NOT FOR CIRCULATION OR CITATION

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

Raj Chetty, Harvard University and Opportunity Insights
Rebecca Diamond, Stanford University
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

March 2025

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 revitalized developments to observably similar control developments in public housing and tax records. HOPE VI reduced neighborhood poverty rates by decreasing the number of public housing units and attracting higher-income families to treated areas. Children raised in revitalized public housing from birth earn 40% more in early adulthood than children in non-revitalized developments. The improvements in children's outcomes are driven by changes in neighborhoods' causal effects on upward mobility rather than selection effects in the types of public housing families living in revitalized areas. The treatment effects are strongly related to changes in social interaction: HOPE VI increased the extent to which public housing residents interact with peers in surrounding neighborhoods and generated larger effects on earnings for subgroups with stronger peers. Many low-income families 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 may be a cost-effective, scalable approach to doing so.

*Any opinions and conclusions expressed herein are those of the authors and do not represent the views of the U.S. Census Bureau. The Census Bureau has ensured appropriate access and use of confidential data and has reviewed the results for disclosure avoidance protection (Project 7519874: CBDRB-FY24-CES023-018, CBDRB-FY24-CES023-020, and CBDRB-FY25-CES027-001). We thank Eric Chyn and numerous seminar participants for helpful comments and discussions. We are very grateful to Anna Sander, Arjun Shanmugam, Erica Sprott, Raghav Saraogi, Chase Williamson, Jamie Gracie, Kamelia Stavreva, and other Opportunity Insights predoctoral fellows for outstanding research assistance.

Disclosure: None of the authors have any conflicts of interest associated with this research.

I Introduction

Recent work has shown that opportunities for upward income mobility vary significantly across neighborhoods (Chetty, Hendren and Katz, 2016; Chetty et al., 2018; Chyn, 2018; Chyn, Collinson and Sandler, 2022). These findings have sparked renewed interest in policies that help low-income families "move to opportunity," for instance using subsidized housing vouchers (Bergman et al., 2024) or by building more affordable housing in high-opportunity neighborhoods (Blanco and Sportiche, 2024). Although the moving to opportunity approach has promise, it has limits to scalability, both because some families may not want to move and because of general equilibrium effects that may dampen gains when many families move.

In this paper, we study whether one can bring opportunity to low-income families where they currently live, rather than helping them move to opportunity. Can we turn low-opportunity neighborhoods into high-opportunity neighborhoods? What are the mechanisms through which one can increase economic mobility in low-opportunity areas?

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 to revitalize 262 large, highly distressed public housing projects that housed 182000 people on average. 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 projects. Poverty and crime rates fell significantly, while house prices increased (Tach and Emory, 2017; Zielenbach and Voith, 2010; Sandler, 2017; Bruhn, 2018; Blanco and Neri, 2023). However, it is unclear whether these changes in outcomes were driven simply by reshuffling families across neighborhoods (a selection effect) or whether the HOPE VI program actually improved neighborhoods' *causal effects* on their residents' outcomes. Would assigning a given individual to a project post-revitalization lead to better outcomes than what she would have experienced had she lived in the same neighborhood pre-revitalization?

We analyze the effects of HOPE VI on neighborhoods' causal effects using an anonymized longitudinal dataset covering more than 400,000 public housing residents, constructed by linking public housing records from the U.S. Department of Housing and Urban Development (HUD) to longitudinal information from tax

returns spanning 1979-2019 as well as decennial census data. These data provide administrative information on address histories and a rich set of covariates from public housing records. They also permit linkage of children to their parents (as in Chetty and Hendren (2018*a*)), allowing us to study a broad range of outcomes for both adults and children, including employment rates, earnings, mortality rates, and incarceration.

We divide our empirical analysis into three 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. 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 generated large, persistent reductions in poverty rates of 10 percentage points in targeted neighborhoods relative to control unrevitalized areas in repeated cross-sections, consistent with the findings of Tach and Emory (2017). However, 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 reductions in poverty 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 the outcomes of children who grew up in public housing before vs. after revitalization, measuring children's earnings and other outcomes in their late twenties. We find that the earnings of children who grew up in HOPE VI public housing units post-revitalization rose significantly. Our estimates imply that children raised in revitalized projects from birth earn \$11,000 per year (40%) more at age 30 ($p < 0.01$) than those who grow up in control projects.

This improvement 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 they were revitalized. To distinguish between these hypotheses, we build on the movers design developed in Chetty and Hendren (2018*b*) to estimate the effect of HOPE VI on childhood exposure effects. Focusing on children who moved into a given project in the

same 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 the revitalized neighborhoods to those in control neighborhoods.

The identification assumption underlying this estimator is that the relationship between potential outcomes and years of exposure to public housing is the same in HOPE VI and matched controls (conditional on project-by-move-in-year fixed effects). This assumption would be violated if, for example, families with more social support or resources move out of unrevitalized, distressed housing projects more quickly, which would reduce the correlation between exposure and children's outcomes in unrevitalized projects relative to revitalized projects purely through a selection effect. Two pieces of evidence suggest that such dynamic selection effects are not prevalent in practice. First, predicted adult 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.

Using this research design, we find that the gains from living in a revitalized neighborhood accrue in proportion to the number of years spent there during childhood. The gradient between children's earnings in adulthood and childhood exposure is more positive in revitalized neighborhoods relative to control neighborhoods. Rescaling by the average duration of exposure, the estimates from this movers design are very similar to those from the analysis of repeated cross-sections of children reported above.

We find little heterogeneity in the impacts of HOPE VI on observable dimensions, either by project type or across subgroups. For example, gains are similar for boys (who have poorer baseline outcomes) and girls as well as in more vs. less distressed projects, showing that the intervention had broad impacts beyond the most disadvantaged groups. The one dimension on which we do find heterogeneity is by children's age: exposure to an additional year in a revitalized project vs. a control project generates larger gains in adolescence than at earlier ages. The finding of greater sensitivity to neighborhood conditions in adolescence is consistent with the results of studies that analyze the outcomes of children who move across neighborhoods (Chetty et al., 2018; Deutscher, 2020).

Our estimates imply that each year of childhood exposure to a revitalized project instead of a control public housing project increases children's earnings in adulthood by 2.6% (s.e. = 0.8). Assuming that this

treatment effect remains constant over children's lifetimes, this implies a present-discounted-value earnings gain of $44,000 for the average family (with 2.4 children) from living in a revitalized project for one year or $880,000 in present value (assuming an interest rate of 3%) if the gains from revitalization persist for 20 years. These earnings gains are large relative to the program's upfront costs of $170,000 per revitalized unit. Indeed, if the gains from revitalization persist for 20 years, the gains in income tax revenue would themselves offset program costs.

In the final part of the paper, we characterize the mechanisms through which HOPE VI improved children's outcomes, with the aim of informing other interventions to improve opportunity in neighborhoods. We first examine whether the direct effects of the program on housing in the treated neighborhoods—the demolition of public housing units and the building of market-rate units—explain the treatment effects. To test whether demolition itself had an impact, we analyze how children's outcomes changed in housing projects that were partially demolished, but where there was no active effort to revitalize the neighborhood through new units or programs. For this "demolition only" treatment, we find no change in the outcomes of children who grew up in the remaining public housing units that were not demolished, suggesting that the treatment effects we observe are not a result of simply reducing the density of public housing. To evaluate whether the reduction in poverty rates resulting from building market-rate units in the treated neighborhoods could explain the treatment effects, we compare our estimates to prior experimental and quasi-experimental estimates of the impact of moving to a lower-poverty area (Chetty, Hendren and Katz, 2016; Chetty et al., 2018). The impacts of HOPE VI on children's earnings are over ten times larger than the causal effects one would predict based purely on changes in poverty rates in the treated neighborhoods, indicating that the construction of market-rate housing itself also does not explain the treatment effects.

Having established that the direct effects of the program on the income mix of housing in the treated neighborhoods cannot directly account for observed treatment effects, we next consider other mechanisms. There are many potential channels that could be relevant, ranging from impacts of the supplementary HOPE VI programs (e.g., childcare, job training) to other endogenous changes in the environment (e.g., in crime and policing).

We find that one mechanism that has been emphasized in recent work on neighborhood effects—the role of social interactions with nearby peers—has the potential to explain nearly all of the HOPE VI treatment effect. To analyze the role of peers, we first define potential peer groups for each child in the HOPE VI neighborhoods as the set of children who live within a one-mile radius of the project and who are of the

same age, gender, and race (given evidence of strong homophily in friendships by age, gender, and race in prior work). The HOPE VI intervention has significantly larger treatment effects for subgroups with stronger peers, as measured based on peers' observed outcomes.[1] 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.

Nearby peer outcomes appear central to understanding the HOPE VI treatment effect: the estimated treatment effect is zero for children with the weakest peers and twice as large as the overall mean for children with the stronger peers. These results hold even when comparing children living within the same housing project (thereby identifying solely from differences in peer groups across children within a given project), ruling out the possibility that common area-level shocks drive these results. Instead, the results support the view that the HOPE VI treatment led to greater connections between children living in public housing and their nearby peers. When peers are stronger, children gain more from the HOPE VI treatment, perhaps because of changes in their behaviors, aspirations, or connections to job opportunities.

Motivated by these correlations with peer outcomes, we test whether social interaction changed directly through an anonymized dataset on cell phone pings (Cook, 2024) that we use to obtain information on the locations that public housing recipients visit in 2019. People residing in HOPE VI revitalized projects are significantly more likely to spend time in other residences outside the public housing projects than individuals residing in matched control projects. They are also more likely to spend time in higher-income neighborhoods (block groups) than those who live in control projects. Consistent with these data on where people spend their time, Facebook data show that low-income children also have more high-income friends in schools in treated neighborhoods than those who attend schools in control neighborhoods.

We find no change in outcomes for children growing up in the neighborhoods immediately adjacent to revitalized projects, suggesting that increased interactions with public housing residents did not negatively impact the outcomes of children in surrounding areas. Although our ability to detect spillover effects in the broader community is constrained by limitations in statistical power, this finding is consistent with observational evidence showing that communities with greater cross-class interaction exhibit better outcomes for children raised in low-income families, but no worse outcomes for those raised in high-income families

[1] In principle, this approach could suffer from a reflection problem (Manski, 1993), whereby peers' outcomes might be influenced by the better outcomes of those treated by HOPE VI rather than the other way around. In practice, there are 44.4 times as many children in the peer groups as those directly treated by HOPE VI, suggesting that spillovers in the opposite direction are likely to be small.

(Chetty et al., 2022).

Finally, to obtain insight into why patterns of interaction changed so much in revitalized projects, we investigate how rates of crime changed in treated vs. control projects. Using data on incarceration rates among public housing residents and neighborhood-level data on arrest records, we show that crime rates fell by more than 30% in treated projects relative to control projects, consistent with previous research on public housing demolitions and crime (Aliprantis and Hartley, 2015; Sandler, 2017) as well as with the findings of Sharkey and Torrats-Espinosa (2017) on the link between crime and children's outcomes. The results point to improvements in safety as one reason that social interaction between those living in housing projects and surrounding areas increased.

In summary, our analysis of mechanisms suggests that distressed public housing projects were essentially islands cut off from 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. This mechanism suggests that there may be other neighborhoods, even outside public housing, that could benefit from interventions that connect them more effectively to surrounding higher-opportunity communities. Using an index of social disconnection based on the covariance between children's outcomes and those in surrounding areas, we show that XX% of public housing projects and XX% of high-poverty neighborhoods more generally (which house XX million children) are more socially disconnected than the average HOPE VI project was prior to revitalization. We conclude that there is substantial scope to create higher-opportunity neighborhoods by better connecting higher-poverty areas to surrounding communities, either using housing revitalization programs as in HOPE VI that create mixed-income neighborhoods or other approaches.

This paper builds on and relates to several literatures. First, in the context of the HOPE VI program itself, past research 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 changes 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.[2]

[2]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 relocation of families.

Second, our analysis relates to the broader literature on neighborhood effects, which has primarily analyzed families who move across areas (see Chyn and Katz (2021) for a summary). 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 key contribution of the present study is to show that neighborhood effects can be *changed* through policy interventions.[3]

Third, our paper relates to a large literature on urban renewal (Baum-Snow and Marion, 2009; Collins and Shester, 2013; Busso, Gregory and Kline, 2013; Blanco and Neri, 2023; Almagro, Chyn and Stuart, 2023), which has primarily examined how place-based policies impact neighborhoods via residential sorting. We directly analyze impacts on residents' *outcomes*, demonstrating that urban renewal may not only attract higher-income, more productive residents to a particular area but also change the productivity of the area itself.

Finally, our analysis relates to the broader literature that analyzes 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 all of these family-level and institutional inputs—school spending, labor market conditions, family resources—one can achieve substantial gains in mobility purely through neighborhood-level investments. In this sense, our analysis provides further evidence that neighborhoods are a key determinant of mobility and that place-based investments offer a promising path for increasing economic mobility in future generations.

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. We discuss in Section VII the implications for policies to create higher opportunity neighborhoods.

II HOPE VI Program Background

This section briefly summarizes the HOPE VI program, focusing on the processes used to select sites and tenants that inform our research design. For a more comprehensive summary of the HOPE VI program, see

[3] Our findings on changing opportunity are consistent with recent studies by Garin and Rothbaum (2024) and Chetty et al. (2024), 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.

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 national commission found that 86,000 of the 1.4 million public housing units nationwide were "severely distressed," with elevated rates of poverty, 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. The HOPE VI program aimed to redevelop severely distressed public housing developments into lower-density, mixed-income communities.[4] Between 1993 and 2010, HUD awarded 262 revitalization grants totaling $6.2 billion to local public housing authorities (PHAs) around the country (HUD, n.d.).

The HOPE VI revitalization program had three primary elements. First, it replaced the housing stock in distressed projects with high-quality construction integrated into the surrounding neighborhood. Second, it aimed to deconcentrate poverty via the construction of mixed-income housing rather than building housing exclusively for low-income residents. Third, it provided funding for additional programmatic services (such as child care and job training), with the general goal of revitalizing and improving the neighborhood. PHAs were directed to use at least 80 percent of federal funding for housing construction costs and up to 20 percent for programmatic services.

In a departure from the public housing program, HOPE VI utilized public-private partnerships for the construction and operation of mixed-income developments. Grantees (local PHAs) were authorized to partner with private developers and property managers and to employ mixed-finance development strategies, which used HOPE VI grants to leverage additional public and private sector funding. Over the course of the program, $6 billion in direct HOPE VI funding was used to leverage almost $11 billion in non-HOPE VI funding, for a leverage ratio of about $1.80 in non-HOPE VI funding for every $1 in HOPE VI funding (Gress, Joseph and Cho, 2019). This mixed-finance strategy allowed HOPE VI developments to contain a mix of unit types catering to different income levels, including a combination of public housing units, affordable non-public housing (e.g., Low-Income Housing Tax Credit (LIHTC)) units, and market-rate

[4]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 we refer to simply as HOPE VI for simplicity) throughout this paper, except in Section VI.A where we examine the impacts of the Demolition program as a comparison to study mechanisms.

units.[5]

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 engineers), 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 a mix of 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 and in 2005, after 95% of HOPE VI grants had been award, Turner, Popkin and Kingsley (2005) estimate that 63,000 distressed public housing units had been demolished but between 47,000 and 82,000 severely distressed units remained. This creates a control group of comparably distressed public housing projects that were not revitalized, which we make use of 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 developments (including HOPE VI developments). The PHA screens for eligibility and adds their name to site-based waitlists. As units become available, PHAs select eligible tenants from the waitlists typically based on date of application, sometimes prioritizing tenants with certain characteristics, such as those experiencing 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 key challenge that we address in formulating 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 percent of public housing residents stay for more than 10 years (Dantzler, 2021; Freeman, 2005; McClure, 2018; Bahchieva and Hosier,

[5]Nationally, 57 percent of HOPE VI development units are public housing, 30 percent are LIHTC units, and 13 percent are unsubsidized market-rate units (Gress, Joseph and Cho, 2019).

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 (e.g., for nonpayment of rent or criminal activity that threatens other tenants) (HUD, 2013).

Because the HOPE VI program reduced density and created mixed-income housing, HOPE VI developments generally had fewer available public housing units after revitalization. As a result, original residents were not necessarily guaranteed a right to return. In practice, 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 such as 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 families rather than the original residents who lived in the treated projects prior to revitalization.

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.[6] 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., 2024). This section describes how we construct our primary analysis sample using these data, defines key variables of interest, 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 that was below the 10th or above the 90th percentile, as

[6]The HUD files we use are the Tenant Rental Assistance Certification Systems (TRACS) and Public and Indian Housing Information Center (PIC) files.

these projects may have been affected by HOPE VI or other related programs. The resulting sample includes approximately 200 HOPE VI projects and 2,500 non-HOPE VI projects.

Within these housing projects, we analyze outcomes for two groups: adults (in Section 4) and children (in Section 5). When analyzing adults, we focus on individuals who ever lived in one of the public housing projects in our sample when they were older than 18. When analyzing children's outcomes, we focus on individuals born between 1978 and 1989 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 1989 because we generally measure children's outcomes in adulthood at age 30 and the last year for which we have tax data is 2019. We link children who live in public housing to parents using the HUD records to identify the household head associated with each child in the year that the child moves into public housing.

For certain analyses, we require information on the residential addresses of children who are not living in public housing. For example, we analyze the outcomes of children who live in the neighborhood surrounding the public housing projects. For these analyses we use the address data of the individual public housing residents to measure the location of the public housing projects (see Appendix C.A for details) and parental address (defined below) to measure the residential address of the child. We link parents and children for non-public housing residents by identifying the adult who most recently claimed the child as a dependent.

III.B Variable Definitions

Variable Definitions for Adults

Public Housing Residents. We use the HUD data to identify residents of public housing projects. 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 classify an individual as being in public housing in a given calendar year if they appear in the HUD data in that year. The HUD data also indicate the specific public housing project in which the individual lives.

Residential Addresses. We use tax returns to measure address of residence in calendar years when an individual is not in public housing. If an individual does not file taxes, then we use any available address data from the HUD records (these data contain address information from programs other than public housing, such as the Section 8 Housing Choice Voucher program). If the individual has no address data from either

source, then we use address data from Infutor in 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. We measure annual income of adults using the total pre-tax income of the primary tax filer and their spouse (if applicable) as in Chetty et al. (2018), which we label family or 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 the sum of the W-2 income of the claimer and their spouse when available. Otherwise, we consider household income for non-filers to be zero. Household income and all other monetary variables are measured in real 2022 dollars, deflated using the CPI-U.

Poverty Status. We define an individual as being poor in a given year if their average household income in the current and previous four years does not need exceed $15,000, which is approximately equal to the poverty line for a three-person family in 2007.

Family Income for Public Housing Residents. The HUD data contain information on total family income for individuals in public housing. We use this variable to supplement the household income measured based on the tax returns.

Variable Definitions for Children

Residential Location in Childhood. We use the HUD data to measure the calendar year in which a child enters and exits a given public housing project; see Appendix C.B for details. While the HUD data provide accurate move-in and move-out dates, these variables are not always populated and we sometimes limit the sample to cases with non-missing entry and exit dates. We are able to measure an entry and exit date for 49.5% of the children in the target sample. We use the address of the parent who most recently claimed the child as a dependent to measure residential location when the child is not in public housing.

Year of Birth and Death. We measure children's year of birth and year of death (if applicable) using the Census Numident.

Parental Income. For our primary analysis of 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 analyses of non-public housing residents, we define parental income as the mean of parents' household income over five years: 1994, 1995, and 1998-2000, as tax records are unavailable in

1996 and 1997.

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 main race and ethnicity groups: non-Hispanic White, non-Hispanic Black, Hispanic, non-Hispanic Asian, non-Hispanic American Indian or Alaskan Natives, and non-Hispanic Other.

Income in Adulthood. In our primary analysis, we measure children's adult income 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.[7]

Employment in Adulthood. We define children as employed at age 30 using an indicator for positive W-2 income.

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

Additional Characteristics for Public Housing Residents. We obtain information on the following additional characteristics of children and their parents from the HUD records: an indicator for having a disability, an indicator for being homeless at the time of admissions, an indicator for whether the household experienced hardships that allow them to quality for additional subsidies, and the number of individuals in the household.

III.C Summary Statistics

Table I presents pre-award summary statistics from a "stacked" dataset, where observations are defined at the public housing project and grant-year level. HOPE VI projects appear only in their funding grant year, while non-HOPE VI projects appear between 1990 and 2007, which is three years before the first and last HOPE VI award, respectively. Baseline characteristics are measured three years prior to the grant year.

Projects that received HOPE VI funding (column 3) were more disadvantaged along multiple dimensions compared to those that did not (column 1). Residents of HOPE VI projects are more likely to be poor, have lower household income, lower adult earnings, and have higher adult incarceration rates than residents

[7] We include children with zero income, assigning them the mean rank of the individuals in that group.

of non-HOPE VI projects. HOPE VI projects house a higher share of Black residents, a lower share of elderly and disabled residents, and larger households than typical public housing projects. The HOPE VI projects also are older, larger, and more densely populated than non-HOPE VI projects. The matched control projects (column 2) have similar resident and project characteristics to the HOPE VI projects as detailed below in Section IV.A.

IV Impacts on Adults' Incomes in Revitalized Neighborhoods

In this section, we analyze how HOPE VI affected the income of adults living in the revitalized neighborhoods. We first describe the research design we use to estimate neighborhood-level treatment effects and then present the results.

IV.A Research Design: Difference-in-Differences with Matched Controls

As discussed in Section II, HOPE VI sought to revitalize severely distressed public housing projects, defined based on ex-ante criteria such as poverty rates. Our target estimand is the treatment effect of HOPE VI revitalization on the average incomes of adults living in neighborhoods with severely distressed public housing projects (i.e., the average treatment effect on the treated neighborhoods). An ideal experiment to identify this estimand would involve random assignment of HOPE VI grants to a subset of severely distressed public housing projects. Comparing average incomes post-intervention in the treated and control sites would then identify the parameter of interest.

We approximate this experiment in the observational data we have – where sites were selected to receive HOPE VI grants based on the criteria described in Section II – 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 adults' incomes in treated vs. control neighborhoods before vs. after the HOPE VI intervention. Conceptually, the post-intervention comparison identifies the treatment effect of interest, while the pre-period comparison serves as a placebo test evaluating whether our matching estimator was effective in identifying comparable projects. We now describe how we implement each of these two steps in the design.

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.4A plots the distribution of pre-

award poverty rates for adults in HOPE VI and non-HOPE VI projects.[8] Although HOPE VI projects had poorer residents on average, 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 by limiting the analysis sample of projects defined in Section 3 to projects that either received a HOPE VI award in a given grant year g or never received a HOPE VI award. Next, we estimate a logistic regression of HOPE VI award receipt on the following baseline characteristics: mean household incomes of public housing residents (using both the HUD public housing data and tax data measures defined in Section III.B 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 limit the sample to the HOPE VI projects and all non-HOPE VI projects that were selected as matched control projects.[10] The matched control projects are assigned the (placebo) grant year g corresponding to the HOPE VI project to which they are matched.

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 score weights for each project site s as the predicted values from this regression. We use the inverse of these propensity scores ω_{gs} as weights throughout our empirical analysis (Rosenbaum and Rubin, 1983, 1984).[11]

[8] For HOPE VI projects, we measure the poverty rate three years prior to the award. For non-HOPE VI projects, we measure the poverty rate using an annual project-level panel containing observations between 1990-2007, which is three years before the first and last HOPE VI award, respectively.

[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. The HUD data are not available prior to 1995. For the earlier years we use publicly available data from HUD User to measure the average family income of the public housing residents in 1993. There are no missing values for the other variables as we use the tax records to measure the characteristics of individuals living in the physical footprint of the public housing project.

[10] Non-HOPE VI projects that are matched to multiple HOPE VI projects only show up once within each grant year. We do not duplicate these observations, but simply subset the original dataset to include only the HOPE VI and matched controls.

[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 multiplying the inverse propensity score weights by the baseline population for each project.

The dataset that results from this process, which we use throughout our empirical analysis below, 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. Project sites s are unique within grants years g but matched control projects may appear in multiple grant years. Each observation has an associated weight ω_{gs}.[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. For example, Figure A.11B shows that the distribution of project-level poverty rates in the matched control projects (weighted by inverse propensity scores) is closely aligned with that in the HOPE VI sites, unlike the raw comparison in Figure A.4A.

To evaluate balance more systematically on poverty rates and other dimensions, 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. We cluster standard errors at the level of the project to account for the fact that matched control projects may appear as controls in multiple grant years. Figure IB 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 IA), 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 percent 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 around the grant year. For each grant year g from 1993 to 2010, we collapse the individual-level data (computing a statistic such as mean incomes or poverty rates) to create a panel that includes one observation per calendar year t and project site s in grant year g. We stack these panel datasets to generate a single dataset with one observation per housing project (s), calendar year (t), and grant year (g).

For control projects, we also multiply these weights by the ratio of total baseline population in HOPE VI projects to that in control projects, within each grant year.

[12] In practice, this second rebalancing step has little impact on the results below; 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.

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

$$y_{gst} = \sum_{j \neq -3} \beta^j H^j_{gst} + \delta_{gs} + \lambda_{gt} + \varepsilon_{gst}, \tag{1}$$

where y_{gst} denotes an outcome in project site s with grant year g in calendar year t (e.g., the poverty rate), δ_{gs} is a site by grant-year fixed effect, λ_{gt} is a calendar year by grant year fixed effect, and H^j_{gst} is a treatment indicator equal to one if site s received a HOPE VI grant $j = t$ years after the award. 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 (1) is a standard difference-in-differences specification within each grant year g (with site, year, and treatment-by-event-year fixed effects), pooled across grant years to obtain a single estimate of the path of treatment effects. We estimate (1) using weighted least squares using the inverse propensity scores ω_{gs} defined above as weights, with standard errors clustered at the site level.

Assumption 1. Parallel Trends in HOPE VI and Matched Control Sites. The estimator in equation (1) identifies the causal effect of the HOPE VI program under the following standard parallel trends assumption:

$$\mathbb{E}[\varepsilon_{gsm} - \varepsilon_{gsn} | H^j_{gsm} = H^j_{gsn} = h, \delta_{gs}, \lambda_{gt}] = \tau \text{ for all } \{m,n\} \text{ and } h \in \{0,1\}. \tag{2}$$

This assumption requires that outcomes in HOPE VI and matched control sites would have evolved identically absent the intervention. We evaluate the validity of this assumption after presenting baseline results.

IV.B Empirical Results

Figure II shows how the incomes of adults evolved in HOPE VI revitalized sites relative to control sites by plotting estimates of β^j from equation (1) for various outcomes and subgroups.

We begin in Panel A by analyzing impacts on poverty rates, which have been a focus of prior cross-sectional research on HOPE VI revitalization programs. As discussed in Section III.B, we measure the poverty rate in our data as the share of adults with family income (based on tax records) below $15,000 (coding non-filers as having zero income).

The repeated cross-section series in green circles in Panel A includes all adults living in the Census blocks in the footprint of the 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 have very similar trends in poverty rates prior to revitalization.

After the grant award, poverty rates fall rapidly in HOPE VI sites relative to control projects, as expected given the design of the HOPE VI program: the total amount of public housing (which housed the poorest families) was significantly reduced, while new construction included both market-rate units and units with shallower subsidies (e.g., LIHTC) that catered to higher-income families. The decline in poverty rates begins four years after the date the grant is received, as a result of time to build and have families occupy the new market-rate units. Poverty rates continue to decline until 10 years after the award, at which point they stabilize approximately 11 percentage points (17.5%) below the pre-revitalization level of 68.5%. This estimate of the impact on neighborhood poverty rates in repeated cross-sections is similar to that obtained Tach and Emory (2017) using publicly available survey data.

Poverty rates might have fallen not just because of changes in the mix of families living in revitalized sites, but also as a result of changes in the incomes of families living in public housing itself, either as a result of sorting (e.g., more stringent selection into HOPE VI units as discussed in Section II) or treatment effects on the incomes of adults living in public housing. To distinguish between these explanations, the orange series in Figure II replicates the green series, restricting the sample to public housing residents in each event year.

To identify treatment effects on public housing residents directly, the orange series in Figure II replicates the green series, but restricts attention to adults living in public housing one year before the grant (in event year $t-1$). We follow this set of individuals using panel data, tracking them regardless of whether they stay in public housing or not, and measure the fraction with household income below $15,000 in this group. We find no change in the poverty rates among the original residents after the HOPE VI intervention relative to those living in control projects, with an upper bound of an XX pp reduction on the 95% confidence interval, consistent with prior work tracking the outcomes of adults who received vouchers to move to lower-poverty neighborhoods (Chetty, Hendren and Katz, 2016).[13]

Panel B of Figure A.5 shows that we find similar results when analyzing impacts on poverty rates in-

[13]Furthermore, most prior residents had little exposure to the revitalized neighborhoods. Only 12.0% of adults who lived in a HOPE VI project before the award lived in the same neighborhood 5 years later (Figure A.8). 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.8).

stead of mean income: a significant increase in mean incomes in repeated cross-sections at the neighborhood level, but no effect on the incomes of public housing residents.[14]

Table A.2 assesses the sensitivity of these findings to alternative specifications by estimating variants of the OLS regression model specified in equation 1. Based on the non-parametric evidence in Figure II, which shows stabilization of treatment effects after 10 years, we parametrize the coefficients as XX and report the mean value of β_j for $j \in [10, 15]$ as well as the linear trend from $j \in [-10, -5]$ as a pre-period placebo. Columns 1-3 of panel A replicate the baseline estimates shown in Figure II, using average income as the outcome and reporting impacts for all adults, public housing residents, and pre-grant public housing residents. Panel B replicates panel A using housing projects that applied for HOPE VI but were not granted an award as the control group. In Panel C, we match HOPE VI projects to other projects run by the same public housing authority to generate controls rather than the full national sample.

We find very similar results when using failed applicants or within-PHA projects as controls. In all cases, we find no differential trends between HOPE VI sites and controls prior to the intervention and a sharp reduction in cross-sectional poverty rates when pooling all residents after the intervention. The lack of pre-trends and robustness of treatment effect estimates across these alternative specifications supports the parallel trends assumption (Assumption 1) underlying our estimator.

We conclude that the sharp reduction in poverty rates in neighborhoods revitalized by HOPE VI documented in prior work is driven primarily by changes in the mix of families living in those neighborhoods rather than improvements in the outcomes of public housing residents.

V Impacts on Children's Outcomes in Revitalized Neighborhoods

We structure our analysis by building on the statistical model of intergenerational mobility developed in Chetty and Hendren (2018*b*). We allow neighborhood effects to vary by child age, indexed by a. Let y_i denote a child $i's$ income, measured in adulthood. Each child lives in a set of neighborhoods over their 18 years of childhood. Each neighborhood's effect on children's outcomes depends on whether it receives a HOPE VI revitalization grant. Denote the treatment effect of living in neighborhood j in year t with HOPE VI funding $H \in \{0, 1\}$ on child $i's$ who is age a adult income as μ_{at}^{jH}. Superscripts refer to the neighborhood

[14] Figure II uses an unbalanced panel, with different numbers of projects in different event years, which could potentially lead to biased estimates in the presence of heterogeneity across projects. In Appendix Figure A.6, we show that results are very similar if we restrict attention to a balanced panel of projects where we have data from 5 years before the intervention to 15 years after the intervention.

j and its HOPE VI treatment status H, subscripts to the child i's age a and calendar year t. Without loss of generality, we can write child $i's$ adult income as a function of the sum of treatment effects of the set of neighborhoods child $i's$ family lives in during her 18 years of childhood, plus a residual θ_i. μ_{at}^{jH} is the treatment effect for child i of neighborhood j with HOPE VI treatment H in year t. Child $i's$ adult income can be written as:

$$y_i = \sum_{a=0}^{18} \mu_{at}^{j(i,t)H} + \theta_i, \tag{3}$$

where $j(i,t)$ is the neighborhood j child i lives in year t.

We wish to study the set of children who grew up in the HOPE VI revitalized sites. For a given calendar year t, we take the set $HOPE_t$ of children living in all HOPE VI revitalized sites. Our ideal experiment would compare the average outcome of children in the $HOPE_t$ set to what these children's outcomes would have been had they lived in the these same places during their childhood, but there had been no HOPE VI revitalizations. Define $\bar{y}_{HOPE_t}^{H^{obs}}$ as the average adult earnings of children in set $HOPE_t$ where the set of HOPE VI treatments across neighborhoods is equal to the vector H^{obs}.

$$\bar{y}_{HOPE_t}^{H^{obs}} = \frac{1}{N_{HOPE_t}} \sum_{i \in HOPE_t} \left(\sum_{a=0}^{18} \mu_{at}^{j(i,t)H^{obs}} + \theta_i \right) \tag{4}$$

Define $\bar{y}_{HOPE_t}^{0}$ as the average adult earnings of children in set $HOPE_t$, who still make the same neighborhood choices as above, but where the effect of each neighborhood j is now equal to the neighborhood effect when there is no HOPE VI revitalization, $H = 0$.

$$\bar{y}_{HOPE_t}^{0} = \frac{1}{N_{HOPE_t}} \sum_{i \in HOPE_t} \left(\sum_{a=0}^{18} \mu_{at}^{j(i.t)0} + \theta_i \right) \tag{5}$$

Our ideal estimand would then be:

$$\bar{y}_{HOPE_t}^{H^{obs}} - \bar{y}_{HOPE_t}^{0} = \frac{1}{N_{HOPE_t}} \sum_{i \in HOPE_t} \sum_{a=0}^{18} \left(\mu_{at}^{j(i.t)H^{obs}} - \mu_{at}^{j(i,t)0} \right). \tag{6}$$

For neighborhoods untreated by HOPE VI, they drop out of this equation since $\mu_{at}^{j(i.t)H^{obs}} = \mu_{at}^{j(i,t)0}$. This leaves us with:

$$\bar{y}^{H^{obs}}_{HOPE_t} - \bar{y}^{0}_{HOPE_t} = \frac{1}{N_{HOPE_t}} \sum_{i \in HOPE_t} \sum_{a=0}^{18} \left(\mu_{at}^{j(i.t)1} - \mu_{at}^{j(i,t)0} \right) * H_j^{obs}, \tag{7}$$

where H_j^{obs} indicates if neighborhood j was treated by HOPE VI.

Among the set of children living in HOPE VI revitalized sites in year t, the treatment effect of HOPE VI on their adult earnings is equal to the treatment effect of HOPE VI on each treated neighborhood $\mu_{at}^{j1} - \mu_{at}^{j0}$, weighted by the number of child-years the set of $HOPE_t$ kids live in theses treated places.

V.A Ideal Experiments

Since it is impossible to directly observe what children's outcomes would have been, had they grown up in the same HOPE VI sites absent revitalization, we need to develop a research design that allows us to estimate these outcomes. Concretely, we directly observe the outcomes of kids who grew up in revitalized HOPE VI sites, $\bar{y}^{H^{obs}}_{HOPE_t}$. We need a research design to estimate $\bar{y}^{0}_{HOPE_t}$.

An ideal setting would utilize a randomized controlled trial, however this is very challenging in this setting as it would require two dimensions of randomization: randomization over which sites are treated by HOPE VI and randomization over which individuals get to live in the treated and control HOPE VI sites. Consider the set of children living public housing untreated by HOPE VI, $NOHOPE_t$. The adult earnings of the $NOHOPE_t$ group can be written as:

$$\bar{y}^{H^{obs}}_{NOHOPE_t} = \frac{1}{N_{NOHOPE_t}} \sum_{i \in NOHOPE_t} \left(\sum_{a=0}^{18} \mu_{at}^{j(i.t)H^{obs}} + \theta_i \right). \tag{8}$$

Define $\bar{\mu}_{HOPE_t}$ as the child-year weighted average neighborhood effect for children in set $HOPE_t$ and $\bar{\theta}_{HOPE_t}$ as the average family effect for children in set $HOPE_VI$. Taking the difference in average adult earnings for children in set $HOPE_t$ with children in set $NOHOPE_t$ gives:

$$\bar{y}^{H^{obs}}_{HOPE_t} - \bar{y}^{H^{obs}}_{NOHOPE_t} = \bar{\mu}_{HOPE_t} - \bar{\mu}_{NOHOPE_t} + \bar{\theta}_{HOPE_t} - \bar{\theta}_{NOHOPE_t} \tag{9}$$

Equation 9 shows the difference in these groups' adult earnings are driven by a difference in neighborhood effects, $\bar{\mu}_{HOPE_t} - \bar{\mu}_{NOHOPE_t}$ and difference in family effects, $\bar{\theta}_{HOPE_t} - \bar{\theta}_{NOHOPE_t}$. The difference in neighborhood effects are driven by a mixture of the treatment effects of HOPE VI, along with a site selection effect and exposure selection effect. The site selection effect is driven by the possibility that sites untreated

by HOPE VI have different neighborhood effects, regardless of HOPE VI treatment. Further, if the time spent living in public housing by the $NOHOPE_t$ group is different than the $HOPE_t$ group, then differences in exposure would also contribute to these child-year weighted average differences in neighborhood effects. Random assignment of HOPE VI treatment would solve the site selection effect.

Even with random assignment of HOPE VI treatments, the types of families that choose to move into each public housing site can differ across sites. The difference in average adult earnings of children growing up in sites randomly treated by HOPE VI versus control sites reflect differences in types of families and the treatment effects of HOPE VI. To solve this family selection effect, we need (quasi)-random assignment of families to treatment versus control sites.

Finally, even if we can quasi-randomly assign HOPE VI treatment to public housing sites and quasi-randomly assign families to lives in these sites, it is possible that the duration for which these families choose to remain living in these public housing sites differ between treatment and control sites, leading to different exposures to the HOPE VI treatment and control neighborhood effects. To account for this, we would need to adjust the estimated neighborhood effects by the observed number of years families choose to live in treatment versus control sites.

V.B Impact on Children in the Repeated Cross-Section

In our analysis of the children's outcomes, we account for the non-random assignment of HOPE VI grants across projects using the same matching and difference-in-differences methodology described in Section IV.A. To start off, we do not address the non-random assignment of children across public housing projects or the duration of their exposure. We simply ask whether children who lived in the revitalized public housing units after the award earned more in adulthood than those living in our matched controls.

We use a variant of equation 1 to estimate the effect on the long-run outcomes of children who lived in the HOPE VI public housing projects. In particular, we construct a repeated cross-section sample, in which we compare children living in a HOPE VI treated site in a given calendar year to children living in our match control sample in the same calendar year. We compare this cross-sectional difference in children's outcomes to the difference observed in these same sites prior to the HOPE VI award. Our analysis only includes children in public housing and excludes children who moved into the newly constructed market-rate units in

the revitalized sites. We then estimate the following specification on this individual-level dataset:

$$y_{gsit} = \sum_{j \neq -3} \beta^j H^j_{gst} + \delta_{gs} + \lambda_{gt} + \phi X_{gsit} + \varepsilon_{gsit}, \tag{10}$$

where project site s received a grant in year g and individual i lives in the project in year t; δ_{sg} is a site (s) by grant-year (g) fixed effect; λ_{gt} is a fixed effect for grant-year by year relative to award; X_{gsit} is a vector of individual-level covariates including parental income, the measure of upward mobility associated with the census tract in which the individual lived prior to moving into public housing, and the number of years spent in public housing; and H^j_{gst} is a treatment indicator equal to one if project s received a HOPE VI grant and year t is j years after the award. This the same specification used in equation 1 to estimate effects on adults, just with the added vector of individual-level covariates, X_{gsit}. These additional controls help to ensure we are comparing similar families with similar neighborhood exposures prior to moving into public housing and similar exposure to public housing. As before, the specification is estimated via weighted least squares using inverse propensity scores as weights, with standard errors clustered at the site level.

Interpreting estimates based on the full repeated cross section sample is complicated by the fact that some children who lived in the project after the award may have also lived there before the award, leading them to be exposed to both pre and post treatment. As a result, a comparison of outcomes between children who lived in the HOPE VI and matched control projects before the award may not accurately reflect pre-award neighborhood conditions, as some residents might have been directly exposed to the intervention. To address this, our preferred specification limits the repeated cross-sectional data to children from HOPE VI and matched control projects that belonged to one of the following three distinct groups: *prior residents:* children who moved out of the public housing projects at least two years before the award, *original residents:* children who lived in the project in the year before the award, and *new residents:* children who moved into the public housing projects in the year of the award or later.[15]

The green series in Figure IIIA presents estimates from equation 10. The estimates for the prior residents are depicted in the ten to two years before the award (β^j for $j \in [-10, -2]$). The coefficient estimate of 0 ten years prior to the award implies that there are no differences between the average adult income ranks of prior residents in the HOPE VI and matched control sites.[16] Furthermore, there are no differential

[15]To ensure that we accurately categorize children into these three groups, we require that the new residents have a confirmed move out date, the new residents have a confirmed move in date, and the original residents have a confirmed move out and move in date. Children appear in the sample for each year that they appear in public housing.

[16]This is a result of our matching procedure. Figure IA illustrates that, prior to matching, children from HOPE VI projects

pre-trend in the HOPE VI sites, supporting the parallel trends assumption underlying our design.

Next we move to the original residents. Recall, that the original residents include all children who lived in the public housing projects in the year before the award. The impacts on the original residents are depicted by the effect in the year prior to the award in Figure IIIA. We find evidence of substantial gains: HOPE VI increased the adult income of the original residents by 1.5 ranks. This evidence is consistent with prior work by Chyn (2018) and Haltiwanger et al. (2024), who also find evidence of long-run earnings gains for children displaced by public housing demolitions. However, most of the original residents moved away to different neighborhoods and were therefore not exposed to the changes in the targeted HOPE VI sites. Thus, the gains for these residents tell us little about the changes in the targeted HOPE VI sites (the gains are likely attributable to the children moving to different, higher-opportunity neighborhoods).

Lastly, we move to the new residents, who are the children who move into the HOPE VI and matched control public housing units after the grant year. Figure IIIA presents the impacts on the new residents in the year of the award through ten years after the award. We find substantial gains: new residents who move who live in the revitalized public housing units ten years after the award earn 3 ranks more than children who move into the matched control sites.

We explore the robustness of our estimates to not removing public housing residents that live in the same public housing site in both the pre- and post-treatment period. Figure A.9 shows qualitatively similar results, although the magnitude of the post-treatment gains is somewhat smaller and there is some evidence of a pre-trend. Both of these differences are a predictable consequence of the change in the sample. In the years leading up to the award, children who lived in the public housing projects before the award are increasingly likely to also have been directly exposed to the intervention, which is why we see the average outcomes of these residents trend up. In the years after the award, some of the new residents also lived there before the award. The change in outcomes for this group is mechanically zero, which explains the muted gains.

The long-run income gains for the new residents in the HOPE VI projects provide the first indications that the HOPE VI program improved economic opportunity in the targeted sites. However, as noted above, these gains could be explained by positively selected families moving into the revitalized units. We will return to this issue, but first we explore the robustness of our results to the selection of the control group of

earned significantly less in adulthood compared to those from non-HOPE VI projects. We did not directly match on this outcome; matching on other observables is adequate to achieve balance in these ex-post outcomes for children.

public housing sites.

We estimate equation 10 using two alternative comparison groups. First, we compare HOPE VI sites to public housing projects that applied for but never received funding. These failed applicants shared characteristics (both observed and observed) that led them to apply for funding and might be correlated with potential outcomes.[17] Second, we compare HOPE VI sites to all other public housing projects in the same Public Housing Authority (PHA). For this latter comparison, we include PHA by time fixed effects, so that all comparisons are made within a PHA. Figure IIIB plots our treatment effects over time using failed HOPE VI applicant sites and other public housing sites in the same PHA as the treated HOPE VI sites. We find very similar estimates compared to our preferred estimates based on the comparison to the matched controls. Obviously these results could be driven by selection into the types of families living in treatment versus control site, which we investigate next.

V.C Impact on Neighborhood Exposure Effects

Returning to equation 9, cross-section comparisons of children growing up in public housing randomly treated versus not by HOPE VI contain the treatment effect on neighborhoods, $\bar{\mu}_{HOPE_t} - \bar{\mu}_{NOHOPE_t}$, and the family selection effect $\bar{\theta}_{HOPE_t} - \bar{\theta}_{NOHOPE_t}$. We explore alternative research designs to assess the role of the family selection effect.

V.C.I Selection on Observables

We investigate whether the family selection effect shows up on observable family characteristics. While there is no reason to believe that selection would be based *only* on observables, it would be surprising if it were based only on unobservables. Obviously this does not address the role of selection on unobservables, but it is a first place to start. If families are differentially selected into HOPE VI revitalized sites, relative to controls, then assumption 2, stated below, holds.

Assumption 2. Conditional Random Assignment of Families to Projects. The estimates from equation 10 will identify the difference in neighborhood effects between the HOPE VI and matched control sites under a selection on observables assumption:

$$E[\theta_i | i \in HOPE_t, X_i] = E[\theta_{i'} | i \in NOHOPE_t, X_i]. \tag{11}$$

[17] See Staiger, Palloni and Voorheis (2024) for a more detailed discussion of using the failed applicants as a control group.

To assess the selection on observables relevant for children's adult income, we predict each child's adult income in our control sample using a detailed list of kid and family observables XX add variables here. This predicted income regression has an R-squared of 0.08, suggesting we are able to get some signal from family characteristics to forecast children's future income, but much is determined by other forces than family background. We can then compute each kid's predicted income, based solely on the child's family and individual characteristics determined prior to their move in to public housing. If these new families moving in to HOPE VI sites are the types that produce higher income children, we would see what looks like a treatment effect of predicted income among the new HOPE VI residents, as compared to the controls. Figure IIIA plots these estimates and shows there is no statistically significant difference in predicted children's adult incomes between treatment and control both prior to treatment and after HOPE VI treatment. This further suggests that the in-migrant kids, post HOPE VI revitalization, do better in adulthood due to the opportunity in the neighborhood improved. Obviously, this is a weak test since we know from our analysis of the adults that in-migrants to HOPE VI revitalized sites are positively selected on contemporaneous household income. The fact that this alone does not lead to higher children's predicted income is because the other family characteristics that predict income must be varying enough to make this signal statistically and economically insignificant.

V.C.II Selection on Unobservables

As a complementary and possibly more convincing research design, we next use variation in the exposure of children to sites treated by HOPE VI versus controls.

This second empirical strategy exploits variation in the timing of when children move into and out of public housing and accounts for the possibility that children who moved into HOPE VI public housing units after the award may differ along unobserved dimensions. Our strategy is to compare children who move into the same project in the same year, examine how earnings in adulthood vary with years spent in the public housing project, and compare this gradient in the HOPE VI and matched control projects. To operationalize this comparison we estimate the following specification,

$$y_{gsit} = \phi e_{gsit} + \beta e_{gsit} \times H_s + \lambda_{gst} + \varepsilon_{gsit}, \tag{12}$$

where individual i moves into project site s with grant year g in year t; e is the number of years the individual

is exposed to (i.e., lives in) the public housing project as a child; and H is an indicator for whether the project received a HOPE VI grant. By including site-grant year-calendar year fixed effect (λ_{gst}), we allow for arbitrary level shifts in kids' expected adult income by project over time. This allows our exposure estimates to only use the relationship between exposure duration and income within sites to identify our treatment effect. We estimate this specification via weighted least squares using the inverse propensity score as weights and cluster standard errors at the level of the site. To estimate pre- and post-award differences we separately estimate this specification on a sample of children who move out before or move in after an award, respectively. To minimize measurement error in the exposure variable, we require that children have non-missing move in and move out dates in the public housing residency file and do not appear in more than one public housing project during childhood.

Equation 12 will identify the difference between the neighborhood effect in the HOPE VI and matched control sites under the assumption that the relationship between unobservable characteristics and years spent in public housing is the same in both groups. Let $\beta^j(y,e)$ be the coefficient from regressing y on x for children in the HOPE VI ($j=H$) or matched control ($j=C$) projects. Then from equation 3 we can see that $\beta^j(y,e) = \mu_j + \beta^j(\eta,e)$, and

$$\beta^H(y,e) - \beta^C(y,e) = \mu_H - \mu_C + [\beta^H(\eta,e) - \beta^C(\eta,e)]. \tag{13}$$

Equation 13 demonstrates that the difference in gradients between adult income and exposure duration in HOPE VI versus matched control projects can be decomposed into two components: the differential neighborhood effect, $\mu_H - \mu_C$, and a selection term, $\beta^H(\eta,e) - \beta^C(\eta,e)$. If, conditional on the project by entry year fixed effects, the relationship between unobservable characteristics and years spent in public housing is the same in HOPE VI and matched control projects, $\beta^H(\eta,e) = \beta^C(\eta,e)$, then the estimates from equation 12 identify the difference in the neighborhood effects: $\beta^H(y,e) - \beta^C(y,e) = \mu_H - \mu_C$. Note that this research design allows for arbitrary differences in selection of family types into treated versus control sites, as long as those differences are not correlated with the duration that the family stays in public housing.

In column 1 of Table II we first report the treatment effect of exposure to HOPE VI sites versus control sites for prior resident children, namely those who move out prior to treatment. Here we see an additional year spent in sites that will be treated by HOPE VI in the future has an economically small and statistically insignificant effect of -0.05 income ranks with a standard error of 0.16. This is a good check on our research

design that prior to treatment, an additional year spent in HOPE VI sites and controls have the same effect on kids' adult incomes. Column 2 of Table II now focuses on kids who move into these sites after treatment. Here we see an extra year spent in the HOPE VI sites, relative to control sites, improves kids' adult income ranks by 0.47 with a standard error of 0.15. The non-parametric binscatter version of this estimate is plotted in Figure IVA. To help interpret the magnitude, note that the adult income rank in the control group is 18, which implies a 2.6 percent increase in adult income rank per year spent in a revitalized public housing project (0.026=0.47/18). Alternatively, if use the level of adult income as the outcome variable, we find that spending an addition year in a revitalized public housing project increases adult annual income by $632. Table II investigates robustness in these estimates by including household income quantile of parents before moving in, a sex fixed effect, race/ethnicity fixed effects, year of birth fixed effects, prior tract predicted mobility, prior tract predicted mobility if spent all other years there, a missing prior tract indicator, a disability indicator, an elderly indicator, a female-headed household indicator, a homelessness indicator,a hardship indicator, a household member with disability indicator, total household member count, and observed in one project indicator as controls in the regression. Column 3 shows this large set of controls barely changes the point estimates or standard errors. Column 4 investigates an a slightly alternative version of the research design where we restrict to families for whom all stay living in public housing until the child is 18 years old. In this sub-sample of the data the identifying variation comes from the age of the time at move-in to public housing. The point estimate here is again almost identical to our baseline specification, although the standard errors widen a bit, consistent with using a smaller sample of data. The effect is still statistically significant below the 1 percent level.

The key identifying assumption to interpret these post-revitalization effects as causal is that, conditional on calendar year of move-in to public housing, families that choose to stay longer in HOPE VI-treated sites are not unobservably more likely to have children that earn higher incomes as adults. As we did in the repeated cross-section research design of in Section V.B, we can test the observable version of this assumption. Namely, the observable characteristics of the families who stay longer in HOPE VI-treated sites do not have observable characteristics predictive of their children earning more as adults. Figure IVA this estimate in orange. We see based on family observables, the families who choose to stay an additional year in HOPE VI sites have characteristics that predict their kids would earn -0.01 higher income ranks as adults, with a standard error of 0.04. This precisely estimate zero effect strongly corroborates are research design assumptions.

As a final robustness exercise, we now include a household fixed effect as a control in the regression. This now zooms in on the differences in HOPE VI exposure to different children in the same family. Depending on when a given family moves into public housing, the set of kids within the family exposed to this move will depend on the age of the children during the time the family lives in public housing. Figure IVB plots the effect of an additional year of exposure to HOPE VI public housing leads kids to earn 0.50 higher income ranks as adults. The standard error of 0.19 is wider than our prior estimates, since we only use families with multiple children that are differentially exposed to HOPE VI treated sites. This smaller sample is still statistically powerful enough to rule out null effects at standard significance levels. Further, the identifying assumption is even weaker than before. Now, even if families that produce higher income kids were to stay longer in HOPE VI-treated sites, we only assume now that families are not investing more in their children's adult earnings during exactly the years in which they live in HOPE VI-treated public housing. Across these many different research designs that make different identifying assumptions, we find very similar estimates. HOPE VI revitalizations made a meaningful improvement in kids' opportunities.

Our main results focus on adult income measured between the ages of 29 and 30. Table A.4 presents estimates from equation 12 where adult income is measured between the ages of 24 and 30. Panel B presents analogous estimation from the model with household fixed effects.

Table A.5 presents estimates from equation 12 on addition outcomes measured at age 30. The outcomes include AGI, log of AGI, individual wages, log of individual wages, an indicator for being employed (wages are positive), an indicator for being alive, an indicator for being married, the poverty rate of the census tract of residence, and an indicator for being incarcerated in the 2020 Decennial Census.

Our estimates based on the movers design described in equation 12 (Figure IVA) are quantitatively consistent with the estimates based on the analysis of the repeated cross-section described in equation 10 (Figure IIIA). Specifically, children who move into the revitalized public housing projects spend an average of 5.0 years in public housing. Assuming homogenous treatment effects, the exposure estimates in Figure IVA imply that we should observe a 2.4 rank increase in the repeated cross-section.

Table III explores heterogeneous treatment effects. Columns 1 through 3 present estimates from a modified version of equation 12 in which we interact years exposed to public housing with sex, parental income, and predicted income in adulthood. We find little heterogeneity along any of these measures, suggesting that both boys and girls benefited from the improved neighborhood conditions as well as children from families with different levels of disadvantage. Column 4 presents results in which we separately

measure years exposed to public housing before and during adolescence. The estimates show that spending an additional year in a revitalized project increases income rank in adulthood by 0.689 percentile ranks for exposure during adolescence (ages 12-17) but only 0.245 ranks for exposure before adolescence. These results are consistent with work showing that neighborhood environment is particularly important during adolescence, perhaps because of the role played by social connections during this period. We explore the role of social connections more directly in Section VI.

V.D Costs vs. Benefits of the Intervention

Our estimates imply that each year of childhood exposure to a revitalized project instead of a control public housing project increases children's earnings in adulthood by 2.6% (s.e. = 0.8). Chetty et al. (2018) use data from the 2015 ACS and estimate a present-discounted-value (PDV) of lifetime earnings of $709,000 (assuming an interest rate of 3 percent). Assuming that the treatment effect remains constant over children's lifetimes, this implies a PDV earnings gain of $44,000 for the average family (with 2.4 children) from living in a revitalized project for one year ($44,000 = 709,000 \times .026 \times 2.4$). These earnings gains are large relative to the program's upfront costs. The HOPE VI program cost taxpayers $170,000 per revitalized unit. If the gains from revitalization persist for at least 20 years, the gains in income tax revenue would themselves offset program costs (assuming a tax rate of 20 percent), implying that the earnings gains to beneficiaries of the program come at no cost to taxpayers ($176,000 = 44,000 \times 20 \times 0.2$).

VI Mechanisms

In this section of the paper, we characterize the mechanisms through which HOPE VI improved children's outcomes.

VI.A Direct Effects of Change in Housing Stock

We first examine whether the demolition and removal of public housing units explain the treatment effects. To do so, we analyze how children's outcomes changed in housing projects that were partially demolished, but where there was no active effort to revitalize the neighborhood through new units or programs. Figure V presents estimates from equation 10 where the treatment group is the set of projects that were partially demolished through the HOPE VI Demolition program and the matched controls are selected using the same

methodology presented in IV.A. There is no evidence that children in these partially demolished projects experienced gains in adult income.

The HOPE VI program had an explicit focus on building mixed-income communities, so we next consider whether the reduction in neighborhood poverty rates could explain the gains for children. Figure VI presents a binned scatter plot between upward mobility and neighborhood poverty in 1990 at the Census Tract level. The measure of upward mobility is constructed by Chetty et al. (2018) and is the expected adult income of low-income children who grew up in a given Census tract. The neighborhood poverty rate is measured from the 1990 Decennial Census. As shown in the figure, HOPE VI projects were located in high-poverty and low-mobility neighborhoods prior to the award. The horizontal arrow shows the effect on neighborhood poverty rates from Figure A.5 and the vertical arrow shows the predicted impact on upward mobility (i.e., spending all of childhood in a revitalized public housing project) from Figure IV. The gains in upward mobility are 10 times as large as what would be predicted based on the cross-sectional relationship. As an alternative approach, we adopt the research design from Chetty and Hendren (2018*a*) and exploit variation in the age at which families move between neighborhoods with different poverty rates to estimate the causal effect of being exposed to neighborhoods with lower poverty rates. The black arrow in Figure VI shows the predicted effect on upward mobility based on these estimates of the effect of exposure to neighborhood poverty rate. Again, we find that the observed impacts of the program are large by an order of magnitude. See Table A.6 for alternative versions of this specification. Taken together, these results suggest that the construction of market-rate housing itself does not explain the treatment effects.

VI.B Effects of Social Interaction

Having established that the direct effects of the program on the structure of housing in the treated neighborhoods cannot directly account for observed treatment effects, we next consider other mechanisms. There are many potential channels that could be relevant, ranging from impacts of the supplementary HOPE VI programs (childcare, job training) to other endogenous changes in the environment (e.g., in crime and policing).

We find that one mechanism that has been emphasized in recent work on neighborhood effects—the role of social interactions with nearby peers—explains nearly all of the HOPE VI treatment effect. To analyze the role of peers, we first define a potential peer groups for each child in the HOPE VI neighborhoods as the set of children who live within a one-mile radius of the project at the same time and who are of the same

age, gender, and race (given evidence of strong homophily in friendships by age, gender, and race in prior work). We then estimate the following equation, which is a modified version of 10:

$$y_i = \beta H_s + \theta \bar{y}_{p(i)} + \rho H_s \times \bar{y}_{p(i)} + \phi X_i + \delta_{sg} + \varepsilon_i, \tag{14}$$

where $\bar{y}_{p(i)}$ denotes the average adult income rank of children who belong to the peer group of individual i; H_s is a treatment indicator for whether site s received a HOPE VI grant, which is subsumed by the site (s) by grant-year (g) fixed effect, δ_{sg}; and X_{ist} is a vector of individual-level covariates including parental income, the measure of upward mobility associated with the Census Tract in which the individual lived prior to moving into public housing, and the number of years spent in public housing. As before, the specification is estimated via weighted least squares using inverse propensity scores as weights, with standard errors clustered at the site level. ρ is the main coefficient of interest.

The green series in Figure VIIA presents estimates from equation 14 based on a sample of children who move into the HOPE VI and matched control projects after the award. Using the estimated fixed effects we recover a level difference of 2.0 ranks for children with the average peer adult income of 40.8 ranks. This mechanically lines up with results from Figure IIIA. More interestingly, the positive slope of the green line shows that the HOPE VI intervention has significantly larger treatment effects for subgroups with stronger peers, as measured based on peers' observed outcomes.[18] On the left side of the figure we find no evidence of treatment effects when the nearby peers tend to have the lowest income in adulthood, and on the right side of the figure the treatment effect is twice the ATT. Peer outcomes fully explain the HOPE VI treatment effect: the estimated treatment effect is zero for children with the weakest peers and twice as large as the overall mean for children with the stronger peers. The magnitude of heterogeneity suggests that connections to peers is a key mechanism. The gray series presents estimated based on a sample of children who move out before the award. Here we see no differences between HOPE VI and control sites.

The green series in Figure VIIB presents estimates from a modified version of equation 14 in which we interact the treatment dummies not only with the average adult income rank of peers in ones own cohort, but also peers in the same race and sex who are a year older and a year younger. Here we find that children's outcomes are much more strongly related to those of peers in their own cohort, with whom they are likely to

[18] In principle, this approach could suffer from a reflection problem (Manski, 1993), whereby peers' outcomes might be influenced by the better outcomes of those treated by HOPE VI rather than the other way around. In practice, there are 44.4 times as many children in the peer groups as those directly treated by HOPE VI, suggesting that spillovers in the opposite direction are likely to be small.

interact most in school, than with peers who are a year older or younger. Note that all specifications include site by grant-year fixed effects, which rules out the possibility that common area-level shocks drive these results. Instead, the results support the view that the HOPE VI treatment led to greater connections between children living in public housing and their nearby peers. When peers are stronger, children gain more from the HOPE VI treatment, perhaps because of changes in their behaviors, aspirations, or connections to job opportunities.

Motivated by these correlations with peer outcomes, we test whether social interaction changed directly through an anonymized dataset on cell phone pings (Cook, 2024) that we use to obtain information on the locations that public housing recipients visit in 2019. Table V presents these estimates and shows that people residing in HOPE VI revitalized projects are significantly more likely to spend time in other residences outside the public housing projects than individuals residing in matched control projects. They are also more likely to spend time in higher-income neighborhoods (block groups) than those who live in control projects. Consistent with these data on where people spend their time, Facebook data show that low-income children also have more high-income friends in schools in treated neighborhoods than those who attend schools in control neighborhoods.

Figure VIII shows that we find no change in outcomes for children growing up in the neighborhoods immediately adjacent to revitalized projects, suggesting that increased interactions with public housing residents did not negatively impact the outcomes of children in surrounding areas. Although our ability to detect spillover effects in the broader community is constrained by limitations in statistical power, this finding is consistent with observational evidence showing that communities with greater cross-class interaction exhibit better outcomes for children raised in low-income families, but no worse outcomes for those raised in high-income families (Chetty et al., 2022).

To obtain insight into why patterns of interaction changed so much in revitalized projects, we investigate how rates of crime changed in treated vs. control projects. Using data on incarceration rates among public housing residents and neighborhood-level data on arrest records, Table V shows that crime rates fell by more than 30% in treated projects relative to control projects, consistent with Aliprantis and Hartley (2015); Sandler (2017). Although other factors may have contributed as well, these results point to improvements in safety as one reason that social interaction between those living in housing projects and surrounding areas increased. Figure A.12 presents the relationship between crime and upward mobility, the magnitude suggest that reductions in crime could play an important role.

Table A.7 explores heterogeneous treatment effects by baseline characteristics. We use the estimator described by Dube et al. (2023) in which we regress the change in economic mobility between children who move in after and those who move out before, on an indicator for HOPE VI interacted with a baseline covariate. Here we defined economic mobility as the average adult income rank of children, residualizing out parental income. The results show that there is no evidence of heterogeneity across sites. While this could be because we are underpowered, it also suggests that HOPE VI was not effective only in the most disadvantaged sites. Indeed Figure A.4 shows that prior to the award there were projects with different levels of disadvantaged.

VI.C Broader Implications Beyond HOPE VI Sites

Our analysis of mechanisms suggests that the HOPE VI public housing projects were essentially islands cut off from 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. Is such an approach scalable to more low-income communities? Or is this type of intervention only impactful in highly distressed public housing projects?

To explore this question we develop a new measure of social connection between two neighborhoods, which characterizes the extent to which the outcomes of children in one neighborhood are correlated with outcomes of peers in another neighborhood. Let $\bar{y}_{np}$ denoted the average income of children who live in neighborhood n and peer group p, where a peer group is defined by the interaction between age, gender, and race. Then the proposed measure of social connection between neighborhoods n' and n'' is simply the correlation,

$$\rho(\bar{y}_{gn'}, \bar{y}_{gn''}) = \frac{cov(\bar{y}_{gn'}, \bar{y}_{gn''})}{\sigma_{\bar{y}_{gn'}} \sigma_{\bar{y}_{gn''}}}. \tag{15}$$

Intuitively, neighborhoods are more connected (more positive values of $\rho(\bar{y}_{gn'}, \bar{y}_{gn''})$) if the outcomes of children in the same social group co-move together. Furthermore, the estimator described in equation 14 identifies the difference between the average social connection index in the HOPE VI and matched control neighborhoods. We make this point formally in Appendix D.

Figure IX shows that children in public housing generally tend to have lower adult income when they grow up in projects that are socially disconnected. To construct the figure, we measure construct the social connection index between the public housing project and the children in the surrounding 1 mile radius

for every public housing project in the United States. Figure IX presents a binned scatter plot, with this measure on the horizontal axis, and the gap in expected income on the vertical axis. The positive slopes show that children who grow up in public housing projects that are socially connected to the surrounding neighborhoods achieve similar levels of success as the non-public housing residents, while those who are in socially isolated neighborhoods do far worse. Note that because the social connection measure looks at the correlation across subgroups within neighborhoods, there is no mechanical relationship between this correlation and the average incomes in the two neighborhoods. We makes this point formally in Appendix D.

VII 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 creating greater opportunities for upward income mobility in the public housing projects it targeted. Children who grow up from birth in revitalized public housing projects earn 40% more than comparable children who grow up in the same places prior to revitalization. These earnings gains can be explained in large part by changes in social interaction: children in revitalized projects interact more with higher-income peers in surrounding neighborhoods and gain more when they have stronger peers.

Our estimates imply that each revitalized public housing unit generates $45,000 of additional earnings for the children who grow up there per year, generating large returns relative to the $170,000 per unit up-front cost of revitalization. There are still more than XX children living in housing projects comparable to those that were revitalized through the HOPE VI program in terms of poverty and social isolation; our results suggest that revitalizing these neighborhoods could have substantial benefits going forward.

Although HOPE VI improved neighborhoods' causal effects on economic opportunity, the original residents of public housing typically did not benefit significantly from the program (Staiger, Palloni and Voorheis, 2024). Only a small fraction of 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. Developing policies that achieve similar gains with less displacement would be a valuable direction for future work.

Beyond the HOPE VI program, our findings demonstrate that it is possible to create neighborhoods with substantially higher levels of upward mobility through policy intervention even without changing broader determinants of outcomes such as labor market conditions or schools. The potential for change in short time frames via targeted neighborhood-level interventions is particularly notable given the longstanding roots of neighborhood disadvantage, which can be traced back to factors such as discriminatory credit lending practices in the 1930s (Aaronson, Hartley and Mazumder, 2021; Lane et al., 2022).

More than XX million children currently live in private market housing in neighborhoods that are as socially isolated as the public housing projects revitalized by HOPE VI. Our findings suggest that greater interaction between these neighborhoods and surrounding communities could improve outcomes in these neighborhoods. Place-based investments that create such connections could provide a scalable path to increasing economic mobility more broadly.

References

Aaronson, Daniel, Daniel Hartley, and Bhashkar Mazumder. 2021. "The Effects of the 1930s HOLC "Redlining" Maps." *American Economic Journal: Economic Policy*, 13(4): 355–392.

Aliprantis, Dionissi, and Daniel Hartley. 2015. "Blowing it up and knocking it down: The local and city-wide effects of demolishing high concentration public housing on crime." *Journal of Urban Economics*, 88: 67–81.

Almagro, Milena, Eric Chyn, and Bryan A Stuart. 2023. "Urban Renewal and Inequality: Evidence from Chicago's Public Housing Demolitions." National Bureau of Economic Research.

Bahchieva, Raisa, and Amy Hosier. 2001. "Determinants of tenure duration in public housing: The case of New York City." *Journal of Housing Research*, 307–348.

Baum-Snow, Nathaniel, and Justin Marion. 2009. "The effects of low income housing tax credit developments on neighborhoods." *Journal of Public Economics*, 93(5-6): 654–666. Publisher: Elsevier.

Bergman, Peter, Raj Chetty, Stefanie DeLuca, Nathaniel Hendren, Lawrence F. Katz, and Christopher Palmer. 2024. "Creating Moves to Opportunity: Experimental Evidence on Barriers to Neighborhood Choice." *American Economic Review*, 114(5): 1281–1337.

Blanco, Hector, and Lorenzo Neri. 2023. "Knocking it down and mixing it up: The impact of public housing regenerations." *IZA Discussion Paper*.

Blanco, Hector, and No\'emie Sportiche. 2024. "Local Effects of Bypassing Zoning Regulations." Place: Rochester, NY Type: SSRN Scholarly Paper.

Bruhn, Jesse. 2018. "Crime and Public Housing: A General Equilibrium Analysis."

Buron, Larry, Susan J Popkin, Diane K Levy, Laura E Harris, and Jill Khadduri. 2002. "The HOPE VI Resident Tracking Study."

Busso, Matias, Jesse Gregory, and Patrick Kline. 2013. "Assessing the incidence and efficiency of a prominent place based policy." *American Economic Review*, 103(2): 897–947.

Chetty, Raj, and Nathaniel Hendren. 2018*a*. "The impacts of neighborhoods on intergenerational mobility I: Childhood exposure effects." *The Quarterly Journal of Economics*, 133(3): 1107–1162. Publisher: Oxford University Press.

Chetty, Raj, and Nathaniel Hendren. 2018*b*. "The impacts of neighborhoods on intergenerational mobility II: County-level estimates." *The Quarterly Journal of Economics*, 133(3): 1163–1228. Publisher: Oxford University Press.

Chetty, Raj, John N Friedman, Nathaniel Hendren, Maggie R Jones, and Sonya R Porter. 2018. "The opportunity atlas: Mapping the childhood roots of social mobility." National Bureau of Economic Research.

Chetty, Raj, Matthew O. Jackson, Theresa Kuchler, Johannes Stroebel, Nathaniel Hendren, Robert B. Fluegge, Sara Gong, Federico Gonzalez, Armelle Grondin, Matthew Jacob, Drew Johnston, Martin Koenen, Eduardo Laguna-Muggenburg, Florian Mudekereza, Tom Rutter, Nicolaj Thor, Wilbur Townsend, Ruby Zhang, Mike Bailey, Pablo Barberá, Monica Bhole, and Nils Wernerfelt. 2022. "Social capital I: measurement and associations with economic mobility." *Nature*, 608(7921): 108–121. Publisher: Nature Publishing Group.

Chetty, Raj, Nathaniel Hendren, and Lawrence F Katz. 2016. "The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment." *American Economic Review*, 106(4): 855–902. Publisher: American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203.

Chetty, Raj, Will S. Dobbie, Benjamin Goldman, Sonya Porter, and Crystal Yang. 2024. "Changing Opportunity: Sociological Mechanisms Underlying Growing Class Gaps and Shrinking Race Gaps in Economic Mobility."

Chyn, Eric. 2018. "Moved to opportunity: The long-run effects of public housing demolition on children." *American Economic Review*, 108(10): 3028–56.

Chyn, Eric, and Lawrence F Katz. 2021. "Neighborhoods matter: Assessing the evidence for place effects." *Journal of Economic Perspectives*, 35(4): 197–222.

Chyn, Eric, Robert Collinson, and Danielle Sandler. 2022. "The long-run effects of residential racial desegregation programs: evidence from gautreaux." Working paper.

Cisneros, Henry G, and Lora Engdahl. 2009. *From despair to hope: HOPE VI and the new promise of public housing in America's cities.* Brookings Institution Press.

Collins, William J, and Katharine L Shester. 2013. "Slum clearance and urban renewal in the United States." *American Economic Journal: Applied Economics*, 5(1): 239–273. Publisher: American Economic Association.

Cook, Cody. 2024. "Where to Build Affordable Housing? Evaluating the Tradeoffs of Location." *Working Paper*.

Dantzler, Prentiss A. 2021. "Household characteristics or neighborhood conditions? Exploring the determinants of housing spells among US public housing residents." *Cities*, 117: 103335.

Deutscher, Nathan. 2020. "Place, Peers, and the Teenage Years: Long-Run Neighborhood Effects in Australia." *American Economic Journal: Applied Economics*, 12(2): 220–249.

Diamond, Rebecca, Tim McQuade, and Franklin Qian. 2019. "The effects of rent control expansion on tenants, landlords, and inequality: Evidence from San Francisco." *American Economic Review*, 109(9): 3365–3394. Publisher: American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203.

Dube, Arindrajit, Daniele Girardi, Oscar Jorda, and Alan M Taylor. 2023. "A local projections approach to difference-in-differences event studies." National Bureau of Economic Research.

Freeman, Lance. 2005. "Does housing assistance lead to dependency? Evidence from HUD administrative data." *Cityscape*, 115–133.

GAO. 2003. "HOPE VI Resident Issues and Changes in Neighborhoods Surrounding Grant Sites." Government Accountability Office.

Garin, Andrew, and Jonathan Rothbaum. 2024. "The Long-Run Impacts of Public Industrial Investment on Local Development and Economic Mobility: Evidence from World War II*." *The Quarterly Journal of Economics*, qjae031.

Goetz, Edward G. 2013. *New deal ruins: Race, economic justice, and public housing policy.* Cornell University Press.

Gress, Taryn H, Mark L Joseph, and Seungjong Cho. 2019. "Confirmations, new insights, and future implications for HOPE VI mixed-income redevelopment." *Cityscape*, 21(2): 185–212. Publisher: JSTOR.

Haltiwanger, John C, Mark J Kutzbach, Giordano Palloni, Henry O Pollakowski, Matthew Staiger, and Daniel H Weinberg. 2024. "The children of HOPE VI demolitions: National evidence on labor market outcomes." *Journal of Public Economics*, 239: 105188.

HUD. 2013. "Termination." In *HUD Occupancy Handbook*. Department of Housing and Urban Development.

HUD. 2019. "Waiting List & Tenant Selection." In *Public Housing Occupancy Guidebook*. Department of Housing and Urban Development.

HUD. n.d.. "Revitalization Grants - HOPE VI - Public and Indian Housing."

Jacob, Brian A. 2004. "Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago." *American Economic Review*, 94(1): 233–258.

Joseph, Mark L, and Robert J Chaskin. 2012. "Mixed-income developments and low rates of return: Insights from relocated public housing residents in Chicago." *Housing Policy Debate*, 22(3): 377–405. Publisher: Taylor & Francis.

Lane, Haley M., Rachel Morello-Frosch, Julian D. Marshall, and Joshua S. Apte. 2022. "Historical Redlining Is Associated with Present-Day Air Pollution Disparities in U.S. Cities." *Environmental Science & Technology Letters*, 9(4): 345–350. Publisher: American Chemical Society.

Manski, Charles F. 1993. "Identification of Endogenous Social Effects: The Reflection Problem." *The Review of Economic Studies*, 60(3): 531–542.

Massey, Douglas S, and Shawn M Kanaiaupuni. 1993. "Public housing and the concentration of poverty." *Social science quarterly*, 74(1): 109–122.

McClure, Kirk. 2018. "Length of stay in assisted housing." *Cityscape*, 20(1): 11–38.

Murphy, LaShonia Michelle. 2012. "The Allocation of Funds within HOPE VI: Applicants and Recipients." Publisher: Virginia Tech.

NCSDPH. 1992. "The Final Report of the National Comission on Severely Distressed Public Housing." National Comission on Severely Distressed Public Housing.

Notice of Funding Availability for Revitalization of Severely Distressed Public Housing; HOPE VI Revitalization Grants Fiscal Year 2004, 69 Fed. Reg. 64155-64164 (Nov. 3, 2004)

Popkin, Susan J, Diane K Levy, Laura E Harris, Jennifer Comey, Mary K Cunningham, and Larry F Buron. 2004. "The HOPE VI program: What about the residents?" *Housing Policy Debate*, 15(2): 385–414. Publisher: Taylor & Francis.

Rosenbaum, Paul R., and Donald B. Rubin. 1983. "The central role of the propensity score in observational studies for causal effects." *Biometrika*, 70(1): 41–55.

Rosenbaum, Paul R., and Donald B. Rubin. 1984. "Reducing Bias in Observational Studies Using Subclassification on the Propensity Score." *Journal of the American Statistical Association*, 79(387): 516–524. Publisher: [American Statistical Association, Taylor & Francis, Ltd.].

Sandler, Danielle H. 2017. "Externalities of public housing: The effect of public housing demolitions on local crime." *Regional Science and Urban Economics*, 62: 24–35. Publisher: Elsevier.

Sharkey, Patrick, and Gerard Torrats-Espinosa. 2017. "The effect of violent crime on economic mobility." *Journal of Urban Economics*, 102: 22–33.

Staiger, Matthew, Giordano Palloni, and John Voorheis. 2024. "Neighborhood Revitalization and Residential Sorting." *Center for Economic Studies Working Paper*.

Super Notice of Funding Availability (SuperNOFA) for HUD's Housing, Community Development and Empowerment Programs and Section 8 Housing Voucher Assistance for Fiscal Year 2000, 65 Fed. Reg. 9603 (Feb. 24, 2000)

Tach, Laura, and Allison Dwyer Emory. 2017. "Public housing redevelopment, neighborhood change, and the restructuring of urban inequality." *American journal of sociology*, 123(3): 686–739. Publisher: University of Chicago Press Chicago, IL.

Turner, Margery Austin, Susan J. Popkin, and G. Thomas Kingsley. 2005. "Distressed Public Housing: What It Costs to Do Nothing." Urban Institute.

Wagner, Deborah, and Mary Layne. 2014. "The Person Identification Validation System (PVS): Applying the Center for Administrative Records Research and Applications' (CARRA) Record Linkage Software." *Center for Administrative Records Research and Applications Working Paper*.

Zielenbach, Sean, and Richard Voith. 2010. "HOPE VI and neighborhood economic development: The importance of local market dynamics." *Cityscape*, 99–131. Publisher: JSTOR.

VIII Tables

TABLE I
Summary Statistics

	Non-HOPE VI	Matched Controls	HOPE VI
	(1)	(2)	(3)
A. Public Housing Residents			
Share Non-Hispanic Black	.3817	.655	.6664
Share Non-Hispanic White	.4024	.1195	.1067
Share Hispanic	.1281	.1344	.1157
Share Elderly	30.15	20.54	15.55
Share Disabled	12.24	10.89	10.52
Share H.H. with Female Head	72.08	78.31	77.23
Share Adults Incarcerated in Census	.06794	.09937	.1159
Mean H.H. Size	2.36	2.72	2.67
Mean H.H. Income (HUD)	$21,910	$13,450	$13,430
Share Adults with Wage Income	.2646	.248	.2295
Mean H.H. Income (Tax Data)	$14,790	$12,450	$12,400
Share Adults with H.H. Income Below $15,000	.578	.682	.6853
Mean Income Rank of Prior Residents	42.67	39.06	38.33
B. Adult Outcomes of Children			
Mean H.H. Income Rank, Ages 29-30	38.33	36.36	37.3
Mean H.H. Income, Age 30	$26,240	$23,450	$24,410
Mean Wages, Age 30	$21,920	$19,900	$20,440
Share Married, Age 30	.131	.09729	.106
Share Alive, Age 30			
Mean Tract Poverty Rate, Age 30	.2425	.2762	.2533
C. Projects			
Mean Adults Per Year	1,192,778	596,000	126,000
Mean Children Per Year	316,283	235,000	56,000
Mean Number of Occupied Units	166.1	364.6	391.9
Mean Population	584.2	923.3	910
Mean Public Housing Projects Per Year	2,583	900	200

Notes: This table reports summary statistics for our primary analysis sample. The HOPE VI and matched control samples are limited to observations three years before the grant year. The non-HOPE VI sample is a panel that includes observations between 1990 and 2007. Panel A reports characteristics of the public housing residents, Panel B reports characteristics of the children living in the public housing project at the time of measurement, and Panel C reports characteristics of the public housing projects. See Section III.B for more details and variable definitions. All monetary values are reported in 2022 dollars. All estimates in this and all subsequent tables and figures have been rounded to four significant digits as part of the disclosure avoidance protocol. Counts are rounded in the following manner: numbers between 10,000 and 99,999 are rounded to the nearest 500; between 100,000 and 999,999 to the nearest 1,000; and above 1,000,000 to four significant digits.

TABLE II
Impact on Childhood Exposure Effects, Sensitivity Analysis

	Kid Family Income Rank Measured at Ages 29-30				
	(1)	(2)	(3)	(4)	(5)
Years Exposed x Revitalization	-0.050	0.474***	0.477***	0.458***	0.594***
	(0.163)	(0.147)	(0.133)	(0.175)	(0.225)
Move Out Before Award Only	X				
Stay Until 18 Only				X	
Additional Covariates			X		
IV Using Entry Age					X
Observations	35,000	53,000	53,000	36,000	53,000

Notes: This table reports the estimates of β from equation 12 in which we regress income rank in adulthood (measured between the ages of 29 and 30) on the interaction between years spent in public housing and a HOPE VI indicator. Column 1 restricts to individuals who moved out of project before the grant, while all other columns restrict to individuals who moved into the project after the grant. Column 4 additionally restricts to individuals who stay until age 18. Column 3 adds additional individual-level covariates. Column 5 instruments for the number of years exposed with the individual's entry age. All estimates weight by the individual-level inverse propensity score, control for project-by-grant year-by-entry year fixed effects, and cluster standard errors by project. Standard errors are in parentheses. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$

TABLE III
Heterogeneous Impacts on Childhood Exposure Effects

	Kid Family Income Rank Measured at Ages 29-30			
	(1)	(2)	(3)	(4)
Years Exposed Before 12 x Revitalization	0.245			
	(0.230)			
Years Exposed 12 to 18 x Revitalization	0.689***			
	(0.202)			
Years Exposed x Revitalization		0.463***	0.466***	0.490***
		(0.176)	(0.151)	(0.144)
Years Exposed x Revitalization x Male		0.030		
		(0.226)		
Years Exposed x Revitalization x Parent Income			0.108	
			(0.155)	
Years Exposed x Revitalization x Predicted Outcome				0.053
				(0.145)
Observations	53,000	53,000	53,000	53,000

Notes: This table reports the estimates of β from variants of equation 12 in which we regress income rank in adulthood (measured between the ages of 29 and 30) on the interaction between years spent in public housing and a HOPE VI indicator. Column 1 uses separate interaction terms for revitalization with years exposed before 12 versus during ages 12-17. Columns 2-4 each control for the additional interaction of revitalization and years exposed with an individual-level variable: respectively, a male indicator. parental income pre-move, and predicted income at ages 29-30 based on observable characteristics defined upon entry into public housing. Both parental income and predicted adult income are standardized based on the control mean and standard deviation. All estimates weight by the individual-level inverse propensity score, control for project-by-grant year-by-entry year fixed effects, and cluster standard errors by project. Standard errors are in parentheses. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$

TABLE IV
Calculation of Lifetime Earnings Impact of HOPE VI

	(1)
Treatment effect (TOT) on Upward Mobility (in ranks), Repeated Cross Section Design	.
[Translated to 2020 USD]	.
Treatment effect (TOT) on Upward Mobility (in ranks), Movers Design	.
[Translated to 2020 USD]	.
Average children per public housing unit	.
Average tax rate	.
Present discounted value of sum of lifetime earnings for average American (Opportunity Atlas)	.
Mean cost per unit, total funds	.
Mean cost per unit, direct HOPE VI funds	.
Years after which MVPF infinite, total funds	.
Years after which MVPF infinite, direct HOPE VI funds	.

Notes: This table outlines the steps we use to translate our estimated treatment effects into lifetime earnings effects for the children whose families moved to HOPE VI projects.

TABLE V
Impact on Other Neighborhood Characteristics

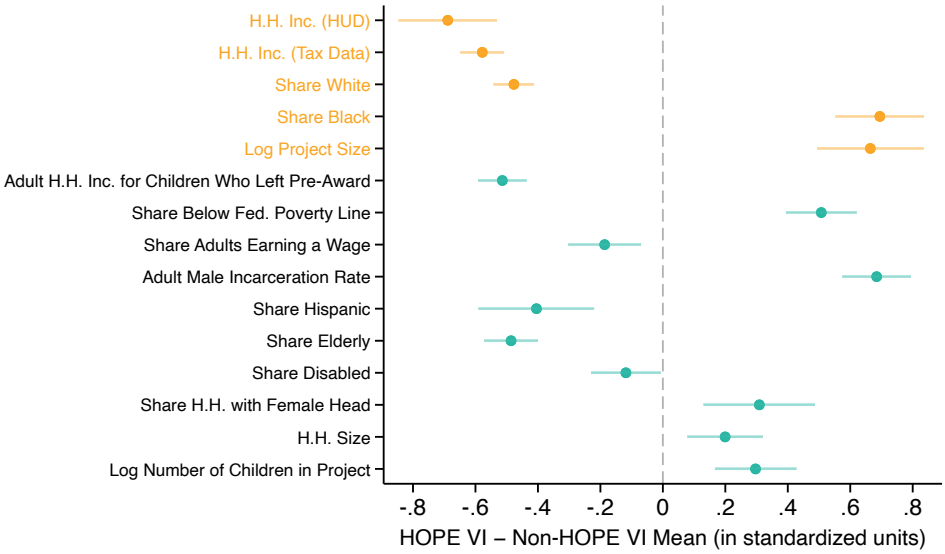
	Ln(Arrests for Violent Crime in Census Tract)	Ln(Share Adults Ever Incarcerated in Site)	Ln(Number of Visits to Other Homes)	Ln(Median Income of Block Groups Visited)
	(1)	(2)	(3)	(4)
Revitalization	-0.290**	-0.231***	0.060***	0.183***
	(0.116)	(0.049)	(0.020)	(0.032)
Observations	130	26,500	100,036	92,358
Control Mean	.	.	.	.
Control SD	.	.	.	.

Notes: This table shows the effects of revitalization on neighborhood characteristics related to crime and community integration. In Column 1, the outcome is the difference in the natural log of arrests for violent crime in 2010 versus 2000 in Census tracts with projects that received an award between 2001 and 2005, as well as Census tracts in the same cities as those that received awards. Column 2 uses project-level repeated cross-sectional data to estimate the effect on the natural log of incarceration rates in HOPE VI projects 15 years after the award, compared to 3 years before the award. The regression weights by the project-level inverse propensity score, controls for project and year after award-by-grant year fixed effects, and clusters standard errors at the project level. Columns 3 and 4 report estimates based on regressions of the natural log of the number of residential visits and the average block group median income for residential parcels visited on whether a project receives a revitalization grant. We exclude visits at home or work, control for project-level covariates and MSA fixed effects, and cluster standard errors by project. Column 4 additionally weights by the time spent in the block group. Standard errors are in parentheses. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$

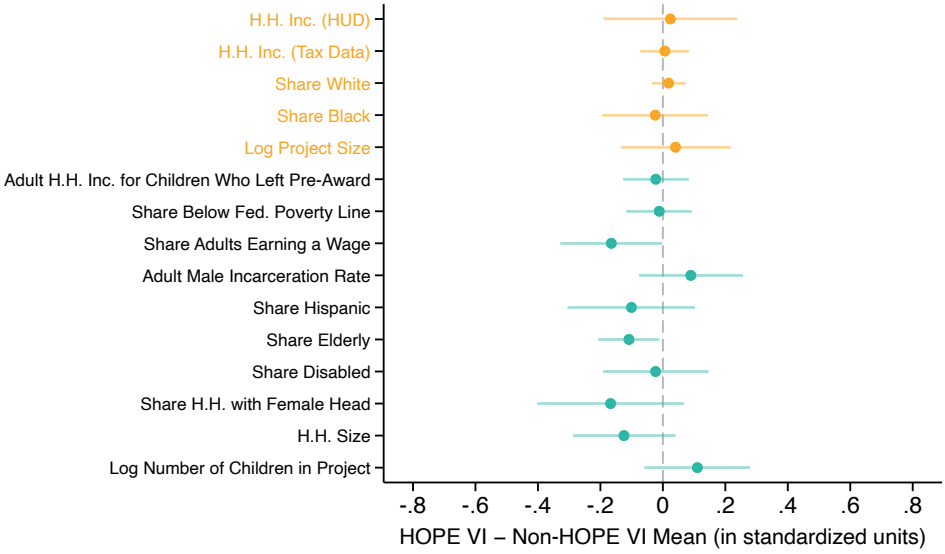
IX Figures

FIGURE I
Pre-Award Differences in Observable Characteristics

A. Before Matching

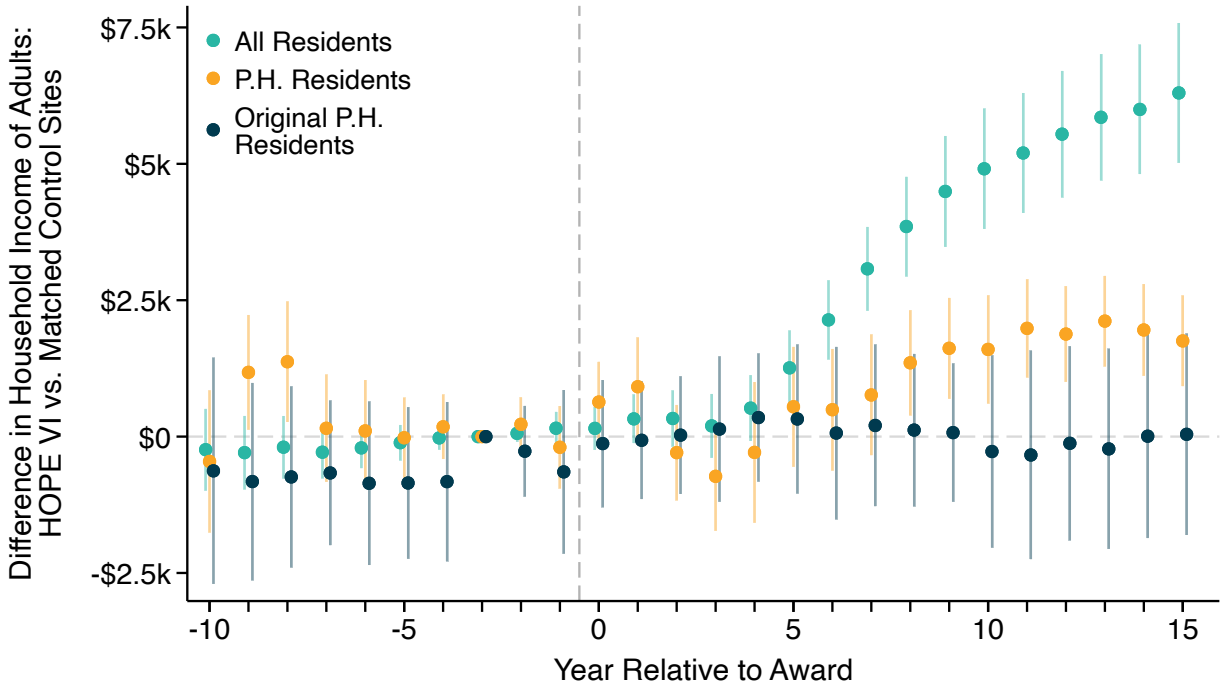


B. After Matching



Notes: These figures show the estimated differences on a number of covariates between HOPE VI and matched control sites, three years prior to the award date. Each point presents an estimate from a separate regression, where the dependent variable is standardized based on the control mean and standard deviation. All regressions weight by the baseline population of the project (Panel A) or by the project-level inverse propensity score (Panel B), control for year of first award fixed effects, and cluster standard errors by project. Variables in orange are used to compute inverse propensity score weights.

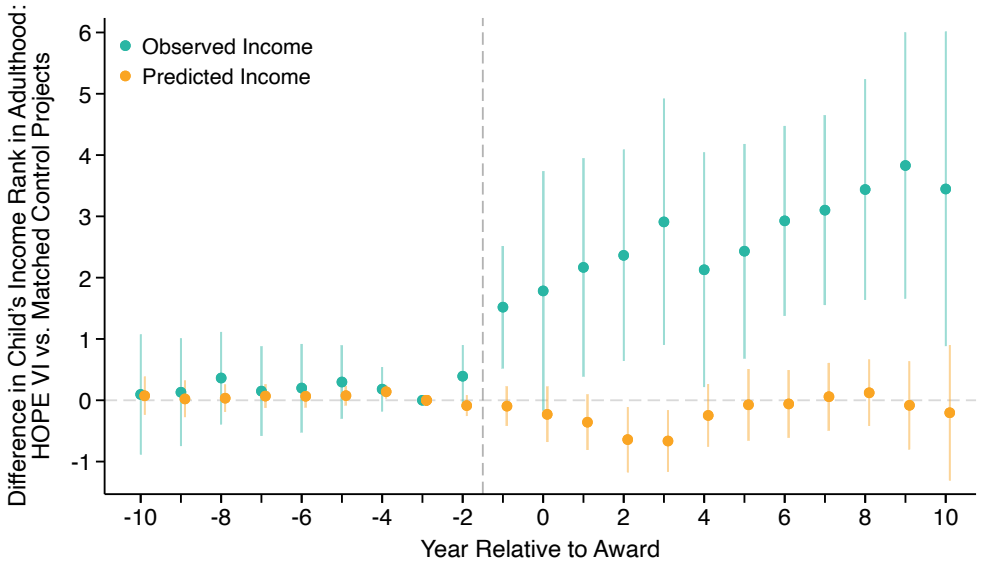
FIGURE II
Impact on the Income of Targeted Sites and Original Resident Adults



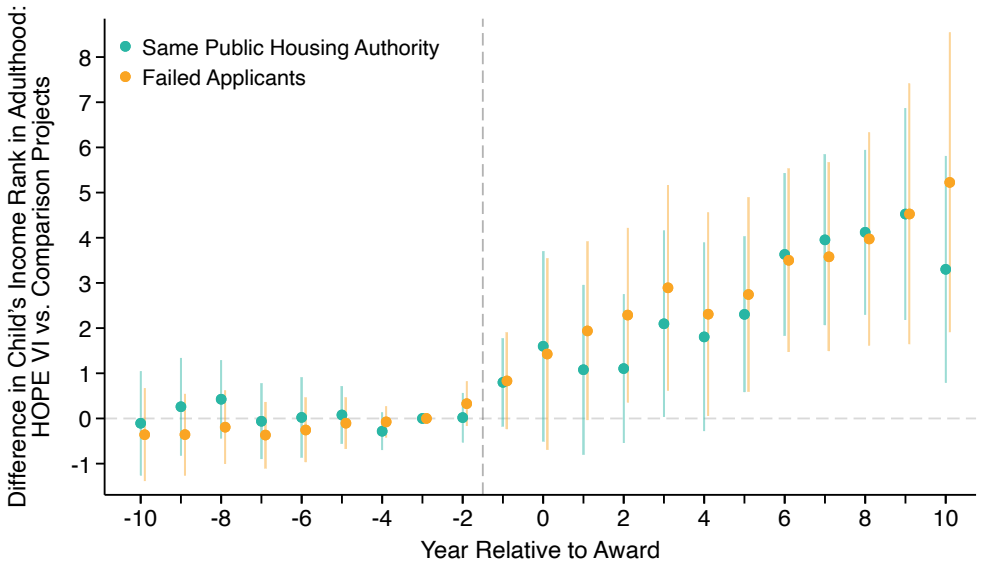
Notes: Each series presents estimates of β from a variant of equation 10 where the outcome is the adjusted gross income (AGI) of a project-level subgroup of individuals in the footprint of HOPE VI and matched control projects. The key dependent variable is the interaction between an indicator for the specific year relative to the grant year and a HOPE VI indicator. The green series measures AGI for everyone in the project footprint, the orange series only public housing residents in the project footprint, and the blue series only residents living in the project in the year before the award. We require incumbent residents to have an observed outcome in the reference year (three years prior to the grant year) and to be observed in the HUD data. Estimates control for project-by-grant year and grant year-by-year after award fixed effects, are weighted by the project-level inverse propensity score, and cluster standard errors by project.

FIGURE III
Impact of HOPE VI on Children's Incomes in Adulthood

A. Actual and Predicted Outcomes



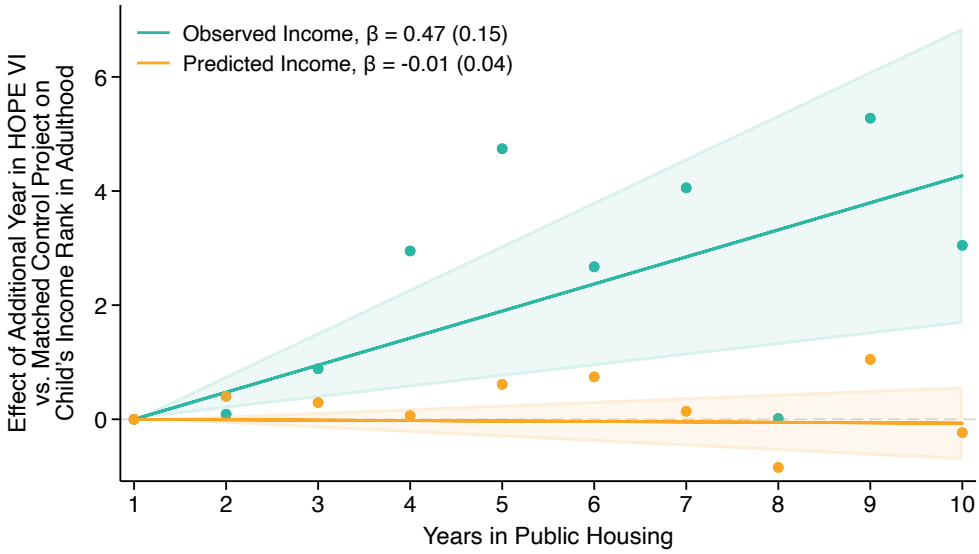
B. Robustness to Comparison Group



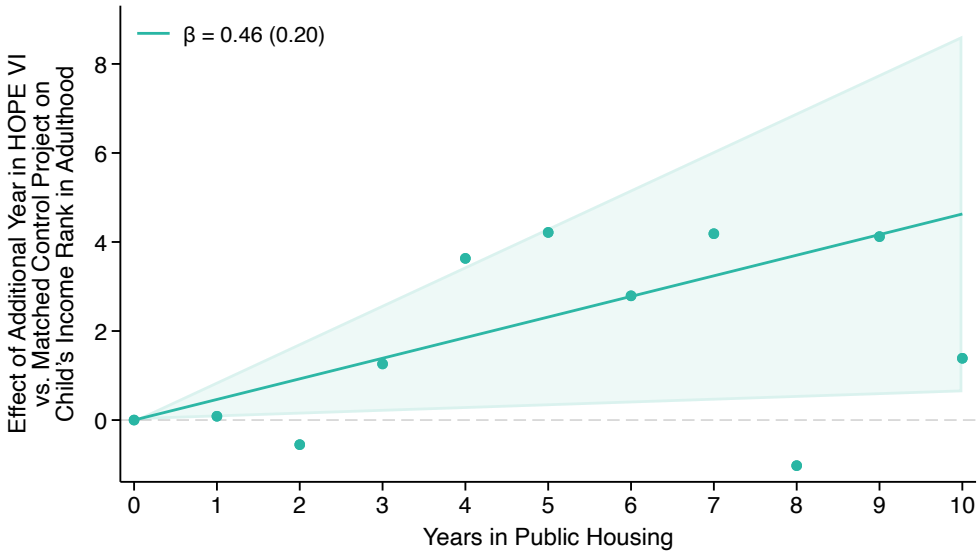
Notes: The green series in Panel A presents estimates of β from equation 10 in which we regress income rank in adulthood (measured between the ages of 29 and 30) on the interaction between an indicator for living in public housing project in a given year and a HOPE VI indicator. The estimates for years -10 to -2 correspond to the prior resident sample who moved out from the site at least two years before the award date. The estimate for year -1 corresponds to the original resident sample who lived in the site the year before the award. The estimates from years 0 through 10 correspond to the new resident sample, children who moved in after the award. Panel A uses as the control group the set of matched control projects, while Panel B shows identical comparisons to two alternative control groups. The orange series in Panel A additionally plots the effect on predicted income in adulthood, based on observable characteristics defined upon entry into public housing. Estimates of the effect on non-predicted income control for pre-move-in parent income percentile, upward mobility in prior neighborhood, and years spent in public housing. All estimates control for project-by-grant year and year after award-by-grant year fixed effects and cluster standard errors by project, and estimates in Panel A weight by the individual-level inverse propensity score.

FIGURE IV
Effect of Additional Year in HOPE VI vs. Matched Control Project

A. Actual and Predicted Outcomes

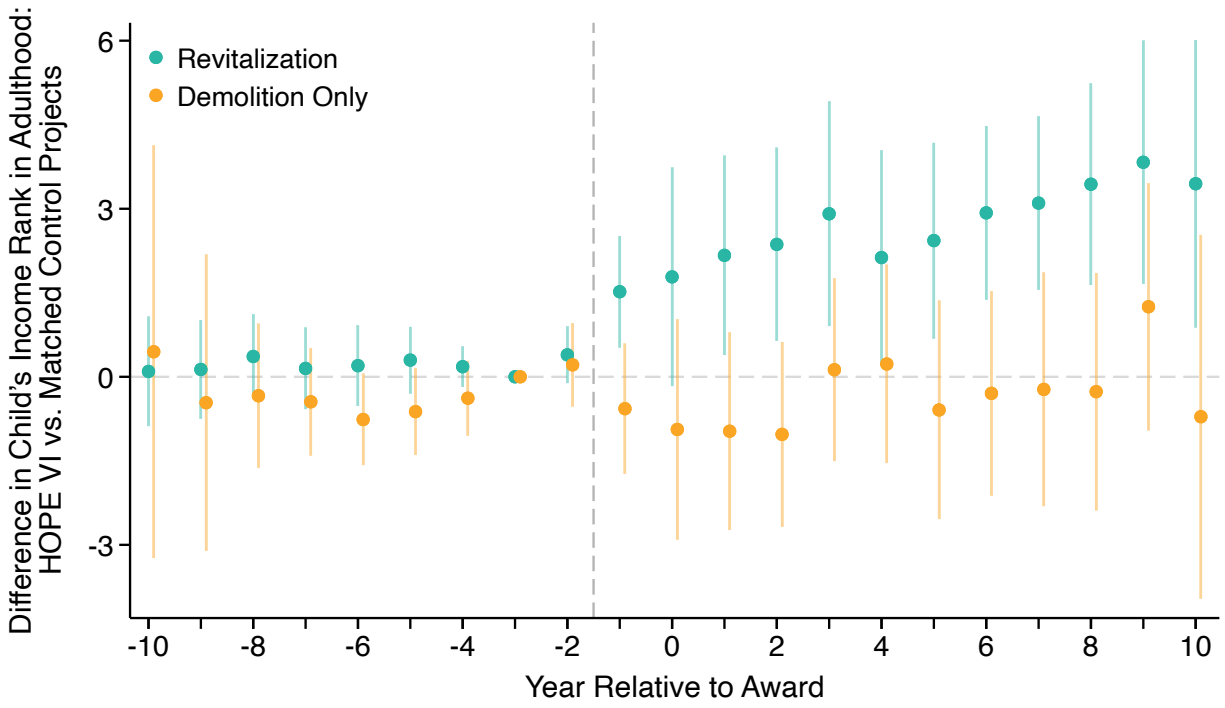


B. Inclusion of Household Fixed Effects



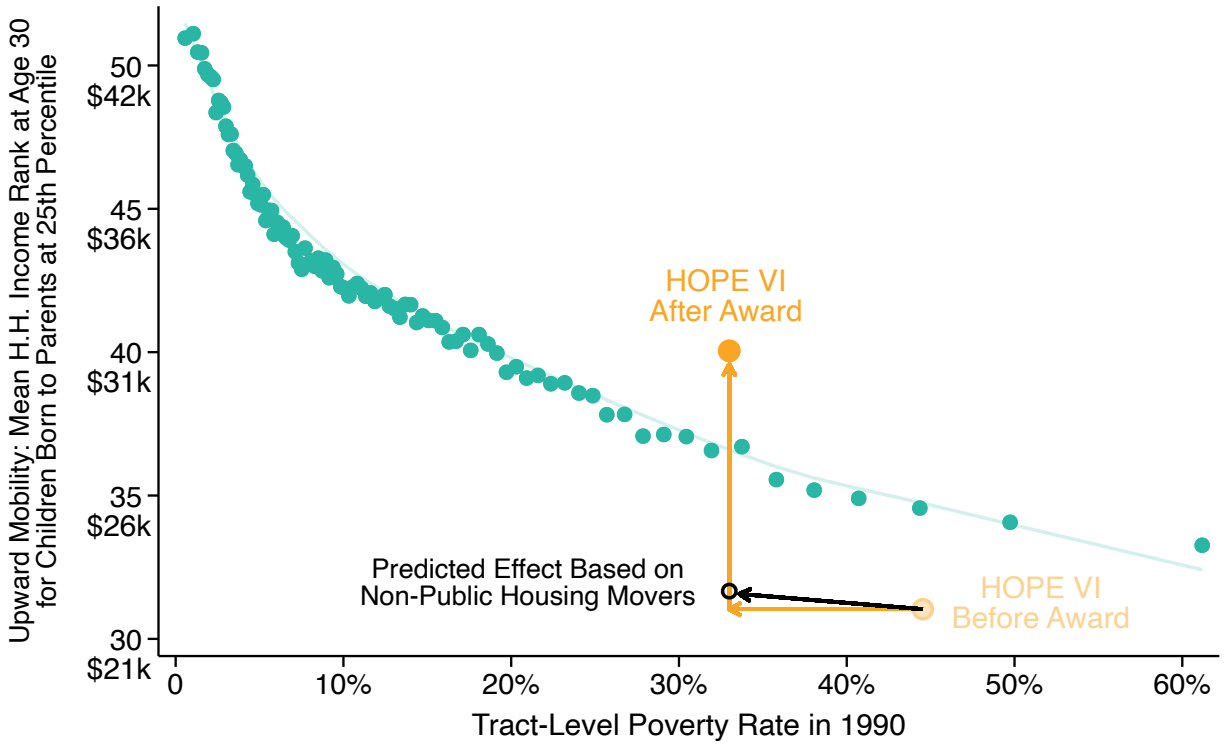
Notes: The green line in panel A presents estimates of β from equation 12 in which we regress income rank in adulthood (measured between the ages of 29 and 30) on the interaction between years spent in public housing and a HOPE VI indicator, controlling for site by a grant year-by-year of move in fixed effect. The orange line presents estimates from the same specification but where the outcome variable is the predicted adult income rank based on observable characteristics defined upon entry into public housing. Panel B presents estimates from a variant of equation 12 which includes household fixed effects. Unlike Panel A, the sample includes siblings who never lived in public housing. In all specifications, the sample consists of individuals who move into the project after the grant year. For every series, the difference between the HOPE VI and matched controls are estimated parametrically and non-parametrically by each number of years in public housing.

FIGURE V
Impact of the Demolition-Only Grants on Children's Income in Adulthood



Notes: The green series in replicates the estimates from Figure IIIA. The orange series presents identical estimates but compares projects that received HOPE VI demolition grants and were partially demolished to a different set of matched control projects.

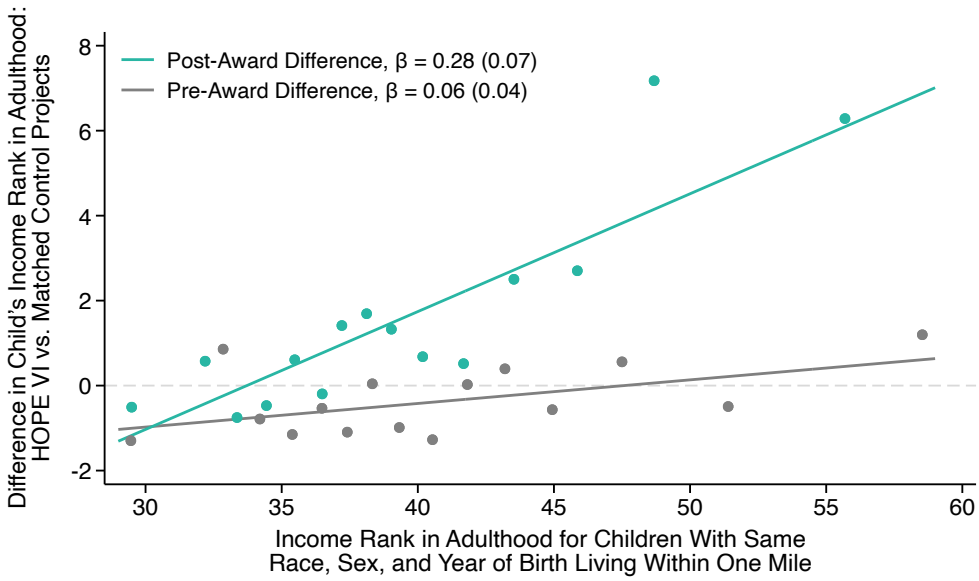
FIGURE VI
Upward Income Mobility vs. Poverty Rates, by Neighborhood



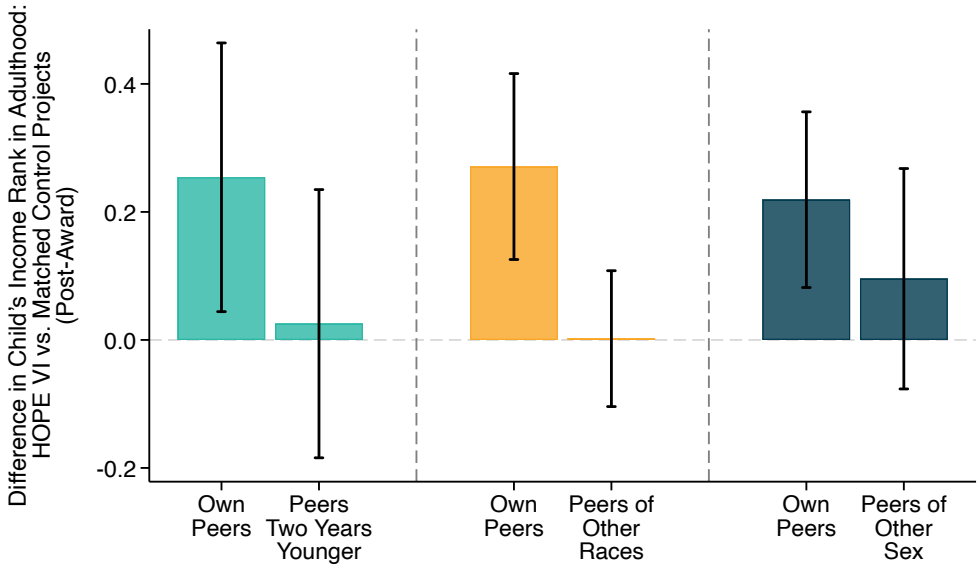
Notes: This figure shows the tract-level distribution of upward mobility by poverty rate. Upward mobility is measured as the mean household income rank at age 30 for children born in that tract with parents at the 25th percentile of the distribution. The mean values for HOPE VI tracts before the award are plotted, as well as the treatment effects on poverty and mobility. The black arrow shows the effect of the same reduction in poverty rates on upward mobility for children not in public housing based on the estimates reported in Table A.6.

FIGURE VII
Heterogeneous Impacts by Outcomes of Nearby Peers

A. Pre-Award versus Post-Award

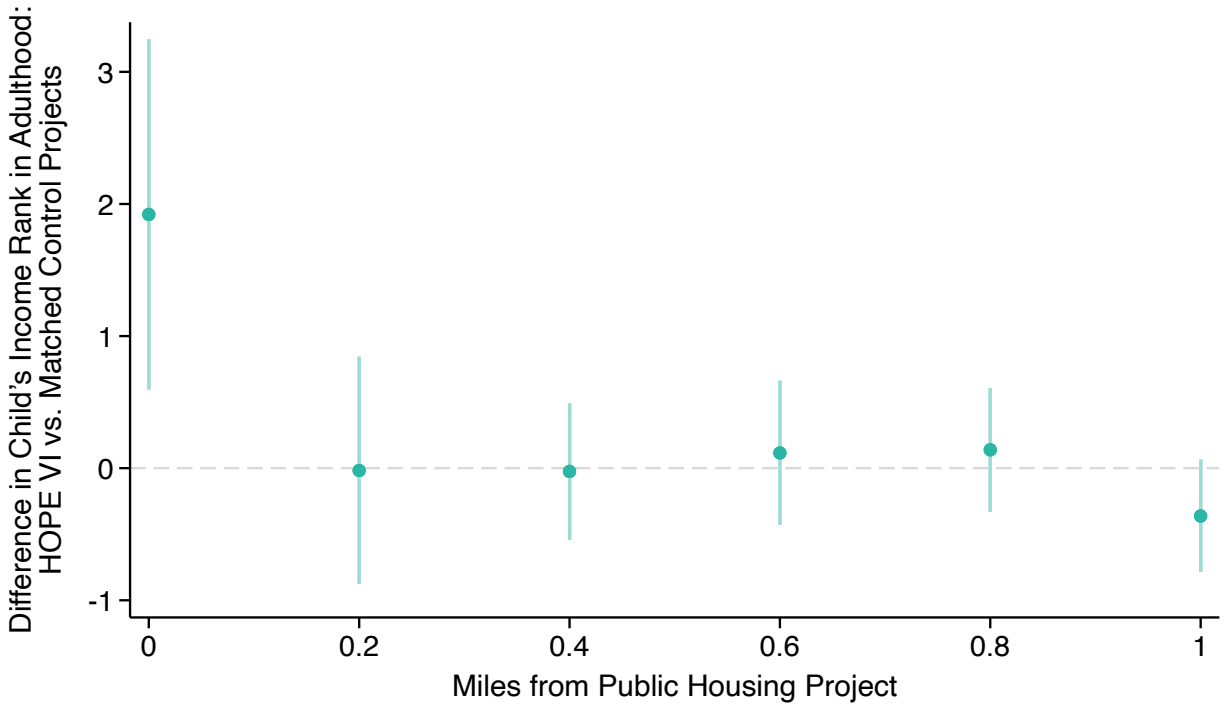


B. Comparison Across Cohorts



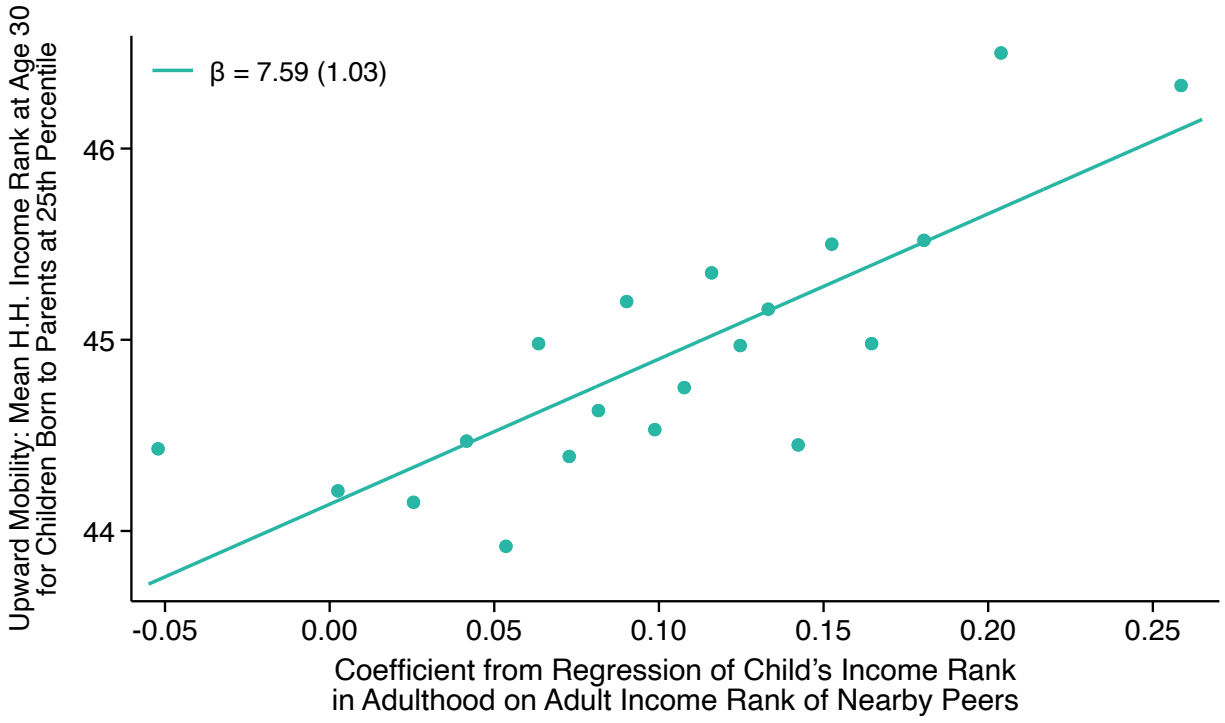
Notes: These figures present estimates of ρ from variations of equation 14, where we show the difference between the correlation of the outcome (income rank in adulthood measured at ages 29-30) of children in public housing and the outcomes of peers for HOPE VI and matched control projects. The outcomes of the peer group are calculated as the exposure-weighted mean income rank in adulthood (measured at ages 29-30) of all individuals with the same race, sex, and year of birth within one mile of the project. Panel A shows the regression coefficient of own income on peer outcome interacted with a revitalization indicator, separately in the pre- and post-award period. For each period, the coefficient is estimated parametrically and non-parametrically by each quantile of the peer measure. Panel B reports coefficients from three separate regressions for individuals living in the projects post-award where we regress own outcome on the outcome of own peers as well as the outcomes of a peer group defined as 1) same race and sex but two years younger, 2) same sex and cohort but different race, or 3) same race and cohort but different sex. All estimates control for pre-move-in parent income percentile, upward mobility in prior neighborhood, and years spent in public housing. We also include demographic fixed effects: in Panel A, race-by-sex; and in Panel B, cohort, race, and sex separately. Estimates are weighted by the individual-level inverse propensity score and cluster standard errors by project.

FIGURE VIII
Effect on Upward Mobility in Surrounding Neighborhoods



Notes: This figure presents estimates of the interaction between indicators for post-award and revitalization on children's income ranks in adulthood (measured at ages 29-30), separately within 0.2 mile-wide bins from a project. Estimates control for several bin-by-project-level covariates (average income, share white, share Black, share Hispanic, average adult age, share of adults with children, block group median rent, block group share receiving public assistance, block group share poor, block group share with at least a college degree, block group share unemployed, and block group share houses that are owner occupied). All estimates weight by the individual-level inverse propensity score and cluster standard errors by project.

FIGURE IX
Mean Outcomes of Children in National Low Income Tracts vs Correlation with Peers' Outcomes



Notes: XX: ANNA TO UPDATE. WATING UNTIL THIS FIGURE IS FINALIZED. This figure presents a binned scatter plot, for non-HOPE VI projects, of the gap in expected income on the vertical axis versus the social connection index on the horizontal axis. The social connection index is the estimate of ρ from Equation 14, which is the correlation between the average adult income (measured at ages 27-28) of individuals in the public housing project with individuals of the same race, sex, and year of birth residing within 1 mile of the project in the same years. We use split-sample instrumental variables to reduce the noise of this estimate. The gap in expected income is calculated as the difference between the adult income (measured at ages 27-28) of individuals in the project and the outcomes of their peers, after residualizing on neighborhood and demographic group fixed effects. For more details on the calculations, see Appendix D. The x-axis and y-axis are calculated on separate samples of the individual children residing within each project. Estimates are then collapsed to the project-level. The gray dotted line shows the average value of the social connection index in HOPE VI sites pre-award with the orange arrow showing the estimated effect of HOPE VI on the social connection index; together, they add to the orange dotted line, which shows the average social connection index in HOPE VI sites post-award.

A Appendix Tables

TABLE A.1
Summary Statistics on HOPE VI Program, Per Project

	Mean	SD
	(1)	(2)
Units, All	361.3	245.3
Units, Public	211.6	172.1
Units, Affordable	107.4	113.3
Units, Market Rate	42.6	79.8
Years of Project, Total	6.7	3.3
Years of Project, Demolition	2.6	2.8
Years of Project, Construction	5.4	3.0
Funding, Total	$64,490,700	$41,873,284
Funding, HOPE VI	$22,826,106	$11,365,952
Funding Per Unit, Total	$312,407	$1,306,167
Funding Per Unit, HOPE VI	$105,228	$317,153

Notes: This table reports per-project summary statistics on the HOPE VI program. The estimates are based on publicly available data of 222 projects that received revitalization grants. We report the average number of total units and units of each subtype; the average amount of time of the process; and the overall and per-unit average level of funding, also broken into HOPE VI-funds only. Total time is calculated as the latest date of full availability minus the earliest date of relocation; the timelines of demolition and construction are similarly defined as the latest finish date minus the earliest start date.

TABLE A.2
Effect on Neighborhood Income

	Adjusted Gross Income of		
	All Residents	P.H. Residents	Incumbent Residents
	(1)	(2)	(3)
A. Comparison Group: Matched Control Projects			
Years 0-5 After Award x Revitalization	555.3**	42.5	733.5*
	(247.8)	(289.2)	(435.2)
Years 6-15 After Award x Revitalization	4815***	1467***	583.8
	(474.5)	(327.0)	(521.2)
Observations	26,500	21,000	15,500
B. Comparison Group: Projects in the Same Public Housing Authority			
Years 0-5 After Award x Revitalization	1312***	338.8	1654***
	(170.7)	(269.6)	(297.3)
Years 6-15 After Award x Revitalization	5680***	2058***	2155***
	(451.1)	(314.8)	(386.1)
Observations	102,000	67,500	38,500
C. Comparison Group: Failed Applicant Projects			
Years 0-5 After Award x Revitalization	765.5**	-410.1	-73.7
	(332.2)	(410.0)	(452.5)
Years 6-15 After Award x Revitalization	4322***	976.6**	172.9
	(799.5)	(483.7)	(569.8)
Observations	71,000	57,500	44,500

Notes: This table presents estimates of β from a variant of equation 10 in which we regress the adjusted gross income (AGI) of a project-level subgroup of individuals in the footprint of HOPE VI and matched control projects on the interaction between an indicator for the specific time period to the grant year and a HOPE VI indicator. We measure three time periods: 10 to 1 years before the award, 0 to 5 years after the award, and 6 to 15 years after the award. Column 1 measures AGI for everyone in the project footprint, Column 2 only public housing residents in the project footprint, and Column 3 only residents living in the project in the year before the award. We require incumbent residents to have an observed outcome in the reference year (three years prior to the grant year) and to be observed in the HUD data. Panel A uses as the comparison group matched control projects, Panel B projects in the same public housing authority, and Panel C failed applicant projects. Estimates control for project-by-grant year and grant year-by-year after award fixed effects, are weighted by the project-level inverse propensity score, and cluster standard errors by project. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$

TABLE A.3
Impact of HOPE VI on Children's Incomes in Adulthood

	Kid Family Income Rank Measured at Ages 29-30					
	(1)	(2)	(3)	(4)	(5)	(6)
There At Award x Revitalization	1.310***	1.418***	2.173**	0.784	0.901*	
	(0.502)	(0.492)	(0.880)	(0.476)	(0.536)	
Move In After Award x Revitalization	2.310***	2.420***	3.155***	2.273***	2.646***	
	(0.750)	(0.733)	(1.087)	(0.795)	(0.935)	
There At Award x Revitalization						0.250*
						(0.147)
There After Award x Revitalization						0.482**
						(0.202)
Control Group	Matched Controls	Matched Controls	Matched Controls	Same PHA	Failed Applicants	Matched Controls
Additional Covariates		X				
Revitalization FE			X			
No Timing Restriction						X
Observations	427,000	427,000	427,000	249,000	708,000	2,079,100

Notes: This table presents estimates of β from a parametric variant of equation 10 in which we regress income rank in adulthood (measured between the ages of 29 and 30) on the interaction between an indicator for living in the project in the specific time period relative to the grant year and a HOPE VI indicator, controlling for project-by-grant year and grant year-by-year after award fixed effects. For Columns 1-5, the sample consists of individuals who moved out from the site at least two years before the award date, children who lived in the site the year before the award, and children who moved in after the award. Column 6 removes the restriction on when the individual moved into or out of the project. Columns 1, 2, 3, and 6 use as the control group the set of matched control projects, while Columns 4 and 5 use, respectively, projects in the same public housing authority and failed HOPE VI applicants. All estimates control for pre-move-in parent income percentile, upward mobility in prior neighborhood, years in public housing, and project-by-grant year fixed effects. Column 2 adds additional individual-level observable characteristics defined upon entry into public housing. Columns 1, 2, 4, 5, and 6 control for project-by-grant year fixed effects; Column 3 instead controls for a revitalization fixed effect. All estimates weight by the individual-level inverse propensity score, control for year after award-by-grant year fixed effects, and cluster standard errors by project. Standard errors are in parentheses. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$

TABLE A.4
Impact on Childhood Exposure Effects, Additional Outcome Ages

	Kid Family Income Rank Measured at Age						
	24	25	26	27	28	29	30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Standard Movers Design							
Years Exposed x Revitalization	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
B. Movers Design with a Household Fixed Effect							
Years Exposed x Revitalization	0.125	0.246	0.347	0.493***	0.411**	0.617***	0.552**
	(0.223)	(0.200)	(0.215)	(0.178)	(0.192)	(0.218)	(0.245)
Observations	73,500	73,500	73,500	73,500	60,500	49,000	38,500

Notes: This table reports the estimates of β from equation 12 in which we regress income rank in adulthood (measured at each age 24 to 30) on the interaction between years spent in public housing and a HOPE VI indicator, controlling for site by grant year-by-year of move in fixed effect. Panel A replicates the analysis in Column 2 of Table II with different outcome ages, and all regressions similarly weight by the individual-level inverse propensity score, control for project-by-grant year-by-entry year fixed effects, and cluster standard errors by project. Panel B additionally controls for a household fixed effect, replicating the same specification as the green series in Figure IVB. Standard errors are in parentheses. $*$ $p < 0.1$; $**$ $p < 0.05$; $***$ $p < 0.01$

TABLE A.5
Impact on Childhood Exposure Effects, Additional Outcomes

	At Age 30								In 2020
	AGI	Ln(AGI)	Wages	Ln(Wages)	Employed	Married	Alive	Tract Poverty Rate	Incarcerated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Years Exposed									
x Revitalization	645.4***	0.086**	407.0**	0.090**	0.009*	0.004	0.000	-0.002	.
	(233.1)	(0.042)	(194.8)	(0.045)	(0.005)	(0.003)	(0.001)	(0.002)	(.)
Boys Only									X
Control Mean	23,360	7.801	19,990	7.200	0.745	0.104	0.979	0.267	.
Control SD	23,980	4.150	22,130	4.375	0.436	0.305	0.144	0.151	.
Observations	41,000	41,000	41,000	41,000	41,000	40,000	41,000	40,000	.

Notes: This table reports the estimates of β from variants of equation 12 in which we regress several adulthood outcomes on the interaction between years spent in public housing and a HOPE VI indicator, controlling for site by grant year-by-year of move in fixed effect. Outcomes in Columns 1-8 are measured when an individual is 30, whereas the outcome in Column 9 is measured using the 2020 Census, regardless of an individual's age. Column 1 uses as the outcome an individual's family income, while Column 2 uses the natural log of that measure plus 1. Columns 3 and 4 replicate Columns 1 and 2, but use an individual's wages instead of family income. Columns 5-7 and 9 use as outcomes several indicator variables: whether an individual is employed, alive, married, and incarcerated, respectively. Column 9 restricts the sample to only males. Column 8 uses as an outcome the poverty rate of the individual's census tract. All regressions weight by the individual-level inverse propensity score, control for project-by-grant year-by-entry year fixed effects, and cluster standard errors by project. Standard errors are in parentheses. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$

TABLE A.6
Effect of Childhood Exposure to Neighborhood Poverty

	Kid Family Income Rank Measured at Ages 29-30				
	(1)	(2)	(3)	(4)	(5)
Δ Share >$15k x Age ≤ 18	-0.004***	-0.004***	-0.003**		
	(0.000)	(0.000)	(0.001)		
Δ Share >$15k x Age >18	-0.000	0.002	0.001		
	(0.001)	(0.001)	(0.004)		
Δ Share $15k-$30k x Age				-0.004**	-0.004
				(0.002)	(0.005)
Δ Share $30k-$50k x Age				-0.002	-0.010**
				(0.001)	(0.005)
Δ Share $50k-$150k x Age				-0.003***	-0.001
				(0.001)	(0.002)
Δ Share >$150k x Age				-0.004***	-0.008**
				(0.001)	(0.003)
Geography of Measure	Block Group	Tract	Block Group	Block Group	Block Group
Low Income/Public Housing Movers Only			X		X
Observations	2,292,900	2,293,100	152,000	1,784,700	111,000

Notes: This table estimates the effect of moving to a neighborhood with 1 percentage point lower poverty rate, interacted with age at move, on a child's family income rank as measured at ages 29-30. We restrict to children who move once and to a place at least 25 miles away. Columns 1-3 separately interact the change in the share of individuals earning greater than $15,000 on an indicator for moving at or before age 18 versus after age 18. Columns 1 and 3 calculate the share at the block-group level, while Column 2 uses the tract. Columns 1-3 control for age-by-origin neighborhood share above $15,000, age-by-parental income rank, and age-by-indicator for zero parental income fixed effects. Columns 4 and 5 split the share earning above $15,000 into four mutually exclusive buckets ($15,000-$30,000, $30,000-$50,000, $50,000-$150,000, and greater than $150,000) and interact each separately with age at move. Columns 3 and 5 limits to children who move into or out of public housing and whose parent's income rank is at most 25. All columns control for the following fixed effects: age-by-parental income pre-move and age-by-origin neighborhood share above $15,000 (Columns 1-3) or share of all four income categories (Columns 4 and 5). Standard errors are in parentheses. $* p < 0.1$; $** p < 0.05$; $*** p < 0.01$

TABLE A.7
Heterogeneity in Childhood Exposure Effects by Project Characteristics

	Kid Family Income Rank Measured at Ages 29-30				
	(1)	(2)	(3)	(4)	(5)
Revitalization x Pre-Award Poverty Rate	0.088				
	(0.610)				
Revitalization x After x Pre-Award Poverty Rate	0.447				
	(1.020)				
Revitalization x Pre-Award Share Black		-0.256			
		(0.520)			
Revitalization x After x Pre-Award Share Black		-0.947			
		(0.710)			
Revitalization x Ln(Pre-Award Project Size)			0.026		
			(0.498)		
Revitalization x After x Ln(Pre-Award Project Size)			0.544		
			(0.825)		
Revitalization x Pre-Award Incarceration Rate				-0.623	
				(0.484)	
Revitalization x After x Pre-Award Incarceration Rate				-1.000	
				(0.678)	
Revitalization x Nearby Peers Adult Income Rank					0.562
					(0.465)
Revitalization x After x Nearby Peers Adult Income Rank					1.722***
					(0.589)
Observations	140,000	140,000	140,000	140,000	137,000

Notes: This table estimates the effect of revitalization and its interaction with project characteristics on a child's family income rank as measured at ages 29-30, for those who move in before versus after the award. All pre-award covariates (poverty rate, share Black, log project size, and incarceration rate in Columns 1-4, respectively) are measured three years before the award. The nearby peer adult income rank is calculated as the exposure-weighted mean income rank in adulthood (measured at ages 29-30) of all individuals with the same race, sex, and year of birth within one mile of the project. All project-level variables interacted with revitalization are standardized using the control mean and standard deviation. The reported regressions weight by the project-level inverse propensity score, control for individual-level covariates (pre-move-in parent income percentile pre-move-in, upward mobility in prior neighborhood, and years in public housing) and project-by-grant year-by-entry year fixed effects, and cluster standard errors by project. Standard errors are in parentheses. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$

TABLE A.8
Heterogeneous Impacts by Outcomes of Nearby Peers

	Kid Family Income Rank Measured at Ages 29-30									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Revitalization x Nearby Peers Adult Income Rank	0.057		0.055	0.024	0.062	0.048	0.052	0.036	0.064	0.053
	(0.045)		(0.044)	(0.064)	(0.116)	(0.040)	(0.036)	(0.046)	(0.056)	(0.036)
Revitalization x After x Nearby Peers Adult Income Rank	0.213***	0.274***		0.219***	0.274*	0.182**	0.150**	0.211**	0.243**	0.150**
	(0.077)	(0.075)		(0.083)	(0.157)	(0.076)	(0.071)	(0.107)	(0.105)	(0.069)
Revitalization x County Peers Adult Income Rank						0.055				
						(0.073)				
Revitalization x After x County Peers Adult Income Rank						0.121				
						(0.096)				
Move In After Award Only		X								
Move Out Before Award Only			X							
Nearby Peers: Project Only				X						
Nearby Peers: Project Only, Demographic-Adjusted					X					
Project FE							X	X		X
Split Sample IV								X	X	
Fully Interacted Demographic FE										X
No Individual Covariates										X
Observations	137,000	41,500	95,500	140,000	140,000	137,000	137,000	134,000	134,000	137,000

Notes: This table presents estimates of ρ from variations of equation 14, where we show the difference between the correlation of the outcome (income rank in adulthood measured at ages 29-30) of children in public housing and the outcomes of peers, for HOPE VI and matched control projects. For Columns 1-3 and 6-10, the outcomes of the nearby peer group are calculated as the exposure-weighted mean income rank in adulthood (measured at ages 29-30) of all individuals with the same race, sex, and year of birth within one mile of the project. Column 4 calculates this measure for all peers nearby the project, regardless of demographic group, and Column 5 reweights the Column 5 measure by the average demographic distribution in all projects. Column 6 additionally controls for the triple interaction of demographically-similar peers in the county, excluding those within 1 mile of the project. Columns 1 and 4-10 interact a revitalization indicator and nearby peer outcomes with a post-award indicator in a set of stacked regressions; Column 2 restricts to those who move in after the award and Column 3 to those who move out before the award. Columns 1-6 and 9 control for revitalization fixed effects, while Columns 7, 8, and 10 control for project-by-post-period fixed effects. Columns 8 and 9 calculate the peer measure using a split sample of the nearby peers. Columns 1-9 control for individual-level covariates (pre-move-in parent income percentile pre-move-in, upward mobility in prior neighborhood, and years in public housing), while Column 10 controls for cohort-by-race-by-sex fixed effects instead. Estimates are weighted by the individual-level inverse propensity score and cluster standard errors by project. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$

TABLE A.9
Heterogeneous Impacts by Outcomes of Nearby Peers, Controlling for Other Demographic Peer Groups

	Kid Family Income Rank Measured at Ages 29-30			
	(1)	(2)	(3)	(4)
Revitalization x After x Nearby Peers Adult Income Rank	0.2249*	0.3141**	0.2709***	0.2191***
	(0.1284)	(0.1355)	(0.07410)	(0.07002)
Revitalization x After x Nearby Peers (Two Years Older) Adult Income Rank	0.04832			
	(0.1310)			
Revitalization x After x Nearby Peers (Two Years Younger) Adult Income Rank	0.003478	-0.02065		
	(0.1291)	(0.1489)		
Revitalization x After x Nearby Peers (Three Years Younger) Adult Income Rank		-0.06422		
		(0.1520)		
Revitalization x After x Nearby Peers (Four Years Younger) Adult Income Rank		0.1083		
		(0.1331)		
Revitalization x After x Nearby Peers (Different Race) Adult Income Rank			0.002003	
			(0.05416)	
Revitalization x After x Nearby Peers (Different Sex) Adult Income Rank				0.09559
				(0.08784)
Observations	41,000	31,000	41,000	41,000

Notes: This table presents estimates of ρ from variations of equation 14, where we show the difference between the correlation of the outcome (income rank in adulthood measured at ages 29-30) of children in public housing and the outcomes of peers for HOPE VI and matched control projects. The nearby peer adult income rank is calculated as the exposure-weighted mean income rank in adulthood (measured at ages 29-30) of all individuals with the same race, sex, and year of birth within one mile of the project. We include only individuals who moved into the project post-award. Each column additionally controls for the outcomes of at least one other peer group, interacted with dummies for revitalization and post-award. In Column 1, we control for the outcomes of peers of the same sex and race but two years older or younger. In Column 2, we control for the outcomes of peers of the same sex and race but two, three, or four years younger. In Column 3, we control for the outcomes of peers of the same sex and cohort but a different race. In Column 4, we control for the outcomes of peers of the same race and cohort but a different sex. All estimates control for individual-level covariates (pre-move-in parent income percentile, upward mobility in prior neighborhood, and years in public housing), are weighted by the individual-level inverse propensity score, and cluster standard errors by project. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$

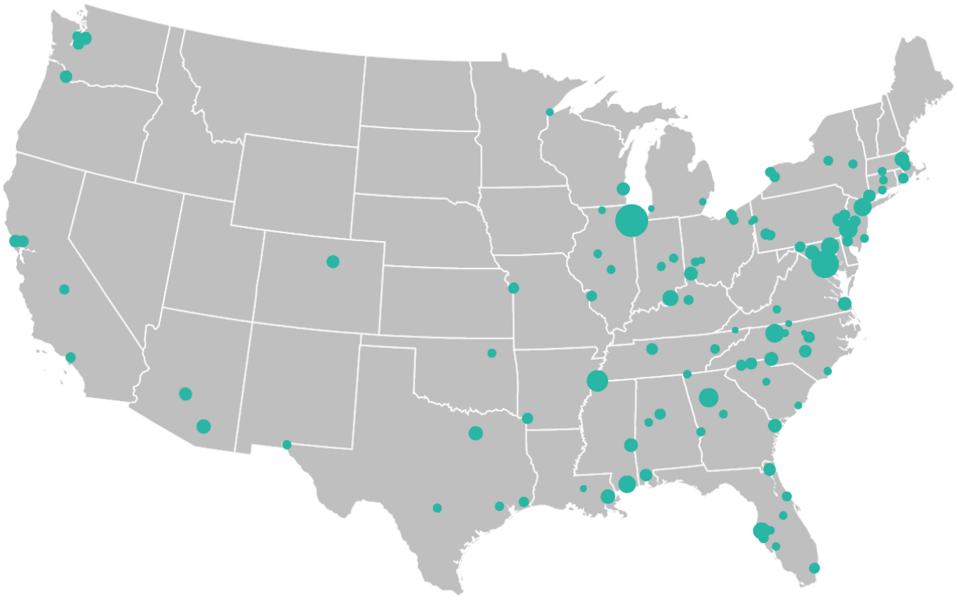
TABLE A.10
Correlation Between Own and Nearby Peers' Outcomes

	Kid Family Income Rank Measured at Ages 29-30				
	(1)	(2)	(3)	(4)	(5)
Nearby Peers Adult Income Rank	0.1792***	0.1116***	0.1146***	0.1106***	0.1058***
	(0.0531)	(0.0099)	(0.0157)	(0.0098)	(0.0030)
Sample	Revitalization Projects	Non-Revitalization, Non-Matched Control Projects	Matched Control Projects	Census Tracts Containing Matched Control Projects	Low Income Census Tracts
Mean Own Outcome	35.39	38.49	36.51	42.12	47.43
Mean Peer Outcome	40.21	44.82	41.52	44.45	48.57
Observations	4,400	81,000	28,000	28,000	345,000

Notes: This table presents estimates of ρ from a variation of equation 14, where we regress the average income rank in adulthood for individuals of a specific demographic group living in either a focal project or a focal tract on the average outcome (income rank in adulthood measured at ages 29-30) of nearby peers. The demographic groups are defined by race, sex, and cohort, and the nearby peer adult income rank is calculated as the exposure-weighted mean income rank in adulthood (measured at ages 29-30) of all individuals with the same race, sex, and year of birth nearby. In Columns 1-3, the sample consists of individuals in revitalized, non-revitalized non-matched control projects, and matched control projects, respectively. For these columns, peers within 1 mile of the project contribute to the nearby peer measure. For Column 1, we restrict to individuals who moved out of the project before the award. In Columns 4 and 5, the sample consists of individuals living in either Census tracts containing a matched control project or a sample of low-income (defined as below median average parental income) Census tracts, respectively. For these columns, peers in the nearest 10 tracts by centroid distance to the focal tract contribute to the nearby peer measure. All estimates control for project/tract and race-by-sex-by-cohort fixed effects, are weighted by the number of individuals in each demographic group-by-project cell, and cluster standard errors by project. We additionally report the means of the focal group and peer outcomes for each sample. $* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$

B Appendix Figures

FIGURE A.1
Location of HOPE VI Sites



Notes: This map shows the location of all sites that received HOPE VI funding. The size of the circles represents the total amount of funding awarded in that city.

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

A. Before Revitalization

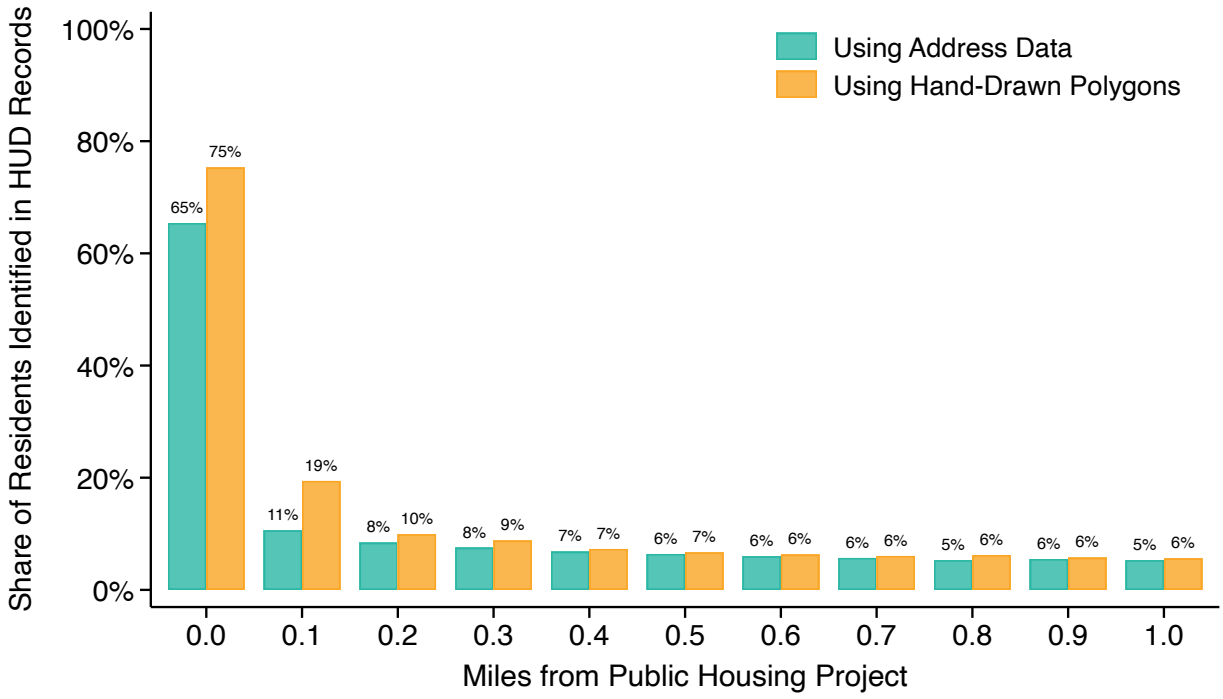


B. After Revitalization



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).

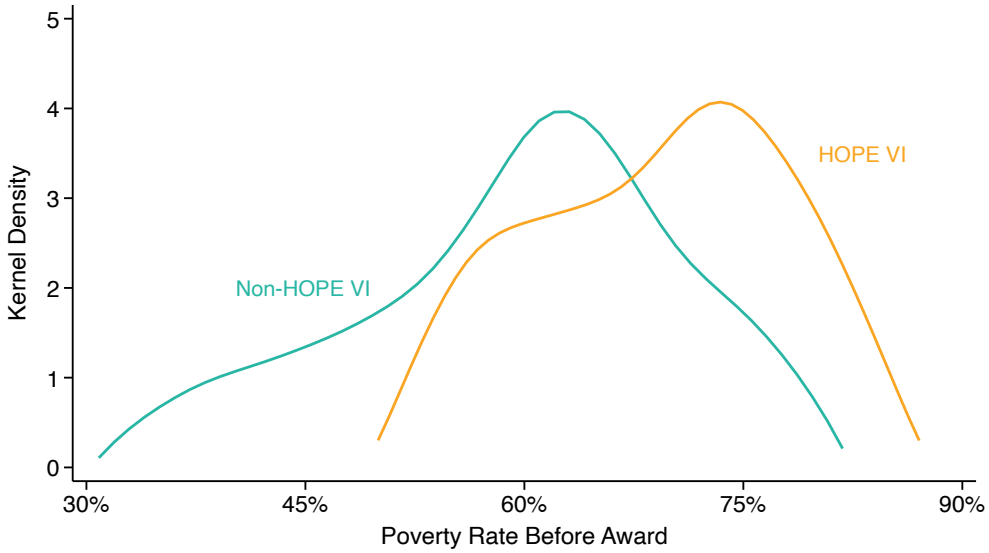
FIGURE A.3
Share of Residents in Public Housing Records in and Around the Public Housing Sites



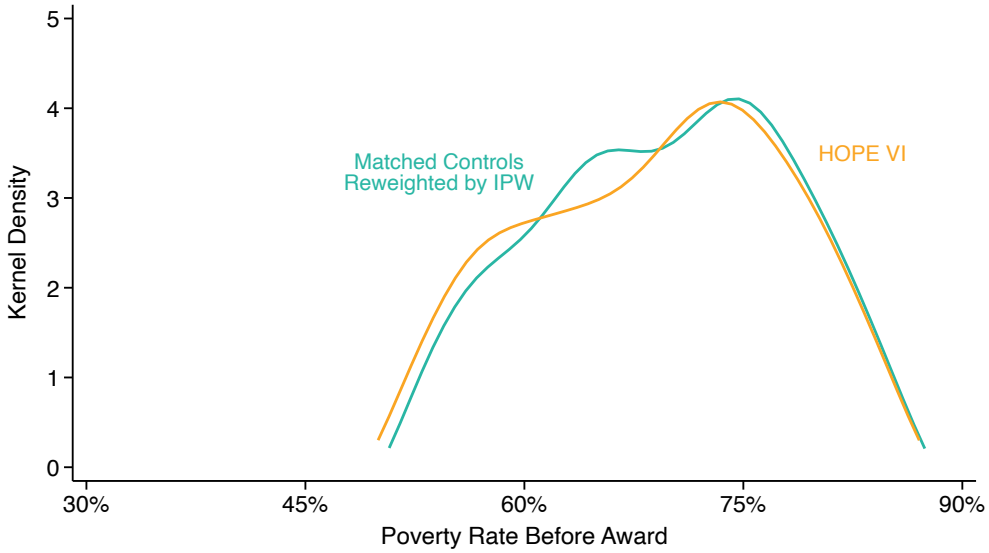
Notes: The figure shows the share of residents within each tenth-mile bucket from a public housing project that are classified as public housing residents in the HUD records. In the green bars, we calculate the footprint of the public housing projects using address data (see Appendix C.A for details). In orange, we instead calculate the footprint using hand-drawn polygons.

FIGURE A.4
Distribution of Poverty Rates in HOPE VI vs. Non-HOPE VI Sites

A. Without Matching

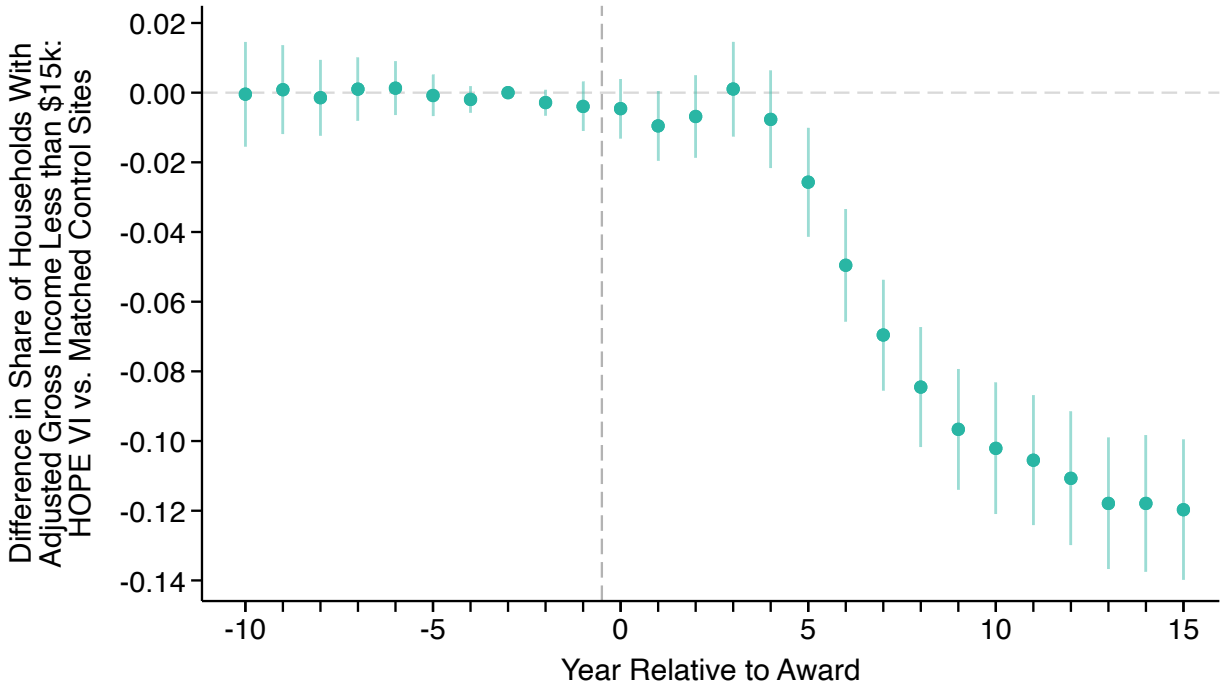


B. With Matching and Weighting by the Inverse Propensity Score



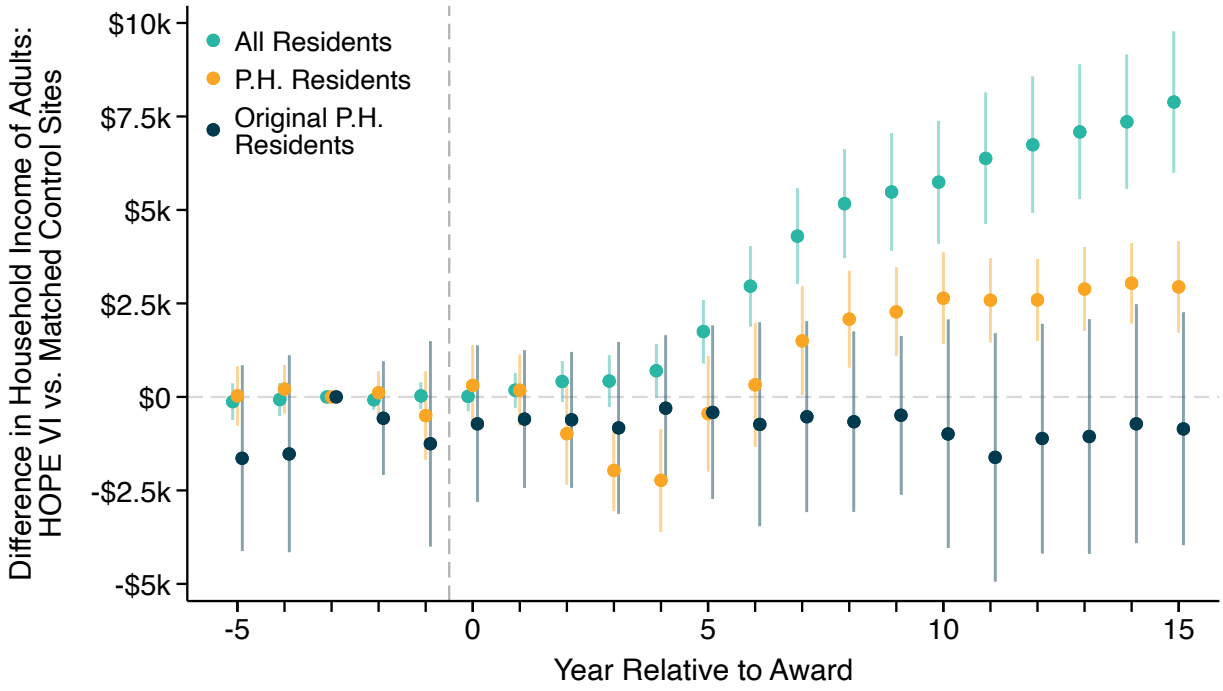
Notes: These figures show the probability density of projects by the poverty rate of their residents. Panel A shows, in green, all non-HOPE VI projects and, in orange, HOPE VI revitalization projects. Panel B shows, in green, matched control projects reweighted by the project-level inverse propensity score. The HOPE VI and matched control samples are limited to observations three years before the grant year. The non-HOPE VI sample is a panel that includes observations between 1990 and 2007.

FIGURE A.5
Impact on Poverty in Targeted Sites



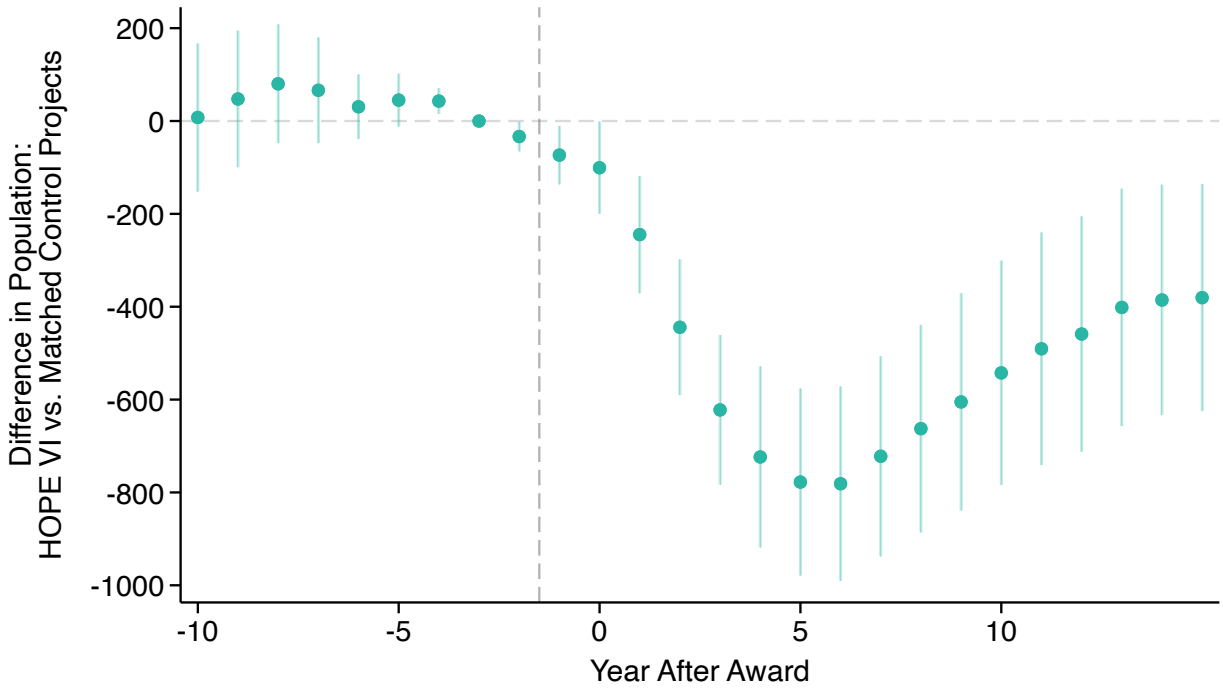
Notes: This figure presents estimates of β from a variant of equation 10 in which we regress the share of households in the project footprint with adjusted gross income less than $15,000, for HOPE VI and matched control projects, on the interaction between an indicator for the specific year relative to the grant year and a HOPE VI indicator. Estimates control for project-by-grant year and grant year-by-year after award fixed effects, are weighted by the project-level inverse propensity score, and cluster standard errors by project.

FIGURE A.6
Impact on Income in Targeted Sites, Balanced Panel



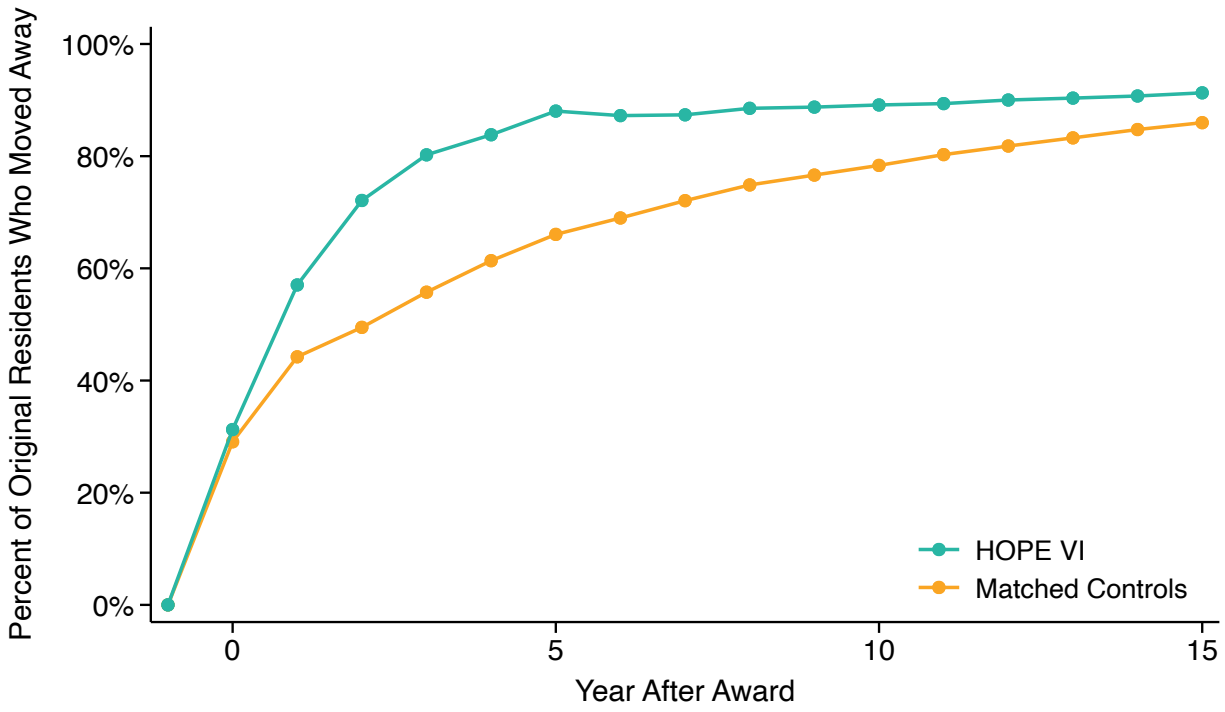
Notes: This figure replicates our main estimates from Figure II but using a balanced panel. Specifically, we exclude observations more than five years before the award and limit to projects with award years between 2000 and 2004.

FIGURE A.7
Impact on Population in Targeted Sites



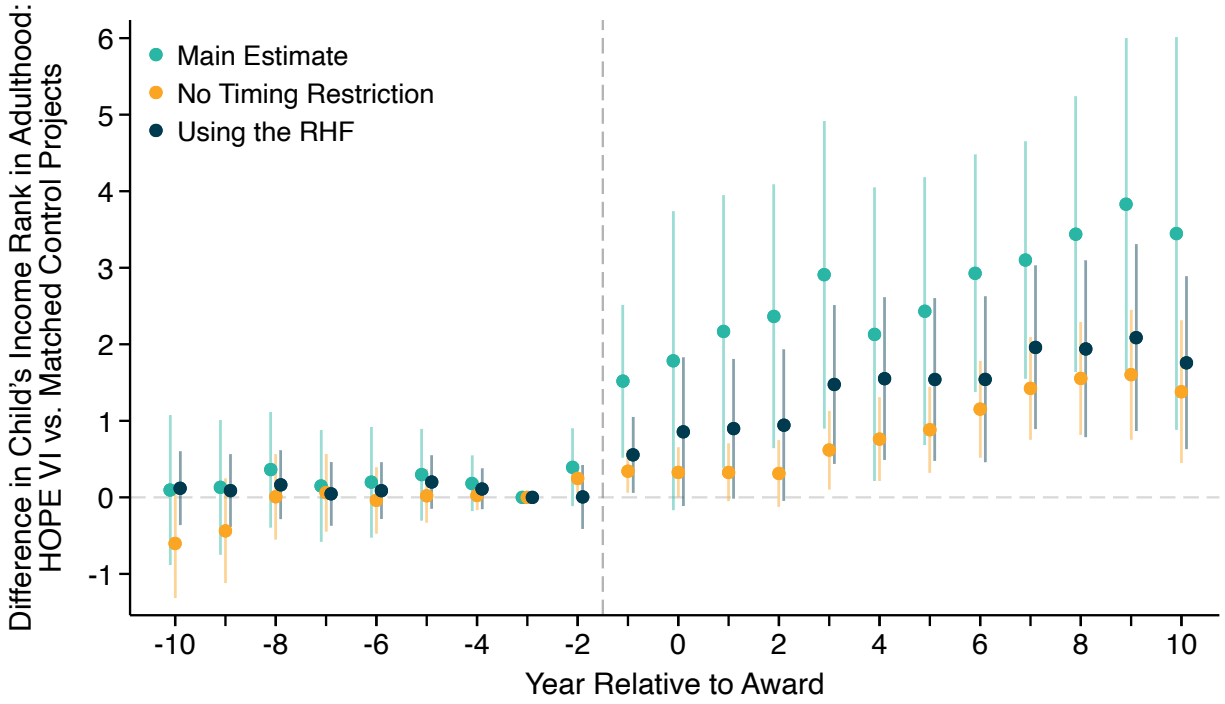
Notes: This figure presents estimates of β from a variant of equation 10 in which we regress the population of the project on the interaction between an indicator for the specific year relative to the grant year and a HOPE VI indicator, controlling project-by-grant year and grant year-by-year after award fixed effects. Estimates are weighted by the project-level inverse propensity score and cluster standard errors by project.

FIGURE A.8
Residential Mobility of Original Residents



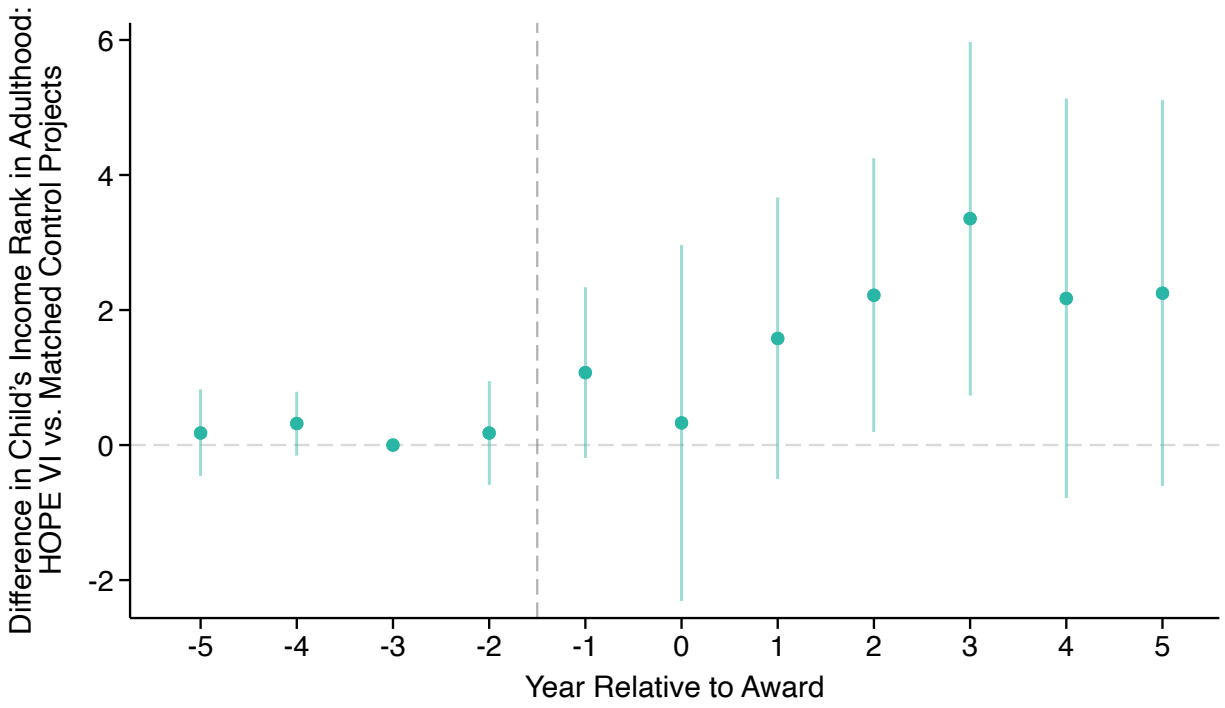
Notes: This figure shows the proportion of residents who lived in either a HOPE VI and matched control site one year prior to the award who had left the site by each year after the award.

FIGURE A.9
Impact of HOPE VI on Children's Incomes in Adulthood, Robustness to Timing and Address Measurement



Notes: The green series in replicates the estimates from Figure IIIA. The orange series presents identical estimates as the green series but does not restrict to individuals who move out before the award, are living in the project in the award year, or move in after the award. The blue series presents identical estimates as the green series but uses the residential history file, instead of the HUD records, to determine move dates.

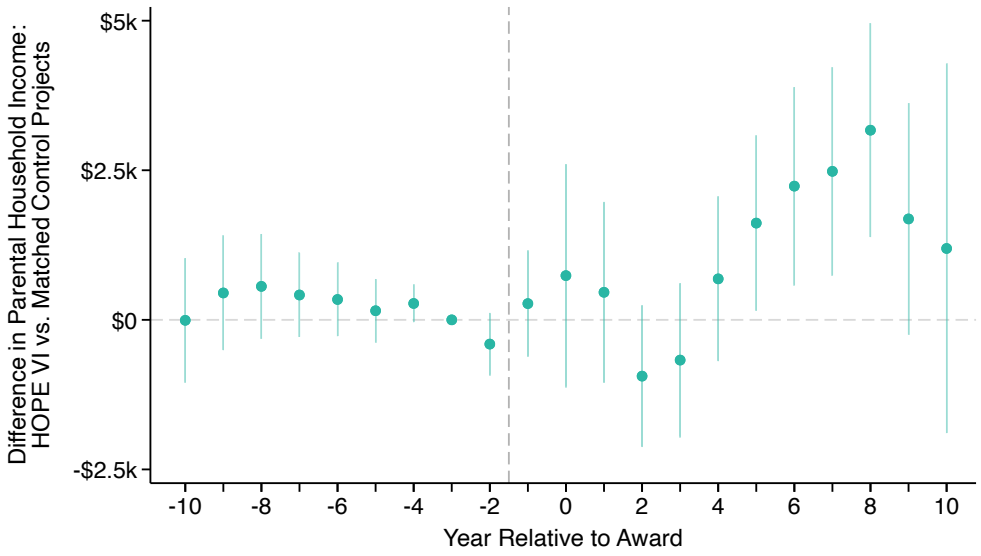
FIGURE A.10
Impact of HOPE VI on Children's Incomes in Adulthood, Balanced Panel



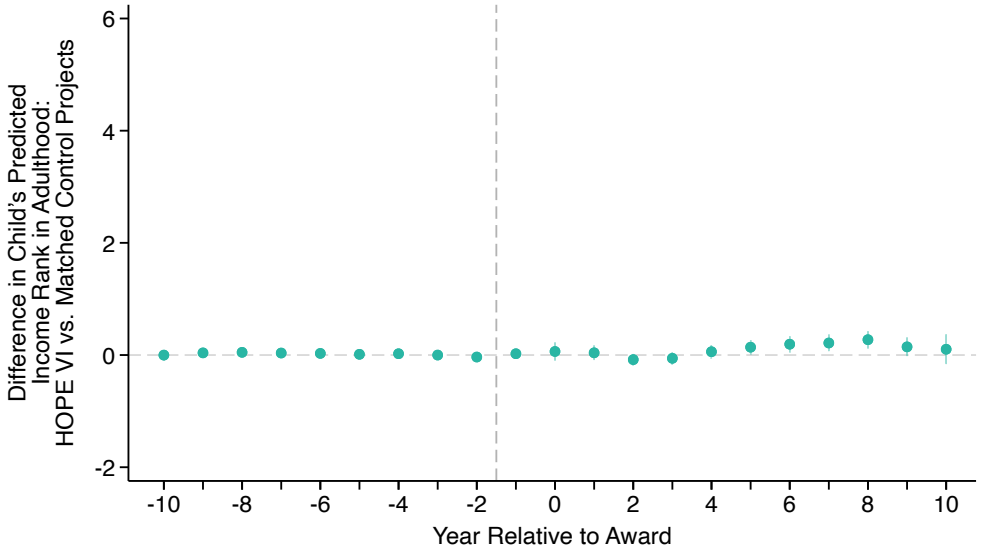
Notes: This figure replicates our main estimates from Figure IIIA but using a balanced panel. Specifically, we include observations five years before and after the award and limit to projects with award years between 2000 and 2003.

FIGURE A.11
Repeated Cross Section Estimator, Testing Selection on Parental Income

A. Outcome: Parental Income

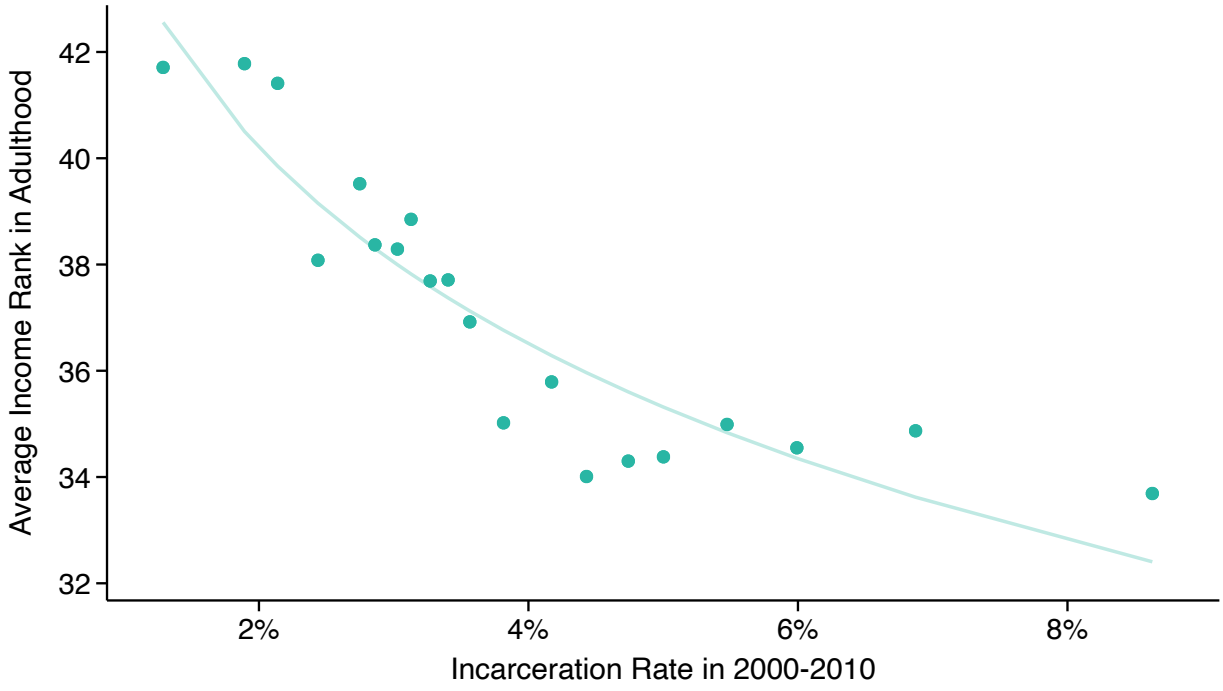


B. Outcome: Predicted Adulthood Income, Predicted Using Only Parental Income



Notes: This figure presents identical estimates as the green series in Figure IIIA but with alternative outcome variables: in Panel A, parental income, and in Panel B, predicted income in adulthood (using only parental income).

FIGURE A.12
Upward Income Mobility vs. Incarceration Rates, by Project



Notes: This figure shows the project-level distribution of upward mobility by incarceration rate for non-HOPE VI projects between 1990 and 2007, weighted by the population in the project. Upward mobility is measured as the mean household income rank at age 30 for children born in that tract with parents at the 25th percentile of the distribution. Incarceration rates are measured as the share of residents incarcerated in 2000 or 2010.

C Description of Data

C.A Measuring the Location of Public Housing Projects

Our procedure for measuring the location of public housing projects involved two steps. First, we use the address data of the public housing residents to group the HUD project identifiers into a single development or "public housing project." Second, we use the address data of the public housing residents to identify the set of census blocks in which each public housing project is located. We discuss both steps below.

The project identifiers in the HUD data sometimes refer to distinct buildings within a public housing project. They are also restructured in 2008, which makes it difficult to track projects longitudinally. To account for these issues, we use the following four-step process to cluster project identifiers that are located in the same area into a single development: 1) Use the address data for the public housing residents to measure the set of census blocks in which each project identifier is located using the methodology described in Appendix C.A. 2) Construct a dataset at the project-by-census-block level, including only census blocks that contain at least 20 percent of a given public housing project's population. 3) Merge this dataset with itself using census block identifiers, creating a Cartesian product that encompasses all combinations of projects located within the same census block. 3) Define single developments or "public housing projects" as the connected sets formed between project identifiers and census blocks.

For each project identifier in the HUD data, we identify the set of census blocks in which the project identifier is located. Our methodology involves several steps: 1) Identify all individuals who have resided in public housing and determine their census block of residence during their tenure. 2) Exclude observations from HOPE VI projects post-award. 3) For each project, compile the set of census blocks in which residents lived and drop blocks that are further than one mile from the center of the project, defined as the median latitude and longitude coordinates among all blocks associated with the project. 4) Calculate the proportion of adults in public housing for each Census block in each year, identifying the maximum value across years. 5) Define a census block as part of the public housing site if it meets two criteria: a) at least five unique individuals from that public housing project resided in the block, and b) the share of public housing residents in the block is at least 20 percent. The public housing site is the union of census blocks for all project identifiers that are grouped together in the public housing project.

To show that the methodology performs well, Figure A.3 plots the share of people living in the public housing site who are in public housing according to the HUD records, as well as this same statistic for people living in 0.1 mile rings within 1 mile of the project.

C.B Measuring Exposure to Public Housing

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, then we assume that the child lived in the project through age 18.

D Social Connection Index

Let y_{ign} be the adult income of individual i, who belongs to demographic group g, and grew up in neighborhood n. We can decompose adult income as,

$$y_{ign} = \alpha_n + \lambda_g + \delta_{gn} + \varepsilon_{ign}, \tag{D.1}$$

where α_n, λ_g, and δ_{gn} are neighborhood, demographic group, and neighborhood-by-demographic group components, respectively; ε_{ign} is an individual component; and for each neighborhood α_n is normalized such that $\mathbb{E}[\delta_{gn}|n] = 0$ and $\mathbb{E}[\varepsilon_{ign}|n] = 0$.

We measure the social connectedness between two neighborhoods, n' and n'', by the strength of the relationship between their neighborhood-by-demographic group components: i.e., the strength of the relationship between $\delta_{gn'}$ and $\delta_{gn''}$. This proposed measure relies on the intuition that 1) children are likely to interact more with other children of the same demographic group, and 2) children who interact are likely to have similar outcomes due to peer effects.

As a step towards understanding what the measure captures, consider collapsing the data to the neighborhood-by-demographic group cell, $\bar{y}_{gn} = \alpha_n + \lambda_g + \delta_{gn}$, and calculating the covariance between the average income for demographic group g in neighborhoods n' and n'', $cov(\bar{y}_{gn'}, \bar{y}_{gn''})$. We can write out that covariance term as,

$$cov(\bar{y}_{gn'}, \bar{y}_{gn''}) = var(\lambda_g) + cov(\lambda_g, \delta_{gn''}) + cov(\delta_{gn'}, \lambda_g) + cov(\delta_{gn'}, \delta_{gn''}) \tag{D.2}$$

The first three terms are a function of the demographic components and do not capture the specific relationship between neighborhoods n' and n''.[19] The fourth term captures the relationship between the neighborhood-by-demographic group components and is the object of interest. Equation D.2 highlights the importance of controlling for differences across demographic groups when constructing the measure.

Our proposed measure of social connectedness between neighborhoods n' and n'' is simply the correlation between $\delta_{gn'}$ and $\delta_{gn''}$,

$$\rho(\delta_{gn'}, \delta_{gn''}) = \frac{cov(\delta_{gn'}, \delta_{gn''})}{\sigma_{\delta_{gn'}} \sigma_{\delta_{gn''}}}. \tag{D.3}$$

In practice, we can estimate δ_{gn} by residualizing y_{ign} on α_n and λ_g in the national data and then collapse to cells defined by n and g.[20] Importantly, note that the difference in average income between neighborhoods n' and n'',

$$\bar{y}_{n'} - \bar{y}_{n''} = (\alpha_{n'} + \mathbb{E}[\lambda_g|n']) - (\alpha_{n''} + \mathbb{E}[\lambda_g|n']) \tag{D.4}$$

is unrelated to $\rho(\delta_{gn'}, \delta_{gn''})$. As mentioned above, we normalize α_n such that $\mathbb{E}[\delta_{gn}|n] = 0$ and $\mathbb{E}[\varepsilon_{ign}|n] = 0$.

Equations D.3 and D.4 illustrate that the social connectedness index, $\rho(\delta_{gn'}, \delta_{gn''})$, is unrelated to differences in average outcomes, $\bar{y}_{n'} - \bar{y}_{n''}$. Thus, $\rho(\delta_{gn'}, \delta_{gn''})$ does not capture the extent to which the outcomes of individuals in one neighborhood converge to another neighborhood (in a level sense), as there might be neighborhood-specific factors that lead to different average outcomes across neighborhoods. Rather, our measure captures the degree to which idiosyncratic differences across demographic groups within one neighborhood are related to these differences in another neighborhood. As an implication of this, the relationship in Figure 9 (the concluding slide of the NBER deck) is not driven by a mechanical relationship.

Social interactions and peer effects are not the only mechanisms that could produce a positive value of $\rho(\delta_{gn'}, \delta_{gn''})$. For example, White and Black workers might have access to different labor markets due to discrimination. Thus, the outcomes of the White children in one neighborhood may be related to the

[19] Note that the term $cov(\lambda_g, \delta_{gn'})$ would be identical for any neighborhood n'.

[20] Given the findings of the race trends paper, when controlling for λ_g, we may actually want to control for the interaction between country, birth cohort, sex, and race.

outcomes of White children in another neighborhood because they are both exposed to the same labor market shocks, not because they interact during childhood. We can use the sharp cohort variation as a way of assessing this possibility. In the paper, Figure 8(B) shows that the correlation is specific to children in one's own cohort, and we find no increase in the correlation between outcomes of children in the cohorts one year above and one year below. This sharp variation across cohorts is consistent with peer effects/social interactions and is inconsistent with other explanations that could produce a positive value of $\rho(\delta_{gn'}, \delta_{gn''})$.

Figure 8 presents estimates from a regression specification that can be tightly linked to the above framework. In particular we estimate the following regression,

$$y_{igmt} = \alpha_n + \lambda_g + \beta \bar{y}_{gs(n)t} + u_{igmt}, \tag{D.5}$$

where individual i belongs to demographic group g (defined by the interaction between race and ethnicity, sex, and year of birth) and lives in project n in time period t. And where $s(n)$ denotes the neighborhood surrounding neighborhood n. We estimate this regression on a sample of children who move into revitalized HOPE VI public housing projects after the award and $\bar{y}_{gs(n)}$ denotes the average adult income of children of the same demographic group who lived in the surrounding neighborhoods in the years that i was in public housing. Because the regression includes both project fixed effects (α_n) and demographic controls (λ_g), β will identify a weighted average of the project-level measures of $cov(\delta_{gn}, \delta_{gs(n)})/var(\delta_{gs(n)})$.