correlated with peer income, such as programmatic choices or school quality.

As a complement to the heterogeneity analysis, we evaluate the role of social interaction as a mediator by estimating the impacts of HOPE VI on three measures of interaction between public housing residents and peers in surrounding neighborhoods. First, using residential data from the decennial Censuses, we show that HOPE VI increased the likelihood that children who grew up in public housing later lived with peers who grew up in the surrounding one-mile radius. Second, using anonymized data from cell phone pings, we show that HOPE VI increased the time that people spent in the surrounding neighborhood outside their public housing development. Third, using social network data from Facebook, we show that HOPE VI increased friendships between children from low- and high-income families in high schools near public housing sites. Using estimates of the causal effect of cross-class friendships on children's incomes in adulthood from Chetty et al. (2026*b*), we show that the effects of HOPE VI on cross-class friendships can explain its treatment effect on children's incomes under plausible assumptions.[4]

Together, these results indicate that distressed public housing projects were essentially islands that had limited social interaction with nearby communities. The HOPE VI program built a bridge to surrounding communities, allowing public housing residents to benefit from interacting with those residents. When those peers were higher-income, children gained more from the HOPE VI treatment, perhaps because of changes in their behaviors, aspirations, or connections to economic and social opportunities.

In the final part of the paper, we assess whether increasing social integration with surrounding areas is a scalable approach to creating higher-opportunity neighborhoods beyond the distressed public housing projects targeted by HOPE VI. Using an index of social integration based on the covariance between children's outcomes and those in surrounding areas, we first show that the average low-income neighborhood in the United States is roughly as isolated from surrounding areas as HOPE VI public housing sites were from their neighbors pre-revitalization. We then show that neighborhoods that are better integrated with surrounding communities, especially higher-income areas, tend to have higher levels of upward income mobility for children from low-income families, consistent with our design-based findings from HOPE VI. There is no significant association between integration with lower-income neighborhoods and the outcomes of children from high-income families, suggesting that the gains for children from low-income families do not come at the expense of their higher-income peers. These asymmetric correlations parallel causal estimates from social network data showing that greater cross-class interaction improves outcomes for children from low-income families but has much smaller effects on outcomes for children from high-income families (Chetty et al., 2022*a*, 2026*b*).

We conclude that many low-opportunity neighborhoods in America are as socially isolated as HOPE VI sites were pre-revitalization. Connecting these low-opportunity areas to surrounding communities could

[4]To be clear, this analysis suggests that social interaction plays an important role in mediating the effects of HOPE VI on children's outcomes on average, but does not imply that social interaction is the only mediator: there is still residual heterogeneity in treatment effects across sites that may be driven by other factors.

significantly increase economic mobility without harming others' outcomes significantly. Our results suggest that revitalizing neighborhoods while preserving affordable housing as HOPE VI did can be one cost-effective approach to creating such connections. More broadly, our findings call for further analysis of other approaches that facilitate cross-neighborhood integration, which could range from improvements in transit to efforts to increase public safety to new programs or venues that facilitate cross-class interaction.

Related Literature. This paper builds on and relates to several literatures. First, prior work on the HOPE VI program has shown that children displaced by the demolitions ultimately had better long-term outcomes because they moved to better neighborhoods (Jacob, 2004; Chyn, 2018; Haltiwanger et al., 2024). Here, we do not focus on prior residents as they move to different areas; instead, we show how the causal effect of a *given* neighborhood itself changed after revitalization. Our findings coupled with the long-term gains even for displaced children suggest that HOPE VI increased upward income mobility significantly for the children it impacted. Although these gains are encouraging, a complete welfare analysis of the HOPE VI program would also need to account for the disruption costs of the demolitions and displacement of families, such as loss of social ties (Clampet-Lundquist, 2004*a*,*b*), social tensions within redeveloped neighborhoods (Chaskin and Joseph, 2015; Chaskin, Khare and Joseph, 2012; McCormick, Joseph and Chaskin, 2012), and aggregate loss in housing supply (Almagro, Chyn and Stuart, 2024).

Second, our analysis connects to work on neighborhood effects, which has primarily analyzed families who move across areas (for a summary, see Chyn and Katz, 2021). Our findings of gains for children, especially adolescent youth, but little change in adults' economic outcomes echo the findings of prior work showing similar patterns for movers from low-opportunity to higher-opportunity areas within a commuting zone. The contribution of the present study is to show that neighborhood effects can be *changed* through policy interventions.[5]

Third, our paper relates to the research on urban revitalization (Baum-Snow and Marion, 2009; Collins and Shester, 2013; Busso, Gregory and Kline, 2013; Diamond and McQuade, 2019; Blanco and Neri, 2025; Almagro, Chyn and Stuart, 2024; Staiger, Palloni and Voorheis, 2024) examining the effects of place-based policies on residential composition, house prices, and other outcomes at the neighborhood level. We directly analyze impacts on residents' *outcomes* using longitudinal data, demonstrating that neighborhood revitaliza-

[5]Our findings on changing opportunity are consistent with recent studies by Garin and Rothbaum (2024) and Chetty et al. (2026*c*), who analyze changes in economic mobility at the county level; we focus on much narrower geographies and policy interventions that can transform local neighborhoods without broader changes in the labor market. Neri (2024) finds that a similar public housing redevelopment program in London improved the educational outcomes of incumbent children.

tion may not only attract higher-income, more productive residents to a particular area and increase property values, but also change the productivity of the area itself, particularly for children.

Finally, our analysis relates to the literature on the determinants of economic mobility, which has investigated a broad range of factors and interventions from the quality of schools to the strength of the local labor market to the role of cash transfers. We show that even holding fixed key family-level and institutional inputs—school spending, labor market conditions, and family resources—one can achieve substantial gains in mobility through neighborhood-level investments. Our analysis provides further evidence that neighborhoods are an important determinant of outcomes and shows that place-based investments offer a promising path for increasing economic mobility.

The paper is organized as follows. The next section summarizes the HOPE VI program. Section III describes our data. We present results of the impacts of HOPE VI on adults in Section IV and children in Section V. Section VI analyzes the mechanisms through which the program improved children's outcomes. Section VII presents evidence on neighborhood integration and children's outcomes beyond public housing sites. Section VIII concludes.

II HOPE VI Program Background

This section briefly summarizes the HOPE VI program, focusing on the processes used to select sites and tenants that are relevant for our research design. For a more comprehensive summary of the HOPE VI program, see Cisneros and Engdahl (2009).

Program Design and Selection of Sites. The federal public housing program has provided subsidized housing for low-income households in government-owned buildings since the 1930s. As the program grew, it was criticized for contributing to the concentration of poverty, crime, and segregation, especially through high-rise developments in cities (Goetz, 2013; Massey and Kanaiaupuni, 1993). In 1992, a commission found that 86,000 of the 1.4 million public housing units nationwide were "severely distressed," with high rates of poverty and crime and poor housing quality (NCSDPH, 1992; Cisneros and Engdahl, 2009).

In response to these concerns, Congress appropriated funding for the HOPE VI revitalization program in 1992, which aimed to redevelop severely distressed public housing developments into lower-density, mixed-income communities.[6] Between 1993 and 2010, HUD awarded 262 revitalization grants (mapped

[6] There was also a second branch of the program that provided funding for the demolition of public housing units and the relocation of affected residents, without funds for neighborhood revitalization. We focus on the Revitalization program (which

in Figure A.1 and listed in Appendix A) totaling \$6.2 billion to local public housing authorities (PHAs) around the country (HUD, n.d.). The \$6.2 billion spent by the federal government on HOPE VI was used to leverage almost \$11 billion in non-HOPE VI funding, the vast majority of which came from private developers (Gress, Joseph and Cho, 2019).

The HOPE VI revitalization program had three primary elements. First, it replaced the housing stock in distressed projects with higher-quality construction integrated into the surrounding neighborhood, as illustrated in Figure A.2. Second, it built mixed-income communities by constructing revitalized public housing units alongside affordable non-public housing (e.g., Low-Income Housing Tax Credit (LIHTC) units and market-rate units).[7] Third, it provided funding for additional programmatic services (such as child care and job training) in the neighborhood. PHAs were directed to use at least 80% of federal funding for housing construction costs and up to 20% for programmatic services.

HOPE VI revitalization grants were awarded based on a competitive process in which PHAs submitted applications to revitalize public housing developments. Eligibility was limited to "severely distressed" public housing projects, defined based on physical deterioration of the structure (as certified by inspectors), levels of poverty, and crime (HUD SuperNOFA, 2000). HUD then selected a subset of applicants to receive grants (Murphy, 2012). HUD evaluated applications based on multiple factors, including need (physical distress of the development), the PHA's capacity to develop and manage affordable housing, funds available from other sources, strength of community and supportive services, and plans for resident relocation (HOPE VI Revitalization NOFA, 2004).

Because funding was insufficient to revitalize all projects that met the "severely distressed" eligibility criteria, 43.3% of applicants never received funding. In 2005, after 95% of HOPE VI grants had been awarded, Turner, Popkin and Kingsley (2005) estimated that 63,000 distressed public housing units had been demolished but between 47,000 and 82,000 severely distressed units remained. The limitations on HOPE VI funding create a control group of comparably distressed public housing projects that were not revitalized. We make use of this control group in our empirical analysis.

Selection of Tenants into Public Housing and HOPE VI Sites. Households interested in public housing generally submit an application to their local PHA listing their preferences over public housing develop-

we refer to simply as HOPE VI for simplicity) throughout this paper, except in Section V.B where we examine the impacts of the Demolition program as an alternative comparison group.

[7]Nationally, 57% of units in redeveloped sites are public housing, 30% are LIHTC units, and 13% are unsubsidized market-rate units (Gress, Joseph and Cho, 2019).

ments (including HOPE VI developments). The PHA screens for eligibility and adds their name to site-based waitlists. As units become available, PHAs select tenants from the waitlists typically based on date of application, sometimes prioritizing tenants with certain characteristics, such as homelessness (HUD, 2019).

HOPE VI sites used a similar process for tenant selection, but some sites implemented stricter screening criteria for tenants, such as criminal background checks, credit checks, drug testing, or participation in economic self-sufficiency programs (Joseph and Chaskin, 2012; Buron et al., 2002; GAO, 2003). These features of the program create scope for differential selection of families into revitalized vs. non-revitalized public housing sites, a challenge that we address in our research design.

There is also selection in exits from public housing. Although public housing is not time-limited, most spells in the public housing program are less than 5 years, and only about 12% of public housing residents stay for more than 10 years (Dantzler, 2021; Freeman, 2005; McClure, 2018; Bahchieva and Hosier, 2001). Some tenants leave voluntarily, for example, as their incomes approach the income limit for eligibility. Others leave because they are evicted or violate their lease agreements such as for nonpayment of rent or criminal activity that threatens other tenants (HUD, 2013).

Because the program reduced density and created mixed-income housing, HOPE VI developments generally had fewer public housing units after revitalization. Original residents were therefore not guaranteed a right to return—for which the program was widely criticized (Popkin et al., 2004). Only 27.6% of original public housing households returned to the development where they previously lived by 2014 (Gress, Joseph and Cho, 2019), as a result of factors including the underlying high rate of churn in public housing, more stringent selection criteria, and the availability of housing vouchers for relocated households (Popkin et al., 2004). The revitalization of neighborhoods through HOPE VI thus largely affected a new set of public housing tenants rather than the original residents who lived in the treated projects prior to revitalization.

The HOPE VI program has been heavily debated since its inception. Advocates argued that creating mixed-income communities would provide new opportunities for low-income families. Critics countered that the program displaced families and provided "False HOPE" because "there is no strong evidence that exposing low-income public housing tenants to higher-income residents has any effect on their employment or educational outcomes" (National Housing Law Project, 2002). We aim to shed light on this debate by analyzing how the program affected residents' outcomes.

III Data

Our primary analysis uses a dataset constructed by linking HUD records on individuals living in public housing from 1995–2019 to federal income tax returns in 1979, 1984, 1989, 1994–1995, and 1998–2019. Individuals are linked across these datasets using protected identification keys (PIK) generated by the Census Bureau for each dataset (Wagner and Layne, 2014). 97.3% of individuals in the HUD data and approximately 90% of individuals in the income tax data are assigned a PIK (Chetty et al., 2026*c*). This section describes how we construct our primary analysis sample, defines key variables, and presents summary statistics.

III.A Sample Definition

We begin by identifying a set of public housing projects for our analysis. Starting from the universe of public housing projects in 1993, we exclude projects that never exceed 50 occupied units by 2000 or whose adult population had an average age of over 55 (to exclude projects that cater to older residents). We also drop non-HOPE VI projects that were either within one mile of a HOPE VI project or experienced a change in project size between the start and end of the panel below the 10th or above the 90th percentile, as these projects may have been affected by HOPE VI or other related programs. The resulting sample includes approximately 200 HOPE VI projects and 2,700 non-HOPE VI projects.

Within these housing projects, we analyze outcomes for two groups: adults (in Section IV) and children (in Section V). When analyzing adults, we focus on individuals who ever lived in one of the public housing projects in our sample when they were 18 or older. When analyzing children, we focus primarily on individuals born between 1978 and 1990 who ever lived in one of the public housing projects in our sample before the age of 18. We limit our analysis to children born in or after 1978 because the first year in which we have individual-level data from HUD is 1995. We limit our analysis to children born in or before 1990 because we generally measure children's outcomes in adulthood between the ages of 29 and 30 and the last year for which we have tax data in this project is 2019.[8]

III.B Variable Definitions

<u>Variable Definitions for Adults</u>

[8] Certain outcomes, such as college attendance and incarceration, are observed at earlier ages; for these outcomes, we include data from more recent cohorts to maximize precision.

Public Housing Residents and Sites. We use the HUD data to identify residents of each public housing project on an annual basis. Individuals appear in the HUD data when they enter or exit public housing or when they are re-certified (typically on an annual basis). We use the address data of public housing residents to measure the geographical footprint of each public housing project, which we then use to identify residents of the same neighborhood after revitalization and to identify nearby residents (see Appendix B for details).

Residential Addresses Outside Public Housing. We use tax returns to identify residential addresses for individuals who do not live in public housing. If an individual does not file taxes, we use any available address data from the HUD records (as these data contain address information from programs other than public housing, such as the Housing Choice Voucher program). If the individual has no address data from either source, we use address data from Infutor between 1980–2019. Infutor is a data aggregator of address data using many sources; see Diamond, McQuade and Qian (2019) for details. We forward fill address histories, assuming that an individual remains in a given address until we have data on a new address.

Household Income. In our primary analysis, we measure annual income of adults using the total pre-tax income of the primary tax filer and their spouse (if applicable) as reported on IRS Form 1040, which we label "household income." We use the term household income for simplicity, but we do not include incomes from cohabiting partners or other household members aside from the primary tax filer's spouse. In years where an individual files a tax return, we define household income as Adjusted Gross Income (AGI). In years where an individual does not file a tax return, we define household income as W-2 income when available. Otherwise, we define household income for non-filers to be zero, so that income is never missing. Household income and all other monetary variables are measured in 2022 dollars, deflated using the CPI-U.

Family Income for Public Housing Residents. The HUD data also contain information on total family income for individuals in public housing. We use this variable to supplement our primary tax-return-based household income measure as controls in certain analyses.

Variable Definitions for Children

Residential Location in Childhood. We identify residential locations for children living in public housing using HUD data (see Appendix B for details). For all other children, we use the address of the adult who claimed the child as a dependent to measure residential location in a given year. For children who are not claimed by any adult in a given year, we impute location using the address of the adult who claimed the child most recently.

Sex and Year of Birth. We measure children's sex and year of birth using the Census Numident.

Race and Ethnicity. We measure children's race and ethnicity using the information they or a household member report on the 2000 and 2010 Census short forms and the American Community Survey (ACS). We prioritize the 2010 Census short form, then the 2000 Census short form, and finally the ACS. We use these data to construct six race and ethnicity groups: non-Hispanic White, non-Hispanic Black, Hispanic, non-Hispanic Asian, non-Hispanic American Indian or Alaskan Native, and non-Hispanic Other.

Parental Income. For children in public housing, we define parental income as the mean household income (as defined above) of their parent over the five years prior to entering the public housing project. For non-public housing residents, we define parental income as the mean household income of the adult(s) who first claimed the child, averaged over five years: 1994, 1995, and 1998–2000 (tax records are unavailable in 1996 and 1997).

Household Income in Adulthood. In our primary analysis, we measure children's income in adulthood as their average household income (as defined above) between the ages of 29 and 30. We calculate an individual's percentile rank based on their position in the national distribution of incomes relative to all others in their birth cohort. We include children with zero income, assigning them the mean rank of the individuals in that group. We focus primarily on within-cohort ranks because ranks stabilize earlier in the lifecycle than levels of income and are less influenced by outliers (Chetty et al., 2014), but also report results for levels of income at ages 29–30.

Individual Income in Adulthood. We define children's individual income in adulthood as their household income minus the W-2 earnings of their spouse if married.

Incarceration in Adulthood. We define individuals as incarcerated if they live in a federal detention center, federal prison, state prison, local jail, residential correctional facility, military jail, or juvenile correctional facility on the day of the 2020 Census.

College Attendance. We measure college attendance using 1098-T forms, which are available from 2011–2014 and 2016–2022 and provide a near-complete roster of college students in the United States (Chetty et al., 2020). We define college attendance as having received a 1098-T form at least once between the ages of 18 and 23.

Additional Characteristics for Public Housing Residents. We obtain information on the following additional characteristics of children and their parents from HUD records: the number of individuals in the household and indicators for having a disability, being homeless at entry, experiencing hardships that qualify households for additional subsidies.

III.C Summary Statistics

Table 1 presents summary statistics for public housing projects that did not receive HOPE VI grants (Column 1) and projects that received HOPE VI grants (Column 2). Summary statistics for non-HOPE VI projects are constructed using a dataset with one observation per project per year from 1990–2007, three years before the first and last HOPE VI grant, respectively. Summary statistics for HOPE VI projects are based on data three years before the grant year in order to measure baseline pre-revitalization characteristics.

Consistent with the program's aims, projects that received HOPE VI funding were more disadvantaged than those that did not. For example, residents of HOPE VI projects have an average household income of \$13,600 according to tax data, compared with \$21,910 for non-HOPE VI residents. 70% of the residents of HOPE VI projects are Black, compared with 38% of residents of non-HOPE VI projects. HOPE VI projects are also larger and more densely populated than non-HOPE VI projects.

Children raised in HOPE VI projects grow up to have household incomes of \$25,680 on average between the ages of 29–30, again lower than their non-HOPE VI counterparts. Approximately 109,000 children in the 1978–90 cohorts live in HOPE VI projects during the period we study.

Table A.1 compares the characteristics of HOPE VI residents to residents of newly constructed market-rate units within HOPE VI sites and other adults living within a one-mile radius of the developments, whom we analyze when examining spillover effects and peer interaction. In this table, we report statistics ten years after revitalization. Residents of revitalized public housing units have lower incomes than residents of market-rate units within the HOPE VI sites, who in turn have lower incomes than adults living in nearby areas outside the developments.

IV Impacts of HOPE VI Revitalization on Adults' Incomes

In this section, we analyze the effect of HOPE VI on the incomes of adults living in the revitalized neighborhoods. We first define our estimands of interest and then present research designs and empirical estimates.

IV.A Target Estimands

Consider a two-period model in which i indexes individuals, t indexes time periods, and n indexes a set of neighborhoods with public housing sites eligible for revitalization. Let $H_n \in \{0,1\}$ denote an indicator for receipt of a HOPE VI revitalization grant. In period $t = 0$ (the pre-period), all neighborhoods are unrevitalized; in period $t = 1$ (the post-period), neighborhoods with $H_n = 1$ are revitalized.

The income of adult i living in neighborhood n in period t is

$$y_i^A = \underbrace{\mu_{nt}^{A0} + \mu_n^A H_n \mathbb{1}\{t > 0\}}_{\text{Neighborhood Effect}} + \underbrace{\theta_i^A}_{\text{Indiv. Fixed Effect}}, \tag{1}$$

where the first component is the neighborhood's causal effect on earnings in period t. The neighborhood effect includes factors unrelated to revitalization such as differences in local job availability in period t, μ_{nt}^{A0}, and any incremental impact of neighborhood revitalization on earnings, μ_n^A. The second component, θ_i^A, captures individual-level determinants of income such as education, which we assume are fixed over the time horizon we analyze.

The average income of adults living in neighborhood n in period t can be written as

$$\bar{y}_{nt}^A = \underbrace{\mu_{nt}^{A0} + \bar{\theta}_{nt}^{A0}}_{\phi_{nt}} + (\underbrace{\mu_n^A + \Delta\bar{\theta}_n^A}_{\pi_n}) H_n \mathbb{1}\{t > 0\}, \tag{2}$$

where $\bar{\theta}_{nt}^{A0}$ is the average individual effect among people who live in neighborhood n absent revitalization and $\Delta\bar{\theta}_n^A$ denotes how revitalization changes the average individual effect through resorting. Let ϕ_{nt} denote the average income in the neighborhood absent HOPE VI. The parameter π_n represents the effect of revitalization on mean incomes of residents in neighborhood n, which combines the causal effect of revitalization on a given individual's income (μ_n^A) and sorting effects arising from changes in the set of residents who choose to live in neighborhood n after it is revitalized ($\Delta\bar{\theta}_n^A$).

Effect of Revitalization on Mean Neighborhood-Level Income. Our first target estimand in this section is the average treatment effect of HOPE VI on mean neighborhood-level incomes:

$$\bar{\pi} = \mathbb{E}[\pi_n \mid H_n = 1]. \tag{3}$$

To see how $\bar{\pi}$ can be identified, for any outcome variable x, let

$$D(x \mid t) \equiv \mathbb{E}[x \mid t, H_n = 1] - \mathbb{E}[x \mid t, H_n = 0]. \tag{4}$$

denote the cross-sectional difference between the expected value of the outcome in treated ($H_n = 1$) and control ($H_n = 0$) neighborhoods in period t. The difference in expected mean incomes between residents in treated and control sites in the post-period can be written as the sum of the treatment effect of interest $\bar{\pi}$ and a selection term reflecting underlying differences in mean incomes between treated and control neighborhoods unrelated to the intervention (e.g., due to differences in job availability):

$$D(\bar{y}^A_{nt} \mid t = 1) = \bar{\pi} + D(\phi_{nt} \mid t = 1). \tag{5}$$

An ideal experiment would randomly assign HOPE VI grants to a subset of distressed public housing sites. Random assignment would guarantee that treated and control sites are comparable in terms of other characteristics such as job availability: $D(\phi_{nt} \mid t = 1) = 0$. As a result, the post-period comparison of mean incomes in equation (5) would identify $\bar{\pi}$.

Effect of Revitalization on a Given Individual's Income. Next, we identify how much of the change in mean income at the neighborhood level is due to a causal effect of HOPE VI on a given individual's income vs. changes in the types of individuals living in revitalized neighborhoods. Our second target estimand is the average treatment effect of HOPE VI on the causal neighborhood effect on adults' incomes:

$$\bar{\mu}^A = \mathbb{E}[\mu^A_n \mid H_n = 1]. \tag{6}$$

An ideal experiment to identify $\bar{\mu}^A$ would additionally randomly assign individuals to treated vs. control sites. This second level of randomization would ensure that $\mathbb{E}[\Delta\bar{\theta}^A_n \mid H_n = 1] = 0$, implying that $D(\bar{y}^A_{nt} \mid t = 1) = \bar{\mu}^A$.

The next two subsections discuss how we estimate $\bar{\pi}$ and $\bar{\mu}^A$ in observational data in turn.

IV.B Effect on Mean Neighborhood-Level Income

Estimating $\bar{\pi}$. We approximate the ideal experiment to identify the effect of HOPE VI on mean neighborhood-level incomes ($\bar{\pi}$) using a difference-in-differences design with matched controls. We first match treated projects to control projects on ex-ante proxies for distress such as poverty rates. We then compare changes in adults' mean incomes in treated vs. control neighborhoods before vs. after the HOPE VI intervention using a difference-in-differences (DD) estimator. Let

$$DD(x) \equiv D(x \mid t=1) - D(x \mid t=0) \tag{7}$$

denote the difference-in-differences of the expected value of an outcome x between HOPE VI and control sites in the post-period vs. the pre-period. The difference-in-difference of the average income of adults living in neighborhood n can then be written as

$$DD(\bar{y}^A_{nt}) = \bar{\pi} + DD(\phi_{nt}). \tag{8}$$

The identification assumption underlying our research design is the following.

Assumption 1a: Parallel Trends in Adults' Incomes in HOPE VI and Matched Control Sites. Other determinants of mean neighborhood incomes ϕ_{nt} evolve identically in treated and control sites:

$$DD(\phi_{nt}) = 0. \tag{9}$$

Assumption 1a implies that the DD estimator in equation (8) recovers the target estimand: $DD(\bar{y}^A_{nt}) = \bar{\pi}$.

We now describe how we implement each of the two steps in the design: matching and difference-in-differences.

Matching. Although the HOPE VI program targeted distressed public housing projects, many similar projects did not receive a grant because of funding constraints. Figure A.3.A plots the distribution of pre-grant poverty rates (defined as the share with household income below $15,000) for adults in HOPE VI and non-HOPE VI projects. HOPE VI projects had poorer residents on average, but many non-HOPE VI projects had comparably impoverished populations. The overlap in the two distributions creates the scope for comparing projects treated by HOPE VI to observably similar untreated projects.

To construct such matched controls, we implement the following procedure separately for each HOPE VI grant award year g between 1993 and 2010. We start with the set of projects that either received a HOPE VI award in a year g or never received a HOPE VI award. We then estimate a logistic regression of HOPE VI award receipt on the following baseline project characteristics: mean household incomes of public housing residents (using both the HUD public housing data and tax data measures as separate variables), racial composition (proportion of Non-Hispanic White and Non-Hispanic Black residents), and project size (number of residents). These characteristics are measured in year $g-3$, three years before the grant year being analyzed.[9] We weight the regression by the baseline project size. Using the predicted values from this regression (propensity scores for treatment), we select with replacement the five non-HOPE VI projects with the closest propensity scores (nearest neighbors) as controls for each HOPE VI project. We then define our analysis sample as HOPE VI projects and all non-HOPE VI projects that were selected as matched controls. The matched control projects are assigned the (placebo) grant year g_n corresponding to the HOPE VI project to which they are matched.

The dataset that results from this process includes one observation for each project awarded a HOPE VI grant and a separate observation for each instance a matched control project appears in a given grant year. Projects are unique within grant years but matched control projects may appear in multiple grant years. To simplify exposition, we use n to denote a project-by-grant-year observation, but refer to n as a "project" in what follows.[10]

The matched control projects are similar but not identical to the corresponding HOPE VI projects on observables. To adjust for remaining differences in observables, we re-estimate the logistic regression of HOPE VI award receipt described above within the subsample of HOPE VI and matched control projects (again, separately in each grant year). We then construct propensity scores p_n for each project n as the predicted values from this regression. We use weights of $\omega_n = 1$ for treated projects and $\omega_n = p_n/(1-p_n)$ for control projects throughout our empirical analysis (Rosenbaum and Rubin, 1983, 1984).[11,12]

The matched control projects are observably similar to HOPE VI projects, both on the variables used in the matching procedure (by construction) as well as on several other dimensions, as can be seen by comparing Columns 2 and 3 of Panel A in Table 1.[13] For example, Figure A.3.B shows that the distribution of project-level poverty rates in the matched control projects (weighted by inverse propensity scores) is

[9] For example, when estimating this regression for the 1993 HOPE VI grants, we measure baseline characteristics of both HOPE VI and non-HOPE VI projects in 1990. Before 1995, we use publicly available data from HUD User to measure the average family income of the public housing residents in 1993, as HUD data are not available before 1995.

[10] We cluster standard errors in all regressions at the level of the project, not the project by grant year, to account for the fact that matched control projects might appear multiple times in the sample across different grant years.

[11] For analyses based on project-level data, we normalize these weights to account for differences in baseline population, so that larger projects receive proportionally more weight and the weighted size of the control group equals that of the HOPE VI group within each grant year. We do so by defining a population-adjusted weight as the product of the weights ω_n and the baseline population for each project. For control projects, we also multiply by the ratio of the sum of this product in the HOPE VI projects to the sum in the control projects, within each grant year.

[12] This second rebalancing step has little impact on our results; the estimates are very similar if we weight the five nearest neighbors equally. Our estimates are also insensitive to the specification of the nearest-neighbor matching procedure, such as changing the number of matches used (e.g., to 3 or 10) or using additional observables for matching.

[13] The children's outcomes in Panel B of Table 1 are measured post treatment and hence should not be compared to evaluate balance.

closely aligned with that in the HOPE VI sites, unlike the raw comparison in Figure A.3.A.

To evaluate balance between the HOPE VI and matched control projects more systematically, we regress baseline characteristics (normalized to have a mean of 0 and standard deviation of 1) on an indicator for HOPE VI receipt, controlling for grant year fixed effects and weighting by inverse propensity scores. Figure 1.B presents estimates from such regressions for 15 baseline variables. The matched controls are very similar to HOPE VI sites on all dimensions, unlike the average housing project (Figure 1.A), even for the ten variables that were not used in the matching procedure itself, such as household size, share disabled, and the incomes in adulthood of children who left the public housing projects prior to revitalization. Only one of the fifteen balance tests is statistically significant at the 10% level.

Difference-in-Differences. We use a stacked difference-in-differences estimator to compare changes in the incomes of residents living in HOPE VI sites around the grant year to changes in the incomes of residents living in the matched control sites. For each grant year g from 1993 to 2010, we collapse the individual-level data (computing a statistic such as mean incomes) to create a panel that includes one observation per project and calendar year (s). We stack these panel datasets across grant years to generate a single dataset with one observation per grant year by housing project and calendar year.

Using the stacked project-level panel, we estimate the following regression specification:

$$y_{ns} = \sum_{j \neq -3} \beta^j \mathbb{1}(j = s - g_n) H_n + \delta_n + \lambda_{g_n s} + \varepsilon_{ns}, \tag{10}$$

where y_{ns} denotes an outcome in project n in calendar year s (e.g., mean income), δ_n is a project fixed effect, $\lambda_{g_n s}$ is a calendar-year-by-grant-year fixed effect, H_n is a treatment indicator equal to one if site n received a HOPE VI grant, and $\mathbb{1}(j = s - g_n)$ is an indicator for whether calendar year s is j years after the grant year g_n. We omit $j = -3$ from the set of indicators so that the β^j coefficients can be interpreted as differential changes in outcomes in the treated group relative to the control group, normalized to baseline values measured three years before the grant year. Intuitively, equation (10) is a standard difference-in-differences specification within each grant year, pooled across grant years to obtain a single estimate of the path of treatment effects. We estimate equation (10) using weighted least squares using the propensity score weights ω_n (defined above), with standard errors clustered at the project level. We restrict the sample to projects where we observe the outcome y_{ns} for at least one year both before and after the grant year.

The estimator in equation (10) identifies the causal effect of the HOPE VI program on average incomes

$\bar{\pi}$ under Assumption 1a. We evaluate the validity of this assumption after presenting baseline results.

Results. The green series in Panel A of Figure 2 shows how the incomes of adults evolved in HOPE VI revitalized sites relative to control sites by plotting estimates of β^j from equation (10). The sample includes all adults living in the Census blocks in the footprint of the original housing project (defined as in Section III.B) in each event year, including those living in market-rate housing post-revitalization within the footprint of the original public housing project. Over the ten years prior to a HOPE VI grant, the series is flat, showing that treated and control sites had very similar trends in mean incomes prior to revitalization.

There was little change in mean resident incomes in HOPE VI sites relative to control projects in the first three years following the grant award when demolitions and revitalization were being undertaken and new market rate units were not yet available for occupancy.[14] Mean incomes then increased in HOPE VI revitalization sites relative to control projects starting 4 to 5 years after the grant award as new construction combining public housing with both market-rate units and units with shallower subsidies (e.g., LIHTC) that catered to higher-income families were occupied.

Mean annual incomes in revitalized neighborhoods continued to rise relative to the control neighborhoods over the next decade, leading to a \$6,000 increase in mean incomes in revitalized neighborhoods 15 years after the grant, a 45% increase relative to average income in matched control sites. We find similar results when analyzing impacts on poverty rates instead of mean income: poverty rates in revitalized areas fell by 12 percentage points (Figure A.5), consistent with the findings of Tach and Emory (2017) using publicly available survey data.

We assess the sensitivity of our findings to alternative specifications by estimating variants of the OLS regression model specified in equation (10) in Table A.2. Based on the non-parametric evidence in Figure 2, which shows stabilization of treatment effects after 10 years, we parametrize the coefficients in three periods: before the award ($j < 0$), short-run ($j \in [0,5]$), and long-run ($j \in [6,15]$). Column 1 of Panel A of Table A.2 replicates the baseline estimates in the green series shown in Figure 2. Panel B replicates Panel A but compares HOPE VI projects to housing projects that applied for HOPE VI but were not granted an award (failed applicants). We find very similar estimates when using failed applicants as controls.

[14]Total population declined immediately after the HOPE VI grant was awarded and began to recover only 5 years later, once people started moving into the newly constructed housing units (Figure A.4).

IV.C Causal Effects on Individuals' Incomes vs. Sorting

The increase in mean neighborhood-level income in Figure 2 following revitalization could arise from an improvement in the neighborhood's causal effects on income ($\bar{\mu}^A > 0$) or from individuals with higher latent earnings potential moving into revitalized neighborhoods ($\mathbb{E}[\Delta\bar{\theta}_n^A \mid H_n = 1] > 0$). We use two approaches to identify $\bar{\mu}^A$ and distinguish between these two channels.

First, we directly shut down the sorting channel by analyzing the outcomes of a fixed set of individuals over time, guaranteeing that $\Delta\bar{\theta}_n^A = 0$ in each revitalized neighborhood n. In particular, we identify adults living in public housing one year before the grant (in event year $j = -1$) and track their incomes over time, regardless of where they subsequently move. The orange series in Figure 2 shows that HOPE VI had no impact on the incomes of these pre-revitalization public housing residents. The estimates are precise enough to rule out gains larger than 14% at the 5% level (Column 2 of Table A.2).

This finding suggests that much of the gain in mean neighborhood-level incomes is driven by sorting rather than causal effects on a given individual's income. However, it does not directly identify $\bar{\mu}^A$ because many of the original residents moved to different neighborhoods after revitalization.[15] The change in their incomes therefore may capture not just the change in the causal effect in revitalized neighborhoods $\bar{\mu}^A$ but also differences in neighborhood effects μ_{nt}^{A0} in the neighborhoods they move to relative to their original public housing projects.

We therefore turn to a second, complementary approach that controls for compositional changes in earnings potential $\Delta\bar{\theta}_n^A$ by conditioning on individuals' observable characteristics. The most direct channel through which HOPE VI altered resident composition in each neighborhood was by replacing some public housing units—which are restricted to lower-income families—with market-rate units available to the general public. To account for the compositional change arising from this change in housing availability, we examine how incomes evolved around revitalization among individuals living in public housing units. The navy series in Figure 2 restricts the sample to current public housing residents in each event year j and plots the mean change in income from the year before each individual moved into public housing to year j (thereby netting out individual fixed effects θ_i^A as in our analysis of the original residents). There was no significant growth in incomes for individuals living in revitalized public housing units relative to matched

[15] Only 11.9% of adults who lived in a HOPE VI project before the grant award lived in the same neighborhood 5 years later (Figure A.6). While some moves are attributable to the displacement effects from the demolitions, most of these families would have moved even absent the demolitions, as seen by the comparably high migration rates from control neighborhoods (Figure A.6).

controls (Column 3 of Table A.2), consistent with our findings of null effects for prior public housing residents.[16]

We conclude that HOPE VI significantly increased mean neighborhood incomes by attracting higher-income adults to move into market-rate units ($\bar{\pi} = \mathbb{E}[\Delta\bar{\theta}_n^A \mid H_n = 1] > 0$), but had little causal impact on the incomes of adults living in public housing themselves ($\bar{\mu}^A = 0$).

V Impacts of Revitalization on Children's Outcomes

In this section, we estimate the impacts of the HOPE VI program on children's long-run outcomes. We begin by defining the estimand of interest. Next, we present empirical estimates from a simple matched difference-in-differences estimator that compares changes in the long-term outcomes of children who grew up in HOPE VI projects before vs. after revitalization to matched control projects. We then present estimates from a movers research design that relaxes the identification assumptions underlying our baseline estimator. The last subsection contextualizes the magnitudes of the treatment effect estimates.

V.A Target Estimand

Statistical Model. Building on the childhood exposure effect model in Chetty and Hendren (2018), we model children's outcomes in adulthood as the sum of two factors: the quality of the neighborhoods in which they grow up from birth until age 18 and the effects of all other factors, such as family inputs. Child i's income in adulthood (measured after age 18) is

$$y_i = \sum_{a=1}^{18} \mu_{n(i,a),t(i)} + \theta_i, \tag{11}$$

where $n(i,a)$ denotes the neighborhood n in which i lives at age a; $t(i) \in \{0,1\}$ denotes the time period in which i was a child; μ_{nt} is the causal effect of spending a year growing up in neighborhood n in period t; and θ_i captures family inputs and all other determinants of income.[17] As above, let $H_n \in \{0,1\}$ denote an indicator for receipt of a HOPE VI grant. In period $t = 0$, all neighborhoods are unrevitalized; in period $t = 1$, neighborhoods with $H_n = 1$ are revitalized.

[16] As above, we find similar results when using failed applicants as controls (Panel B of Table A.2).

[17] This specification assumes that the childhood exposure effect μ_{nt} is constant for ages $a \leq A$ and zero thereafter. Recent work suggests that neighborhood exposure effects may be larger during adolescence than earlier in childhood (Deutscher, 2020; Chetty et al., 2026*b*). We augment our model to permit heterogeneous exposure effects by age below and find larger impacts of revitalization during adolescence as well, but start with the constant-exposure-effect by age to simplify exposition.

The neighborhood exposure effect μ_{nt} consists of two components:

$$\mu_{nt} = \mu_{nt}^0 + \tau_n H_n \mathbb{1}\{t > 0\}, \tag{12}$$

where μ_{nt}^0 captures background factors unrelated to revitalization (such as expenditure on local schools or labor market conditions) and τ_n is the incremental impact of HOPE VI revitalization on the neighborhood exposure effect.

Target Estimand. Our goal is to estimate the average treatment effect of HOPE VI on the neighborhood exposure effect in treated neighborhoods:

$$\bar{\tau} = \mathbb{E}[\tau_n \mid H_n = 1]. \tag{13}$$

This target estimand differs from that identified in prior studies of the effects of HOPE VI revitalization on children's outcomes (Chyn, 2018; Haltiwanger et al., 2024). Prior studies follow children living in public housing prior to demolition and examine their outcomes in adulthood after they were displaced to other neighborhoods, i.e., how the average μ_{nt} of the neighborhoods to which children who were displaced by HOPE VI compares to μ_{n0} in public housing projects. In contrast, we seek to identify $\mu_{n1} - \mu_{n1}^0$, the effect of HOPE VI on a *given* neighborhood's causal effect on children's outcomes.

Ideal Experiment. To see how we can identify $\bar{\tau}$ using a randomized experiment, consider a child i who spends e_{int} years of their childhood living in neighborhood n in time period t. We can write child i's earnings as:

$$y_i = e_{int}\mu_{nt} + \underbrace{(18 - e_{int})\bar{\mu}_{i,-n} + \theta_i}_{\nu_{int}}, \tag{14}$$

where $\bar{\mu}_{i,-n} \equiv \frac{1}{18-e_{int}} \sum_{a:n(i,a)\neq n} \mu_{n(i,a),t}$ is the average neighborhood effect of all other neighborhoods that i lives in during childhood. To simplify notation, we group all factors that affect children's outcomes other than the causal effect of neighborhood n into a single term, ν_{int}.

The difference in the mean earnings in adulthood of children who spent e years growing up in revitalized vs. unrevitalized sites in the post period $t = 1$ is

$$\begin{aligned} D(y_i|e_{in1} = e, t = 1) &= eD(\mu_{nt} \mid t = 1) + D(\nu_{in1} \mid e_{in1} = e, t = 1) \\ &= e\bar{\tau} + eD(\mu_{nt}^0 \mid t = 1) + D(\nu_{in1} \mid e_{in1} = e, t = 1), \end{aligned} \tag{15}$$

where the first line follows from equation (14) and the second follows from equation (12). Intuitively, the difference in mean earnings between treated and control sites consists of three components: the treatment effect of HOPE VI, baseline differences in neighborhood effects between treated and control sites (e.g., local school expenditures), and differences in unobservables arising from family inputs or the quality of other neighborhoods in which children grow up. Analogous to the identification of impacts on adults' outcomes in Section IV.A, an ideal experiment to identify impacts of revitalization on children's outcomes requires two levels of randomization to purge the two selection terms:

(i) Random assignment of HOPE VI grants across projects, which guarantees $D(\mu^0_{nt} \mid t=1) = 0$.

(ii) Random assignment of children across projects, which guarantees $D(\nu_{in1} \mid e_{in1} = e, t = 1) = 0$.

With these two levels of random assignment, comparing children's mean earnings in adulthood between treated and control projects in the post period would identify $e\bar{\tau}$, the effect of HOPE VI on the neighborhood exposure effect multiplied by years of exposure.

V.B Difference-in-Difference Estimates

To identify $\bar{\tau}$ in observational data, we begin with an estimator that adapts the matched difference-in-differences estimator that we used to analyze impacts on adults' income in Section IV to children. We compare changes in the outcomes in adulthood of children who grew up in public housing units before vs. after HOPE VI revitalization to analogous changes in matched control projects.[18] Because some of the children who were living in HOPE VI sites prior to revitalization continue to live there after revitalization and are thereby partially treated, we focus on two distinct subsamples of these children: (1) *prior residents:* children who moved out of the public housing project n at least two years before the grant award and thus were exposed to project n only pre-revitalization and (2) *new residents:* children who moved into the public housing project n in the year of the HOPE VI grant g or later and thus were exposed to project n only post-revitalization. We compare the difference in earnings in adulthood y_i for new residents vs. prior residents in revitalized vs. matched control sites.

Formally, consider the set of children who spend e years in HOPE VI or matched control public housing units either before or after the intervention (i.e., those with $e_{in0} = e$ or $e_{in1} = e$ and $e_{in0} \times e_{in1} = 0$). Among

[18]Throughout this section, unless otherwise noted, we analyze only children living in public housing units even after revitalization, excluding those who move into newly built market-rate units at the same project site.

this subset of children, equation (15) implies that the difference-in-differences in mean earnings in adulthood is

$$DD(y_i \mid e_{int} = e) = e\bar{\tau} + eDD(\mu_{nt}^0) + DD(\nu_{int} \mid e_{int} = e). \tag{16}$$

We make two assumptions to identify $\bar{\tau}$.

Assumption 1b: Parallel Trends in Children's Outcomes in HOPE VI and Matched Control Sites. Other determinants of children's outcomes μ_{nt}^0 evolve identically in HOPE VI and matched control sites:

$$DD(\mu_{nt}^0) = 0. \tag{17}$$

Assumption 2. Conditional on a vector of observable characteristics X_{it}, there is no differential sorting into HOPE VI vs. matched control public housing projects after the grant award:

$$DD(\nu_{int} \mid e_{int} = e, X_{it}) = 0. \tag{18}$$

Assumptions 1b and 2 imply that the difference-in-differences estimator $DD(y_i \mid e_{int} = e, X_{it}) = e\bar{\tau}$. Assumption 1b, which is analogous to Assumption 1a made above to identify impacts on adults' mean incomes, ensures that any differential change in average outcomes across treated vs. control areas is due to the treatment effect of HOPE VI. Assumption 2 further requires that changes in unobservable family-level determinants of children's outcomes are the same on average in HOPE VI and control sites (conditional on observable characteristics X_{it} such as parent income). This assumption ensures that any change in outcomes can be attributed to changes in neighborhood effects μ_{nt} resulting from HOPE VI rather than changes in the types of families living in revitalized areas. We first present set of baseline DD estimates under these assumptions and then evaluate their validity using alternative estimators.

Implementation. We implement the DD estimator in a dataset with one observation per child per year in which they appear in a HOPE VI or matched control public housing project during childhood. We estimate the following regression model, restricting the sample to prior residents and new residents:

$$y_i = \sum_{j \neq -3} \beta^j \mathbb{1}(j = s - g_n) H_n + \delta_n + \lambda_{g_n s} + \phi X_{in} + \varepsilon_{ins}, \tag{19}$$

where child i lives in project n in calendar year s, y_i denotes child i's outcome in adulthood (e.g., earnings at age 30), δ_n is a project fixed effect, $\lambda_{g_n s}$ is a grant-year-by-calendar-year fixed effect, H_n is a treatment indicator equal to one if project n received a HOPE VI grant, $\mathbb{1}(j = s - g_n)$ is the indicator function for whether calendar year s is j years after the grant award year g_n, and X_{in} is a set of individual-level covariates. As above, the specification is estimated via weighted least squares using inverse propensity scores as weights, with standard errors clustered at the project level.[19]

To account for potential selection effects arising from differences in family characteristics or time spent in other neighborhoods (v_{int}), we control for the following vector of characteristics X_{in} in our baseline specification: parental income, upward mobility (the mean income rank of children growing up at the 25th percentile) in the Census tract where the child lived prior to moving into public housing, and the number of years spent in public housing during childhood. On average, children in our new residents sample spent 5 years growing up in public housing among HOPE VI sites; hence the estimates of β^j should be interpreted as the effect of HOPE VI revitalization on outcomes for children who spent 5 out of 18 years of their childhood in a revitalized rather than unrevitalized public housing unit.

Results. The green series in Figure 3.A presents estimates from equation (19), using children's mean income ranks at ages 29–30 as the outcome variable. The estimates for the prior residents are depicted in the years before the HOPE VI grant (β^j for $j \in [-10, -2]$). The coefficients exhibit no trend pre-revitalization, supporting the assumption that there were no underlying differences in trends in neighborhood-level outcomes between the treated and matched control projects (Assumption 1b).[20]

The estimates for the new residents, who moved in after revitalization, are shown in the years after the HOPE VI grant (β^j for $j \in [0, 10]$). Children's outcomes improved significantly post-revitalization in HOPE VI public housing developments.[21] Children living in revitalized public housing units ten years

[19] This model is analogous to that in equation (10) to estimate effects on adults, except that it includes the individual-level covariates, X_{in}. Because the covariates vary at the individual level, we estimate the model in the individual-level microdata rather than in a dataset collapsed to the project-by-year level. Omitting covariates and estimating the model in a project-by-year level dataset yields very similar results to those reported below.

[20] In this figure, we normalize the difference between the HOPE VI treated and matched control sites to 0 in event year $j = -3$, but there is no difference in the level of children's income ranks in adulthood prior to revitalization between the two groups, as shown in Figure 1.A. We did not directly match on this outcome, showing that our approach of matching on other observables is adequate to achieve balance in these ex-post outcomes for children pre-revitalization.

[21] In Figure A.7, we expand the sample to include children who lived in projects in the year before the HOPE VI grant ($j = -1$) and who were displaced to other neighborhoods when their developments were demolished. We estimate that the HOPE VI treatment increased income ranks in adulthood for this group of children by 1.52 percentiles, as shown by the gray dot at -1, consistent with the findings of Chyn (2018) and Haltiwanger et al. (2024), who also find evidence of long-run earnings gains for children displaced by public housing demolitions. This treatment effect largely reflects the effect of moving to *other* higher-opportunity neighborhoods rather than changes in the treatment effect of the revitalized public housing sites themselves, since very few of these children grew up in the HOPE VI sites post-revitalization.

after the grant award had household incomes that are 3.45 percentiles higher on average at age 29–30 than children of parents with comparable incomes who lived in matched control sites.[22]

Table 2 quantifies the treatment effects and assesses the sensitivity of the estimates to alternative specifications. Each column of the table reports estimates from an OLS regression analogous to equation (19), replacing the set of event time indicators with a single post-revitalization indicator $\mathbb{1}(s - g_n > 0)H_n$. The coefficient on the post-revitalization indicator is an estimate of growing up (for 5 years of childhood on average) in a revitalized public housing site.

Column 1 of Table 2 uses the same sample and controls as Figure 3.A. We estimate that, on average, children who grew up in revitalized projects (pooling all ten post-revitalization years in Figure 3.A) had 2.41 percentile higher household income ranks at ages 29–30. This estimate is significantly different from 0 with $p < 0.01$. Column 2 shows that we obtain very similar estimates using a more parsimonious specification that replaces the large number of project fixed effects δ_n with a single indicator for being a HOPE VI site. Column 3 shows that the estimates remain similar when we fully interact all independent variables with the de-meaned value of exposure—a linear approximation to the conditioning on exposure in equation (16)—rather than simply controlling for exposure linearly. Column 4 replicates Column 1 using the level of household income (in dollars rather than ranks) as the outcome. We estimate that HOPE VI revitalization increased the household incomes of children who grew up in revitalized public housing units by $3,633, which is 16% of mean income in the control group ($22,740) at ages 29–30 (Figure A.8). Column 5 shows that the estimated effect on individual income (excluding spousal earnings) is $2,935; the treatment effect on household income is slightly larger because of a small, statistically insignificant increase in rates of marriage.

We find positive impacts of revitalization on non-monetary outcomes as well. Figure 4.A replicates Figure 3 using an indicator for attending college between the ages of 18 and 23 as the outcome. Growing up in a revitalized public housing unit increased children's chances of attending college by 6.0 pp (17% of the control group mean of 35.3 pp), as shown in Column 6 of Table 2. Figure 4.B shows that boys who grow up in revitalized units were 1.3 pp (20%) less likely to be incarcerated in 2020 (when they are between the ages of 20 and 30). There was no impact on incarceration rates for girls, since their baseline pre-revitalization rates of incarceration are already very close to zero. Mirroring these reductions in incarceration rates,

[22] The treatment effects increase slightly over time, which may be driven in part by the time it takes for revitalization to be completed as well as changes in the period of childhood during which children are exposed to revitalized neighborhoods, as we show below.

national data from police department records show that rates of violent crime fell by 41% in revitalized projects relative to control projects (Figure A.9), consistent with prior work examining the effects of public housing demolitions on crime in Chicago (Aliprantis and Hartley, 2015; Sandler, 2017).

Although impacts on incarceration differ by sex, treatment effects on income do not differ by sex, as shown in Column 1 of Table A.3, where we replicate the specification in equation (19), interacting all the independent variables with sex.[23] Column 2 replicates the same specification, interacting the independent variables with parental income rank instead of sex. We find no significant heterogeneity in treatment effects of revitalization by parental income (within the public-housing-eligible sample of low-income families). In Column 3, we further analyze whether treatment effects are heterogenous by the degree of disadvantage that children face by predicting children's income ranks using a rich set of family characteristics beyond parental income, including race, household size, disability status, and an indicator for experiencing homelessness prior to entering public housing (see the notes to Table 2 for the full list). Again, we find no evidence of heterogeneity in treatment effects by this predicted income measure.

Evaluating the Identification Assumptions. The preceding estimates can be interpreted as the treatment effect of HOPE VI only if the identification assumptions discussed above hold. We assess the validity of Assumption 1b—which requires parallel trends in other neighborhood characteristics among treated and control sites—in three ways. First, we control for a rich set of additional neighborhood characteristics such as poverty rates and racial demographics (listed in the notes to Table 2) interacted with a post-treatment indicator. Including these controls has little impact on our treatment effect estimate (Column 9 of Table 2).

Second, we consider alternative control groups, starting with failed HOPE VI applicants, as in Section V. Figure 3.B and Column 10 of Table 2 show that we obtain nearly identical estimates (with slightly larger standard errors, due to the smaller size of the control group) when using failed applicants as controls. As another comparison, we consider public housing sites that were partially demolished as part of the HOPE VI program but where there was no active effort to revitalize the neighborhood through new units or programs (a "demolition only" treatment). The navy series in Figure 3.B replicates the baseline estimator in equation (19) for demolition-only projects, using the same matching procedure discussed in Section IV.B to identify control projects for demolition-only sites. We see no change in the outcomes of children who

[23]The fact that revitalization improved earnings for both boys and girls but reduced incarceration rates only for boys indicates that the income gains cannot be mechanically explained by lower rates of incarceration and must instead arise from downstream benefits from growing up in neighborhoods with less crime (as documented by Sharkey and Torrats-Espinosa, 2017) or other channels unrelated to crime.

grow up in public housing sites after parts of their housing developments were demolished. Put differently, using partially demolished sites as a control group for revitalized sites yields very similar results, further supporting the view that our results are not sensitive to the choice of control projects.[24]

Third, we compare changes in children's outcomes in revitalized public housing units to changes in nearby neighborhoods. If other factors μ_{nt}^{0}—such as the quality of local schools—improved contemporaneously in areas that received HOPE VI revitalization grants relative to controls, we might see improvements in the outcomes of children raised in the neighborhood surrounding the revitalized public housing project as well (since nearby children attend the same public schools). Figure 5 first plots the treatment effect among public housing residents using the specification in Column 9 of Table 2 and then replicates exactly the same specification using sets of children raised in distinct concentric rings ("donuts") around the housing project.[25] The estimated treatment effect decays sharply with distance. The impact falls by 86% for children living just 0.2 miles away from the revitalized project, and is near 0 for children raised in a 0.4–1 mile radius around the project. This spatial comparison effectively shows that using nearby residents as a control group for the outcomes of children in revitalized public housing again yields similar results, further supporting Assumption 1b.[26]

Next, we evaluate Assumption 2, which requires that the unobserved family-level determinants of children's outcomes are comparable in HOPE VI and control sites conditional on our baseline controls. To do so, we first predict each child's adult income in our control sample using a rich set of pre-determined family characteristics observed in public housing files upon entry into public housing: number of household members, and indicators for sex, race, year of birth, having a household member with disability, an elderly household member, female-headed household, whether homeless, whether experienced hardship, and whether observed in only one project. We then estimate a variant of our baseline specification that drops the individual-level covariates and uses *predicted* income ranks instead of actual income ranks as the outcome.

The orange series in Figure 3.A plots these estimates and show there is no difference in predicted children's adult incomes between treatment and control both before and after revitalization. Consistent with this result, Column 11 of Table 2 shows that controlling for the additional family-level characteristics

[24] In addition to providing a method of assessing robustness, these results for demolition-only sites show that the mechanism underlying the HOPE VI revitalization effect is not simply a reduction in the density of high-poverty public housing in a neighborhood, but rather a consequence of investment in revitalization, as we discuss further in Section VI.

[25] To make estimates comparable across samples, we use address histories from the tax data to measure the timing of moves for both public housing and nearby residents in Figure 5.

[26] The fact that there were no changes in outcomes for nearby residents also suggests that the treatment effects of HOPE VI do not spill over to neighbors living near revitalized public housing projects, a point we revisit in Section VII.

described above has little impact on the estimated treatment effect of HOPE VI on children's incomes. Column 12 shows that removing all covariates from the specification also yields very similar estimates.

These findings show that the families who move into revitalized public housing units do not appear to be differentially selected relative to those in control units on characteristics that we observe in public housing files. These characteristics contain considerable additional information about children's incomes in adulthood, tripling the R-squared in the prediction regression relative to a regression based only on our baseline controls from 0.024 to 0.075. However, since most of the variation in children's outcomes remains unexplained by these variables, there is still considerable scope for selection on unobservables. We therefore turn to a second research design that relaxes Assumption 2 to address such selection concerns directly.

V.C Movers Exposure Effect Estimates

In our second research design, we compare the outcomes of children who move into and out of public housing projects at different ages to estimate the causal effect of an additional year of childhood exposure to a given project (μ_{nt}), as in Chetty and Hendren (2018). We then estimate the mean change in μ_{nt} in revitalized projects relative to control projects, $DD(\mu_{nt})$, using the same difference-in-differences design as above.

Formally, let β_{nt} denote the coefficient from an OLS regression of y_i on e_{int} for children who live in public housing units in neighborhood n in period t. Equation (14) implies that

$$\beta_{nt} = \mu_{nt} + \eta_{nt}, \tag{20}$$

where $\eta_{nt} = \frac{Cov(v_{int}, e_{int})}{Var(e_{int})}$. Intuitively, regressing outcomes on length of exposure yields a coefficient that reflects the sum of the exposure effect μ_{nt} and a selection term η_{nt} reflecting the covariance between unobservable determinants of income and exposure. For example, if children with better family inputs stay in public housing for fewer years, $\eta_{nt} < 0$.

We identify the effect of HOPE VI on μ_{nt} by estimating the differential change in the estimated exposure effect β_{nt} in treated vs. matched control sites before vs. after revitalization, $DD(\beta_{nt})$. This differential change can be decomposed into three terms:

$$DD(\beta_{nt}) = \bar{\tau} + DD(\mu_{nt}^0) + DD(\eta_{nt}), \tag{21}$$

corresponding to the HOPE VI treatment effect $\bar{\tau}$ and two selection bias terms, arising from (i) differential changes in treated neighborhoods' effects due to other factors (μ^0_{nt}) and (ii) differential changes in the covariance between the duration of exposure to public housing and unobservables such as family inputs. Assumption 1b guarantees that the first of the two selection bias terms is 0, as above. The following assumption guarantees that the second selection bias term is zero.

Assumption 3. There is no differential change in selection by age in revitalized vs. control sites:

$$DD(\eta_{nt}) = 0 \tag{22}$$

Assumption 3 relaxes Assumption 2 by permitting level shifts in unobservables across treated vs. untreated sites. It instead requires that the gradient of potential outcomes with respect to years of exposure does not change with revitalization—a weaker restriction but one that may nevertheless fail to hold. We first present a set of baseline results under Assumption 3 and then evaluate its validity.

Baseline Results. We implement the movers exposure design using the following OLS regression specification:

$$y_i = \theta e_{int} + \zeta e_{int} \times \mathbb{1}\{t > 0\} + \tau^{\text{pre}} e_{int} \times H_n + \tau^{\text{post}} e_{int} \times H_n \times \mathbb{1}\{t > 0\} + \lambda_{ns} + \varepsilon_{in}, \tag{23}$$

where individual i lives in project n in time period t and e_{int} is the number of years the individual i is exposed to (i.e., lives in) public housing project n during childhood in period t. As above, we limit the sample to children who either move out before the HOPE VI grant or move in after the grant (excluding those who stayed through the revitalization) to separate the pre-revitalization period ($t = 0$) from the post-revitalization period ($t = 1$).[27] The project-by-year-of-move-in fixed effects (λ_{ns}) permit arbitrary differences in children's potential outcomes across projects and time periods, subsuming H_n, $\mathbb{1}\{t > 0\}$, and the interaction between the two. As a result, τ^{pre} and τ^{post} are identified purely from the relationship between outcomes and years of exposure among children who move into the same project in the same year. We estimate equation (23) in

[27] Constructing the exposure variable requires that children have non-missing move in and move out dates in the HUD public housing residency file. We are able to measure *both* entry and exit dates for 48.8% of the children in our baseline analysis sample. To assess whether this subsample is representative of the broader population of public housing residents, we replicate the repeated cross-section estimates shown in Figure 3 on this subsample. We find that results are very similar to the estimates in our baseline sample, which only requires non-missing exit dates for prior residents and non-missing entry dates for new residents (the green and orange series in Figure A.10). Furthermore, we find similar repeated cross-section estimates in the full cross-sectional HUD data that includes all observations regardless of whether they have an entry or exit date (the navy series in Figure A.10).

a dataset with one observation per child per project using weighted least squares as above, using the inverse propensity score as weights and clustering standard errors by site.

We first analyze impacts on children's household income ranks at ages 29–30. We begin by estimating a non-parametric analog of equation (23), replacing the linear exposure measure e_{int} with indicators for years of exposure to public housing. The green series in Figure 6 plots the coefficients on these exposure indicators for children who moved in after revitalization. The coefficients increase with years of exposure (i.e., $\tau^{\text{post}} > 0$): growing up for more years in a revitalized project (instead of a matched control project) increased children's earnings in adulthood. The series in Figure A.11 replicates the same estimates for the pre-revitalization period. Before revitalization, we see no differential effect of childhood exposure on children's outcomes between HOPE VI and matched control projects (i.e., $\tau^{\text{pre}} = 0$). Under Assumptions 1b and 3, these estimates imply that the HOPE VI treatment increased neighborhoods' exposure effects on children's earnings.

To quantify the magnitude of this treatment effect, we estimate variants of the linear exposure effect model in equation (23). In Column 1 of Table 3, we present estimates corresponding to the green series in Figure 6, by estimating equation (23) in the subsample of children who moved into revitalized or matched control housing developments after revitalization to identify τ^{post}. Consistent with the results in the figure, the estimated annual childhood exposure effect (μ_{nt}) is 0.474 ranks higher in revitalized public housing units relative to matched controls. This estimate implies that every additional year a child spent growing up in a revitalized public housing unit increased their income rank in adulthood by 0.474 percentiles. Column 2 shows that for children who move out before the HOPE VI intervention, there was no significant difference in the association between income in adulthood and childhood exposure in revitalized vs. matched control sites. Column 3 estimates the full difference-in-differences specification in equation (23), including both prior and new residents. Columns 4 and 5 replicate Column 1 using income measured in dollars instead of ranks. Each year of exposure to a revitalized project increased children's annual household incomes at ages 29–30 by \$632 (2.77%) and their individual income by \$441 (2.22%).[28]

In Columns 6–8 of Table 3, we examine impacts on college attendance rates between the ages of 18–23 and rates of incarceration in 2020 (separately for boys and girls). Consistent with the evidence using

[28] When we estimate income in adulthood at earlier ages, we find statistically significant but smaller treatment effects (Table A.4) consistent with prior work showing a "fanning out" pattern of treatment effects of childhood interventions on incomes between ages 20–30 as children complete higher education and enter the labor market. Put differently, growing up in a revitalized neighborhood puts children on a steeper earnings trajectory in their twenties.

the difference-in-differences design, every additional year spent growing up in a revitalized public housing unit increased the probability that children attend college by 0.215 pp. It also reduced the probability of incarceration for boys by 0.207 pp (and had no impact on incarceration for girls).

Testing for Differential Changes in Selection by Age. Our interpretation of these estimates as the causal effect of HOPE VI revitalization on neighborhood effects rests on the validity of Assumption 3. This assumption would be violated if, for example, families with poorer unobservables ν_{int} stayed in non-revitalized public housing units for more time than in revitalized public housing units. We evaluate such concerns using three approaches.

First, in our baseline specification, we use variation in exposure arising both from when a family enters and leaves public housing. One may be especially concerned about endogeneity of exits, as families with poorer outside options may be forced to stay in less desirable (unrevitalized) public housing units for longer periods, whereas the time of entry into public housing from waitlists is more idiosyncratic. When we isolate variation arising solely from age at entry—either by limiting the sample to families who stay in public housing until the child is 18 years old (Column 9 of Table 3) or instrumenting for childhood exposure with the child's age at entry (Column 10 of Table 3)—we obtain estimates of τ^{post} that are similar to and not statistically distinguishable from our baseline estimates.

Second, we use predicted income ranks in adulthood based on family characteristics to test for selection on observables. We use the same prediction model for children's income ranks in adulthood as in Figure 3.A above. The orange series in Figure 6 replicates the estimates in the green series using predicted income ranks instead of actual income ranks. We find no significant difference between HOPE VI and control projects in the association between childhood exposure and predicted income ranks, supporting the view that the association between children's potential outcomes and exposure does not vary between revitalized and control sites. Consistent with this result, Column 11 of Table 3 shows that controlling for the interaction between exposure and a rich set of variables measured before entry into public housing has no impact on our estimate of τ^{post}.

Finally, to test for potential selection on unobservables across families, we compare the outcomes of siblings by including family fixed effects in the specification in equation (23). This specification nets out any unobservable family-level factors that may influence children's outcomes that we do not observe (such as parental education or wealth). This approach relaxes Assumption 3 by permitting the possibility that families with better unobservables stay longer in revitalized public housing units. In this siblings comparison design, τ^{post} is identified from differences in siblings' ages—when a family moves into a public housing unit, younger children will generally be exposed to it for a larger portion of their childhood than older children.

Figure 7 replicates the green series of Figure 6 with family fixed effects. There is again a clear upward-sloping relationship. Replicating the linear specification in Column 1 of Table 3 with family fixed effects, Column 12 of Table 3 shows that an additional year of childhood exposure to revitalized public housing increased income ranks in adulthood by 0.495 percentiles (s.e. = 0.209). This estimate is very similar to our baseline estimate based on cross-family comparisons, supporting the validity of Assumption 3.[29]

Heterogeneity. In Table 4, we analyze whether the effects of HOPE VI revitalization on exposure effects vary across subgroups. Columns 1–3 present estimates from variants of equation (23) in which we interact years exposed to public housing with the HOPE VI indicator and sex, parental income, or predicted income in adulthood. As with the repeated cross-sectional estimator, we find little heterogeneity along these dimensions in treatment effect on earnings in adulthood.

In Column 4 of Table 4, we examine heterogeneity in exposure effects by age by analyzing whether exposure to a revitalized project is more important in certain years of childhood. Motivated by prior work showing that neighborhood exposure effects are especially large during adolescence (Deutscher, 2020; Chetty et al., 2026*a*), we separately measure years exposed to public housing before and during adolescence. Spending an additional year of adolescence (ages 12–17) in a revitalized project increased income ranks in adulthood by 0.689 percentile ranks, compared with 0.245 ranks before adolescence. These larger effects during adolescence may point to mechanisms underlying the HOPE VI impacts that affect older children (such as social interaction with peers) more than younger children (such as reduced exposure to lead paint), a point we revisit in Section VI.

The estimates from the movers design are quantitatively consistent with the difference-in-differences estimates based on repeated cross-sections reported in Figure 3.A. Children who moved into the revitalized public housing projects spent an average of 5 years in public housing. Our exposure effect estimate therefore implies that we should observe a $0.474 \times 5 = 2.38$ rank increase in the repeated cross-section. This is similar to the observed impact in Figure 3.A. Furthermore, the temporal dynamics of treatment effects in the difference-in-differences estimator closely match what one would predict based on our exposure effect estimates with the movers design (Figure A.12).[30] These findings indicate that the repeated cross-sectional

[29] We include older siblings who move out of the house before their parents and younger siblings move into public housing to maximize precision; the observations with zero years of exposure in Figure 7 are these older siblings.

[30] In Figure A.12, we predict the treatment effects one would see based on the duration for which children in each event year

difference-in-differences estimator used in Figure 3 is not significantly biased by differential selection in to revitalized projects that would violate Assumption 2. The institutional features of public housing likely limit the scope for such selection, as all families who qualify for public housing are low-income and long waitlists constrain their ability to choose among projects.

V.D Interpreting the Magnitudes of the HOPE VI Impacts

To interpret the magnitudes of the impacts of HOPE VI, we compare its impacts on children's earnings to two benchmarks: the costs of the program and the impacts of other programs that help families move to higher-opportunity neighborhoods.

The estimates in Column 5 of Table 3 imply that each year of childhood exposure to a revitalized project instead of a control public housing project increases children's individual earnings at ages 29–30 by $441, a 2.22% increase relative to mean individual incomes in control projects. We estimate that the present-discounted-value (PDV) of lifetime earnings for children who grow up in public housing is $467,400.[31] Assuming that the 2.22% HOPE VI treatment effect remains constant over children's lifetimes, this implies a PDV earnings gain of $24,900 for the average family (with 2.4 children) from living in a revitalized project for one year ($24,900 = 467,400 \times 2.22\% \times 2.4$).

The total impacts of revitalization on children's earnings depend on the durability of the gains from revitalization. Treatment effects are stable over the 10-year horizon we can analyze at present, as shown in Figure 3. A stream of constant treatment effects of $24,900 per year over ten years translates to a present value gain of $218,800; if the gains persist for 30 years—which is plausible given the persistence of increases in mean neighborhood incomes in revitalized neighborhoods—the present value gain would be $502,700.

These earnings gains exceed the program's upfront costs. The HOPE VI program cost $168,200 in

cross-section live in housing projects, using the (calendar-time-invariant) age-specific exposure effect estimates reported in Column 4 of Table 4. The predicted treatment effects grow slightly over time because the sample in later (more recent) years consists of older children (whose earnings in adulthood can be observed before our data ends), and the gains from exposure to revitalized projects were larger for older children, as shown in Table 4.

[31] We construct this number as follows. Using the publicly available 2015 ACS, we first tabulate mean individual earnings w_a by age a. Average individual earnings at age 30 are $38,998 in the full population (Chetty et al., 2014), compared to $20,040 in our sample of children who grew up in public housing. Assuming that the ratio of these two earnings measures remains constant over the life cycle, the average earnings for children in our sample is given by κw_a, where κ = 20,040/38,998. We then incorporate a 1% annual wage growth rate, g, and age-specific survival probabilities, s_a, from Chetty et al. (2016). Average earnings at age a for children who grew up in public housing can therefore be written as $y_a = \kappa w_a s_a \frac{1}{(1-g)^{a-18}}$. To compute the present value from age 8—the average age at which children enter HOPE VI revitalized projects—we apply a 3% annual discount rate, r. The resulting present discounted value of lifetime earnings for the average child in public housing is $\$467,400 = \sum_{a=18}^{97} y_a \frac{1}{(1+r)^{a-8}}$, and total undiscounted lifetime earnings is $\$1,389,500 = \sum_{a=18}^{97} y_a$.

public funds and \$310,800 in total funds (including investments by private developers) per revitalized unit (in 2022 dollars). Much of this up-front cost is ultimately recouped in the form of subsequent income tax revenues and reduced spending on Medicaid and other public assistance programs resulting from the increase in children's earnings. Assuming an 18.4% effective marginal income tax rate (excluding payroll taxes) for low-income households (Congressional Budget Office, 2015), taxpayers would save \$92,500 in present value if the benefits of revitalization persist for thirty cohorts of children. Net of these taxes, the cost to taxpayers would be \$75,700=\$168,200-\$92,500 and the benefits to the recipients would be \$410,200=\$502,700-\$92,500, implying a marginal value of public funds of 5.42 = \$410,200/\$75,700, a rate of return that is higher than many existing anti-poverty and transfer programs (Hendren and Sprung-Keyser, 2020). The gains in income tax revenue also broaden the scope for incentivizing private investment in neighborhood revitalization using tax increment financing (TIF), which offers developers a share of future increases in tax revenues. TIF was used to attract private investment in some HOPE VI sites, but was based purely on expected gains in property and sales tax revenues (Salama, 1999).

Although these calculations indicate that HOPE VI increased children's earnings substantially, they do not necessarily imply that the program increased net social welfare. A complete evaluation of the program would need to account for many other costs and benefits, including costs for displaced residents, reductions in the total stock of affordable housing, as well as other gains that are not captured by earnings impacts, such as reductions in crime and improvements in health.[32]

As another benchmark, we compare the impacts of the HOPE VI treatment effect to the impacts of the Moving to Opportunity (MTO) experiment. In the MTO experiment, a randomly selected group of families living in high-poverty public housing projects (similar to HOPE VI projects pre-revitalization) were given vouchers to move to Census tracts with poverty rates below 10%. Chetty, Hendren and Katz (2016) estimate that children who were assigned to the experimental voucher group before the age of 13 (who were 8.2 years old on average at the point of random assignment) earned 17.6% more in early adulthood than children in the control group. On average, children in the experimental voucher group grew up in neighborhoods with 10.3% lower poverty rates between the point of random assignment and age 18. Assuming constant childhood exposure effects by age, the MTO estimates imply that each year of childhood spent in a Census tract with a 10% lower poverty rate increases children's earnings in early adulthood by 2.1%.

[32] A complete welfare analysis would also consider general equilibrium effects arising from changes in prices and how families sort across areas, which could have downstream impacts on social interaction and children's outcomes. Our partial-equilibrium parameter estimates could be used to calibrate models of neighborhood choice (e.g., Chyn and Daruich, 2025).

HOPE VI reduced poverty rates in the Census tract containing the HOPE VI project by 12%. Assuming that treatment effects vary linearly with poverty rates, our treatment effect estimate of a 2.77% increase in earnings from spending an additional year in a revitalized HOPE VI unit implies that growing up in a neighborhood where poverty rates fall by 10% as a result of revitalization increases earnings by 2.3%. This magnitude is very similar to the estimated impact of the MTO intervention, showing that the impacts of revitalizing high-poverty public housing developments are comparable to those of helping children move out of those developments to lower-poverty neighborhoods.

VI Mechanisms: The Role of Social Interaction

The HOPE VI program changed many aspects of children's environments, from improving the physical condition of the public housing units in which they lived to changing the socioeconomic backgrounds of their neighbors to increasing availability of childcare and other programmatic resources. Understanding which of these mechanisms contribute to the treatment effects identified above is important for developing new interventions to create high-opportunity neighborhoods, particularly since it may be infeasible to replicate exactly the same bundle of changes due to budget limits or other constraints.

A complete characterization of the mechanisms through which HOPE VI changed outcomes is challenging because it is difficult to measure and identify the causal effects of all potential mediating factors. Instead, we focus primarily on one mechanism that has been emphasized in recent work as a central driver of neighborhood effects on economic mobility: social interaction with higher-income peers (Chetty et al., 2022*a*; Cattan, Salvanes and Tominey, 2025; Mallah, 2025).

In this section, we show how increased interaction with higher-income peers explains much of HOPE VI's treatment effect on children in two steps. We first show that the treatment effects of HOPE VI on children's outcomes are highly heterogeneous by peer income. We then show that HOPE VI increased interaction with nearby higher-income peers and that these impacts are large enough to explain the program's mean treatment effects on children's earnings.

VI.A Heterogeneity in Treatment Effects on Earnings by Peer Strength

Prior research using social network data shows substantial homophily in childhood friendships by age, sex, and race (Kao and Joyner, 2004; Chetty et al., 2026*b*). In light of this evidence, for each child living in public housing, we define their peer group as children of the same age, sex, and race who live within a one-mile radius.

We begin by testing whether HOPE VI had larger treatment effects on children with more affluent peers, as measured by their peers' average incomes in adulthood. To do so, we estimate the treatment effects of HOPE VI using the same difference-in-differences specification as in equation (19), interacting the HOPE VI treatment variable with the average income rank of each child's peers in adulthood:

$$\begin{aligned} y_i = & \beta H_n + \theta \bar{y}_{p(i,n)} + \zeta \mathbb{1}\{t > 0\} + \psi H_n \times \mathbb{1}\{t > 0\} + \Gamma \bar{y}_{p(i,n)} \times \mathbb{1}\{t > 0\} \\ & + \rho^{\text{pre}} H_n \times \bar{y}_{p(i,n)} + \rho^{\text{post}} H_n \times \bar{y}_{p(i,n)} \times \mathbb{1}\{t > 0\} + \varphi X_{in} + \varepsilon_{int}, \end{aligned} \tag{24}$$

where child i lives in project n in time period t; $\bar{y}_{p(i,n)}$ denotes the average income rank in adulthood of children who belong to child i's peer group when living in project n; H_n is an indicator for whether project n received a HOPE VI grant; and X_{in} is a vector of the following individual-level covariates: fixed effects for year of birth, sex, and race/ethnicity; parental income; the number of years spent in public housing; and upward mobility in the most recent Census tract in which the individual lived prior to moving into public housing, defined as the average income rank in adulthood of children who grew up with parents at the 25th percentile of the income distribution in that tract. As in Section V.B, the estimation sample includes children who were either prior or new residents with one observation per child per project. The specification is estimated with weighted least squares using inverse propensity scores as weights, with standard errors clustered at the project level. The main coefficient of interest is ρ^{post}, which measures whether the HOPE VI treatment effect varies with peer income.

Baseline Results. Figure 8.A presents estimates from a semi-parametric analog of equation (24), where we replace the linear specification of $\bar{y}_{p(i,n)}$ with indicators for 15 quantiles for the level of $\bar{y}_{p(i,n)}$. The green series shows estimates of ρ_j^{post}—the difference in income ranks in adulthood for children who grew up in revitalized vs. matched control projects in each quantile—for the post-revitalization period, based on children who moved in after revitalization. The gray series shows analogous estimates for the pre-period, based on children who moved out before revitalization.

Before revitalization, there was little difference in outcomes for children who grow up in HOPE VI and matched control sites: the estimates in the pre-period series are all close to zero. After revitalization, outcomes improved in revitalized sites relative to matched controls, especially for children with affluent peers (as measured by their incomes in adulthood). We find a positive slope of the green line of $\rho = 0.27$ (s.e. $= 0.08$)—the HOPE VI treatment effect is 0.27 ranks higher for children who have peers with 1 percentile higher incomes on average. Strikingly, revitalization has zero effect on outcomes for children with the lowest-income surrounding peers, indicating that simply improving the physical structure of housing has little impact on economic outcomes in areas where the outcomes of nearby peers are poor.

Because peer income in adulthood is measured after treatment, the heterogeneous treatment effects documented above could be driven by correlated effects (Manski, 1993) that affect both public housing residents and their peers (e.g., improvements in school quality) rather than causal effects of peers on public housing residents' outcomes. To test between these possibilities, we examine heterogeneity by the *parental* income of nearby peers in the same age-sex-race group. Parental income cannot be directly affected by common shocks while individuals live in public housing because we measure it before children enter public housing. Figure 8.B replicates Figure 8.A, replacing peers' own mean income ranks $\bar{y}_{p(i,n)}$ with their parents' mean income ranks. We find a similar pattern: the HOPE VI treatment effect on children's income increases substantially with the parental income of nearby peers in a child's race-sex-birth cohort group. Revitalization leads to larger gains for children when their peers in the surrounding neighborhood come from ex-ante more affluent families, ruling out the possibility that the heterogeneity is driven purely by common shocks.

The heterogeneous treatment effects are also not driven by differential selection of families into revitalized projects with higher- vs. lower-income peers. When we replicate equation (24) using children's predicted outcomes (based on the same observables as those used in the orange series of Figure 3.A), we obtain estimates of $\rho = 0$ (Table A.5, Column 4). We also obtain similar results using our movers design: spending more years growing up in revitalized site increased children's incomes in adulthood more when their peers had higher mean incomes (Table A.5, Column 5).[33]

The incomes of surrounding residents may be correlated with other factors that underlie the heterogeneous treatment effects. Controlling for observable project-level characteristics such as poverty rates, demographic characteristics, and project size does not change our estimates of heterogeneity by peer income, as these other factors do not explain significant variation in the treatment effects (Table A.7). Of course, these observable characteristics are only a subset of potential factors that may vary across projects.

[33]The differential treatment effects by peer income in our baseline design (reported in Column 3 of Table A.5) are driven by heterogeneous exposure effects by peer income rather than heterogeneity in the amount of time children spend in projects with higher- vs. lower-income peers. Although children spent slightly longer living in HOPE VI sites post-revitalization than pre-revitalization (Column 1 of Table A.6), the number of years spent growing up in HOPE VI projects did not vary with peer income (Column 2 of Table A.6).

To isolate heterogeneity by peer characteristics controlling for project-level differences more directly, we next examine heterogeneity in outcomes within projects.

Within-Project Heterogeneity. The treatment effects of HOPE VI on earnings are more correlated with the incomes of peers with whom children interact more within a given project. As a first step to demonstrate this, we estimate variants of equation (24) in which we focus purely on the post-period and interact the treatment dummies not only with the average adult income rank of peers in a child's own cohort, but also peers who are 2–4 years younger, with whom children have far fewer connections. The first panel in Figure 9 shows that children's outcomes are much more strongly related to those of peers in their own cohort ($\rho = 0.31$) than with younger peers ($\rho = 0.01$). The second and third panels of Figure 9 report estimates from analogous specifications, including interactions with the average incomes of peers from other racial groups and the opposite sex. Again, the point estimates indicate that children's outcomes are more strongly related to the outcomes of own-race and own-sex peers.

Building on these results, we include project-by-time-period fixed effects when estimating equation (24), so that the interaction effect ρ is identified purely from comparisons across children living in the *same* public housing project at the same time, but with different peer groups (purely as a result of their age or demographics). Including these fixed effects yields very similar estimates of ρ to those from our baseline design.[34] This result demonstrates that the relationship between HOPE VI treatment effects and nearby peers' incomes cannot be driven by differences across projects in other correlated factors such as the amount of expenditure on revitalization; instead, what appears central is own-peer-group affluence.

A different way to interpret the preceding results is that HOPE VI increased the correlation between the outcomes of children living in public housing and those of their surrounding peers. Prior to revitalization, the correlation between children's outcomes and their peers' outcomes was similar in HOPE VI and control sites (as shown by the flat gray series in Figure 8.A). After revitalization, the correlation became much stronger in HOPE VI sites as the outcomes of children in public housing began to converge towards the outcomes of peers who lived nearby.

[34] Column 6 of Table A.5 reports estimates from a specification that includes project fixed effects. The coefficient falls to 0.15, but this decline primarily reflects exacerbated measurement error: project fixed effects disproportionately absorb signal variation across projects. To address this, Column 7 presents estimates based on a split-sample instrumental variables design, where we construct two independent measures of $\bar{y}_{p(i,n)}$ using disjoint random samples and instrument for one estimate with the other. The coefficient from this specification is 0.21, similar to our baseline estimate. Column 8 drops the project fixed effects but continues to use the split-sample IV approach. The resulting estimates closely match our baseline specification, indicating that, as expected, measurement error in $\bar{y}_{p(i,n)}$ does not substantially attenuate the estimates when project fixed effects are excluded.

VI.B Treatment Effects on Interaction with Nearby Peers

Motivated by the convergence of public housing residents' outcomes to nearby peers' outcomes, we next directly examine HOPE VI's treatment effects on three proxies for interaction with nearby peers and assess whether these treatment effects can explain the program's treatment effects on earnings.

Cohabitation with Surrounding Peers. Our first and most granular measure of interaction with nearby peers is the fraction of children raised in public housing who subsequently live with a child who grew up in the surrounding one-mile radius around the project (excluding the project itself). We measure cohabitation after children are 18 using data from the 2020 Census, and limit the analysis sample to individuals who lived with at least one other person at that point.[35]

The first panel of Figure 10 presents estimates of the impact of the HOPE VI treatment on this measure of social interaction (also see Column 2 of Table A.8). The left bar shows the fraction cohabiting with nearby peers for HOPE VI revitalized projects post-revitalization; the right bar shows the corresponding fraction for the matched control sites in the post period. HOPE VI significantly increased interaction with nearby peers using this proxy: 13.44% of children in revitalized public housing units lived with a child who grew up nearby in early adulthood, a 66% increase relative to the 8.11% who did so in matched control projects.[36]

The impacts of HOPE VI on cohabitation with surrounding peers do not vary with peer affluence as measured by either their own incomes in adulthood or parental income (Columns 4 and 5 of Table A.8). Revitalization appears to increase interaction between public housing residents and surrounding peers irrespective of their affluence; when peers happen to be higher-income, children growing up in public housing benefit more.

The "partial demolition only" intervention (without revitalization efforts) had no impact on the remaining public housing residents' probability of cohabiting with nearby peers (Column 6 of Table A.8), consistent with the lack of a treatment effect on earnings for that intervention. HOPE VI also had no effect on the likelihood that individuals living near revitalized projects (0.1–0.2 miles) ended up cohabiting with their surrounding peers (0.3–1 miles) (Column 7 of Table A.8), consistent with the finding that earnings did

[35]HOPE VI had no impact on the probability of cohabiting (Column 1 of Table A.8), suggesting that differential selection into this sample does not bias our findings.

[36]We do not have pre-period data on the cohabitation measure; however, our preceding analyses show that HOPE VI sites are comparable to matched controls pre-revitalization in terms of other outcomes. Assuming the same holds true for our measures of social interaction, we can interpret the post-period comparisons plotted in Figure 10 as estimates of the causal effect of the HOPE VI program on interaction. Reassuringly, we also find similar estimates on rates of cohabitation using the movers exposure effect design, as shown in Column 3 of Table A.8.

not change for nearby residents either.

Time Spent in Surrounding Neighborhood. Rates of cohabitation with nearby peers presumably increased as a result of spending more time in the surrounding neighborhood. We next directly evaluate this channel by measuring the amount of time that public housing residents spend in the neighborhood outside their public housing development. We use anonymized information from cell phone pings, as in Cook (2024), to measure the amount of time that public housing residents spent in locations within a one-mile radius outside the public housing project in 2019 (see Appendix C for details). The second panel of Figure 10 (and Column 1 of Table A.9) shows that public housing residents in revitalized projects spent 35% more time in nearby locations outside the project than those in matched control projects, consistent with the results on social interaction documented above.

Correspondingly, we find that residents of revitalized projects spent less time within the footprint of the public housing project itself (Column 2 of Table A.9). Alternative measures such as the number of visits per day to nearby locations outside the housing projects and specifications with additional controls yield similar results (Columns 3–4 of Table A.9).

Cross-Class Friendships. Finally, we turn to a third indicator that measures a broader set of relationships, using friendships from Facebook data. Following Chetty et al. (2022*a*), we measure the share of high-income friends that low-income students have in the high schools near public housing projects (see Appendix D for details). The advantage of this Facebook-based measure is that it captures a broader set of interactions than our cohabitation-based indicator and can be examined both pre- and post-revitalization because it measures contemporaneous friendships in high school. The disadvantage is that we cannot distinguish public housing residents from other low-income families whose children attend nearby high schools. Children living in public housing account for only 8% of students in nearby schools (see Appendix D), muting the estimated treatment effect.

The third panel of Figure 10 shows that low-income children attending high schools near revitalized projects had 1.67 pp more high-income friends. Reassuringly, we find no difference in the share of high-income friends—or in pre-intervention trends—between schools near HOPE VI vs. control sites for students who graduated from high school prior to the HOPE VI grant (Figures A.13 and A.14).

To evaluate whether this increase in cross-class interaction can explain the effects of HOPE VI on children's income in adulthood, we use estimates of the causal effect of cross-class interaction on children's incomes in adulthood from Facebook data. Chetty et al. (2026*b*) use fluctuations in the share of high-income children across cohorts within high schools to estimate that a 1 pp increase in economic connectedness (EC)—defined as the share of high school friends with high-income parents—increases a child's own income in adulthood by 0.30 percentiles. For changes in cross-class interaction to fully mediate the effect of HOPE VI on children's earnings of 3.45 percentiles, EC among children in public housing would have had to increase by 11.5 pp = 3.45/0.30. Since children in public housing account for 8% of students in nearby schools, this would require that changes in the friendship patterns of public housing residents account for $55\% = (0.08 \times 11.5)/1.67$ of the observed 1.67 pp increase in EC among all low-income students in nearby high schools.

It is plausible that the school-level changes in EC resulting from HOPE VI were driven disproportionately by changes in public housing residents' friendship patterns for two reasons. First, HOPE VI had no significant impact on the cohabitation-based measure of social interaction for children living in surrounding neighborhoods (Table A.8), indicating that the social connections of public housing residents changed more than those of surrounding peers. Second, the arrival of higher-income residents in market rate units in HOPE VI sites likely changed the friendships of public housing residents most because children tend to befriend school peers who live nearby at much higher rates (Preciado et al., 2012; Liu et al., 2021; Andrews et al., 2025).[37] We therefore conclude that the degree of increased interaction with more affluent peers could plausibly explain much of HOPE VI's impact on children's incomes in adulthood, consistent with our conclusions from the heterogeneity analysis showing that HOPE VI had no effect on outcomes for children surrounded by low-income peers (Figure 8.A).

VII Implications for Neighborhood Revitalization Beyond Public Housing

In this section, we assess whether increasing social integration with surrounding areas is a scalable approach to creating higher-opportunity neighborhoods beyond the distressed public housing projects targeted by the HOPE VI program. Were the HOPE VI public housing sites exceptionally isolated pre-revitalization, or can increasing integration with surrounding neighborhoods improve children's outcomes in other low-opportunity neighborhoods as well?

Measuring Social Integration Across Neighborhoods. In light of our finding that HOPE VI increased

[37] Part of the association between HOPE VI's treatment effects and the incomes of nearby residents could flow through differences in the characteristics of families living in market-rate units within the revitalized site, as projects near more affluent neighborhoods tended to attract higher-income families to the market-rate units in the revitalized development.

the correlation between the outcomes of children living in public housing and their nearby peers (Figure 8.A), we measure the connection between children growing up in each Census tract c and children in nearby areas based on the correlation between their outcomes.[38] We assign children to the modal Census tract c in which they live during childhood and define their peer group as children of the same race, sex, and birth cohort living in the ten closest tracts.[39] We then estimate the following regression model:

$$\bar{y}_{cd} = \phi_c + \theta \bar{y}_{p(cd)} + \delta_d + \varepsilon_{cd}, \tag{25}$$

where $\bar{y}_{cd}$ denotes the average adult income rank of children in tract c and demographic group d; $\bar{y}_{p(cd)}$ denotes the average income rank in adulthood of children who belong to the peer group of individuals in c and d; ϕ_c is a tract-level fixed effect, and δ_d is a fixed effect for demographic group (race, sex, and year of birth). The key parameter of interest, θ, provides a summary measure of social integration: it measures the degree to which the outcomes of children in a given tract covary with those of children in surrounding neighborhoods on average. We estimate the regression using weighted least squares, weighting by the number of children within each cell and excluding tracts with fewer than 500 children in the sample (where tract-level estimates are imprecise).

We focus on low-income neighborhoods, which we define as tracts with mean parental income rank below the median of the distribution of mean parental income ranks across tracts. Among low-income neighborhoods, $\hat{\theta}$ is 0.103. That is, a 1 rank increase in nearby peers' mean income ranks in adulthood is associated with an increase of 0.103 in a child's own income rank. This value is very similar to and not statistically distinguishable from the corresponding estimate of 0.110 for tracts that contain non-revitalized (matched control) public housing projects (Columns 2 and 3 of Table A.10). Hence, the average low-income neighborhood in the U.S. is as isolated from surrounding areas as distressed public housing projects are from their neighbors.[40]

[38] We focus on this outcome-based measure rather than the direct measures of social interaction analyzed above because it can be measured at a sufficiently granular level to compare public housing projects to other neighborhoods (unlike the Facebook school-level measures) and yields more precise estimates of integration than proxies such as cohabitation rates.

[39] We measure distance using tract centroids and only include tracts within five miles of the focal tract, even if there are fewer than ten tracts in that radius. We use a tract-based measure of surrounding areas—which has similar population counts in each cell—rather than the distance-based measure we used in the HOPE VI sample for scalability across areas with different population densities. In the public housing sample, we find similar estimates when using tract- and distance-based measures (Columns 1 and 2 of Table A.10).

[40] This finding may be surprising since levels of poverty in public housing projects are especially high. However, the neighborhoods surrounding these public housing sites also tend to be lower-income (Table A.10), reducing the *difference* in average incomes between public housing residents and the surrounding neighborhoods. It is plausible that the degree of interaction across

Association Between Children's Outcomes and Social Integration. Next, we examine whether increased integration with surrounding peers is associated with better outcomes in other neighborhoods, as in HOPE VI projects. Let $\bar{y}_c^{P=35}$ denote the mean income rank in adulthood for children growing up in tract c in families at the 35th percentile of the national income distribution, the mean parental income rank in low-income neighborhoods.[41] Let $\hat{\theta}_c$ denote an estimate of social integration in tract c, obtained by estimating a variant of equation (25) that allows the coefficient on mean peer incomes to vary by tract (i.e., replacing θ with θ_c). Figure 11.A presents a binned scatter plot of $\bar{y}_c^{P=35}$ vs. $\hat{\theta}_c$. We split the sample in each tract into two and estimate $\bar{y}_c^{P=35}$ in half the sample and $\hat{\theta}_c$ in the other half to eliminate any mechanical correlation between the dependent and independent variables and adjust for attenuation bias due to noise in the estimates of $\hat{\theta}_c$ by shrinking them toward the grand mean based on the estimate's reliability.[42]

Figure 11.A shows that, controlling for own parents' income, children who grow up in low-income neighborhoods that are more integrated with nearby communities have higher incomes in adulthood on average. This association remains similar when controlling for the average parental income in the child's own neighborhood (Table A.11).

Low-income children have better outcomes when their neighborhoods are integrated with high-income neighborhoods. To demonstrate this, we re-estimate equation (25), defining children's peers as children in the 10 nearest *high-income* Census tracts—tracts where mean parent income rank is above the national median. The coefficients from this regression, which we denote by $\hat{\theta}_c^H$, can be interpreted as an index of how integrated low-income neighborhoods are with nearby high-income areas. The green series in Figure 11.B presents a binned scatter plot of $\bar{y}_c^{P=35}$ vs. the high-income neighborhood integration index $\hat{\theta}_c^H$, using split samples and shrinkage as in Figure 11.A. The relationship between low-income children's outcomes and the high-income integration index ($\beta^H = 9.41$) is steeper than with the integration measure pooling all neighborhoods ($\beta = 5.25$).[43]

The correlations between integration and upward mobility across low-income neighborhoods are consistent with our design-based findings that HOPE VI improved children's outcomes by increasing integration

neighborhoods depends more on the relative difference in income than the levels.

[41]We estimate $\bar{y}_c^{P=35}$ using a linear regression of children's mean income ranks at ages 29–30 on their parents' income ranks and taking the predicted value at the 35th percentile of parental income.

[42]We estimate reliability as the ratio of the signal variance of $\hat{\theta}_c$ to the sum of the signal variance and the sampling variance of $\hat{\theta}_c$. We estimate the signal variance using the covariance of the $\hat{\theta}_c$ estimates across split samples and estimate the tract-specific sampling variance using the standard error of the regression coefficient.

[43]The mean of $\hat{\theta}_c^H$ (0.040) is lower than the mean of $\hat{\theta}_c$ (0.103), reflecting the fact that low-income neighborhoods are less well integrated with higher-income areas than other low-income areas.

with higher-income peers (Figure 8). Combining the estimate of $\beta^H = 9.41$ in Figure 11.B with the impact of the HOPE VI intervention on our measure of social integration ($\rho = 0.27$ from Figure 8.A), we predict that HOPE VI will improve children's income ranks in adulthood by $\beta^H \times \rho = 2.54$ percentiles, similar to the actual treatment effect estimate of 2.41 percentiles (Column 1 of Table 2).[44]

Finally, we analyze whether increased integration has harmful effects on higher-income children in surrounding areas. Consider the children growing up in high-income tracts, defined as tracts where the average parental income rank is above the national median. In each high-income tract, we regress children's mean incomes in adulthood on the mean incomes in adulthood of peers living in the 10 closest low-income Census tracts using the specification in equation (25). Let $\hat{\theta}_c^L$ denote the estimated regression coefficient on $\bar{y}_{p(i)}$. Let $\bar{y}_c^{P=65}$ denote mean income rank in adulthood for children with parents at the 65th percentile of the national income distribution, the mean parental income rank in the high-income neighborhoods we analyze.

The orange series in Figure 11.B presents a binned scatter plot of $\bar{y}_c^{P=65}$ vs. $\hat{\theta}_c^L$, constructed using split samples and shrinkage of $\hat{\theta}_c^L$ as in Figure 11.A. The relationship is weak and not significantly different from 0 (β^L = -1.54, s.e. = 1.10).[45] We reject the hypothesis that integration has equally large effects on outcomes for low- and high-income children ($\beta^H = -\beta^L$) with $p < 0.01$. These findings suggest that increased integration across low- and high-income neighborhoods is not zero sum: the benefits to low-income children do not come at the expense of children from higher-income families, consistent with recent findings on asymmetric effects of interaction on children's outcomes from social network data (Chetty et al., 2022*a*, 2026*b*).[46]

VIII Conclusion

This paper has shown that the largest effort to revitalize high-poverty public housing projects in the United States—the HOPE VI program—succeeded in increasing economic mobility. Each year a child spent growing up in a revitalized public housing project increased their household income in adulthood by 2.77%,

[44]We use the slope from Figure 11.B for this calculation because the difference between the average parental income is focal and surrounding tracts is similar in this sample to the difference in our sample of public housing projects (Columns 1 and 4 of Table A.10).

[45]We find similar results when we replicate Figure 11.B measuring children's incomes in dollars instead of percentile ranks: the slope for low-income neighborhoods is large and statistically significant, whereas the slope for high-income neighborhoods is small and statistically insignificant.

[46]These correlations are also consistent with our finding that the HOPE VI program did not have negative effects on outcomes for children in surrounding higher-income neighborhoods (Figure 5), although the precision of those estimates limits our ability to make strong claims about the treatment effect of HOPE VI on nearby areas.

implying that growing up from birth in a revitalized public housing project would increase income by 50%. These income gains are substantially larger than the program's up-front costs to taxpayers, demonstrating that it is possible to create higher-opportunity neighborhoods in a cost-effective manner even without changing broader determinants of outcomes such as labor market conditions or schools. The potential for change in short time frames is particularly notable given the historical roots of differences in opportunity across neighborhoods (Aaronson, Hartley and Mazumder, 2021; Lane et al., 2022).

Although HOPE VI improved neighborhoods' causal effects on upward mobility, most original residents of public housing did not benefit significantly from the program (Staiger, Palloni and Voorheis, 2024). Few public housing tenants returned to the neighborhoods after they were revitalized (Popkin et al., 2004). While displaced children ultimately had better outcomes from moving to higher-opportunity areas (Chyn, 2018; Haltiwanger et al., 2024), adults gained little economically and may have faced substantial displacement costs (Almagro, Chyn and Stuart, 2024). Developing policies that achieve similar gains while minimizing costs of displacement would be a valuable direction for future work.

The gains from HOPE VI are largely explained by changes in social interaction: children in revitalized projects interact more with peers in surrounding neighborhoods and gain more when they have higher-income peers. Many low-income neighborhoods in the U.S. are as socially isolated from surrounding areas as HOPE VI projects were prior to revitalization. Increasing connections between these neighborhoods and surrounding affluent communities could bring opportunity to many low-income families. To facilitate such interventions, we provide a list of low-opportunity neighborhoods that are surrounded by higher-opportunity areas using data from the Opportunity Atlas in Appendix E.

The key open question is what interventions are most effective in creating connections between lower-opportunity areas and surrounding communities, particularly given that cross-class interactions may not arise organically even among neighbors (Chetty et al., 2022*b*). Beyond the physical revitalization of distressed housing that was the focus of HOPE VI, there could be many approaches to increasing connections between neighborhoods, such as school integration, investments in public safety, transit infrastructure, and community programs. HOPE VI itself may have increased social interaction through several mechanisms: the creation of mixed-income neighborhoods with affordable and market-rate housing side-by-side, reduced crime that led to increased public safety, and new neighborhood programs that brought children from low- and high-income families together. Understanding how these and other mechanisms shape connections between neighborhoods would facilitate the design of policies to create high-opportunity neighborhoods.

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TABLE 1
Summary Statistics for Public Housing Residents

	Non-HOPE VI	HOPE VI	Matched Controls
	(1)	(2)	(3)
A. Adults Living in Public Housing			
Percent Non-Hispanic Black	38.2%	70.3%	69.4%
Percent Non-Hispanic White	40.2%	7.5%	8.0%
Percent Hispanic	12.8%	10.5%	13.0%
Percent Elderly	30.1%	14.9%	17.3%
Percent Disabled	12.2%	9.7%	9.9%
Percent H.H. with Female Head	72.1%	76.9%	80.4%
Percent Incarcerated in 2000 or 2010 Census	6.8%	11.0%	10.3%
Percent Employed (HUD Data)	26.5%	21.8%	23.7%
Percent with H.H. Income Below $15,000 (Tax Data)	57.8%	67.9%	67.8%
Mean H.H. Income (Tax Data)	$21,910	$13,600	$13,450
Mean H.H. Income (HUD Data)	$14,790	$12,170	$11,990
Mean H.H. Size	2.36	2.76	2.83
Mean Occupied Units	166	538	554
B. Children's Outcomes in Adulthood			
Mean H.H. Income Rank, Ages 29–30	38.45	37.30	35.69
Mean H.H. Income, Ages 29–30	$27,330	$25,680	$22,850
Mean Individual Income, Ages 29–30	$23,250	$22,420	$20,040
Percent Attended College, Ages 18–23	37.9%	36.4%	35.0%
Percent Married, Age 30	13.2%	10.6%	9.7%
C. Counts of Unique Individuals/Projects, 1995–2019			
Adults	5,999,000	326,000	1,033,000
Children	5,168,000	357,000	1,138,000
Children Born 1978-1990	1,178,000	109,000	251,000
Public Housing Projects	2,700	200	550

Notes: This table reports summary statistics for public housing residents. Column 1 considers individuals living in non-HOPE VI public housing projects, pooling annual data from 1990 to 2007. Columns 2 and 3 present estimates from the HOPE VI and matched control projects, respectively; these samples are restricted to observations from the three years prior to the year of the HOPE VI grant. Panel A reports characteristics of adults living in public housing, Panel B reports characteristics of their children, and Panel C reports counts of individuals who appear in the projects between 1995 and 2019. See Section III.B for details on data sources and variable definitions. H.H. denotes household. In this and all subsequent tables, all monetary values are reported in 2022 dollars and counts are rounded as follows: numbers between 10,000 and 99,999 to the nearest 500, between 100,000 and 999,999 to the nearest 1,000, and numbers above 1,000,000 to four significant digits; all other statistics are rounded to four significant digits.

TABLE 2
Impact of HOPE VI Revitalization on Children's Outcomes in Adulthood

	Child H.H. Income Rank at Ages 29–30			Child H.H. Income at Ages 29–30	Child Individual Income at Ages 29–30	Percent Attending College	Percent Incarcerated in 2020		Child H.H. Income Rank at Ages 29–30			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HOPE VI x Moved In After	2.413*** (0.789)	3.165*** (1.086)	2.627** (1.075)	3,633*** (1,218)	2,935*** (792)	5.952*** (1.207)	-1.287** (0.559)	-0.193 (0.118)	2.295*** (0.818)	2.607*** (0.982)	2.497*** (0.769)	2.390*** (0.787)
Description of Specification		Revita-lization F.E.	Exposure Covariates				Boys Only	Girls Only	Neighbor-hood Covariates	Failed Applicants	Additional Individual Covariates	No Individual Covariates
Dep. Var. Mean In Control Group	36.14	36.14	36.02	22,740	19,550	35.34	6.37	0.51	36.14	37.62	36.14	36.14
Observations	373,000	373,000	341,000	373,000	373,000	1,296,000	545,000	541,000	373,000	634,000	373,000	373,000

Notes: This table reports estimates from variants of the stacked difference-in-differences specification in equation (19), where the dependent variable is a child's outcome in adulthood and the key regressors are an indicator for moving into the project after the year of the HOPE VI grant, an indicator for HOPE VI, and their interaction. Unless otherwise noted, all specifications include project fixed effects and grant-year-by-calendar-year fixed effects as well as the following individual-level covariates: pre-move-in parental income percentile, upward mobility in the prior tract, and years in public housing. The estimation sample contains one observation per child per year they appear in the project and consists of children who moved out more than one year before the grant year and children who moved in during or after the grant year. The specifications are estimated with weighted least squares using inverse propensity scores as weights. The outcome in Columns 1–3 and 9–12 is the percentile rank of household income measured between ages 29 and 30. Column 2 replaces the project fixed effects with an indicator for HOPE VI. Column 3 interacts all covariates (aside from the project and grant-year-by-calendar-year fixed effects) with the demeaned years of exposure to public housing. The outcome in Column 4 is mean annual household income at ages 29 and 30. The outcome in Column 5 is mean annual individual income at ages 29 and 30. Column 6 uses an indicator for college attendance, defined as having received a Form 1098-T at least once between ages 18 and 23; for this column, we restrict the sample to the 1988 and 2004 birth cohorts based on the availability of the 1098-T data. The outcomes in Columns 7 and 8 are indicators for incarceration in the 2020 Census; here, we restrict the sample to individuals who were between ages 20 and 30 in 2020. Column 7 further restricts the sample to boys and Column 8 to girls. Coefficients in Columns 6–8 are multiplied by 100 so that impacts can be interpreted as percentage point changes. All columns except Column 10 use the set of matched control projects as the control group, while Column 10 uses failed HOPE VI applicants. Column 9 additionally controls for the following neighborhood-level characteristics, each interacted with an indicator for the post-award period: poverty rate; average adult age; share of adults with children; shares white, Black, and Hispanic; and Census block-group-level measures of median rent, share receiving public assistance, poverty rate, share with at least a college education, unemployment rate, and owner-occupied rate. Column 11 additionally controls for the following individual-level covariates: number of household members and dummies for male, race, year of birth, household member with disability, elderly household member, female-headed household, whether homeless, whether experienced hardship, and whether observed in more than one project. Column 12 drops all individual-level covariates. H.H. denotes household. Standard errors, clustered by project, are reported in parentheses. $*\ p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$.

TABLE 3

Impact of HOPE VI Revitalization on Childhood Exposure Effects: Movers Analysis

	Child H.H. Income Rank at Ages 29–30			Child H.H. Income at Ages 29–30	Child Individual Income at Ages 29–30	Percent Attending College	Percent Incarcerated in 2020		Child H.H. Income Rank at Ages 29–30			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HOPE VI x Years Exposed After Grant	0.474*** (0.147)		0.524** (0.216)	632*** (182)	441*** (161)	0.215* (0.122)	-0.207* (0.125)	-0.017 (0.033)	0.458*** (0.175)	0.594*** (0.225)	0.480*** (0.128)	0.495** (0.209)
HOPE VI x Years Exposed Before Grant		-0.050 (0.163)	-0.050 (0.163)									
Move In After Grant Only	X			X	X	X	X	X	X	X	X	X
Move Out Before Grant Only		X										
Description of Specification							Boys Only	Girls Only	Stay Until 18 Only	IV Using Entry Age	Additional Covariates	H.H. F.E.
Dep. Var. Mean In Control Group	35.14	36.12	35.51	22,850	19,890	33.35	9.46	0.67	35.75	35.14	35.14	35.04
Observations	53,000	35,000	88,000	53,000	53,000	408,000	71,000	76,500	36,000	53,000	53,000	69,500

Notes: This table reports estimates of τ^{pre} and τ^{post} from the movers specification in equation (23), where the dependent variable is the child's outcome in adulthood and the main regressors are childhood years spent in public housing (years exposed), an indicator for HOPE VI, and their interaction. All specifications control for project-by-year-of-move-in fixed effects. The estimation sample consists of children who moved into a HOPE VI or matched control public housing project during or after the year of the grant in all columns except Columns 2 and 3. The sample in Column 2 consists of children who moved out of these projects at least one year prior to the grant year, and Column 3 uses the union of the samples in Columns 1 and 2. The outcome in Columns 1–3 and 9–12 is the percentile rank of mean household income at ages 29 and 30. Column 4 uses the level of household income at ages 29 and 30, while Column 5 uses the level of individual income at ages 29 and 30. In Column 6, the outcome is an indicator for college attendance, defined as having received a Form 1098-T at least once between ages 18 and 23; for this column, we restrict the sample to the 1988 and 2004 birth cohorts based on the availability of the 1098-T data. In Columns 7 and 8, the outcome is a dummy for incarceration in the 2020 Census, and we restrict the sample to individuals who were between 20 and 30 in 2020. We further restrict to boys in Column 7 and girls in Column 8. Coefficients in Columns 6–8 are multiplied by 100 so that impacts can be interpreted as percentage point changes. Column 9 restricts to individuals who remain in the project until age 18. Column 10 instruments for the number of years exposed using the individual's age upon entering public housing. Column 11 additionally controls for the interaction between years exposed and the following individual-level covariates: parental income pre-move-in; upward mobility in the prior tract; number of household members; and dummies for male, race, year of birth, household member with disability, elderly household member, female-headed household, whether homeless, whether experienced hardship, and whether observed in more than one project. Column 12 includes household fixed effects and extends the sample to include older siblings who moved out of their parents' household before their younger siblings entered public housing. H.H. denotes household. All specifications are estimated with weighted least squares using inverse propensity scores as weights. Standard errors, clustered by project, are reported in parentheses. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$.

TABLE 4
Impacts of HOPE VI Revitalization on Childhood Exposure Effects: Heterogeneity Analysis

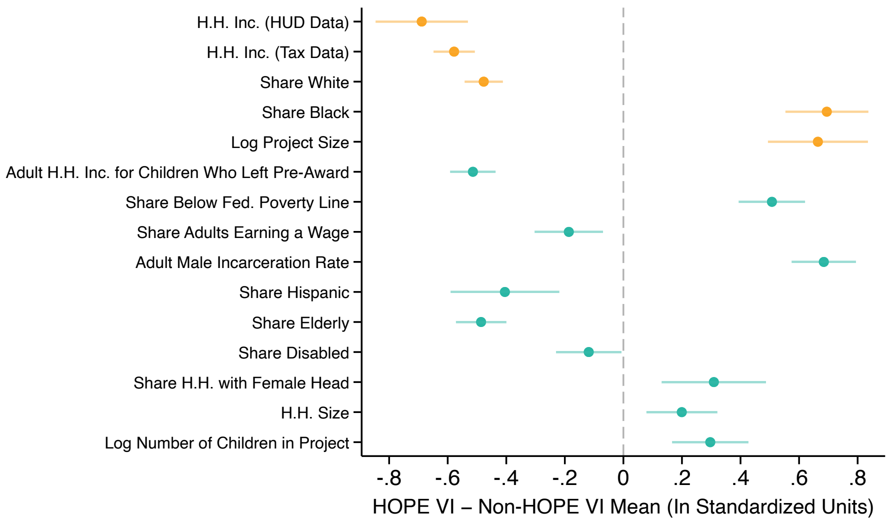
	Child H.H. Income Rank at Ages 29–30			
	(1)	(2)	(3)	(4)
HOPE VI x Years Exposed	0.463***	0.466***	0.511***	
	(0.176)	(0.151)	(0.133)	
HOPE VI x Years Exposed x Male	0.030			
	(0.226)			
HOPE VI x Years Exposed x Parental Income		0.108		
		(0.155)		
HOPE VI x Years Exposed x Predicted Outcome			-0.045	
			(0.146)	
HOPE VI x Years Exposed Before 12				0.245
				(0.176)
HOPE VI x Years Exposed 12 to 18				0.689***
				(0.202)
Observations	53,000	53,000	53,000	53,000

Notes: This table reports estimates from variants of the movers specification in equation (23). In all columns, the dependent variable is the percentile rank of mean household income at ages 29 and 30. In Columns 1–3 the key regressors are childhood years spent in public housing (years exposed), an indicator for HOPE VI, an individual-level characteristic, and interactions of these variables. The individual-level characteristic is sex in Column 1, parental income in Column 2, and predicted income at ages 29–30 in Column 3. Parental income and predicted adult income are each standardized based on the control mean and standard deviation. The prediction of adult income is based on the following child and household characteristics observed at entry into public housing: parental income percentile, upward mobility in the prior tract, years in public housing, sex, race, year of birth, household size, and household-level indicators for disability, elderly, female-headed, prior homelessness, hardship status, and being observed in more than one project. The key regressors in Column 4 are two separate measures of exposure—time spent in public housing before age 12 and time spent between ages 12 and 18—and their interactions with the HOPE VI indicator. All specifications control for project-by-year-of-move-in fixed effects. The estimation sample consists of children who moved into a HOPE VI or matched control public housing project during or after the year of the grant. H.H. denotes household. All specifications are estimated with weighted least squares using inverse propensity scores as weights. Standard errors, clustered by project, are reported in parentheses. $*\ p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$.

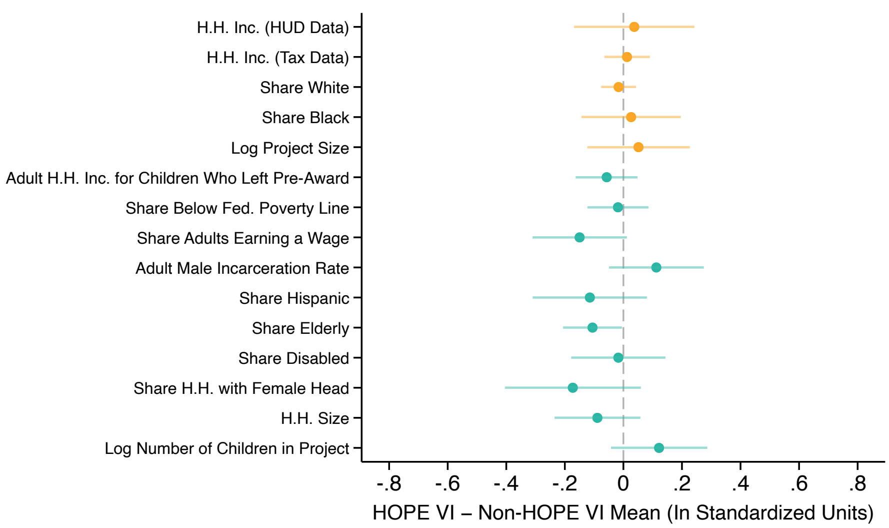
FIGURE 1

Pre-Grant Differences in Observable Characteristics

Panel A. Before Matching



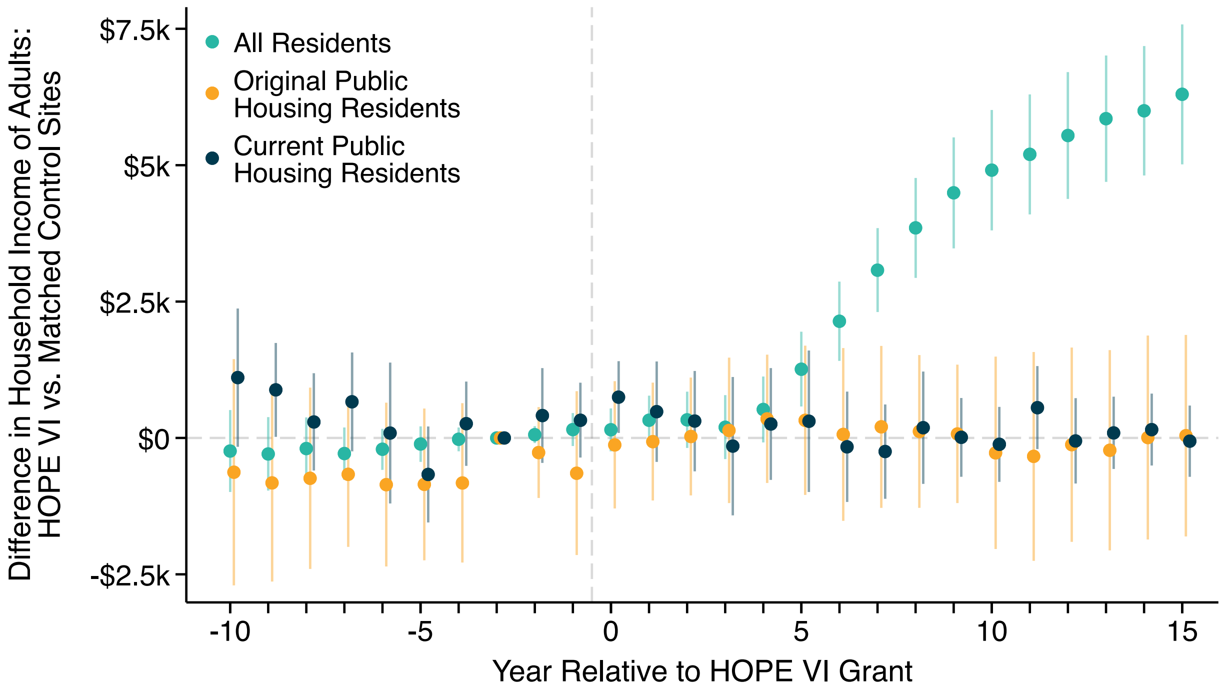
Panel B. After Matching



Notes: These figures plot mean differences in pre-grant characteristics between HOPE VI and non-HOPE VI sites before matching (Panel A) and after matching (Panel B). Each point reports an estimate from a separate regression in which the dependent variable is the pre-grant characteristic measured three years prior to the grant year and is standardized using the control group mean and standard deviation. The main independent variable is an indicator for receiving a HOPE VI grant, and all specifications control for grant-year fixed effects. The sample in Panel A includes all HOPE VI projects and all non-HOPE VI projects. In Panel B, the non-HOPE VI sample is restricted to the set of matched control projects. All regressions are estimated using weighted least squares, with baseline project population as weights in Panel A and project-level inverse propensity scores as weights in Panel B. Standard errors are clustered by project and the horizontal bars denote the 95% confidence intervals. The first five variables (in orange) are used to construct the inverse propensity score weights for the matching procedure. H.H. denotes household. See Section III.B for variable definitions and data sources.

FIGURE 2

Impact of HOPE VI Revitalization on Adults' Incomes

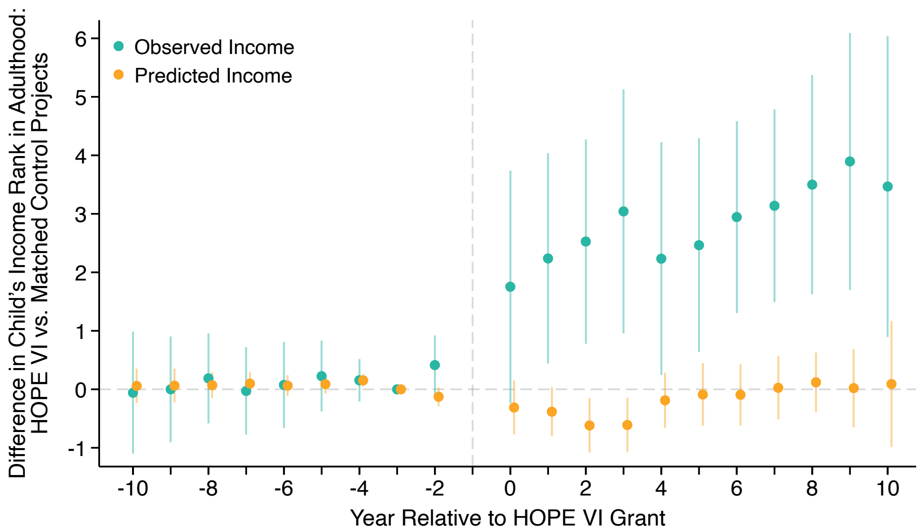


Notes: This figure plots estimates of the difference in adults' mean incomes between HOPE VI and matched control public housing projects j years after a HOPE VI grant. The figure plots estimates of β^j from the stacked difference-in-differences specification in equation (10). All specifications control for project fixed effects and grant-year-by-calendar-year fixed effects. Each series presents estimates from a separate regression with a different outcome variable. For the green series, we construct a repeated cross-sectional dataset consisting of all adults living in the physical footprint of the original public housing project in each year, and the outcome variable is the average household income among this population. For the orange series, we construct an individual-level panel following the original adult residents of the public housing projects—adults who lived in the projects one year prior to the grant year—and the outcome variable is the average household income among this population. For the navy series, we construct a repeated cross-sectional dataset consisting of adults living in public housing in each year. For each individual, we measure the difference between their current household income and their household income one year prior to moving into public housing, and the outcome is the average value of this difference. All specifications are estimated using weighted least squares with inverse propensity scores as weights. Standard errors are clustered by project, and the vertical bars denote 95% confidence intervals.

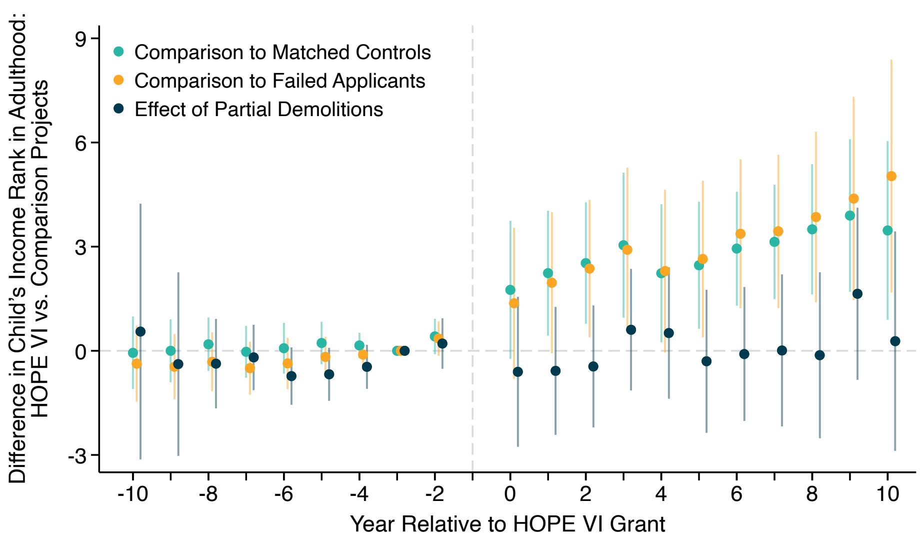
FIGURE 3

Impact of HOPE VI Revitalization on Children's Incomes in Adulthood

Panel A. Observed and Predicted Outcomes



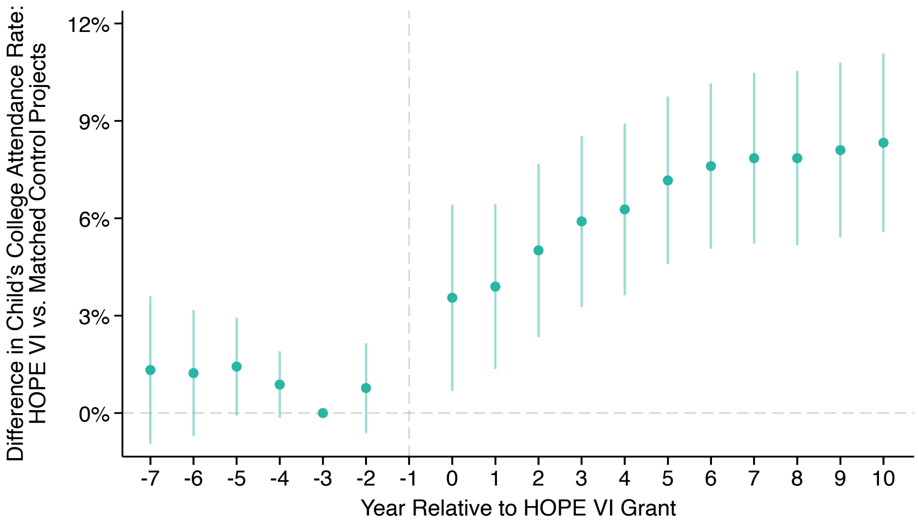
Panel B. Alternative Treatment and Comparison Groups



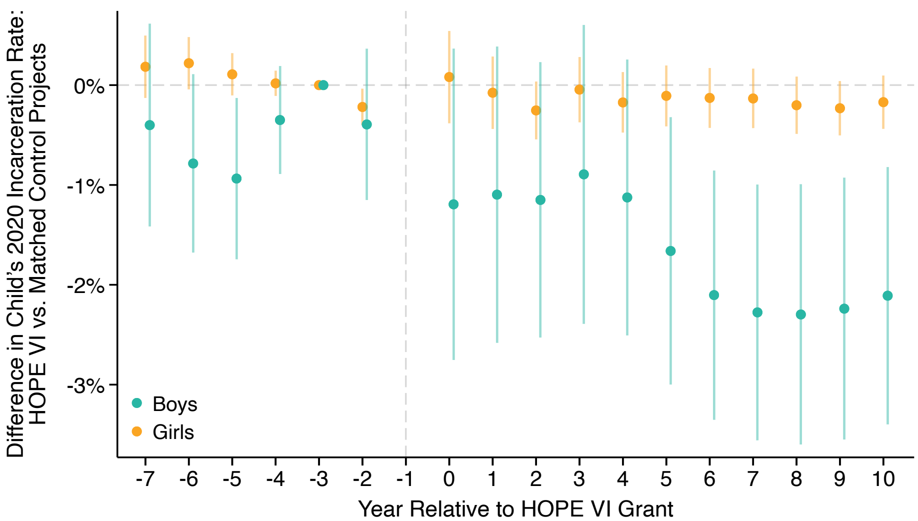
Notes: These figures plot estimates of the difference in mean incomes in adulthood between children in HOPE VI vs. matched control public housing projects j years after a HOPE VI grant. The figures plot estimates of β^j from the stacked difference-in-differences specification in equation (19). Unless noted otherwise, all specifications control for project fixed effects, grant-year-by-calendar-year fixed effects, pre-move-in parental income percentile, upward mobility in the prior tract, and years in public housing. The estimation sample contains one observation per child per year they appear in the project and consists of children who moved out more than one year before the grant year ($j \in [-10,-2]$) and children who moved in during or after the grant year ($j \in [0,10]$). All specifications are estimated with weighted least squares using inverse propensity scores as weights. Panel A compares HOPE VI and matched control projects. The dependent variable is the child's percentile rank of household income measured between ages 29 and 30 for the green series in Panel A and all series in Panel B. The orange series in Panel A drops the individual-level covariates and the outcome is the predicted income rank in adulthood based on the following variables observed at entry into public housing: parental income percentile; upward mobility in the prior tract; years in public housing; sex; race; year of birth; household size; and household-level indicators for disability, elderly, female-headed, prior homelessness, hardship status, and being observed in more than one project. In Panel B, the green series replicates the green series in Panel A, the orange series compares HOPE VI projects to projects that applied for but did not receive HOPE VI funding (estimated using population weights), and the navy series uses the matching procedure to estimate the effect of partial demolitions under the HOPE VI Demolition program. Standard errors are clustered by project, and the vertical bars denote 95% confidence intervals.

FIGURE 4
Impact of HOPE VI Revitalization on College Attendance and Incarceration Rates

Panel A. College Attendance, Ages 18–23

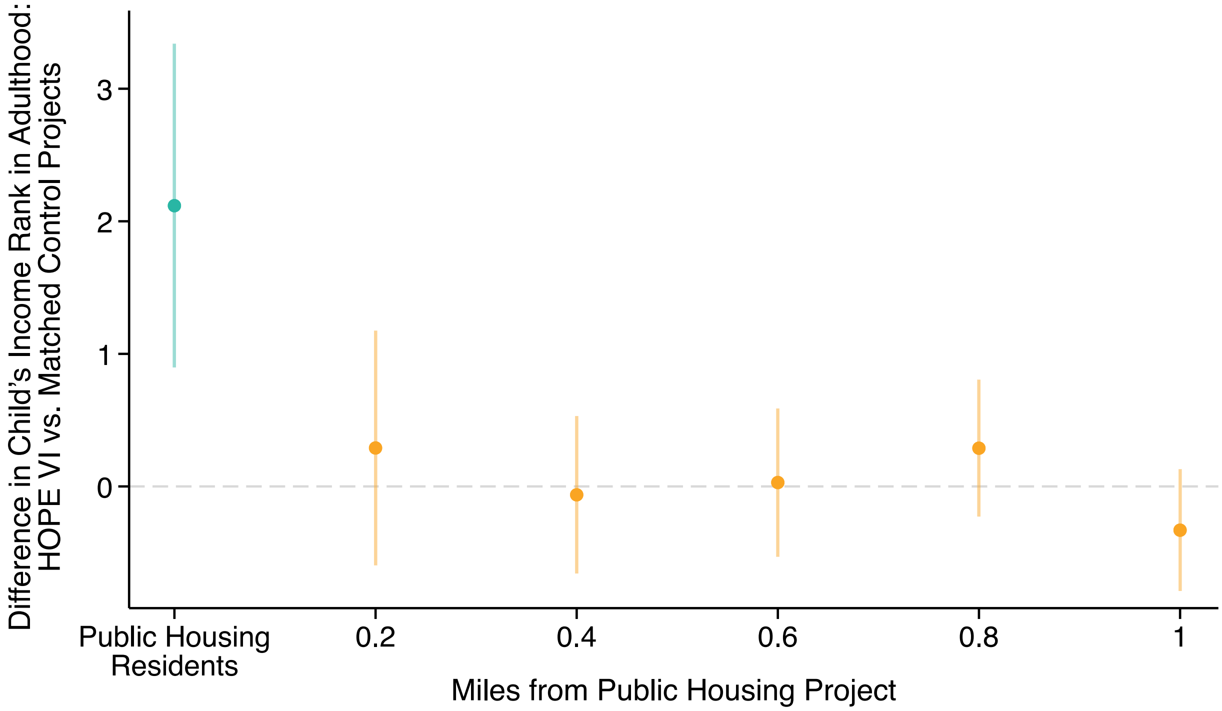


Panel B. Incarceration in 2020



Notes: These figures plot estimates of the difference in college attendance rates (Panel A) and incarceration rates (Panel B) between children in HOPE VI vs. matched control public housing projects j years after a HOPE VI grant. The figures plot estimates of β^j from the stacked difference-in-differences specification in equation (19). The dependent variable in Panel A is an indicator for college attendance, defined as having received a Form 1098-T at least once between ages 18 and 23; for this panel, we restrict the sample to the 1988 and 2004 birth cohorts based on the availability of the 1098-T data. The dependent variable in Panel B is an indicator for being incarcerated in the 2020 Census; we restrict the sample to individuals who were between 20 and 30 in 2020 and the green series presents estimates for boys while the orange series presents estimates for girls. Regression coefficients are multiplied by 100 so that impacts can be interpreted as percentage point changes. All specifications control for project fixed effects, grant-year-by-calendar-year fixed effects, pre-move-in parental income percentile, upward mobility in the prior tract, and years in public housing. The estimation sample contains one observation per child per year they appear in the project and consists of children in HOPE VI or matched control projects who moved out more than one year before the grant year ($j \in [-10, -2]$) or moved in during or after the grant year ($j \in [0, 10]$). All specifications are estimated with weighted least squares using inverse propensity scores as weights. Standard errors are clustered by project, and the vertical bars denote 95% confidence intervals.

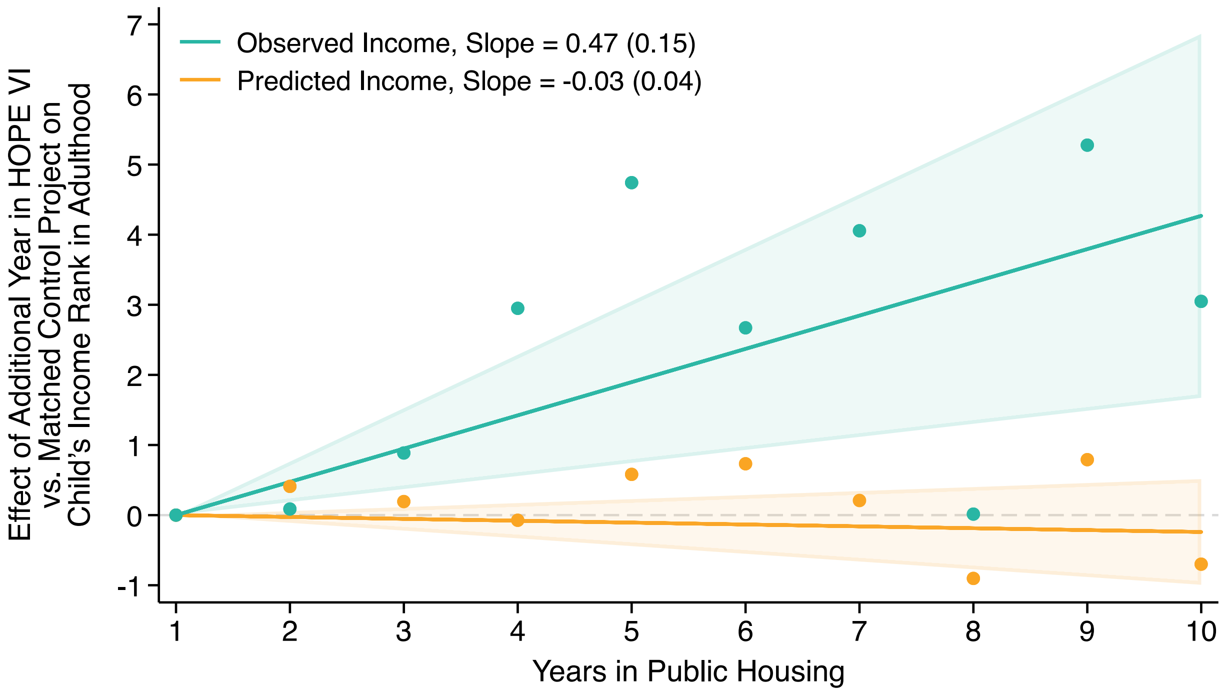
FIGURE 5
Impact of HOPE VI Revitalization on Children Living in Surrounding Neighborhoods



Notes: This figure plots estimates of the difference in mean incomes in adulthood between children living in or within one mile of HOPE VI vs. matched control public housing projects after a HOPE VI grant. The estimates are from a variant of the stacked difference-in-differences specification in equation (19), where the dependent variable is the child's percentile rank of household income measured between ages 29 and 30 and the main regressors are an indicator for moving into the area after the year of the HOPE VI grant, an indicator for HOPE VI, and their interaction (which is plotted above). Each point reports an estimate from a separate regression. The estimation sample for the left-most point consists of children living in public housing. The estimation sample for the remaining points consists of children living within 1 mile of the project, grouped into 0.2-mile rings based on distance to the project. The estimation sample contains one observation per child per year the child appears in the neighborhood and consists of children who moved out more than one year before the grant year and children who moved in during or after the grant year. Children in surrounding neighborhoods are identified using address data from tax records. For comparability, we also use the tax records to identify children living in the footprint of the public housing project and then restrict to those in public housing according to HUD records. All specifications control for project fixed effects, grant-year-by-calendar-year fixed effects, parental income rank, upward mobility in the prior neighborhood, the number of years in the neighborhood, and an indicator for being observed in public housing. All regressions also control for the following neighborhood-level characteristics, each interacted with an indicator for the post-award period (identical to Column 9 of Table 2: poverty rate; average adult age; share of adults with children; shares white, Black, and Hispanic; and Census block-group-level measures of median rent, share receiving public assistance, poverty rate, share with at least a college education, unemployment rate, and owner-occupied rate. All specifications are estimated using weighted least squares with inverse propensity scores as weights. Standard errors are clustered by project, and the vertical bars denote 95% confidence intervals.

FIGURE 6

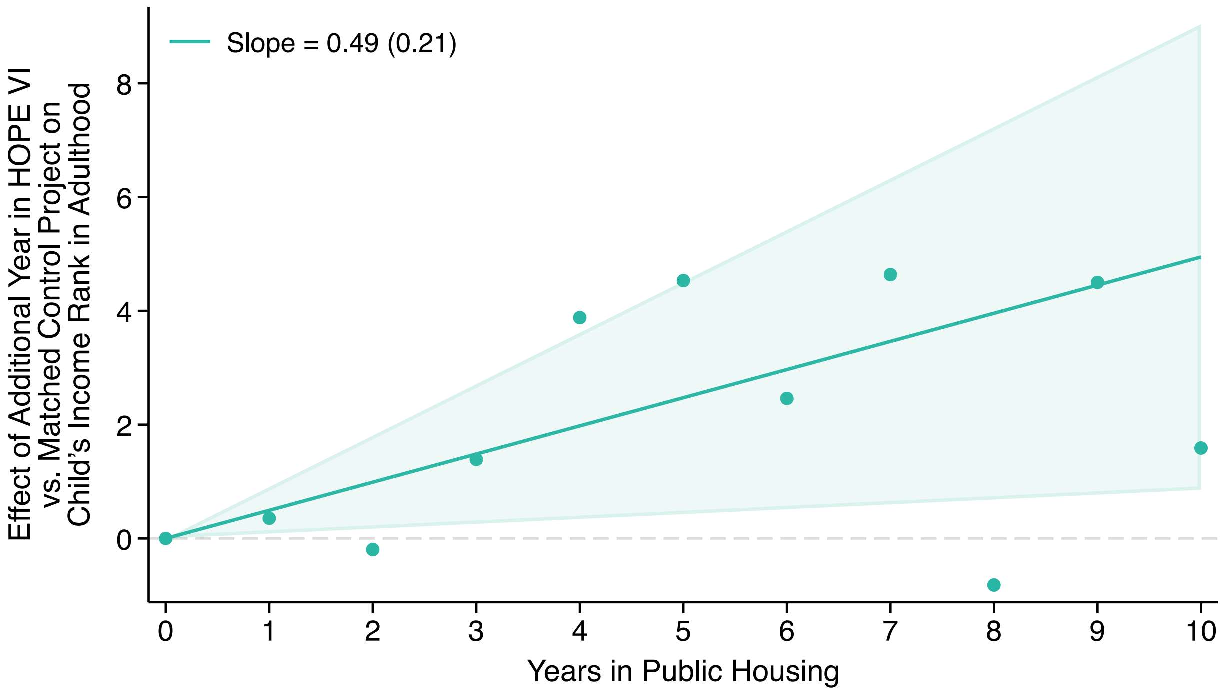
Effects of Childhood Exposure to HOPE VI vs. Matched Control Projects: Movers Analysis



Notes: This figure plots estimates of the difference in mean incomes in adulthood between children in HOPE VI vs. matched control projects vs. years of childhood spent in public housing, for children who move in after the HOPE VI grant. The figure plots estimates of τ^{post} from a variant of the movers specification in equation (23), where the dependent variable is the child's outcome in adulthood and the main regressors are childhood years spent in public housing, an indicator for HOPE VI, and their interaction (which is plotted above). For the green series, the dependent variable is the child's household income rank measured at ages 29 and 30. The orange series uses predicted income rank in adulthood based on the following variables observed at entry into public housing: parental income percentile; upward mobility in the prior tract; years in public housing; sex; race; year of birth; household size; and household-level indicators for disability, elderly, female-headed, prior homelessness, hardship status, and being observed in more than one project. The estimation sample consists of one observation per child per public housing project and includes children who moved into a HOPE VI or matched control public housing project during or after the grant year. The best-fit line is constructed by multiplying the parametric estimate of τ^{post} from equation (23) by the number of years on the horizontal axis. Standard errors are clustered by project, and the shaded region denotes the 95% confidence interval for the line. The points represent estimates from an alternative specification in which childhood years spent in public housing enter non-parametrically (indicators for one to ten years) rather than linearly. Because very few children in our sample live in public housing for more than ten years, we top-code exposure at ten years in this nonparametric specification. All specifications control for project-by-year-of-move-in fixed effects.

FIGURE 7

Effects of Childhood Exposure to HOPE VI vs. Matched Control Projects: Sibling Comparisons

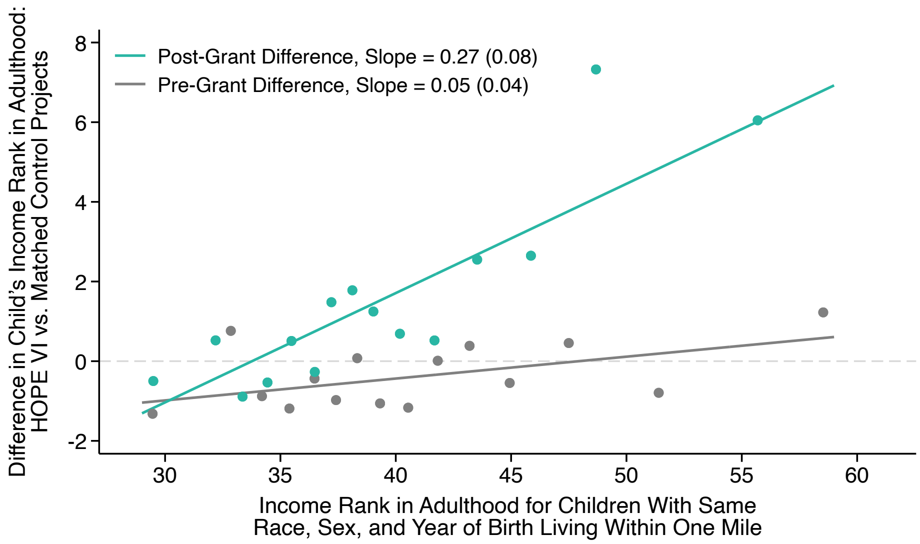


Notes: This figure plots estimates of the difference in mean incomes in adulthood between children in HOPE VI vs. matched control projects vs. years of childhood spent in public housing, for children who move in after the HOPE VI grant. The estimates are obtained by comparing siblings within the same family. The figure plots estimates of τ^{post} from a variant of the movers specification in equation (23), where the dependent variable is the child's household income rank measured at ages 29 and 30 and the main regressors are childhood years spent in public housing, an indicator for HOPE VI, and their interaction (which is plotted above). The specification additionally controls for household fixed effects. The estimation sample includes one observation per child per public housing project and is limited to children who moved into a HOPE VI or matched control public housing project during or after the grant year. We also include older siblings of these children who moved out of their parents' household before their younger siblings entered public housing. The best-fit line is constructed by multiplying the estimate of τ^{post} by the number of years on the horizontal axis. Standard errors are clustered by project, and the shaded region denotes the 95% confidence interval for the best-fit line. The points represent estimates from an alternative specification in which childhood years spent in public housing enter non-parametrically (indicators for zero to ten years) rather than linearly. Because very few children in our sample live in public housing for more than ten years, we top-code exposure at ten years in this nonparametric specification.

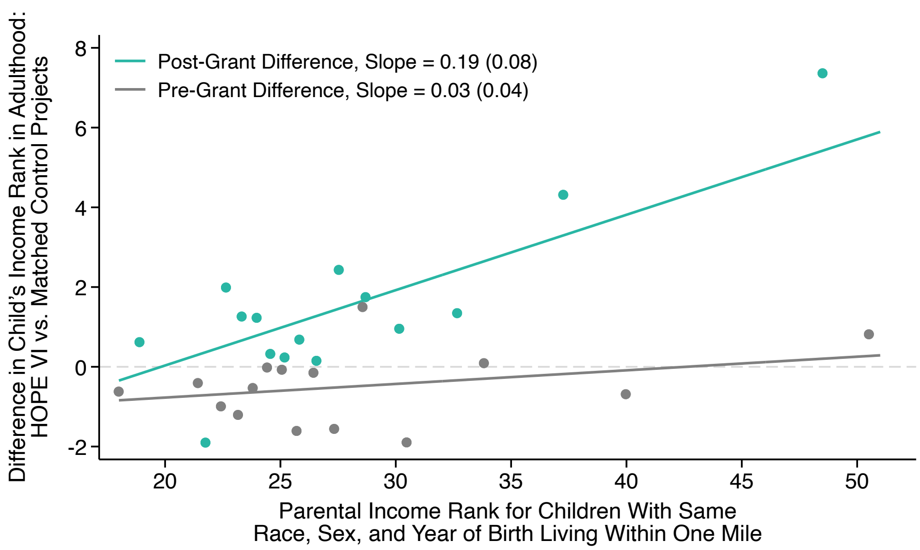
FIGURE 8

Heterogeneity in Impacts of HOPE VI Revitalization by Nearby Peer Income

Panel A. By Peers' Income in Adulthood



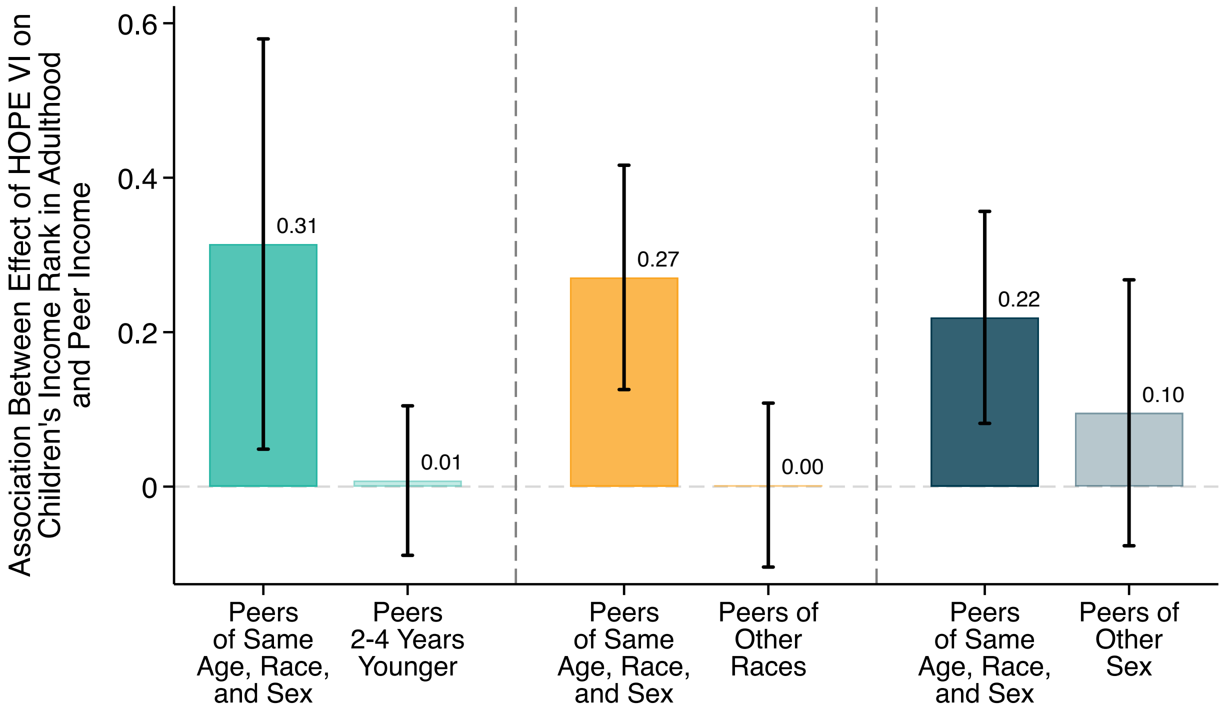
Panel B. By Peers' Parental Incomes



Notes: These figures plot estimates of the difference in mean incomes in adulthood between children in HOPE VI vs. matched control public housing projects vs. their nearby peers' average incomes in adulthood (Panel A) or the average income of their peers' parents (Panel B). The figures plot estimates of ρ^{pre} and ρ^{post} from variants of equation (24), where the dependent variable is the child's household income rank at ages 29 and 30. The main regressors are the average characteristic of children of the same race, sex, and birth year who live within one mile of the project, an indicator for HOPE VI, and their interaction (which is plotted above). Panel A uses peers' household income rank at ages 29 and 30 as the peer characteristic; Panel B uses peers' parental income percentile. For the green series, the estimation sample consists of children who moved into HOPE VI or matched control projects during or after the grant year. The gray series consists of children who moved out of these projects more than one year prior to the grant year. All specifications control for fixed effects for sex, race, and year of birth; grant-year-by-calendar-year fixed effects; pre-move-in parental income percentile; upward mobility in the prior tract; and years in public housing. The best-fit lines are constructed by multiplying the estimates of ρ^{pre} or ρ^{post} by the value of the peer characteristic on the horizontal axis. The points represent estimates from a specification in which the peer characteristic enters non-parametrically (as indicators for bins) rather than linearly; their positions on the horizontal axis correspond to the mean value of the peer characteristic within each bin. All specifications are estimated using weighted least squares with inverse propensity scores as weights, and standard errors are clustered by project.

FIGURE 9

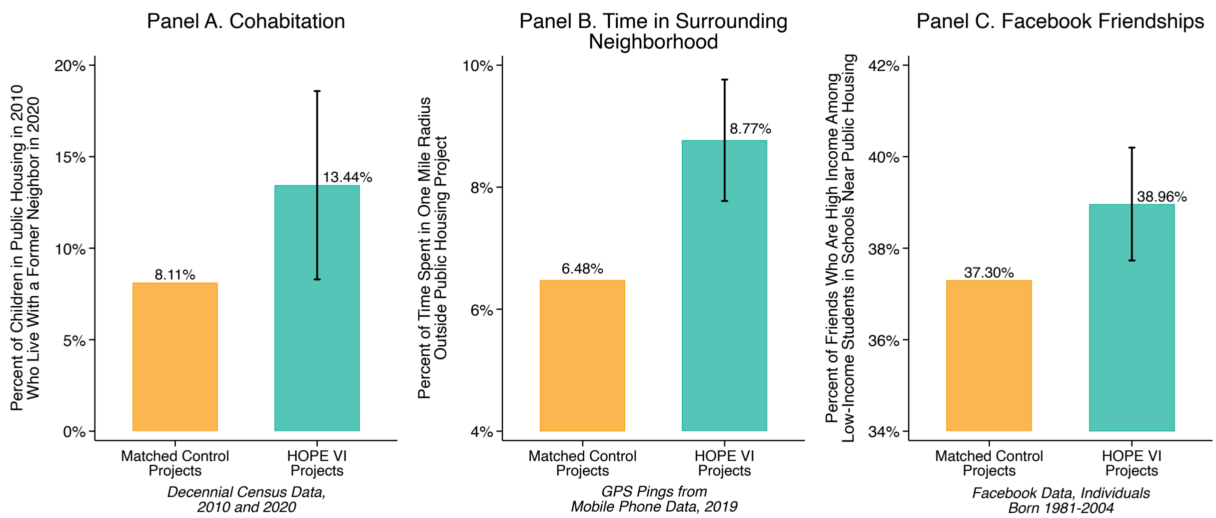
Association Between Impacts of HOPE VI and Peer Income, by Peer Cohort, Race, and Sex



Notes: This figure shows how the treatment effects of HOPE VI on children's incomes in adulthood vary with the incomes of nearby peers based on peers' cohort, race, and sex. The figure plots estimates of ρ^{post} from variants of equation (24), where the dependent variable is the child's household income rank at ages 29 and 30. The key regressors are the average percentile rank of household income in adulthood for children of the same race, sex, and birth year who live within one mile of the project; an indicator for HOPE VI; and their interaction (which is plotted above). The three panels correspond to three separate specifications that additionally control for the average adult income rank of an alternative peer group living within one mile and its interaction with the HOPE VI indicator (shown by the lighter bars to the right in each panel). The first panel uses peers of the same race and sex but who are 2–4 years younger; the second uses peers of the same sex and birth year but of a different race; and the third uses peers of the same race and birth year but of the opposite sex. All specifications control for fixed effects for sex, race, and year of birth; grant-year-by-calendar-year fixed effects; pre-move-in parental income percentile; upward mobility in the prior tract; and years in public housing. The estimation sample consists of children who moved into HOPE VI or matched control projects during or after the grant year. All specifications are estimated using weighted least squares with inverse propensity scores as weights. Standard errors are clustered by project and the vertical bars denote the 95% confidence intervals.

FIGURE 10

Impact of HOPE VI Revitalization on Social Interaction with Nearby Peers

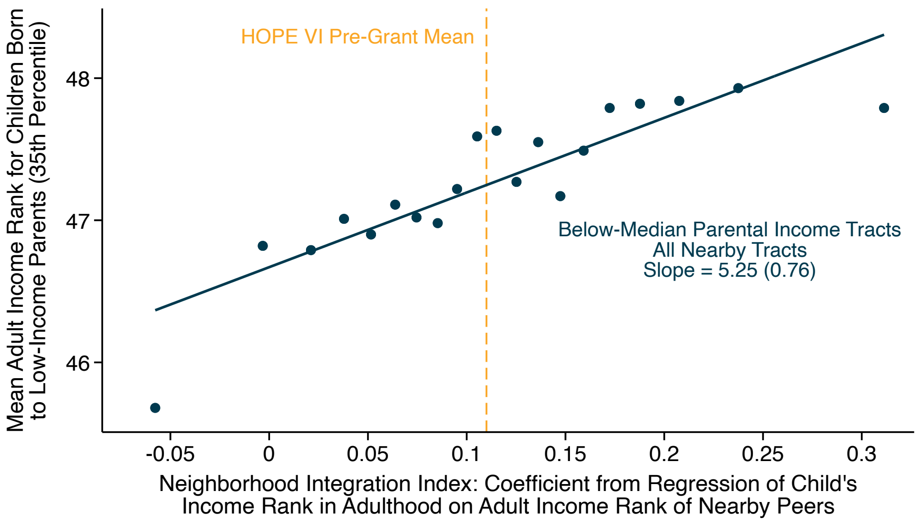


Notes: These figures show how HOPE VI changed the degree to which children in public housing interact with individuals outside public housing, using cohabitation with nearby peers (Panel A), time spent in the surrounding neighborhood (Panel B), and cross-class Facebook friendships (Panel C), as measures of social interaction. Each panel presents the control mean (left bar) and the control mean plus the treatment effect of HOPE VI (right bar) for a different outcome variable. In Panel A, the outcome is an indicator for whether children who lived in public housing cohabit with a partner who grew up in neighborhoods within a one-mile radius of their project in adulthood. The sample includes children who lived in a HOPE VI or matched control public housing project between ages 8 and 17 in 2010 and who were married or cohabiting with a partner in 2020. The outcome is defined as the percent whose partner lived within one mile of the public housing project in 2010, excluding partners who lived in the footprint of the public housing project. The data are collapsed to the project level. The treatment effect is estimated by regressing the outcome on an indicator for HOPE VI, controlling for grant-year fixed effects. In Panel B, the outcome is the share of time that public housing residents spend in the surrounding neighborhood, defined as a one mile radius around the project, excluding the project itself. We identify mobile phone devices belonging to public housing residents using their nighttime locations. For each device-by-quarter in 2019, we compute the share of minutes spent in the surrounding neighborhood out of all tracked minutes and regress this measure on an indicator for HOPE VI. See Appendix C for additional details on the mobile phone sample and analysis. In Panel C, the outcome is economic connectedness—the share of Facebook friends who are high-income. The sample includes Facebook users who attended a high school within 5 km of a HOPE VI or matched control public housing project. The data are collapsed to the project-by-high-school-by-cohort level. Treatment effects are estimated via the specification in Equation (D.1), by regressing the change in the outcome between students who graduated 10 years after the grant year and students who graduated 3 years before the grant year on an indicator for HOPE VI and a set of covariates defined in Appendix D. See Appendix D for additional details on the Facebook sample and analysis and Figure A.14.A for the full event-study estimates. All specifications are estimated using weighted least squares with inverse propensity scores as weights. Standard errors are clustered by project, and the vertical bars denote 95% confidence intervals.

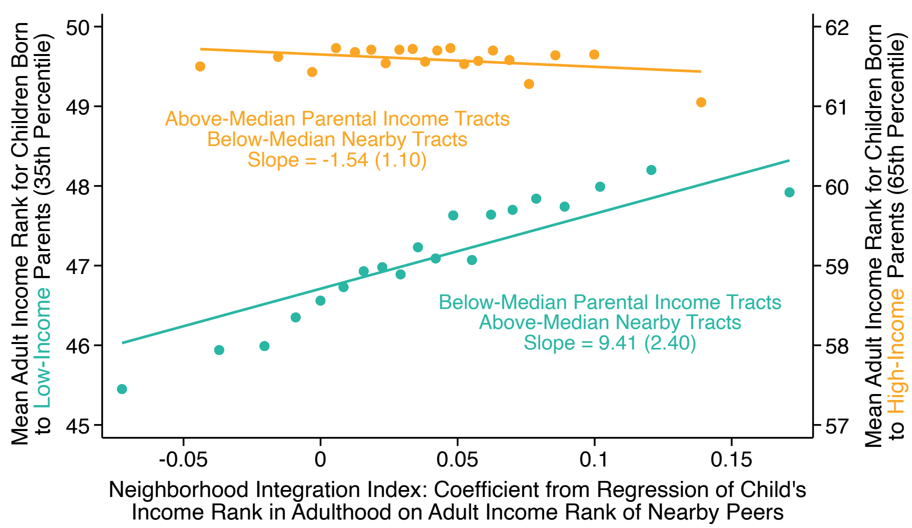
FIGURE 11

Children's Income in Adulthood vs. Social Integration with Nearby Tracts: National Estimates

Panel A. Integration Between Low-Income Tracts and Nearby Tracts



Panel B. Integration Between Low- and High-Income Tracts



Notes: These figures show the relationship between the income in adulthood of children who grew up in a given Census tract and the extent to which that tract is socially integrated with nearby Census tracts, pooling all tracts in the United States (not just public housing sites). In Panel A, we assign children to their modal census tract during childhood and limit the sample to low-income tracts, defined as tracts with mean parental income rank below the national median across all tracts. Within each tract, we regress the child's income rank on their parent's income rank and use the estimates to predict the income rank of children born to parents at the 35th percentile ($\bar{y}_c^{P=35}$) using half the sample. We then use Equation (25) to estimate the tract-level association between the outcomes of children in the focal tract and the outcomes of children of the same race, sex, and year of birth in the ten nearest census tracts within five miles (θ_c) in the other half of the sample. We shrink the tract-level estimates of θ_c toward the grand mean based on the reliability of each estimate and group observations into ventiles based on their shrunk θ_c. Each point plots the average value of $\bar{y}_c^{P=35}$ vs. the mean shrunk value of θ_c for tracts in that ventile. The linear fit shows estimates from a tract-level regression weighted by tract population, with standard errors clustered at the county level. The vertical dashed line displays the average value of θ_c in HOPE VI sites before the award (see Table A.10). The green series in Panel B replicates this analysis but restricts the nearby tracts to high-income tracts, defined as tracts with an average parental income rank above the national median. The orange series presents the mirror image of these results by defining the focal tracts as high-income tracts and measuring their social integration with nearby low-income tracts. For this series, the outcome is the predicted income rank in adulthood for children born to high-income parents ($\bar{y}_c^{P=65}$), shown on the right vertical axis.

Appendix A. List of HOPE VI Revitalization Grants

This section reproduces a list of the 262 HOPE VI revitalization grants compiled by HUD.

PUBLIC HOUSING AUTHORITY	NAME OF DEVELOPMENT	YEAR OF AWARD	AMOUNT OF AWARD
Akron (OH) Metropolitan Housing Authority	Elizabeth Park Homes	2002	$19,250,000
	Edgewood Homes	2005	$20,000,000
Albany (NY) Housing Authority	Edwin Corning Homes	1998	$28,852,200
Alexandria (VA) Redevelopment and HA	Samuel Madden Homes	1998	$6,716,250
Allegheny County (PA) HA	McKees Rocks Terrace	1997	$15,847,160
	Homestead Apartments	1998E*	$2,549,392
	Ohioview Acres	2002	$20,000,000
Allentown (PA) Housing Authority	Hanover Acres and Riverview Terrace	2004	$20,000,000
HA of the City of **Atlanta** (GA)	Techwood/Clark Howell/ Centennial Place	1993	$42,562,635
	Perry Homes	1996	$20,000,000
	Carver Homes	1998	$34,669,400
	Joel Chandler Harris Homes	1999	$35,000,000
	Capitol Homes	2001	$35,000,000
	McDaniel Glen	2003	$20,000,000
	Grady Homes	2005	$20,000,000
Atlantic City (NJ) Housing Authority and Urban Redevelopment Agency	Shore Park Shore Terrace	1999	$35,000,000
Housing Authority of **Baltimore City** (MD)	Lafayette Courts *(later divided into 2 grants, Lafayette Courts $31,015,600 and Homeownership Demonstration $18,648,000)*	1994	$49,663,600
	Lexington Terrace	1995(2)†	$22,702,000
	Hollander Ridge	1996	$20,000,000
	Murphy Homes Julian Gardens	1997	$31,325,395
	Flag House Courts	1998	$21,500,000
	Broadway Homes	1999	$21,362,223
HA of the City of **Beaumont** (TX)	Magnolia Gardens	2006	$20,000,000
Benton Harbor (MI) Housing Commission	Whitfield I	2003	$15,947,404

PUBLIC HOUSING AUTHORITY	NAME OF DEVELOPMENT	YEAR OF AWARD	AMOUNT OF AWARD
HA of the City of **Biloxi** (MS)	Bayview Homes Bayou Auguste	2000	$35,000,000
HA of the **Birmingham** District (AL)	Metropolitan Gardens	1999	$34,957,850
	Tuxedo Court	2003	$20,000,000
Boston (MA) Housing Authority	Mission Main	1993	$49,992,350
	Orchard Park	1995(2)†	$30,000,000
	Maverick Gardens	2001	$35,000,000
	Washington Beech	2007	$20,000,000
	Old Colony	2010	$22,000,000
HA of the City of **Bradenton** (FL)	GD Rogers and Addition	1999	$21,483,332
	Zoller Apartments	2002	$1,822,456
HA of the City of **Bridgeton** (NJ)	Cohansey View	2001	$10,945,944
HA of the City of **Bremerton** (WA)	Westpark	2008	$20,000,000
Buffalo (NY) Housing Authority	Lakeview Homes Lower West Side	1997	$28,015,038
Cambridge (MA) Housing Authority	John F. Kennedy Apartments	1998E*	$5,000,000
Housing Authority of the City of **Camden** (NJ)	McGuire Gardens	1994	$42,177,229
	Westfield Acres	2000	$35,000,000
	Franklin D. Roosevelt Manor	2003	$20,000,000
HA of the City of **Charlotte** (NC)	Earle Village	1993	$41,740,155
	Dalton Village	1996	$24,501,684
	Fairview	1998	$34,724,570
	Piedmont Courts	2003	$20,000,000
	Boulevard Homes	2009	$20,900,000
Chattanooga (TN) Housing Authority	McCallie Homes	2000	$35,000,000
Housing Authority of **Chester City** (PA)	Lamokin Village	1996	$14,949,554
	McCafferey Village	1998	$9,751,178
	Chester Towers	2003	$20,000,000
Chester County (PA) Housing Authority	Oak Street	1997	$16,434,200
Chicago (IL) Housing Authority	Cabrini-Green	1994	$50,000,000
	Henry Horner	1996	$18,435,300
	Robert Taylor	1996	$25,000,000
	ABLA Brooks Extension	1996	$24,483,250
	ABLA	1998	$35,000,000

PUBLIC HOUSING AUTHORITY	NAME OF DEVELOPMENT	YEAR OF AWARD	AMOUNT OF AWARD
	Madden/Wells/Darrow	2000	$35,000,000
	Robert Taylor	2001	$35,000,000
	Rockwell Gardens	2001	$35,000,000
	Stateway Gardens	2008	$20,000,000
Cincinnati (OH) Housing Authority	Lincoln Court	1998	$31,093,590
	Laurel Homes	1999	$35,000,000
HA of the City of **Columbia**, SC	Saxon Homes	1999	$25,843,793
	Hendley Homes	2003	$10,755,952
HA of **Columbus**, Georgia	George Foster Peabody Homes	2002	$20,000,000
Columbus (OH) Metropolitan HA	Windsor Terrace (Rosewind)	1994	$42,053,408
Housing Authority of **Covington** (KY)	Jacob Price Homes	2009	$17,000,000
Cuyahoga (OH) Metropolitan HA	Outhwaite Homes King Kennedy Estate South	1993	$50,000,000
	Carver Park	1995(2)†	$21,000,000
	Riverview	1996	$29,733,334
	Valleyview Homes	2003	$17,447,772
Dallas (TX) Housing Authority	Lakewest	1994	$26,600,000
	Roseland	1998	$34,907,186
	Frazier Courts and Frazier Courts Addition	2002	$20,000,000
	Turner Courts	2009	$22,000,000
Danville (VA) Redev. and HA	Liberty View	2000	$20,647,784
Dayton (OH) Metropolitan HA	Edgewood Court Metro Gardens Metro Annex	1999	$18,311,270
HA of the City of **Daytona Beach** (FL)	Bethune Village and Halifax Park	2002	$17,242,383
	Martin Luther King Jr. Apartments	2003	$7,639,191
Decatur (IL) Housing Authority	Longview Place	1999	$34,863,615
HA of the City and County of **Denver** (CO)	Quigg Newton Homes	1994	$26,489,288
	Curtis Park Arapahoe Courts	1998	$25,753,220
	Arrowhead Apartments and Thomas Bean Towers	2002	$20,000,000

PUBLIC HOUSING AUTHORITY	NAME OF DEVELOPMENT	YEAR OF AWARD	AMOUNT OF AWARD
	South Lincoln	2010	$22,000,000
Detroit (MI) Housing Commission	Jeffries Homes	1994	$39,807,342
	Parkside Homes	1995(1)†	$47,620,227
	Herman Gardens	1996	$24,224,160
District of Columbia HA	Ellen Wilson Homes	1993	$25,075,956
	Valley Green, Skytower	1997	$20,300,000
	Frederick Douglass Homes Stanton Dwellings	1999	$29,972,431
	East Capitol Dwellings Capitol View Plaza	2000	$30,867,337
	Arthur Capper Carrollsburg	2001	$34,937,590
	Eastgate Gardens	2003	$20,000,000
	Sheridan Terrace	2007	$20,000,000
Duluth (MN) Housing and Redevelopment Authority	Harbor View Homes	2002	$20,000,000
HA of the City of **Durham** (NC)	Few Gardens	2000	$35,000,000
HA of **East Baton Rouge Parish** (LA)	East Boulevard and Oklahoma Street	2002	$18,640,495
Housing Authority of **Easton** (PA)	Delaware Terrace and Delaware Terrace Annex	2006	$20,000,000
HA of the City of **El Paso** (TX)	Kennedy Brothers	1995(1)†	$36,224,644
	Alamito Apartments	2004	$20,000,000
HA of the City of **Elizabeth** (NJ)	Pioneer Homes Migliore Manor	1997	$28,903,755
Fayetteville (NC) Metropolitan HA	Delona Gardens and Campbell Terrace	2007	$20,000,000
HA of the City of **Fort Myers** (FL)	Michigan Court/Flossie Riley	2005	$20,000,000
HA of the City of **Frederick** (MD)	John Hanson Homes and Roger B. Taney Homes	2002	$15,889,376
Housing Authority of the City of **Fresno** (CA)	Yosemite Village	2003	$20,000,000
HA of **Fulton County** (GA)	Red Oak Townhomes	2002	$17,191,544
HA of the City of **Gary** (IN)	Duneland Village	1999	$19,847,454
Greensboro, NC Housing Authority	Morningside Homes	1998	$22,987,722

PUBLIC HOUSING AUTHORITY	NAME OF DEVELOPMENT	YEAR OF AWARD	AMOUNT OF AWARD
HA of the City of **Greenville**, SC	Woodland Homes Pearce Homes	1999	$21,075,322
	Jesse Jackson Townhomes	2004	$20,000,000
HA of the City of **Hagerstown** (MD)	Westview Homes	2001	$27,357,875
Hartford (CT) HA	Dutch Point Colony	2002	$20,000,000
Helena (MT) Housing Authority	Enterprise Drive	1997	$939,700
HA of the City of **High Point**, NC	Springfield Townhouses	1999	$20,180,647
Holyoke (MA) Housing Authority	Jackson Parkway	1996	$15,000,000
Houston (TX) Housing Authority	Allen Parkway Village	1993	$36,602,761
	Allen Parkway Village II	1997	$21,286,470
Indianapolis (IN) Housing Authority	Concord Village Eagle Creek	1995(1)[†]	$29,999,010
	Brokenburr Trails	2003	$16,778,288
Jacksonville (FL) Housing Authority	Durkeeville	1996	$21,552,000
	Brentwood Park	2002	$20,000,000
HA of the City of **Jersey City** (NJ)	Curries Woods	1997	$31,624,658
	Lafayette Gardens	2001	$34,140,000
	A. Harry Moore Apartments	2009	$9,700,000
Housing Authority of **Kansas City** (MO)	Guinotte Manor	1993	$47,579,800
	Theron B. Watkins Homes	1996	$13,000,000
	Heritage House	1997	$6,570,500
		1998E[*]	$3,429,500
King County (WA) Housing Authority	Park Lake Homes	2001	$35,000,000
	Park Lake Homes II	2008	$20,000,000
Kingsport (TN) Housing and Redevelopment Authority	Riverview	2006	$11,900,000
Knoxville's (TN) Community Development Corporation	College Homes	1997	$22,064,125
HA of the City of **Lakeland**, FL	Washington Ridge	1999	$21,842,801
Lexington-Fayette Urban County (KY) HA	Charlotte Court	1998	$19,331,116
	Bluegrass/Aspendale	2005	$20,000,000
City of **Long Branch** (NJ) Housing Authority	Sea View	2005	$20,000,000
HA of the City of **Los Angeles** (CA)	Pico Gardens Aliso Apartments	1993	$50,000,000
	Aliso Village	1998	$23,045,297

PUBLIC HOUSING AUTHORITY	NAME OF DEVELOPMENT	YEAR OF AWARD	AMOUNT OF AWARD
Housing Authority of **Louisville** (KY)	Cotter and Lang Homes	1996	$20,000,000
	Clarksdale I	2002	$20,000,000
	Clarksdale II	2003	$20,000,000
	Sheppard Square	2010	$22,000,000
Macon (GA) Housing Authority	Oglethorpe Homes	2001	$19,282,336
Memphis (TN) Housing Authority	LeMoyne Gardens	1995(1)[†]	$47,281,182
	Hurt Village	2000	$35,000,000
	Lamar Terrace	2003	$20,000,000
	Dixie Homes	2005	$20,000,000
	Cleaborn Homes	2009	$22,000,000
Mercer County (PA) Housing Authority	Steel City Terrace Extension	2000	$9,012,288
HA of the City of **Meridian** (MS)	Victory Village	2003	$17,281,075
Miami-Dade (FL) Housing Agency	Ward Towers	1998E[*]	$4,697,750
	Scott Homes Carver Homes	1999	$35,000,000
HA of the City of **Milwaukee** (WI)	Hillside Terrace	1993	$45,689,446
	Parklawn	1998	$34,230,500
	Lapham Park	2000	$11,300,000
	Highland Park	2002	$19,000,000
	Scattered Sites 2003	2003	$19,500,000
	Scattered Sites 2008	2008	$6,759,852
Minneapolis (MN) Public Housing Authority	Heritage Park	2002	$14,193,604
Mobile (AL) Housing Board	Central Plaza Towers	1998E[*]	$4,741,800
	Albert Owens / Jesse Thomas Homes	2003	$20,000,000
HA of the City of **Muncie** (IN)	Muncyana Homes	2002	$12,352,941
Metropolitan Development and Housing Agency - **Nashville** (TN)	Vine Hill Homes	1997	$13,563,876
	Preston Taylor Homes	1999	$35,000,000
	Sam Levy Homes	2002	$20,000,000
	John Henry Hale Homes	2003	$20,000,000
New Bedford (MA) Housing Authority	Caroline Street Apartments	1998E[*]	$4,146,780
HA of the City of **New Brunswick** (NJ)	New Brunswick Homes	1998	$7,491,656
HA of the City of **New Haven** (CT)	Elm Haven Terrace	1993	$45,331,593
	Quinnipiac Terrace and Riverview	2002	$20,000,000
Housing Authority of **New Orleans** (LA)	Desire	1994	$44,255,908

PUBLIC HOUSING AUTHORITY	NAME OF DEVELOPMENT	YEAR OF AWARD	AMOUNT OF AWARD
	St. Thomas	1996	$25,000,000
	William J. Fischer	2003	$8,127,632
	C.J. Peete	2007	$20,000,000
New York City (NY) Housing Authority	Arverne Homes	1995(1)†	$47,700,952
	Edgemere Homes	1996	$20,000,000
	Prospect Plaza	1998	$21,405,213
HA of the City of **Newark** (NJ)	Archbishop Walsh Homes	1994	$49,996,000
	Stella Wright Homes	1999	$35,000,000
Newport, KY Housing Authority	Peter G. Noll Booker T. Washington McDermott-McLane	2000	$28,415,290
Newport, RI Housing Authority One York Street	Tonomy Hill	2002	$20,000,000
Niagara Falls (NY) Housing Authority	Center Court	2006	$20,000,000
Norfolk (VA) Redev. and HA	Roberts Village Bowling Green	2000	$35,000,000
North Charleston (SC) Housing Authority	North Park Village	2001	$30,347,921
HA of the City of **Oakland** (CA)	Lockwood Gardens Lower Fruitvale	1994	$26,510,020
	Chestnut Court	1998	$12,705,010
	Westwood Gardens	1999	$10,053,254
	Coliseum Gardens	2000	$34,486,116
HA of the City of **Orlando** (FL)	Colonial Park	1997	$6,800,000
	Carver Court	2002	$18,084,255
HA of the City of **Paterson** (NJ)	Christopher Columbus	1997	$21,662,344
	Alexander Hamilton	2010	$18,400,000
Peoria (IL) Housing Authority	Colonel John Warner Homes	1997	$16,190,907
Philadelphia (PA) Housing Authority	Richard Allen Homes	1993	$50,000,000
	Schuylkill Falls	1997	$26,400,951
	Martin Luther King Plaza	1998	$25,229,950
	Mill Creek	2001	$34,825,000
	Ludlow Scattered Sites	2004	$17,059,932
City of **Phoenix** (AZ) Housing Dept.	Matthew Henson Homes	2001	$35,000,000
	A.L. Krohn	2007	$8,855,000
	Frank Luke Addition	2010	$20,000,000

PUBLIC HOUSING AUTHORITY	NAME OF DEVELOPMENT	YEAR OF AWARD	AMOUNT OF AWARD
Pittsburgh (PA) Housing Authority	Allequippa Terrace	1993	$31,564,190
	Manchester	1995(2)[†]	$7,500,000
	Bedford Additions	1996	$26,592,764
Pleasantville (NJ) HA with Atlantic City Housing Authority And Urban Redevelopment Agency	Woodland Terrace	2002	$13,446,700
HA of **Portland** (OR)	Columbia Villa, Columbia Villa Additions	2001	$35,000,000
	Iris Court	2005	$16,895,528
	Hillsdale Terrace	2010	$18,500,000
Portsmouth (VA) Redevelopment and HA	Ida Barbour	1997	$24,810,883
	Jeffry Wilson	2005	$20,000,000
HA of the City of **Prichard** (AL)	Bessemer Avenue Apartments	2002	$20,000,000
Puerto Rico Housing Administration	Cristantemos y Manuel A. Perez	1994	$50,000,000
HA of the City of **Raleigh** (NC)	Halifax Court	1999	$29,368,114
	Chavis Heights	2003	$19,959,697
HA of the City of **Richmond**, CA	Easter Hill	2000	$35,000,000
Richmond (VA) Redevelopment and HA	Blackwell	1997	$26,964,118
City of **Roanoke** (VA) Redevelopment and Housing Authority	Lincoln Terrace	1998	$15,124,712
St. Louis (MO) Housing Authority	Darst-Webbe	1995(1)[†]	$46,771,000
	Blumeyer Homes	2001	$35,000,000
	Cochran Gardens	2003	$20,000,000
	Arthur A. Blumeyer	2010	$7,829,750
HA of the City of **St. Petersburg** (FL)	Jordan Park	1997	$27,000,000
San Antonio (TX) Housing Authority	Springview	1994	$48,810,294
	Mirasol	1995(1)[†]	$48,285,500
	Victoria Courts	2002	$18,788,269
City and County of **San Francisco** (CA) Housing Authority	Bernal Plaza East	1993	$49,992,377
	Hayes Valley North and South	1995(2)[†]	$22,055,000
	North Beach	1996	$20,000,000
	Valencia Gardens	1997	$23,230,641
Housing Authority of **Savannah** (GA)	Garden Homes	2000	$16,328,649
Seattle (WA) Housing Authority	Holly Park	1995(1)[†]	$48,116,503

PUBLIC HOUSING AUTHORITY	NAME OF DEVELOPMENT	YEAR OF AWARD	AMOUNT OF AWARD
	Roxbury	1998	$17,020,880
	Rainier Vista Garden	1999	$35,000,000
	High Point Garden	2000	$35,000,000
	Lake City Village and House	2008	$10,486,839
HA of the City of **Spartanburg** (SC)	Tobe Hartwell Tobe Hartwell Extension	1996	$14,620,369
	Phyllis Goins	2003	$20,000,000
Springfield (IL) Housing Authority	John Hay Homes	1994	$19,775,000
Springfield (OH) Metropolitan HA	Lincoln Park	2004	$20,000,000
HA of the City of **Stamford** (CT)	Southfield Village	1997	$26,446,063
	Fairfield Court	2003	$19,579,641
HA of the City of **Tacoma** (WA)	Salishan	2000	$35,000,000
HA of the City of **Tampa** (FL)	Ponce de Leon College Hill	1997	$32,500,000
	Riverview Terrace Tom Dyer	2001	$19,937,572
Taunton (MA) Housing Authority	Fairfax Gardens	2010	$22,000,000
Housing Authority of the City of **Texarkana**, Texas	Covington Homes, Stevens Court and Griff King Homes	2008	$20,000,000
Housing Authority of the City of **Trenton** (NJ)	Miller Homes	2009	$22,000,000
Tucson (AZ) Public Housing Authority	Connie Chambers	1996	$14,600,000
	Robert F. Kennedy Homes	2000	$12,748,000
	Martin Luther King Apartments	2004	$9,825,000
HA of the City of **Tulsa** (OK)	Osage Hills	1998	$28,640,000
Tuscaloosa (AL) Housing Authority	McKenzie Court	2004	$20,000,000
Utica (NY) Municipal HA	Washington Courts	2002	$11,501,039
HA of the City of **Wheeling**, WV	Grandview Manor Lincoln Homes	1999	$17,124,895
Wilmington, DE Housing Authority	Eastlake	1998	$16,820,350
HA of the City of **Wilmington**, NC	Robert S. Jervay Place	1996	$11,620,655
Winnebago County (IL) HA	Champion Park	2002	$18,847,938
HA of the City of **Winston-Salem** (NC)	Kimberly Park Terrace	1997	$27,740,850
	Happy Hill Gardens	2002	$18,264,369

PUBLIC HOUSING AUTHORITY	NAME OF DEVELOPMENT	YEAR OF AWARD	AMOUNT OF AWARD
Municipal Housing Authority of the City of **Yonkers** (NY)	Mulford Gardens	2003	$20,000,000
Youngstown (OH) Metropolitan HA	Westlake Terrace	2002	$19,751,896

* There were two funding rounds in fiscal year 1995.
† 1998E indicates a special grant for Elderly projects.

Appendix B. Construction of Public Housing Data

We use two datasets from HUD to identify public housing residents: the Tenant Rental Assistance Certification Systems (TRACS) and Public and Indian Housing Information Center (PIC) files.

Measuring the Location of Public Housing Projects. To measure the geographic footprint of each public housing project, we use individual-level address data for public housing residents to identify the set of Census blocks associated with each project. We begin by constructing a dataset with one observation per person per project. For each individual, we measure the Census block of residence using address histories from the HUD administrative records when available, and the federal tax records otherwise. For each project, we exclude individuals whose residential coordinates lie more than one mile from the median latitude and longitude coordinates of all residents linked to that same project, thereby removing clear outliers. We then classify a Census block as belonging to a given project if (i) at least five unique residents are ever observed living in that block and (ii) in some year, at least 20% of block residents are identified as living in public housing according to HUD data. The geographic boundary of each public housing site is defined as the union of all Census blocks satisfying these criteria.

We assess the accuracy of our measure of project boundaries by comparing the share of residents living in public housing in blocks that contain a project to those in nearby blocks. We use the tax returns to measure the residential addresses for all adults and then for each Census block, we calculate the share of residents who live in public housing according to the HUD data. 65% of residents in blocks containing a public housing project are coded as public housing residents in the HUD data. This share drops sharply to 11% in blocks within 0.1 miles and to 8% in blocks 0.1 to 0.2 miles away. This spatial discontinuity provides evidence that our project boundary measures are accurate.

To further evaluate the accuracy of our boundary measures, we construct an independent, high-resolution measure of project footprints using historical satellite imagery in Google Earth Pro. We begin with approximate locations based on address and coordinate information from HUD's 1994 "951 file." We then locate the precise location of the projects using satellite imagery from Google Earth Pro. The public housing projects are typically identifiable based on architectural features, but, when available, we also use data from supplemental sources such as ProPublica's housing database, and PHA websites. For HOPE VI sites, we use pre-demolition imagery to recover the original footprint. We then draw parsimonious polygons around the identified structures and convert them to shapefiles. This approach yields precise geographic boundaries for 1,049 projects at a finer spatial resolution than Census blocks. 75% of residents in these hand-drawn polygons are coded as public housing residents in the HUD data. This share drops to 19% in blocks within 0.1 miles and to 10% in blocks 0.1 to 0.2 miles away. The close alignment between the manual and primary measures reinforces the conclusion that our primary methodology accurately captures the spatial boundaries of public housing projects.

Project Identifiers. HUD restructured its project identifiers in 2008, complicating efforts to track public housing projects consistently over time. We construct longitudinally consistent project identifiers using the following procedure. First, using the methodology described above, we construct two datasets in which observations are uniquely defined at the project-by-Census-block level, one based on the pre-2008 project identifiers and the other based on the post-2008 identifiers. We then merge these datasets by Census block and retain pairs of pre- and post-2008 project identifiers if (i) at least 20% of either project's units are located in overlapping blocks, or (ii) the overlapping blocks constitute the largest share of either project in the matched dataset. We define a longitudinal project identifier as the connected set formed by linking all project identifiers (pre- and post-2008) that share overlapping Census blocks under these criteria. In cases where HUD provides an official crosswalk between pre- and post-2008 identifiers, we have verified that our algorithm produces matches that closely align with the HUD crosswalk.

Measuring Entry and Exit Dates for Public Housing Residents. We measure the entry year using the date on the entry record. If this record is missing, then we use the entry date on the annual re-certification record. If the children are missing an entry date, then we use the entry dates of the head of household. We measure the exit year using the date on the exit record. For children, we are primarily interested in measuring exposure during childhood. Therefore, if a child is missing an exit date but they or their parent is observed in that public housing project after age 18, we assume that the child lived in the project through age 18.

Appendix C. Analysis of Cell Phone Data

We use anonymized GPS data from cell phone pings from a mobility data provider, following Cook (2024), to measure how much time public housing residents spend in nearby neighborhoods. The data contain device-level location pings from 2019, which allow us to track mobile phone devices over time and space and construct measures of time spent in different areas. We collapse the data into *staypoints*—spatial clusters indicating locations where a device remains for a sustained period—and restrict the sample to staypoints lasting at least 15 minutes.

We manually identify the physical footprints of public housing projects using historical satellite imagery, as described in Appendix B. We exclude projects that cannot be matched to the HUD USER project-characteristics data from 1993, and we merge projects that are spatially proximate to ensure that project boundaries are non-overlapping. Because some project footprints—especially at HOPE VI sites—include market-rate units, we further isolate the parcels that correspond to public housing. We identify these parcels by linking Landgrid parcel records to CoreLogic parcels using standardized parcel identifiers (APNs), and we supplement this linkage with a spatial match that assigns CoreLogic points to parcels based on geographic containment when an APN is unavailable. We classify a parcel as public housing if it is tax-exempt or if the owner name contains strings indicative of public ownership (e.g., "housing" or "public") and it also lies inside the broader project footprint.

We next match devices to public housing projects using inferred home locations. Home locations are identified from each device's overnight staypoints, calculated separately for each calendar quarter. A quarterly home location is assigned if the device is observed at the same overnight location at least five times during the quarter and if that location accounts for at least 60 percent of all overnight stays in the quarter. We retain devices whose inferred home locations fall within a public housing parcel in our sample.

The resulting sample includes 658 distinct public housing projects, 172 of which received HOPE VI revitalization grants. Within this sample, we estimate propensity scores for receiving a HOPE VI grant using project characteristics from the 1993 HUD USER project-characteristics data. Covariates include measures of project occupancy, tenant income, tenant rent, minority share, average household size, the share of female-headed households, and the share of single-parent households. We match each treated HOPE VI project to two control projects using nearest-neighbor matching on the propensity score. The resulting matched sample includes 67,655 device-quarter observations from 172 HOPE VI projects and 344 control projects. We then apply inverse propensity score weights to further adjust for remaining differences between treated and control projects.

The final analysis uses cross-sectional regressions, weighted by inverse propensity scores, that compare visit patterns of devices associated with revitalized projects to those associated with reweighted control projects. Project-level weights are further adjusted to account for differences in the number of observed device-quarters across projects.

For each device-quarter, we restrict attention to staypoints lasting at least 15 minutes. We compute the total number of minutes spent across all staypoints; the total number of minutes spent at staypoints located within one mile of the public housing project but outside the project footprint; and the total number of minutes spent within the public housing project itself. Our primary outcome is the share of time spent within one mile of the public housing project but outside the project footprint.

We validate our cross-sectional estimator using the cell phone data by examining differences in neighborhood poverty in HOPE VI vs. matched control sites both before and after treatment. Column 5 of Table A.9 regresses the 1990 Census block group poverty rate on an indicator for HOPE VI. We estimate this specification by weighted least squares using inverse propensity score weights, and we cluster standard errors at the project level. Consistent with our earlier results, propensity score reweighting eliminates pre-treatment differences between HOPE VI and comparison public housing sites. Column 6 repeats the analysis using 2017 poverty rates, seven years after the last HOPE VI grant. Public housing projects revitalized under HOPE VI and observed in the cell phone data experience a 7.6 percentage point decline in tract poverty rates.

Appendix D. Analysis of Cross-Class Interaction Using Facebook Data

This appendix describes how we analyze the effects of HOPE VI on cross-class interaction using Facebook data. A summary of the results can also be found on Facebook's "Data for Good" blog, here.

Data. We follow the methodology in Chetty et al. (2022*a*) and Chetty et al. (2022*b*) to construct measures of social capital at the high school-by-cohort level.

We begin by constructing an individual-level index of socioeconomic status (SES) by combining characteristics such as average neighborhood income and self-reported educational attainment using a machine-learning model. We then rank individuals within their national SES distribution by birth cohort to obtain percentile ranks. We link parents and children using a combination of self-reports and various imputation methods and classify individuals into a low-SES group (parental SES below the national median) and a high-SES group (parental SES above the median).[47] For simplicity, we refer to the low-SES group as "low-income" and the high-SES group as "high-income" in the main text.

For each individual, we construct two measures of social capital. (i) *Economic Connectedness (EC):* the share of an individual's high school friends who are high-SES. (ii) *Exposure:* the share of peers in an individual's high school cohort who are high-SES. For both measures, we restrict attention to friendships between users who attended the same high school and graduated within three years of one another.

We restrict the sample to low-SES individuals who were born between 1981 and 2004. We further require that the users have logged into Facebook at least once in the past 30 days, have at least 100 U.S.-based Facebook friends, have a non-missing U.S. ZIP code, and report their sex. We then collapse the data to the high school-by-cohort level, taking the average values of EC and exposure among users in our sample who are in the same cohort and attended the same high school.

[47] "Self-report" refers to reporting another user as a child or parent on one's own Facebook profile. For users who we cannot match using profile self-reports, we use three imputation methods. We use public wall posts that indicate family relationships (e.g., Mother's Day posts), matching based on age and last name, and family relationships (e.g., triadic closures based on siblings who report parents

We use data from the National Center for Educational Statistics (NCES) to measure the location and characteristics of each high school. We match NCES school identifiers to high schools observed in the Facebook data. For each high school, we also measure school characteristics, including the shares of White, Black, and Hispanic students, the race Herfindahl-Hirschman index, total enrollment, number of full-time teachers, and the student-teacher ratio.

We use publicly available data from Staiger, Palloni and Voorheis (2024) to measure the location of public housing projects and their HOPE VI treatment status. These data also include characteristics of the Census tracts in which the projects are located as well as characteristics of the projects themselves, such as total number of occupied units.

For each public housing project, we identify all high schools within a 5 km radius and calculate the mean characteristics (e.g., EC and exposure) of those nearby high schools. We restrict the HOPE VI sample to grants awarded between 1996 and 2003, as most awards occurred during this period and the coverage of the Facebook data is less complete outside these years. We drop non-HOPE VI projects that are located within 5 km of a HOPE VI project.

We estimate that in the average school year, there are 2,000 below-median-SES students on average in the high schools near each HOPE VI site and 160 of those students reside in revitalized HOPE VI projects, with $8\% = 160/2,000$. We arrive at the former number by measuring the total number of high school students using the NCES and multiplying this number by the share of students who have below-median SES according to the Facebook data. We arrive at the latter number by multiplying the average number of units in revitalized projects by 2.4 (since the average household had 2.4 children) and 4/18 (assuming that the age distribution of children in public housing is uniform).

Analysis. Our estimand of interest is the effect of HOPE VI on average measures of EC and exposure in nearby schools. Our empirical strategy is analogous to the approach described in Section IV.B, but adapted to the Facebook data environment.

For each HOPE VI grant year between 1996 and 2003, we construct a dataset that includes the HOPE VI awardees from that year and all non-HOPE VI projects. We measure project, school, and tract characteristics three years before the grant year.[48] Figure A.13.A presents estimates from a balance test in which we regress baseline characteristics on an indicator for HOPE VI receipt; HOPE VI projects differ systematically from other public housing projects along many dimensions.

For each grant year between 1996 and 2003, we estimate a propensity score by estimating a logistic regression of HOPE VI award receipt on tract-level poverty rates, tract-level share Black, the log number of residents in the public housing project, mean exposure in schools within 5 km of the project, mean EC in schools within 5 km of the project, the log number of high-SES students in nearby schools, and the log number of low-SES students in nearby schools. We drop control projects with estimated propensity scores outside the support of the HOPE VI sites and conduct one-to-one nearest-neighbor matching with replacement. We then re-estimate the propensity score within the matched sample and stack the datasets from each grant year to construct a single dataset containing all HOPE VI projects and their matched controls. Matched pairs are indexed by m. The sample contains 203 HOPE VI projects and 164 unique matched control projects (control projects can be matched to multiple HOPE VI projects). We then merge on the nearby high schools to produce a high school-by-project-by-cohort dataset, which we use in our analysis. We reweight the matched control projects using inverse propensity score weights to match the HOPE VI projects on observable characteristics. Figure A.13 replicates the balance tests from panel A, but on the matched sample and using the inverse propensity score weights; our procedure achieves balance along observable dimensions.

We estimate a local projections difference-in-differences (LP-DiD) specification to measure the causal

[48]Tract characteristics are taken from the 1990 and 2000 decennial censuses, with missing values between decennial years forward filled.

effect of revitalization on social capital (Dube et al., 2025). The LP-DiD regression is:

$$\Delta Y_{i,t+j} = \beta^j D_i + \gamma X_{i,t-3} + \eta_m^j + \varepsilon_{i,t}^j, \tag{D.1}$$

where i denote a school by project, t denotes the year the HOPE VI grant was awarded, $j \in \{-9, \dots, 15\}$ indexes years relative to the grant year, and $\Delta Y_{i,t,j} = Y_{i,t+j} - Y_{i,t-3}$ is the difference in outcomes between year $t+j$ and year $t-3$. D_i is an indicator for whether i is impacted by a HOPE VI grant. $X_{i,t-3}$ is a vector of covariates measured in year $t-3$, including shares of students who are White, Black, and Hispanic; the race Herfindahl-Hirschman index; total students; number of full-time teachers; distance between the school and project; measures of economic connectedness for both low-SES students and all students; and the share of students who are low-SES. η_m^j are matched-pair fixed effects. We estimate 25 separate regressions, one for each value of $j \in [-9, 15]$ and plot the resulting estimates of β^j in Figure A.14. We weight the regressions by the product of the inverse propensity score weight and the total number of low-SES students in schools linked to the project. Standard errors are clustered at the project level.

We conduct two robustness checks to assess the validity of our research design. First, we compare the estimated effects on EC and exposure for schools within 5 km of HOPE VI projects to those for high schools located 5–30 km away. As shown in Figure A.15, we find no evidence of impacts on either outcome for schools farther from the projects, consistent with the localized nature of the impacts. Second, we replicate our analysis using as "treated" the set of projects that applied for but did not receive HOPE VI funding. Figure A.16 shows that these failed applicants generate no detectable effects, supporting the view that our main results are not driven by selection into applying for the HOPE VI program.

Appendix E. Candidate Neighborhoods for Connection-Based Revitalization

This appendix describes how we identify a set of low-income, low-opportunity Census tracts that are surrounded by higher-opportunity neighborhoods and thereby are likely to benefit most from efforts to increase integration with surrounding areas, based on the findings in Figure 8.A.

We begin with a tract-level dataset covering the full U.S. From the 2019–2023 American Community Survey (five-year tabulations), we measure population, population density, the share of residents under age 18, share of adults who are full time students, the poverty rate, and median household income. From the Opportunity Atlas, we measure the number of children in the 1978–1983 birth cohorts that grew up in that tract as well as two characteristics of those children: (i) upward mobility, defined as the mean adult income rank for children whose parents were at the 25th percentile of the national income distribution, and (ii) the unconditional mean adult income rank. From the NBER tract distance database, we compute distances between tract centroids and all tracts within 5 miles.

We restrict the sample to residential, disadvantaged census tracts in the 100 largest metropolitan statistical areas (MSAs). We classify a tract as non-residential if the area of the tract is greater than 5 square miles or any of the following measures fall below the 5th percentile: total population, population density, the share of adults who are not students, the share of residents who are children, or the number of children. We define disadvantaged tracts using both absolute and within-MSA thresholds: poverty rates above 10%, median household income below $100,000, and upward mobility below the 50th percentile rank, and—relative to other tracts in the same MSA—poverty above the MSA median, median household income below the MSA median, and upward mobility in the bottom quartile of the MSA distribution.

For each tract, we define the *opportunity gap* as the difference in mean (unconditional) income ranks in adulthood between children who grew up in the 10 nearest tracts within a five-mile radius and the mean income ranks of children raised in low-income (25th percentile) families in the focal tract. Our set of candidate tracts for connection-based revitalization includes any tract that either: (i) has an opportunity gap in the top quartile of the distribution across residential, disadvantaged census tracts in the 100 largest MSAs, or (ii) is among the five tracts with the largest opportunity gap within any of the 50 largest cities (to ensure coverage across major cities).

The resulting list of the selected tracts (along with underlying data for all tracts in the U.S. and additional demographic covariates) is available here and can be visualized in the Opportunity Atlas here.

TABLE A.1
Summary Statistics for Public Housing vs. Nearby Residents

	HOPE VI, P.H. Residents	HOPE VI, Non-P.H. Residents	Individuals Living Within One Mile of HOPE VI Projects	Matched Controls, P.H. Residents
	(1)	(2)	(3)	(4)
Mean H.H. Income	\$11,830	\$27,980	\$37,570	\$9,694
Percent With H.H. Income Below \$15,000	65.94%	49.68%	46.34%	74.85%
Percent With H.H. Income \$15,000–\$30,000	23.73%	18.90%	18.07%	19.60%
Percent With H.H. Income \$30,000–\$50,000	8.57%	15.47%	13.91%	4.47%
Percent With H.H. Income \$50,000–\$150,000	1.77%	13.96%	17.81%	1.07%
Percent Non-Hispanic Black	74.88%	62.91%	49.17%	71.00%
Percent Non-Hispanic White	6.52%	14.36%	27.11%	8.40%
Percent Hispanic	11.49%	11.81%	13.89%	15.39%
Mean Number of Adults	293	256	29,620	491

Notes: This table reports the average characteristics of adults living in or near HOPE VI and matched control public housing projects ten years after the grant. Column 1 includes adults living in the HOPE VI revitalized public housing (P.H.) units. Column 2 includes adults living in the footprint of the original HOPE VI project but in non-public housing units. Column 3 includes adults who live within one mile of a HOPE VI site, excluding the site itself. Column 4 includes adults living in the matched control public housing projects. H.H. denotes household.

TABLE A.2
Impact of HOPE VI Revitalization on Adults' Incomes

	Adjusted Gross Income of		
	All Residents	Original P.H. Residents	Current P.H. Residents (Contemporaneous Minus Pre-Move In)
	(1)	(2)	(3)
Panel A. Comparison Group: Matched Control Projects			
HOPE VI x Years 0-5 After Award	554.9**	733.5*	109.2
	(247.8)	(435.2)	(327.2)
HOPE VI x Years 6-15 After Award	4,808***	583.8	-165.6
	(472.9)	(521.2)	(230.3)
Observations	26,500	15,500	19,500
Panel B. Comparison Group: Failed Applicant Projects			
HOPE VI x Years 0-5 After Award	772.6**	49.8	193.2
	(340.0)	(456.8)	(212.8)
HOPE VI x Years 6-15 After Award	4,365***	326.8	1.5
	(815.1)	(576.6)	(192.2)
Observations	71,500	44,500	52,500

Notes: This table presents parametric estimates corresponding to those shown in Figure 2, which presents the impact of HOPE VI on adults' incomes. While the figure presents estimates for each year relative to the HOPE VI grant, this table presents estimates from a specification in which we parameterize the coefficients in three periods: before the award ($j < 0$), short run ($j \in [0,5]$), and long run ($j \in [6,15]$). We report estimates for the short- and long-run impacts. Columns 1–3 of Panel A correspond to the green, orange, and navy series in Figure 2. Panel B presents estimates using projects that applied for but did not receive HOPE VI funding as the comparison group (instead of the matched controls). P.H. denotes public housing. Standard errors are reported in parentheses. $*$ $p < 0.1$; $**$ $p < 0.05$; $***$ $p < 0.01$.

TABLE A.3
Impact of HOPE VI Revitalization on Children's Outcomes in Adulthood: Heterogeneity Analysis

	Child H.H. Income Rank at Ages 29–30		
	(1)	(2)	(3)
HOPE VI x Moved In After	3.717***	3.532***	2.869***
	(1.270)	(1.219)	(0.772)
HOPE VI x Moved In After x Male	-1.010		
	(1.275)		
HOPE VI x Moved In After x Parental Income		-1.192	
		(0.841)	
HOPE VI x Moved In After x Predicted Adult Income			0.732
			(0.460)
Observations	373,000	373,000	373,000

Notes: This table presents estimates from a variant of the specification in Column 1 of Table 2 in which we include an additional covariate, as well as the full set of interactions between that covariate, an indicator for moving into the project after the year of the HOPE VI grant, and an indicator for HOPE VI. In Columns 1–3, the covariate is an indicator for being male, parental income rank, and predicted income in adulthood, respectively. Parental income rank and predicted adult income are each standardized to have a mean of zero and a standard deviation of one using the control-group mean and standard deviation. Standard errors are reported in parentheses. $*\ p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$.

TABLE A.4
Impact of HOPE VI Revitalization on Children's Incomes in Adulthood, by Age of Income Measurement

	Child H.H. Income Rank at Age						
	24	25	26	27	28	29	30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Baseline Movers Specification							
HOPE VI x Years Exposed	0.062	0.116	0.117	0.182	0.362***	0.483***	0.492**
	(0.103)	(0.101)	(0.110)	(0.135)	(0.131)	(0.145)	(0.207)
Observations	139,000	118,000	99,500	82,000	67,000	53,000	41,000
Panel B. Movers Specification with Household Fixed Effects							
HOPE VI x Years Exposed	0.079	0.177	0.225	0.435**	0.372**	0.595***	0.513**
	(0.152)	(0.162)	(0.191)	(0.177)	(0.189)	(0.211)	(0.249)
Observations	153,000	134,000	117,000	99,500	84,000	69,500	57,000

Notes: Panel A of this table reports estimates from the specification in Column 1 of Table 3, and Panel B reports estimates from the specification in Column 12. The outcome variables in Columns 1–7 are the child's household income percentile rank at ages 24–30, respectively. Standard errors are reported in parentheses. $*\ p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$.

TABLE A.5
Heterogeneity in Impacts of HOPE VI Revitalization by Nearby Peer Income

	Child H.H. Income Rank at Ages 29–30			Predicted Child H.H. Income Rank at Ages 29-30	Child H.H. Income Rank Ages 29–30			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Nearby Peers' Adult Income Rank	0.150***	0.173***	0.189***	0.006**		0.156***	0.596***	0.453***
	(0.046)	(0.030)	(0.028)	(0.002)		(0.027)	(0.097)	(0.058)
HOPE VI x Moved In After x Nearby Peers' Adult Income Rank	0.274***		0.213***	0.001		0.150**	0.211**	0.243**
	(0.075)		(0.077)	(0.004)		(0.071)	(0.107)	(0.105)
HOPE VI x Nearby Peers' Adult Income Rank		0.055	0.057	0.002		0.052	0.036	0.064
		(0.044)	(0.045)	(0.003)		(0.036)	(0.046)	(0.056)
HOPE VI x Years Exposed					0.552***			
					(0.151)			
HOPE VI x Years Exposed x Nearby Peers' Adult Income Rank					0.260*			
					(0.153)			
Move In After Award Only	X				X			
Move Out Before Award Only		X						
Project FE						X	X	
Split Sample IV							X	X
Observations	41,500	95,500	137,000	137,000	53,000	137,000	134,000	134,000

Notes: This table presents parametric regression estimates of how the effects of childhood exposure on children's income in adulthood vary with nearby peers' incomes. Column 1 presents estimates that correspond to the green series in Figure 8.A, which reports an estimate of ρ^{post} from a variant of equation (24). Column 2 presents estimates that correspond to the gray series. Column 3 presents estimates from the stacked regression described in equation (24), where the sample includes both prior and new residents. Column 4 uses the same specification as Column 3, but the outcome is the child's predicted income in adulthood. Column 5 presents estimates from a variant of the specification used in Column 1 of Table 2, additionally controlling for the mean adult income rank of nearby peers, as well as the full set of interactions between this covariate, an indicator for HOPE VI, and years exposed to the public housing project. Column 6 replicates Column 3, including housing project fixed effects. Column 7 modifies the specification in Column 6 by splitting the sample in two, constructing two independent estimates of the mean income rank of nearby peers, and instrumenting for one using the other. Column 8 modifies the specification in Column 7 by replacing the project fixed effects with a HOPE VI indicator. Standard errors are reported in parentheses. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$.

TABLE A.6
Impact of HOPE VI Revitalization on Time Spent Living in Public Housing

	Years in Public Housing During Childhood	
	(1)	(2)
HOPE VI x Moved In After	0.415***	0.287
	(0.128)	(0.571)
HOPE VI x Moved In After x Nearby Peers' Adult Income Rank		0.003
		(0.015)
Observations	102,000	102,000

Notes: This table presents estimates from a variant of the specification in Column 3 of Table A.5. The outcome variable is the number of years the child spent in the HOPE VI or matched control public housing project during childhood. The sample is limited to children who moved out of a HOPE VI or matched control project more than one year before the grant year and children who moved in at least five years after the grant year. Column 1 excludes the average adult income of nearby peers, along with all interactions that include this variable. Standard errors are reported in parentheses. $*\ p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$.

TABLE A.7

Heterogeneity in Impacts of HOPE VI by Nearby Peer Income and Other Project Characteristics

	Child H.H. Income Rank at Ages 29–30					
	(1)	(2)	(3)	(4)	(5)	(6)
Nearby Peers' Adult Income Rank x HOPE VI x Moved In After	1.118***	1.286**	1.348**			
	(0.420)	(0.505)	(0.574)			
Nearby Peers' Adult Income Rank x HOPE VI	0.149	0.182	-0.125			
	(0.319)	(0.335)	(0.352)			
Nearby Peers' Parent Income Rank x HOPE VI x Moved In After				0.981**	1.129**	0.997**
				(0.388)	(0.456)	(0.434)
Nearby Peers' Parent Income Rank x HOPE VI				0.048	0.037	-0.103
				(0.293)	(0.313)	(0.279)
Pre-Award Poverty Rate x HOPE VI x Moved In After		0.501	0.620		0.440	0.582
		(0.608)	(0.573)		(0.624)	(0.554)
Pre-Award Poverty Rate x HOPE VI		0.119	0.129		0.030	0.124
		(0.322)	(0.291)		(0.340)	(0.288)
Pre-Award Incarceration Rate x HOPE VI x Moved In After			-0.385			-0.407
			(0.582)			(0.583)
Pre-Award Incarceration Rate x HOPE VI			-0.094			-0.095
			(0.210)			(0.212)
Pre-Award Share Black x HOPE VI x Moved In After			0.055			-0.327
			(0.641)			(0.627)
Pre-Award Share Black x HOPE VI			0.178			0.211
			(0.368)			(0.348)
Pre-Award Project Size x HOPE VI x Moved In After			0.338			0.331
			(0.477)			(0.491)
Pre-Award Project Size x HOPE VI			0.138			0.102
			(0.243)			(0.243)
Prior Resident Child Adult Income x HOPE VI x Moved In After			-0.520			-0.436
			(0.881)			(0.885)
Prior Resident Child Adult Income x HOPE VI			0.848			0.842
			(0.540)			(0.528)
Observations	137,000	137,000	137,000	137,000	137,000	137,000

Notes: Column 1 of this table presents estimates from a variant of the specification in Column 3 of Table A.5 in which the peer income variable is defined at the project-level, instead of the project-by-demographic-cell-level. Specifically, the peer income variable is the average income rank in adulthood for all children who lived within a one-mile radius of the project. This variable is standardized to have mean zero and standard deviation one using the control-group mean and standard deviation. Column 2 additionally controls for the poverty rate in the project three years prior to the HOPE VI grant (also standardized), as well as the full set of interactions between this variable, an indicator for moving into the project after the grant year, and an indicator for HOPE VI. Column 3 adds the following covariates (also standardized and interacted with the same two indicators): the share of adults who lived in the project three years before the HOPE VI grant and were incarcerated in either the 2000 or 2010 Census, the share of adults who lived in the project three years before the grant who were Black, the number of adults living in the project three years before the grant (project size), and the average adult income rank of children who moved out at least three years before the award. Columns 4–6 are identical to Columns 1–3 except that they replace peers' own adult income rank with the average income rank of peers' parents. Standard errors are reported in parentheses. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$.

TABLE A.8

Impact of HOPE VI Revitalization on Children's Cohabitation Patterns in Adulthood

	Percent Cohabitating with Anyone	Percent Cohabitating with a 2010 Neighbor					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HOPE VI	-0.773	5.327**		5.131**	5.252**		-0.118
	(0.622)	(2.626)		(2.594)	(2.612)		(1.198)
HOPE VI x Years Exposed			0.801*				
			(0.453)				
HOPE VI x Nearby Peers' Adult Income Rank				-2.056			
				(1.900)			
HOPE VI x Nearby Peers' Parent Income Rank					-1.070		
					(1.956)		
Partial Demolition						-1.273	
						(2.376)	
Predicted Cross-Sectional Effect			4.912				
			(2.777)				
Focal Group of Children	P.H. Residents	P.H. Residents	P.H. Residents	P.H. Residents	P.H. Residents	P.H. Residents	Within 0.2 Miles of P.H.
Dep. Var. Mean In Control Group	8.780	8.112	8.245	8.112	8.112	10.220	5.514
Observations (Projects)	700	700		700	700	850	350
Observations (Individuals)	51,500	6,600	5,300	6,600	6,600	10,000	2,700

Notes: This table presents regression estimates of the impacts of HOPE VI on children's cohabitation patterns in adulthood. Column 2 reproduces the estimates reported in Panel A of Figure 10. Column 1 presents estimates from the same specification, but the outcome is an indicator for whether the child is cohabiting with anyone in 2020, measured among all children who lived in a HOPE VI or matched control public housing project between ages 8 and 17 in 2010. Column 3 presents estimates from the movers specification used in Column 1 of Table 3, where the sample is the subset of individuals from Column 2 with valid entry and exit dates. The predicted cross-sectional effect equals the estimated coefficient multiplied by the average years of exposure. Columns 4 and 5 modify the specification in Column 2 by additionally including the mean income rank of nearby peers (Column 4) or the mean income rank of their parents (Column 5), as well as the interaction between this covariate and the HOPE VI indicator (as in Figure 8). Column 6 is analogous to Column 2 except that the treatment group is the set of partially demolished projects (as in Figure 3.B) rather than HOPE VI Revitalization projects. Column 7 is analogous to Column 2 except that focal children are defined as those who live outside but within 0.2 miles of the HOPE VI or matched control projects (instead of public housing residents), and nearby children are defined as those who live within the projects and within 0.2–1 miles (instead of within 1 mile). Columns 1–2 and 4–7 are estimated on project-level data, so we also report the number of projects. P.H. denotes public housing. Standard errors are reported in parentheses. $*\ p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$.

TABLE A.9

Impact of HOPE VI Revitalization on Public Housing Residents' Time Spent in Surrounding Neighborhood

	Percent of Time Spent in One-Mile Radius Outside of Project	Percent of Time Spent in Project	Number of Visits Per Day in One-Mile Radius Outside of Project	Percent of Time Spent in One-Mile Radius Outside of Project	Poverty Rate in Block Group Containing Housing Project in 1990	Poverty Rate in Block Group Containing Housing Project in 2017
	(1)	(2)	(3)	(4)	(5)	(6)
HOPE VI	2.290***	-3.831***	0.068***	2.348***	1.238	-7.637***
	(0.508)	(0.608)	(0.025)	(0.510)	(1.910)	(1.450)
Additional Covariates				X		
Dep. Var. Mean In Control Group	6.479	81.314	0.384	6.479	48.284	41.355
Observations	67,655	67,655	67,655	67,655	67,655	67,655

Notes: This table presents estimates of the effect of HOPE VI on time spent in nearby neighborhoods. Column 1 reproduces the estimates reported in Panel B of Figure 10. Columns 2 and 3 present estimates from the same specification, but the outcome in Column 2 is the percent of time spent in the public housing development and the outcome in Column 3 is the number of visits per day to locations within a one-mile radius of the public housing project. Column 4 presents a variant of the specification in Column 1 that controls for the 1990 poverty rate in the Census block group and for the following project characteristics measured in 1993: number of occupied units; average resident income; average household rent, average household size; shares of households with a female head, with a single parent, or with the majority of income from wages; and shares of residents who are disabled, age 62 or older, overhoused, or minority. Column 5 replicates the specification in Column 1 using the poverty rate in the block group containing the public housing project measured in the 1990 Census (pre-revitalization). Column 6 replicates Column 5 using the poverty rate in 2017 (post-revitalization) as the outcome. Standard errors are reported in parentheses. $* \; p < 0.1$; $** \; p < 0.05$; $*** \; p < 0.01$.

TABLE A.10
Neighborhood Integration Index: Association Between Own and Nearby Peers' Incomes in Adulthood

	Child H.H Income Rank at Ages 29–30				
	(1)	(2)	(3)	(4)	(5)
Panel A. Pooled Regression with Single Coefficient					
Nearby Peers' Adult Income Rank	0.1146***	0.1100***	0.1031***		
	(0.0157)	(0.0098)	(0.0013)		
Panel B. Summary of Tract-Specific Coefficients					
Mean of θ_c			0.1081	0.0403	0.0410
Signal Standard Deviation of θ_c			0.1261	0.0912	0.0855
Panel C. Summary Statistics for Estimation Sample					
Mean Own Adult Income Rank	36.51	42.12	47.61	47.57	61.37
Mean Peer Adult Income Rank	41.52	44.47	48.75	54.24	53.36
Mean Own Parent Income Rank	17.35	27.55	35.42	35.22	63.25
Mean Peer Parent Income Rank	29.03	32.37	37.97	51.80	45.37
Focal Neighborhood	Matched Control Projects	Census Tracts Containing Matched Control Projects	Low-Income Census Tracts	Low-Income Census Tracts	High-Income Census Tracts
Nearby Peers	Blocks Within 1 Mile	10 Nearest Tracts	10 Nearest Tracts	10 Nearest High-Income Tracts	10 Nearest Low-Income Tracts

Notes: This table presents estimates of the association between children's outcomes in adulthood and those of their nearby peers for various subgroups. Panel A presents estimates from equation (25). Panel B report the mean and signal standard deviation of estimates of θ_c when we estimate equation (25), separately for each tract. In Column 1, the focal group consists of children who grew up in matched control public housing projects and nearby peers are those living within a one-mile radius. In Column 2, the focal group consists of all children who grew up in the Census tracts that contain the matched control projects, and nearby peers are those living in the ten nearest Census tracts. In Columns 3 and 4, the focal group consists of children who grew up in low-income tracts, defined as tracts with mean parental income rank below the median across tracts. Nearby peers are those living in the ten nearest Census tracts (Column 3) and those living in the ten nearest high-income Census tracts (Column 4), defined as tracts with mean parental income rank above the median across tracts. In Column 5, the focal group consists of children who grew up in high-income tracts and nearby peers are those living in the ten nearest low-income tracts. Panel C reports the mean income rank of children and their parents in the focal and nearby neighborhoods. Standard errors are reported in parentheses. $*\ p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$.

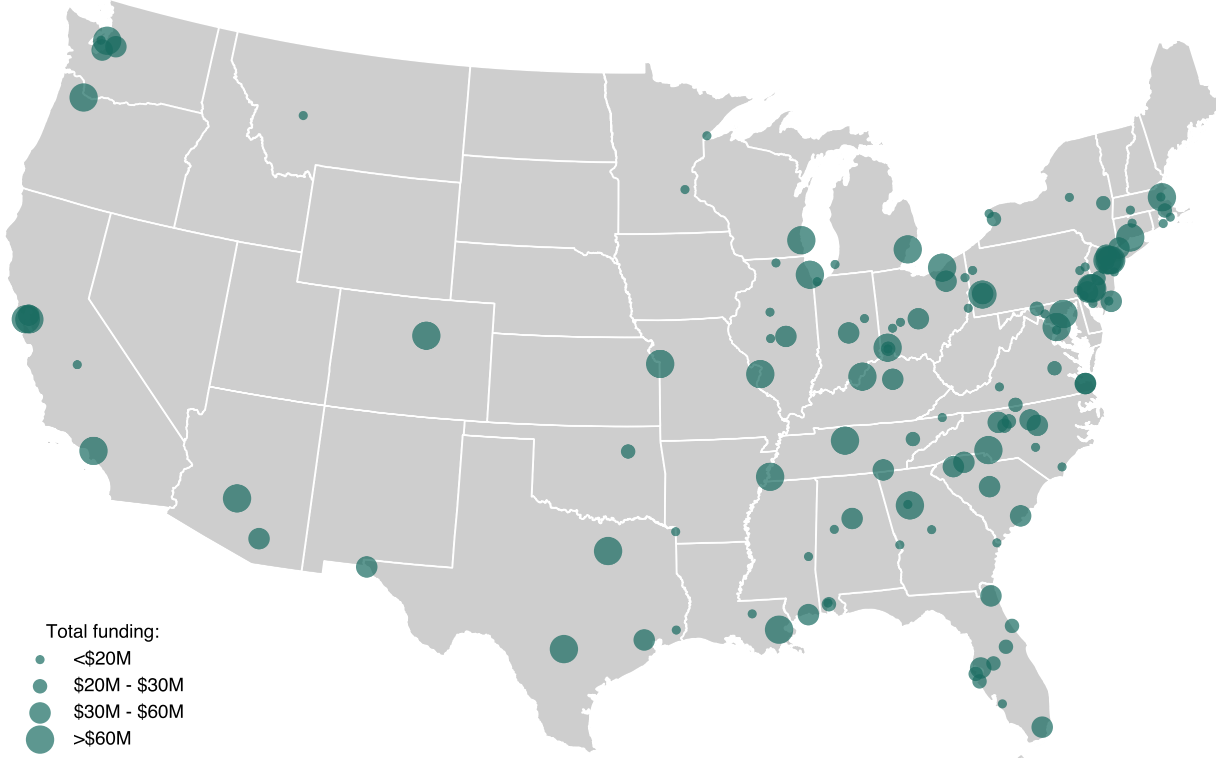
TABLE A.11

Children's Income in Adulthood vs. Social Integration with Nearby Tracts: National Estimates for Low-Income Tracts

	Mean Adult Income Rank for Children Born to Low-Income Parents (35th Percentile)		
	(1)	(2)	(3)
Neighborhood Integration Index	5.254***	6.266***	5.356***
	(0.763)	(0.982)	(1.116)
Split Sample IV		X	X
Parental Income Control			X
Observations	18,500	18,500	18,500

Notes: Column 1 presents regression estimates correponding to the navy series in Figure 11.A. In Column 2, we split the sample used to estimate the neighborhood integration index, $\hat{\theta}_c$, in Column 1 in half and use equation (25) to obtain two independent estimates, $\hat{\theta}_c^1$ and $\hat{\theta}_c^2$. We then estimate a two-stage least squares specification in which $\bar{y}_c^{P=35}$ is the outcome and we instrument for $\hat{\theta}_c^1$ using $\hat{\theta}_c^2$. Column 3 modifies Column 2 by additionally controlling for the average parental income rank of children in the focal tract. Standard errors are reported in parentheses. $*\ p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$.

FIGURE A.1
Location of HOPE VI Revitalization Sites



Notes: This map shows the location of all public housing authorities that received a HOPE VI Revitalization grant, replicating the list in Appendix A. The size of each circle represents the total amount of funding (in nominal dollars) received across all HOPE VI Revitalization grants for each public housing authority.

FIGURE A.2
Mill Creek Homes HOPE VI Site, Before and After Revitalization

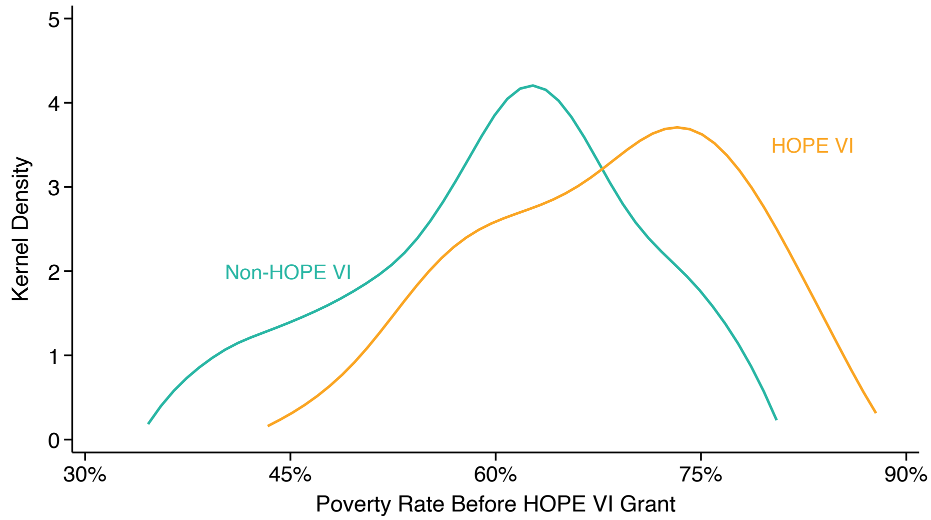
Panel A. Before Revitalization

Panel B. After Revitalization

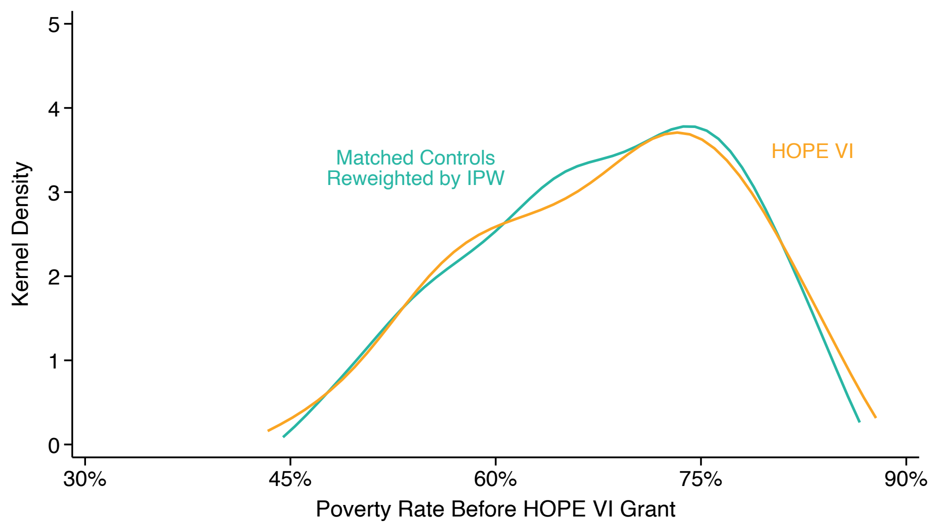
Notes: This figure shows an example of a HOPE VI project before and after revitalization. Panel A shows Mill Creek Homes in Philadelphia, which received a $35 million HOPE VI grant in 2001 (image credit: Louis I. Kahn Collection, University of Pennsylvania and Pennsylvania Historical and Museum Commission, University of Pennsylvania Stuart Weitzman School of Design). Panel B shows Lucien E. Blackwell Homes, a new project built on the footprint of the demolished Mill Creek Homes and constructed using HOPE VI funds (image credit: Apartments.com).

FIGURE A.3
Pre-Grant Distribution of Poverty Rates in HOPE VI vs. Non-HOPE VI Projects

Panel A. Before Matching

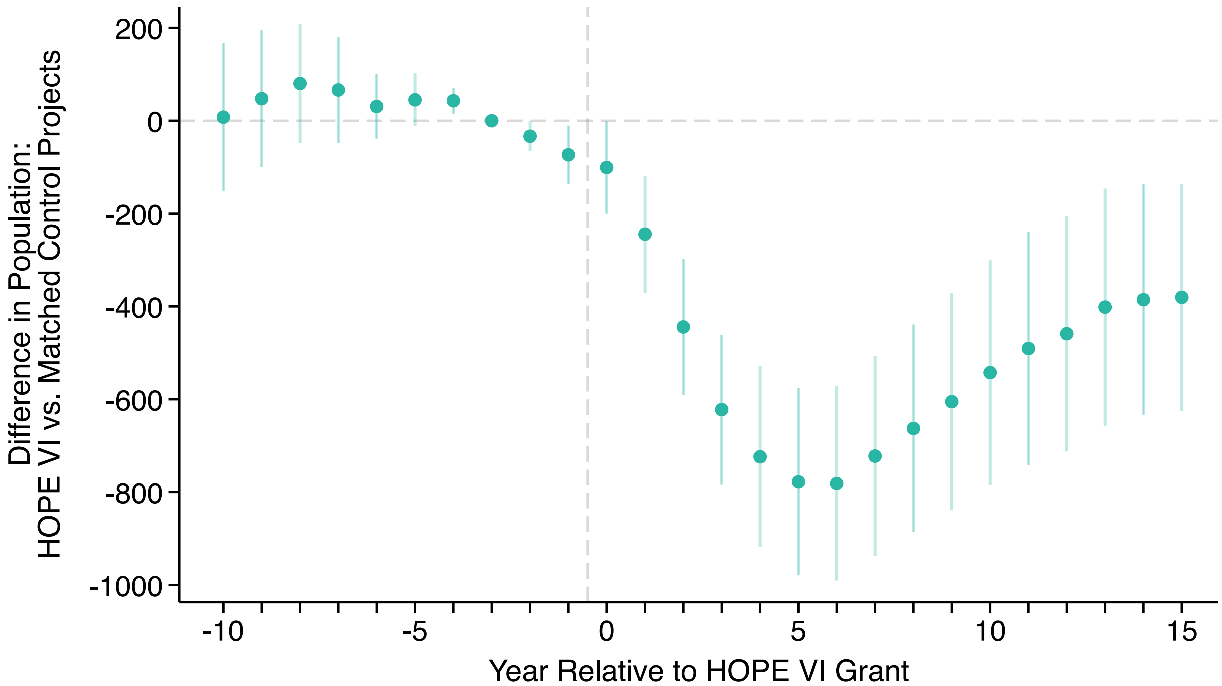


Panel B. After Matching



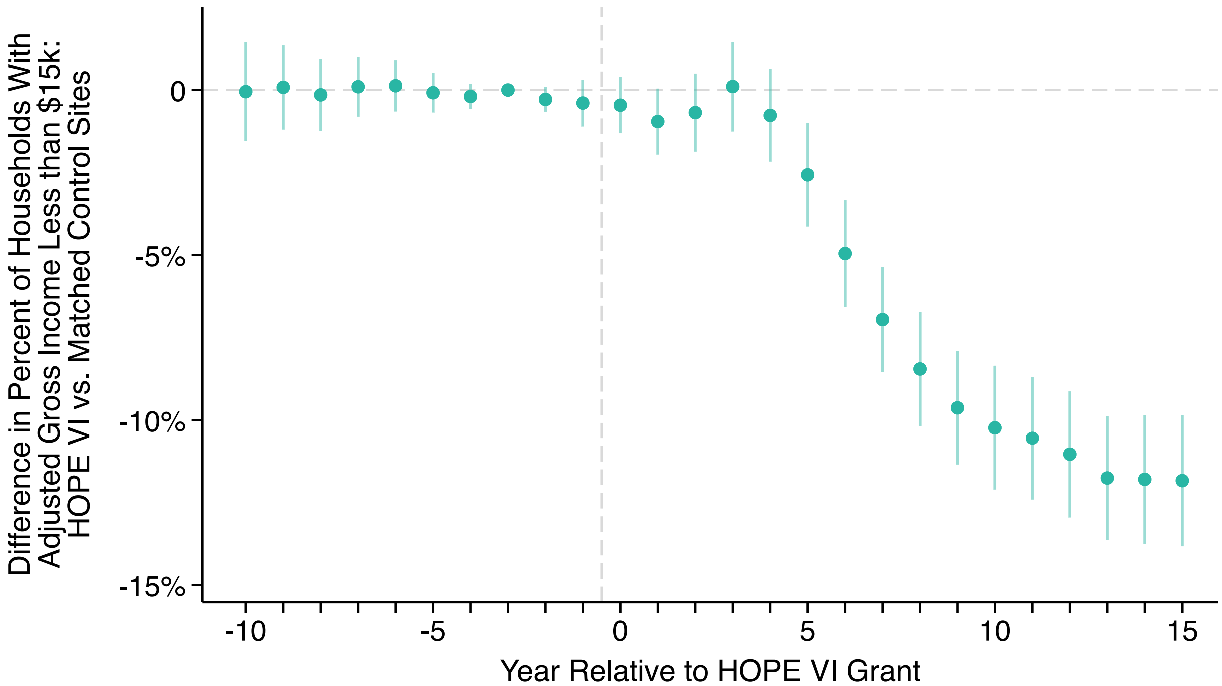
Notes: This figure shows the distribution of poverty rates for HOPE VI and non-HOPE VI projects before and after matching. The poverty rate is defined as the share of adult residents whose adjusted gross income is less than $15,000. Panel A shows, in green, all non-HOPE VI projects and, in orange, HOPE VI projects. Panel B shows, in green, matched control projects. The estimates in Panel A are weighted by project size, while the estimates in Panel B are weighted by inverse estimated propensity scores. The vertical axis reports the kernel density. As in Table 1, the HOPE VI and matched control samples are limited to observations from the three years prior to the grant year and the non-HOPE VI sample is a panel that includes observations between 1990 and 2007.

FIGURE A.4
Impact of HOPE VI Revitalization on Total Population



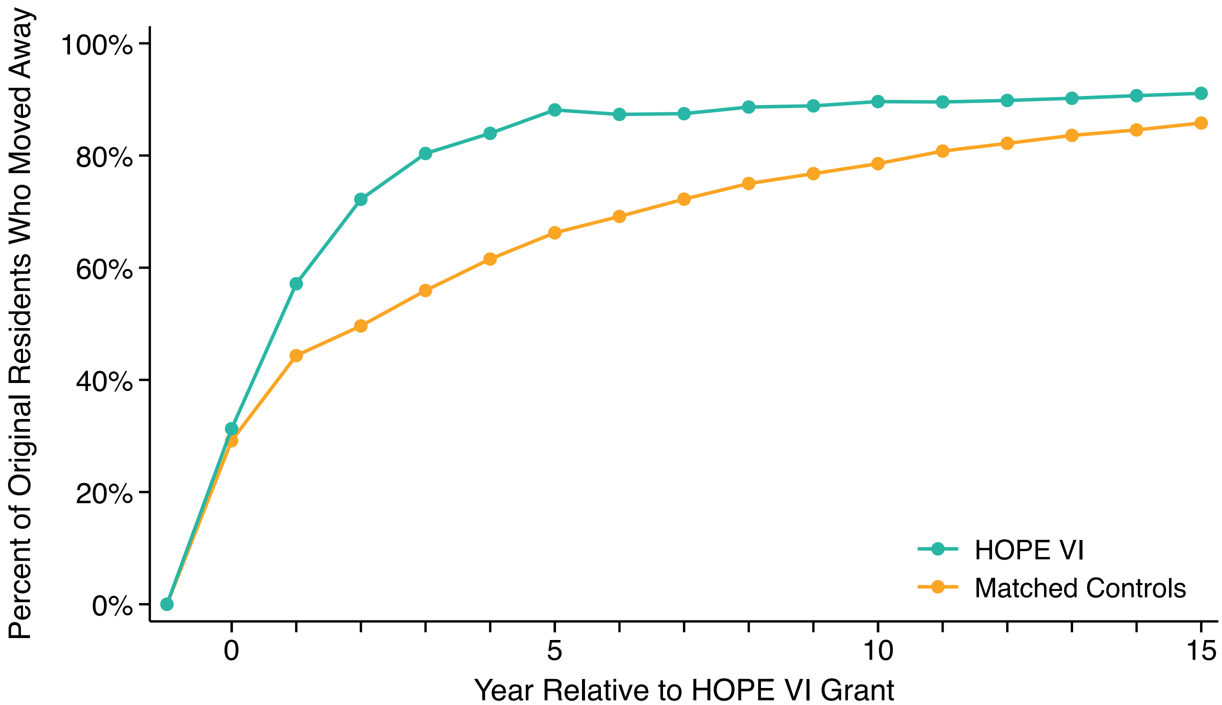
Notes: This figure presents estimates from a variant of the specification underlying the green series in Figure 2, where the outcome variable is the total number of people living in the physical footprint of the public housing project. Vertical bars denote 95% confidence intervals.

FIGURE A.5
Impact of HOPE VI Revitalization on Poverty Rates



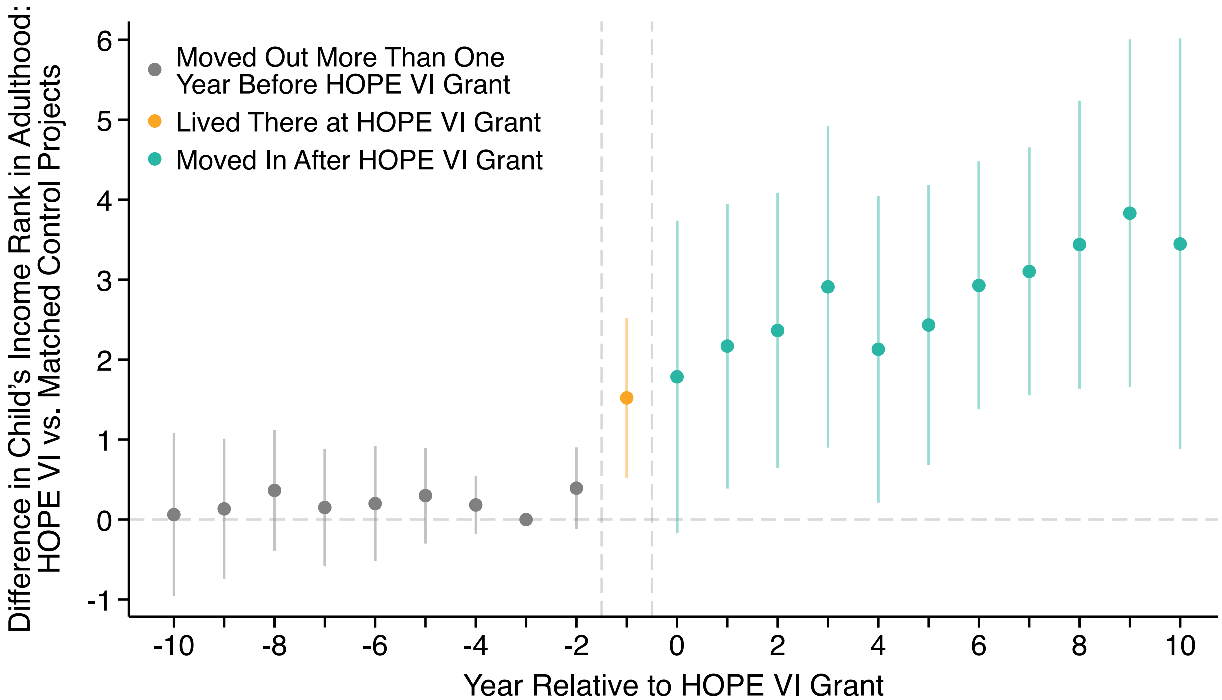
Notes: This figure presents estimates from a variant of the specification underlying the green series in Figure 2, where the outcome variable is the poverty rate, defined as the share of adults whose adjusted gross income is less than $15,000. Vertical bars denote 95% confidence intervals.

FIGURE A.6
Residential Mobility of Public Housing Residents in HOPE VI vs. Matched Control Projects



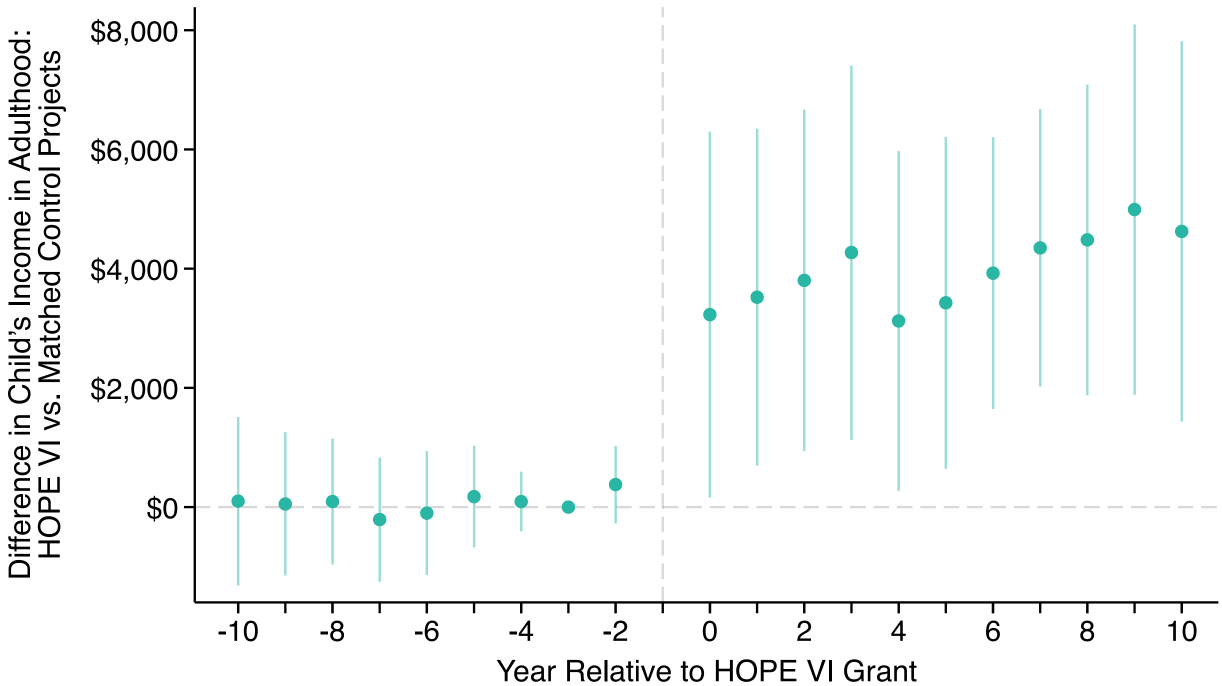
Notes: This figure focuses on public housing residents who lived in either a HOPE VI (green series) or matched control (orange) site one year prior to the grant. It plots the proportion of this sample who had left the site by each year after the grant.

FIGURE A.7
Impact of HOPE VI Revitalization on Children's Incomes in Adulthood, Including Original Residents



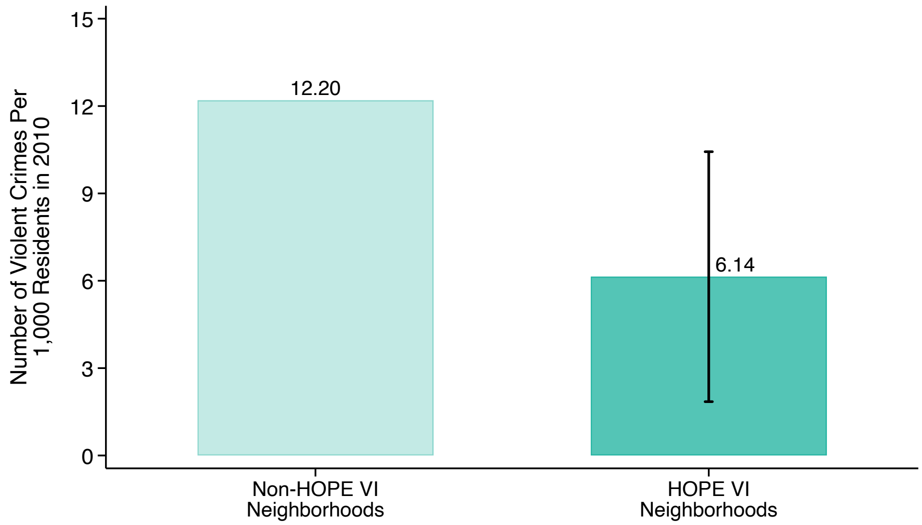
Notes: This figure presents estimates from a variant of the specification underlying the green series in Figure 3.A, where the analysis sample is expanded to include original residents (children who lived in a HOPE VI or matched control project in the year before the grant); estimates for this group are plotted in orange. Vertical bars denote 95% confidence intervals.

FIGURE A.8
Impact of HOPE VI Revitalization on Children's Incomes in Dollars in Adulthood



Notes: This figure replicates the green series in Figure 3.A, changing the outcome variable to the child's average annual household income at ages 29 and 30. Vertical bars denote 95% confidence intervals.

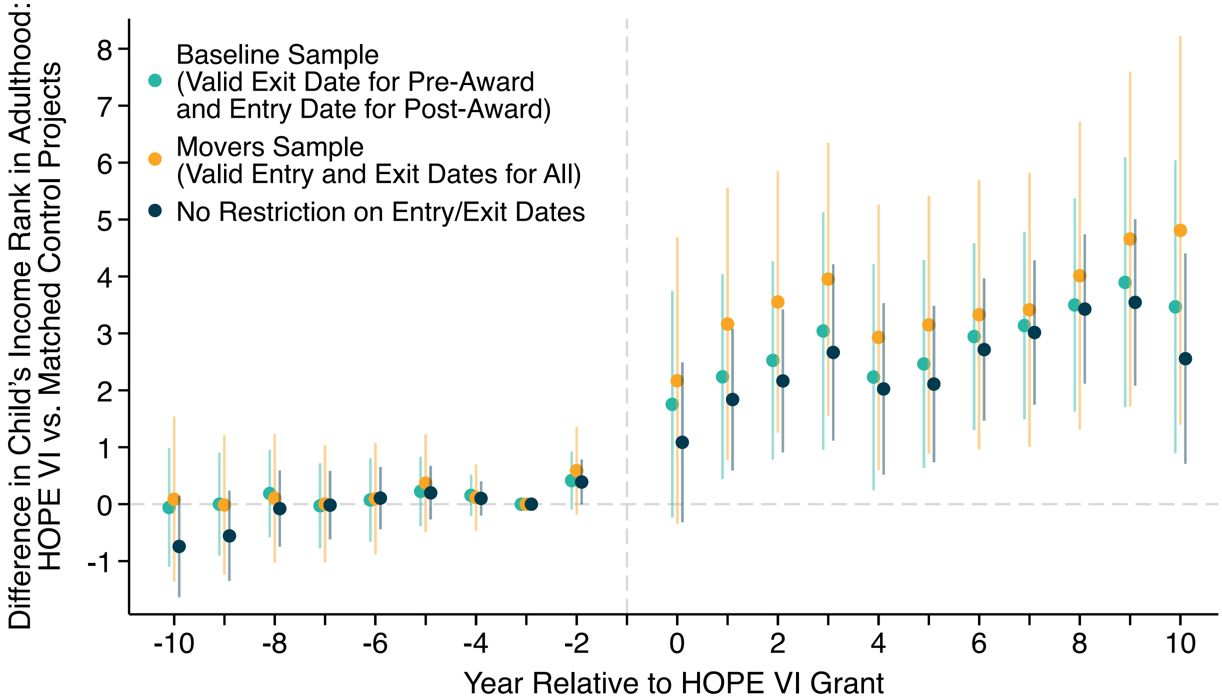
FIGURE A.9
Impact of HOPE VI Revitalization on Crime Rates



Notes: This figure presents the estimated effect of HOPE VI on Census tract-level crime rates. The number of violent crimes per 1,000 residents per year is measured at the tract level using data from the Neighborhood Crime Study, Wave 2. Because coverage varies widely across regions, we compare HOPE VI sites to other public housing sites within the same city in this figure. We limit the sample to tracts that contain public housing projects and are located in a city that received a HOPE VI grant. We restrict to projects that either received a HOPE VI revitalization grant between 2001 and 2005 or never applied for any HOPE VI funding. We then regress the change in the violent crime rate between 2000 and 2010 on an indicator for HOPE VI, controlling for city fixed effects and fixed effects for deciles of the 2000 violent crime rate and the 2000 poverty rate. Regressions are estimated using weighted least squares with 2000 population as weights. The bar on the left reports the mean violent crime rate in 2010 for non-HOPE VI projects; the bar on the right reports this mean plus the estimated treatment effect. Vertical bar denotes 95% confidence interval.

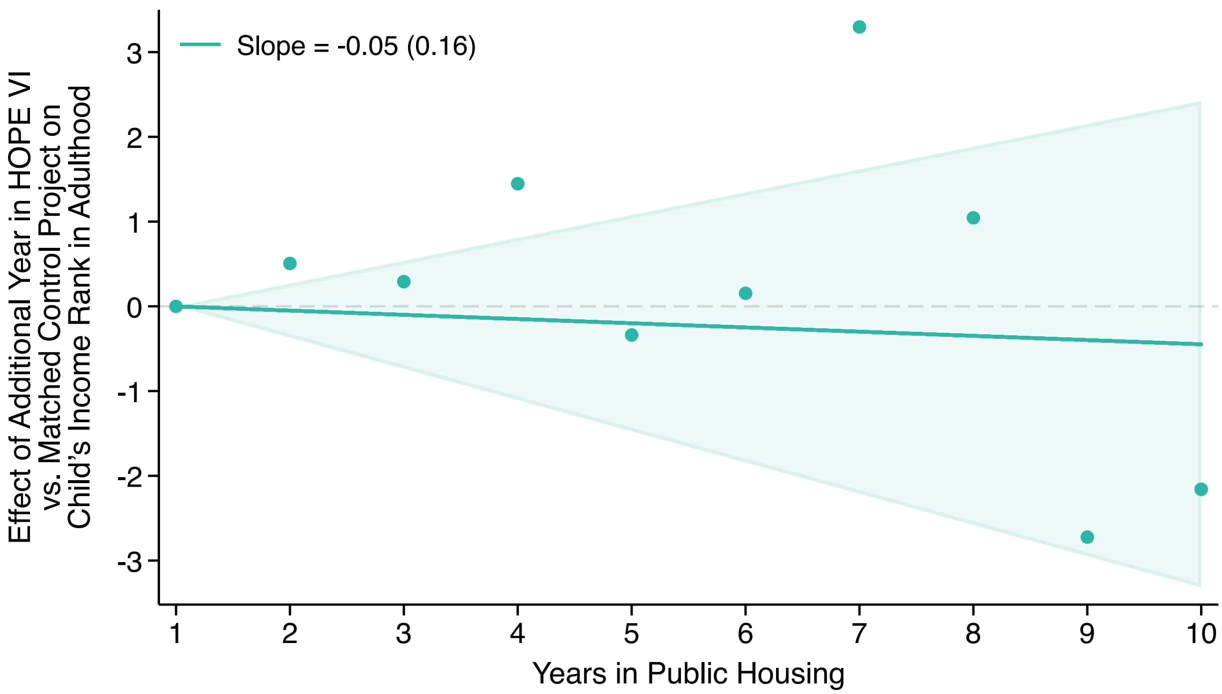
FIGURE A.10

Impact of HOPE VI Revitalization on Children's Incomes in Adulthood: Alternative Sample Restrictions



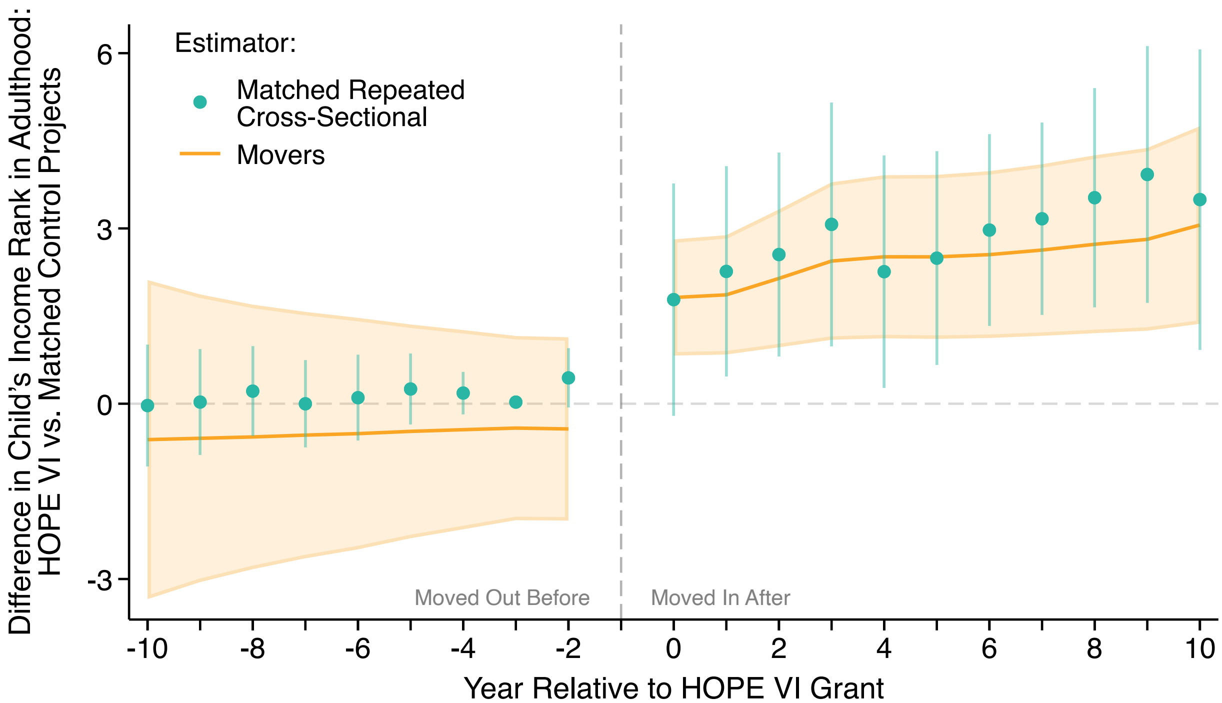
Notes: The green series in this figure replicates the green series in Figure 3.A. The baseline sample used to construct this series includes children who are observed in public housing in event year *j* and additionally requires an exit date for children who move out before the grant year and an entry date for children who move in after the grant year. The orange series replicates the green series, but requires that all children have both an entry and exit date. The navy series expands the sample to include all children living in public housing in a given year according to the HUD data, regardless of whether they have an entry or exit date. Vertical bars denote 95% confidence intervals.

FIGURE A.11
Pre-Grant Placebo Effect of Exposure to HOPE VI vs. Matched Control Projects on Children's Income in Adulthood



Notes: This figure replicates the green series in Figure 6, changing the sample to children who moved out of a HOPE VI or matched control project more than one year before the grant year. Vertical bars denote 95% confidence intervals.

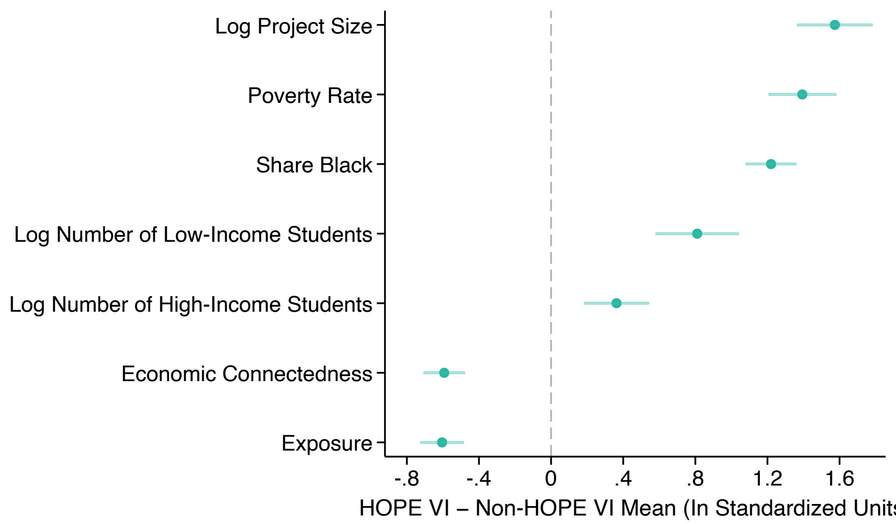
FIGURE A.12
Impact of HOPE VI Revitalization on Children's Income in Adulthood:
Repeated Cross-Sectional vs. Movers Estimates



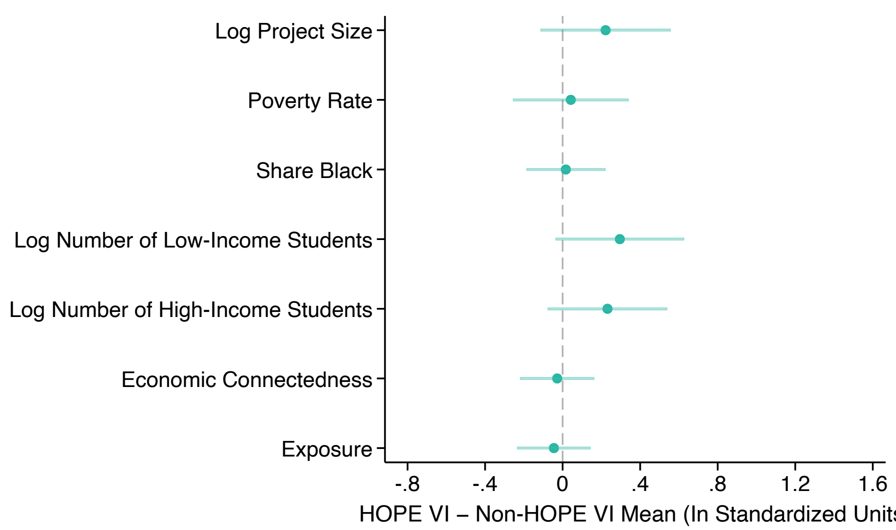
Notes: The green series in this figure replicates the green series in Figure 3.A but adds back pre-grant level differences. To estimate pre-grant differences, we identify children who lived in a HOPE VI or matched control public housing project three years before the grant year and who moved out more than one year before the grant year. We then regress the child's percentile rank of household income measured between ages 29 and 30 on an indicator for HOPE VI, controlling for grant-year-by-calendar-year fixed effects and the following individual-level covariates: pre-move-in parental income percentile, upward mobility in the prior tract, and years in public housing. We add this pre-award difference to each estimate in the green series in Figure 3.A. The orange series displays the predicted cross-sectional difference based on the movers estimates and average exposure. For post-grant event years ($j \geq 0$), we use the estimates from Column 4 of Table 4 . The estimate in each event year j is given by the exposure estimate for children ages 12–18 multiplied by the average number of years children spend in HOPE VI projects during this age range plus the same product for the younger age range. Pre-grant differences ($j < 0$) are constructed analogously, using a variant of the Column 4 movers specification estimated on a sample of children who moved out of the projects more than one year before the grant year. Vertical bars and shaded regions denote 95% confidence intervals.

FIGURE A.13
Pre-Grant Differences in Observable Characteristics in Facebook Data

Panel A. Before Matching



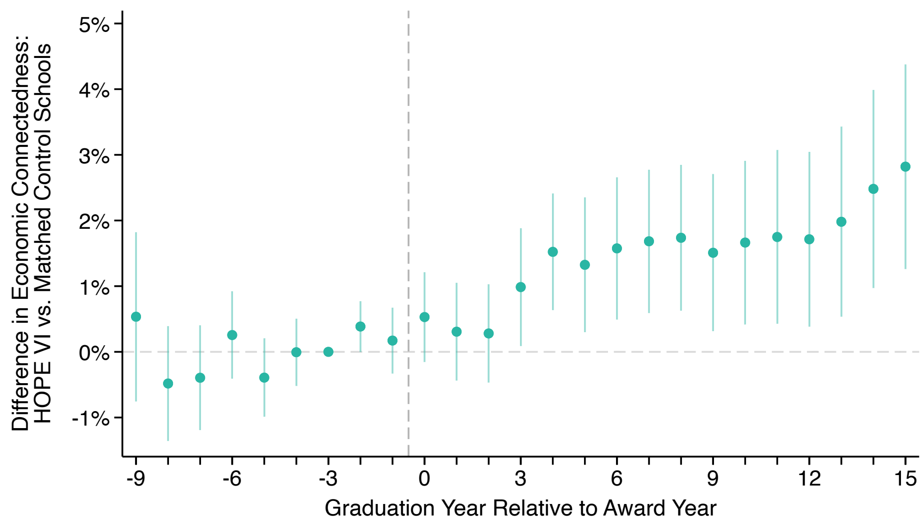
Panel B. After Matching



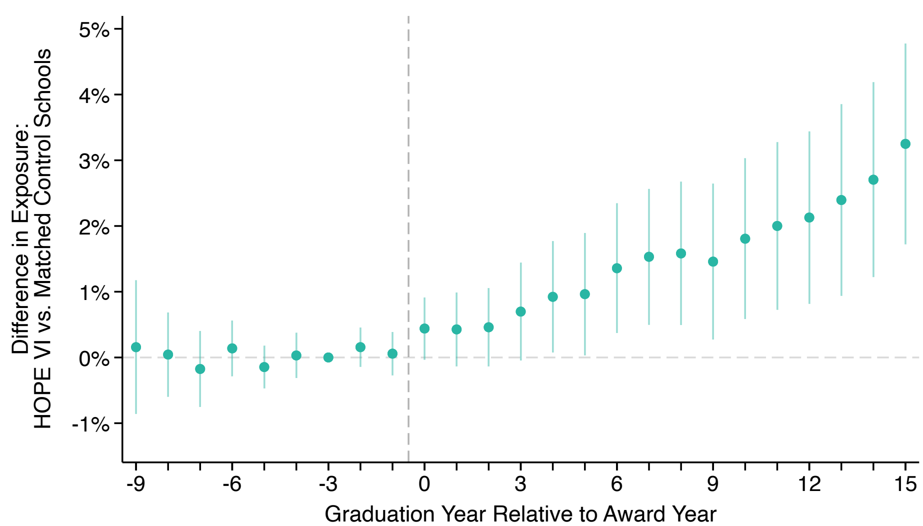
Notes: This figure replicates Figure 1 using the Facebook data. See Appendix D for details. We use data from the decennial Census to measure the poverty rate and the share Black in the Census tract in which the public housing project is located. We use data from Facebook to identify individuals who attended a high school within 5 km of a HOPE VI or matched control public housing project and classify them as high- or low-income according to the economic status of their parents. Economic connectedness is defined as, among low-income students, the share of an individual's high school friends who are high income. Exposure is defined as the share of students who are high income. Horizontal bars denote 95% confidence intervals.

FIGURE A.14
Impact of HOPE VI Revitalization on Cross-Class Friendships in Nearby High Schools

Panel A. Economic Connectedness



Panel B. Exposure

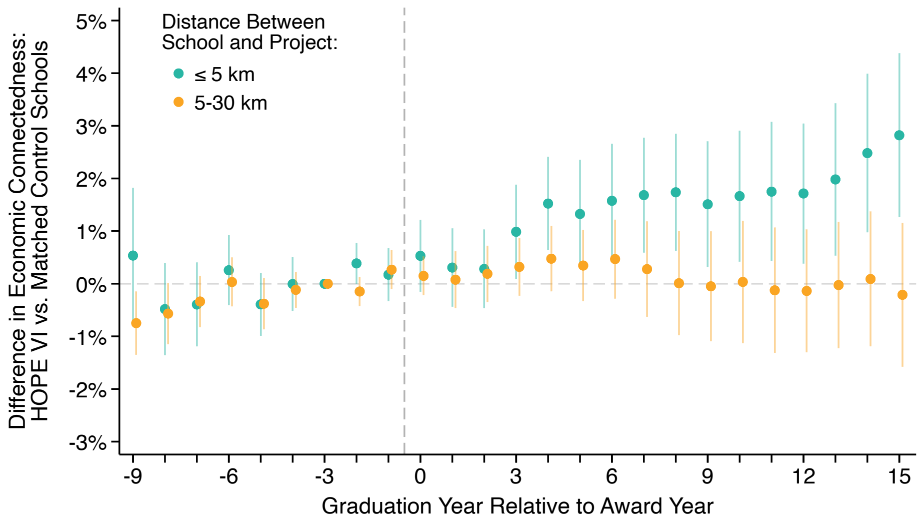


Notes: These figures plot estimates of β^j from the local projections difference-in-differences specification in equation (D.1). In Panel A, the outcome is mean economic connectedness, defined as the share of an individual's high school friends who are high income, among low-income students. The sample consists of individuals who attended a high school within 5 km of a HOPE VI or matched control public housing project in a given cohort (defined by high school graduation year). In Panel B, the outcome is exposure, defined as the share of students in the same schools who are high income. See Appendix D for details. Vertical bars denote 95% confidence intervals.

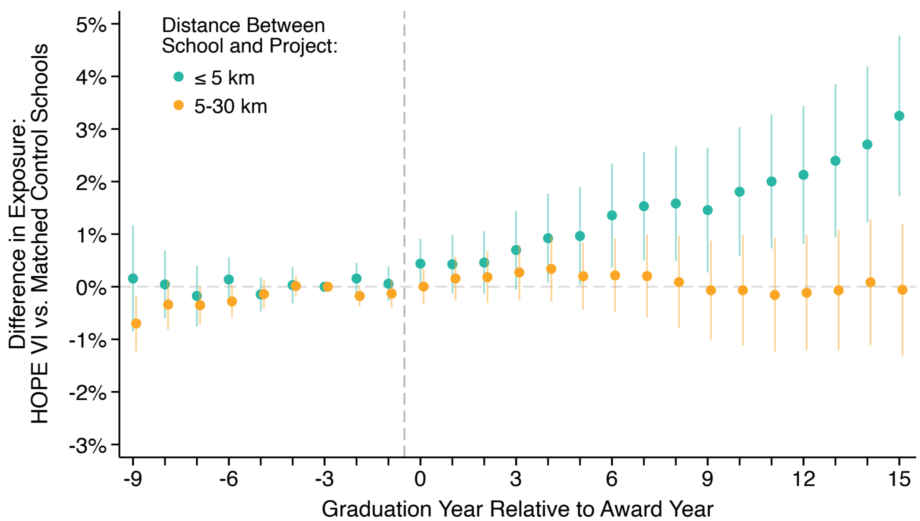
FIGURE A.15

Impact of HOPE VI Revitalization on Cross-Class Friendships in Nearby vs. Further Away High Schools

Panel A. Economic Connectedness



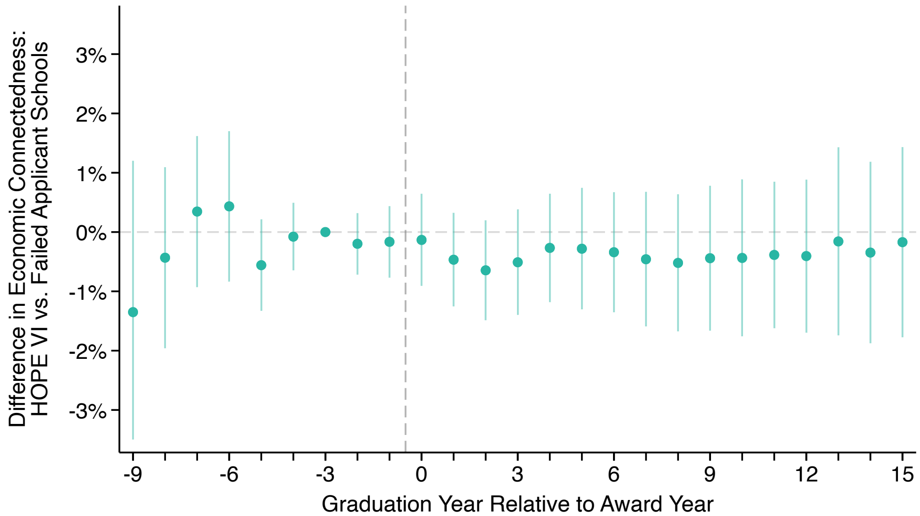
Panel B. Exposure



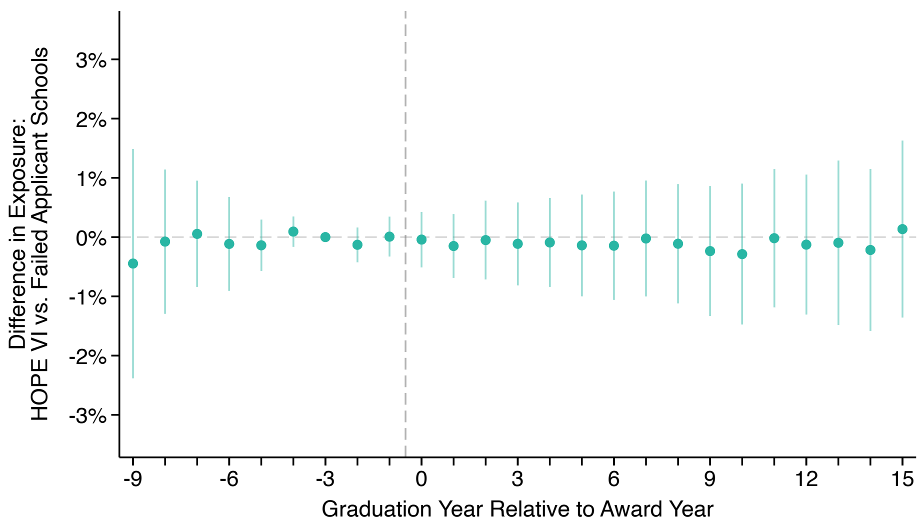
Notes: The green series reproduce the estimates from Figure A.14. The orange series present analogous estimates, but using a sample of students who attended high schools located 5–30 km from the HOPE VI or matched control projects (rather than within 5 km). See Appendix D for details. Vertical bars denote 95% confidence intervals.

FIGURE A.16
Placebo Estimates of Failed HOPE VI Applications on Cross-Class Friendships in Nearby High Schools

Panel A. Economic Connectedness



Panel B. Exposure



Notes: This figure replicates Figure A.14, but defines the treatment group as projects that applied for a HOPE VI grant but did not receive funding (failed applicants). See Appendix D for details. Vertical bars denote 95% confidence intervals.