# The Children of HOPE VI Demolitions: National Evidence on Labor Market Outcomes

October 2022

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## Abstract

We combine national administrative data on earnings and participation in subsidized housing to investigate how the demolition of 160 public housing projects—funded by the HOPE VI Demolition program—affected adult labor market outcomes for 18,500 children. Our empirical strategy compares children exposed to the program between ages 10 and 18 to children drawn from thousands of non-demolished projects, adjusting for observable differences using a flexible estimator that combines features of matching and regression. We find that children who resided in HOPE VI projects earn 15 percent more at age 26 relative to children in comparison projects. Earnings gains are strongest for demolitions in large cities and in the neighborhoods with the highest poverty rates. We investigate pathways including improved parental earnings, childhood exposure to lower poverty neighborhoods, and greater job accessibility. We find the strongest evidence for improved job accessibility facilitating increased employment and earnings for young adults.

NOTE: Any opinions expressed herein are those of the authors and do not represent the views of the U.S. Census Bureau, the U.S. Consumer Financial Protection Bureau, the U.S. Federal Deposit Insurance Corporation, or the United States. All results have been reviewed to ensure that no confidential information is disclosed (q.v., U.S. Census Bureau Disclosure Review Board numbers: DRB-B0038-CED-20190405, DRB-B0071-CED-20190829, and CBDRB-FY20-CED0060029). Much of the work for this analysis was done while Mark Kutzbach was an employee of the Census Bureau, and while Giordano Palloni was an employee of the Census Bureau and the International Food Policy Research Institute. John Haltiwanger was a Schedule A (part-time) employee and Matthew Staiger a Pathways Intern of the U.S. Census Bureau at the time of the writing of this paper. This research has been supported by grant number 98082 from the "How Housing Matters" research program of the John D. and Catherine T. MacArthur Foundation, by NSF grant number 1730108, by a Research Partnership grant from the U.S. Department of Housing and Urban Development, and by a grant from the Russell Sage Foundation. This research uses data from the Census Bureau's Longitudinal Employer-Household Dynamics Program, which was partially supported by the following National Science Foundation Grants: SES-9978093, SES-0339191, and ITR-0427889; a National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation. The authors want to thank Mark Heller for valuable research assistance and Lydia Taghavi of HUD for data assistance. We thank James Spletzer, Erika McEntarfer, Katherine O'Regan, and Amy Ellen Schwarz as well as participants at the Urban Economics Association, American Real Estate and Urban Economics Association, Association for Public Policy and Management, and Southern Economics Association meetings for comments.

# 1. Introduction

The concern that time spent in subsidized housing, especially large public housing projects in high-poverty neighborhoods, could negatively affect children has been the focus of a substantial literature.[1] Based partly on this rationale, the last 40 years of federal assisted housing policy has sought to deconcentrate subsidized housing participants, mainly through the provision of Housing Choice Vouchers (hereafter, vouchers) that subsidize low-income families to live in market-supplied housing. A significant effort to spur the dispersion of these households has focused on the demolition of public housing projects paired with support for existing residents to find alternative housing, most notably under the U.S. Department of Housing and Urban Development's (HUD's) HOPE VI program. Despite the growing availability of vouchers and the continued funding of programs intended to reduce the population living in low-quality public housing projects, there is no nationally representative evidence about how these demolitions impacted short- or long-term outcomes for exposed children and adults.

This paper explores how the HOPE VI Demolition program affected the adult labor market outcomes of children who resided in affected projects. Using linked administrative data on earnings and participation in subsidized housing, we identify approximately 18,500 children exposed to 160 HOPE VI demolitions in diverse environments across the United States.[2] Even though the HOPE VI program systematically targeted "severely distressed" public housing projects, there were many similarly distressed projects in equally disadvantaged neighborhoods that were not demolished through the HOPE VI program. Our empirical strategy compares children living in projects that received a HOPE VI Demolition grant to children living in observably similar projects that were not affected by the HOPE VI program. We leverage the richness and size of the data by using a stratification with regression estimator, which combines features of both matching and regression to account for observable differences between projects that were and were not affected by the program (Imbens and Rubin, 2015).

Our main finding is that exposure to the HOPE VI Demolition program between the ages of 10 and 18 produced substantial long-run labor market benefits, increasing age 26 earnings by

[1] For example, Oreopoulos (2003), Jacob (2004), Chetty et al. (2016), Pollakowski et al. (2022), and Chyn (2018).
[2] In some cases, not all of the housing units were demolished, and our treated sample includes children living in the non-demolished units. Because the program could have affected these individuals by altering the neighborhood in which their HOPE VI project was located, we consider these children to be treated. We provide more discussion of this issue below.

15.3 percent relative to comparable children from non-HOPE VI projects. The positive impacts are driven by children from projects in large metropolitan areas. We estimate that HOPE VI increased earnings by 21.2 percent in large (greater than 2,500 units) Public Housing Authorities (PHAs), compared to a (not statistically significant) 4.6 percent increase in smaller PHAs.

In relation to previous work, the main contribution of our paper is to obtain estimates of the long-term impacts of a large, assisted housing program that are more representative of the full national population of affected projects. Much of the relevant prior empirical research relies on data from a limited set of large metropolitan areas. Figure A.1 plots the distribution of the size of PHAs that participated in three important studies including the HUD Moving to Opportunity (MTO) experiment (Ludwig et al., 2013), the Gautreaux program (Rosenbaum, 1995), and the Effects of Housing Choice Voucher on Welfare Families project (Mills et al., 2006).[3] Approximately half of all public housing units are located in small PHAs, but only two of the ten PHAs in this previous research are located in these smaller locations. In contrast, over two-thirds of the PHAs that received HOPE VI funding are in small PHAs. Thus, our results are likely to be more representative of the effects for the broader population in distressed public housing. Chicago is the third largest PHA and is highlighted in Figure A.1 as it is the setting for Chyn (2018), the closest existing paper to our work. Chyn (2018) studies the long-term earnings impacts of public housing project demolitions and finds that demolitions increased long-term earnings by 16 percent for resident children. We provide additional evidence on the long-term benefits of the demolitions of distressed public housing projects in contexts beyond Chicago.

Several results support a causal interpretation of our estimates. Our most convincing evidence comes from a series of analyses in which we study projects that are similar to the HOPE VI awardees but did not themselves received an award. Specifically, we use two alternative sets of projects: (1) projects that applied for but never received a HOPE VI grant, and (2) observably similar projects located within the same PHA as HOPE VI recipients. We use the same matching estimator to compare children in these two sets of projects to children in other observably similar non-HOPE VI projects and find no difference in long-run earnings for these groups. As none of

[3] An exception is a companion paper—Pollakowski et al. (2022)—which uses a household fixed-effects identification strategy and finds long-term benefits of time spent in public and voucher housing between the ages of 13 and 18. Pollakowski et al. (2022) use data from nearly the universe of assisted housing participants so that the results capture the *typical* effect of participating in the public housing or voucher program. In contrast, the current paper focuses on a population that is more disadvantaged relative to the subsidized housing population as a whole.

these projects were treated, we interpret this as evidence that the results are not driven by differential trends for projects interested in HOPE VI funding or by differential trends in the distressed neighborhoods of metro areas where a project received HOPE VI funding. Furthermore, we find earnings gains similar to our matching estimator-derived main estimates when we compare children in the HOPE VI projects to children in projects from these two other groups. Taken together, these results suggest that our matching estimator selects a valid counterfactual and illustrate that our main findings are robust to using other reasonable comparison groups. We also present evidence that the matching estimator successfully balances treatment and control groups across a rich set of observable baseline covariates.

We next investigate the short- and medium-term impacts of the program. The demolitions led to large changes in housing circumstances, forcing a substantial number of HOPE VI households out of their initial projects and into other public housing projects, the voucher program, or private market housing. While households exited subsidized housing at a higher rate in the year after the demolition, there is no evidence that the program displaced households from subsidized housing entirely in later years.[4] We also find no evidence that HOPE VI changed labor market outcomes for parents 5 or 10 years after the demolitions. Both HOPE VI and non-HOPE VI households moved to neighborhoods with lower poverty rates but, relative to non-HOPE VI households, HOPE VI households resided in neighborhoods with poverty rates only modestly lower 1-5 years after an award. Thus, the effect of HOPE VI on neighborhood poverty is not statistically distinguishable from zero and is small relative to previous work (e.g., Chetty et al. 2016; Chyn 2018), though there is some evidence that certain sub-groups may experience larger poverty reductions. We also find no evidence of larger long-term impacts for children who were younger at the time of a demolition. Together, these findings suggest that for our national sample, reductions in childhood exposure to neighborhood poverty are not likely to represent the *primary* mechanism for our long-run effects. That said, our results are not inconsistent with the neighborhood exposure model or past empirical work that identifies reductions in childhood poverty as being key determinants of long-term economic success (e.g., Chetty et al. 2016; Chyn 2018). This is because for the average household in our sample—who was more likely to be in small a PHA and less likely to have young children—the HOPE VI demolitions did not induce

[4] While it is common for residents of HOPE VI projects to exit subsidize housing, after five years they are no more likely to exit relative to residents of other similarly distressed projects that were not part of the HOPE VI program.

reductions in neighborhood poverty that were as large or that occurred as early as those driving the long-run effects in existing work.

We find strong evidence that HOPE VI affected the local labor market characteristics of the neighborhoods where children resided as adults. Specifically, HOPE VI led to a significant improvement in measures of job accessibility—average commute time, jobs per person, and a job proximity index constructed by HUD—in the neighborhoods that the children were living in 2010, 7-13 years after the demolitions. Improved job accessibility can reduce job search duration (Andersson et al., 2018) and also encourage individuals on the margin between working and not working to participate in the labor market (Smith and Zenou, 2003). Consistent with job accessibility being an important mechanism, we find that a large part of the observed earnings gains is driven by an extensive margin labor supply response, most notably for workers with short commute times.[5] Improvements in job accessibility are attributable both to HOPE VI forcing some residents to move to new neighborhoods with better job accessibility and to changes in job accessibility in the original neighborhood for residents who did not move (primarily by reducing population density). The latter finding provides empirical support for the idea that the children who lived in non-demolished housing units within HOPE VI projects were also affected by the program.

Our results shed light on an open puzzle in the existing literature: Does inducing households to move to new neighborhoods have to occur while children are still young to have long-run benefits? Chyn (2018) and the results in our paper suggest that demolitions do produce long-run benefits for older children (older than 13 at the time of the demolition). Conversely, in their analysis of the MTO experiment, Chetty et al. (2016) find no evidence of long-run gains for older children who transitioned from public to voucher housing.[6] One explanation for this discrepancy suggested by Chyn (2018) is that the projects in his study were in much more disadvantaged neighborhoods relative to those in MTO. If older children only benefit when the

[5] Our findings are consistent with job accessibility at the time of market entry being uniquely important for employment and earnings, or with job accessibility being important for all workers with a low opportunity cost of supplying labor, regardless of age/experience. There is an existing literature that has studied the persistent effects of labor market conditions at the time of market entry (Schwandt and von Wachter, 2019; Arellano-Bover, 2020).

[6] The MTO study randomly assigned 4,600 households living in public housing projects to a control group, a "Section 8" group which was offered standard vouchers, or an experimental group which was offered vouchers that could only be used in census tracts with a 1990 poverty rate below 10 percent. The primary comparison made by Chetty et al. (2016) is between this experimental group and the control group. Their results thus rely on moves to lower poverty neighborhoods, a case in which it makes sense that younger children should benefit more. Survey and administrative data have provided means of evaluating the impact of the two treatments (Ludwig et al., 2013).

origin neighborhood is especially distressed, that could reconcile the findings from MTO, Chyn (2018), and this paper. We exploit the variation in pre-demolition neighborhood characteristics and find that HOPE VI had the largest impact on age 26 earnings for projects located in neighborhoods that had higher poverty rates, were more densely populated, and had lower measures of job accessibility. An explanation consistent with our results is that large distressed public housing projects create an environment in which there are many people competing for nearby jobs and limited transportation options to access jobs located farther away, and this creates barriers to employment that are especially germane for individuals on the margin between working and not working. The children located in these neighborhoods—even if they were exposed to the program only later in adolescence—still benefited from the HOPE VI intervention.

The paper proceeds as follows. Section 2 provides background on the HOPE VI program and related research and discusses the potential mechanisms through which public housing demolitions could affect the long-term well-being of children in affected households. Section 3 describes the data sources and sample construction. Section 4 highlights challenges for the identification of unbiased treatment effects and discusses the stratification with regression estimator. Section 5 presents the empirical results. Section 6 concludes.

## 2. Background and Anticipated Impacts of the Program

HUD launched the HOPE VI initiative in response to the report by the National Commission on Severely Distressed Public Housing (NCSDPH), which, in 1992, found that 86,000 of the 1.4 million public housing units nationwide qualified as "severely distressed" (NCSDPH, 1992; U.S. HUD, 2007).[7] HOPE VI consisted of two main programs designed to address this issue: (1) the *Demolition* program, which provided funding for the demolition of public housing projects and the relocation of affected residents, and (2) the *Revitalization* program, which provided funding to redevelop neighborhoods with public housing into low-density, mixed-income communities. The focus of our paper is strictly on the Demolition program and unless otherwise noted, any mention of HOPE VI refers solely to this program.[8] Between 1996 and 2003, HUD awarded $392 million through 285 HOPE VI grants for the demolition of more than 57,000

[7] The HOPE VI program, originally known as the Urban Revitalization Demonstration, was the sixth of the Housing Opportunities for People Everywhere grants, funded by P.L. 102-389 (U.S. HUD, 2007).

[8] There is some overlap between the Revitalization and Demolition programs so that some recipients of a Demolition grant later received a Revitalization grant. However, the Revitalization intervention typically began years after the demolition occurred. As we discuss in Appendix C, we find no evidence that our estimated impact of the Demolition program is affected by the Revitalization program.

public housing units. Research tracking the former residents of a limited set of demolished public housing projects finds that about half of displaced households moved to a new public housing project, a third were provided with a voucher, and the remainder exited subsidized housing altogether (Kingsley et al., 2003; Popkin et al., 2009).

HOPE VI Demolition grants were awarded based on a competitive process in which HUD posted a notice of funding availability, PHAs submitted applications, and HUD selected a limited set of awardees (Murphy, 2012). Any PHA was eligible to apply for the demolition of severely distressed public housing developments (using the NCSDPH criteria). However, at least in the earliest year, HUD explicitly differentiated between PHAs of various sizes in their call for funding (2,500 units or less, between 2,501 and 10,000 units, and over 10,000 units); applicants were evaluated within these groups and group size determined the amount of funding for which PHAs were eligible. Our analysis often differentiates between large (more than 2,500 units) and small (2,500 or fewer units) PHAs based on these cutoffs.[9] Each year, HUD classified applicants into one of four priority groups, and grants were awarded (conditional on eligibility and approval) on a first-come, first-served basis by priority group until funds were exhausted.[10] Given limited funding, both the number of applicants and eligible projects exceeded the number of awards.[11] Furthermore, many eligible projects never applied for funding while some non-distressed projects received funding, leaving many distressed-projects unaffected by HOPE VI. Indeed, Turner et al. (2007) estimate that there were between 47,000 to 82,000 severely distressed units that remained in public housing inventory as of 2007 (four years after the last demolition grant award). We return to these points later in our discussion of the empirical strategy.

It is not obvious how we should expect HOPE VI to affect the long-term labor market outcomes of displaced children. A primary goal of the program was to move families out of environments characterized by a "high incidence of crime," physical deterioration "that renders

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[9] We do not further differentiate the large PHA sample because there are too few HOPE VI projects in PHAs that exceed 10,000 units in our sample to analyze separately.

[10] Different sources give slightly different accounts of the award process. A Congressional Research Service report (McCarty, 2005) describes the first-come, first-served process and notes that the "priority groups are, in order of priority, (1) approved for a 202 conversion, (2) applied for a 202 conversion, (3) approved for a Section 18 demolition, or (4) approved for a HOPE VI revitalization grant. Section 202 Mandatory Conversion is the conversion of public housing developments to Section 8. If it costs less to give the residents a Section 8 voucher, rather than maintain the low rent public housing building, the building is shut down and the residents are given Section 8 vouchers."

[11] On average only 53 percent of applicants were funded each year. The percentage is based on the authors' calculation using publicly available data (U.S. HUD, 2007) and the statistic excludes data from 1996, for which we do not know the number of applicants.

the housing dangerous to the health and safety of its residents," and "limited opportunities for meaningful employment of residents."[12] Based on these stated objectives, demolitions could have shaped the development of children by improving the home and neighborhood environments they were exposed to while young. This would be consistent with recent empirical evidence suggesting that neighborhood conditions in childhood can affect the development of human capital, which in turn affect long-term labor market outcomes (Chetty et al., 2014 and 2016; Chetty and Hendren, 2018). Alternatively, the program could have affected adult labor market outcomes by changing access to jobs in the neighborhoods where children end up living as young adults. Theory highlighting the potential importance of job accessibility dates back to Kain (1968) and argues that the geographic location of jobs and job seekers can have important implications for labor market outcomes; recent empirical evidence in Andersson et al. (2018) supports this hypothesis. The program also could have had an adverse effect if treated households moved to even more distressed neighborhoods than where their HOPE VI projects were located, or if the financial and non-financial costs of exposure to a demolition (disruption costs) were large enough to outweigh any benefits from moving to a better neighborhood.

The existing empirical research on HOPE VI is largely descriptive but it suggests that the program had limited success in achieving its short-term goals. Popkin et al. (2004; 2009) find that households affected by HOPE VI experienced large changes in housing and most households moved to neighborhoods with lower poverty rates and less crime, and reported being more satisfied with their new neighborhoods, particularly if they received vouchers. However, most research finds little evidence that HOPE VI affected the short-term labor market outcomes of adults (Goetz, 2010; Jones and Paulsen, 2011; Popkin et al., 2009) or the health, educational, or behavioral outcomes of the children (Gallagher and Bajaj, 2007). A limitation of this prior research is that it primarily documents how outcomes changed over time for households exposed to the program. This focus on movers, without an appropriate counterfactual comparison group, is particularly problematic in the HOPE VI setting because, even in absence of demolitions, households in public housing exhibit a high degree of residential mobility (McClure, 2018).

Jacob (2004) and Chyn (2018) are exceptions to this descriptive work, obtaining credible causal estimates of the demolition of public housing projects by comparing outcomes for children who resided in buildings that were demolished to children who resided in buildings that were not

[12] Quotes are from NCSDPH (1992).

demolished but were located within the same project. Jacob (2004) finds no evidence of short-term gains in educational outcomes. In his research on the long-term outcomes of demolitions for children, Chyn (2018) uses a similar empirical strategy and finds positive impacts on adult labor market outcomes. However, the results from Jacob and Chyn are limited to public housing in Chicago and therefore may not be representative of the HOPE VI program more broadly. A contribution of our paper is to obtain nationally representative estimates of the impact of the HOPE VI program by studying 160 demolitions that occurred in diverse environments across the United States. In contrast to Jacob (2004) and Chyn (2018), we observe a great deal of variation in project and neighborhood characteristics within our empirical sample. This enables us to empirically assess how the impact of the HOPE VI program differed across projects located in heterogeneous pre-program contexts.

## 3. Description of the Data

We use administrative data to identify children and parents affected by public housing project demolitions, track exposed and non-exposed residents as they move across subsidized housing programs and neighborhoods, and match the children's housing and residential experiences to their labor market outcomes as adults.

We rely on five sources of data. First, we identify who is in subsidized housing using the Public and Indian Housing Information Center (HUD-PIC) administrative records. These data record every individual participating in public or voucher housing in each year between 1997 and 2010. Second, we measure earnings using the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) Infrastructure Files. These data are based on the unemployment insurance (UI) wage records and cover more than 95 percent of wage and salary civilian jobs, including both private sector and state and local government workers. Third, we measure and residential location using the 2010 Decennial Census and the Composite Person Record (CPR). The CPR identifies residence Census tract for each child and adult from 1999-2010 where available (approximately 10 percent of children are missing a CPR residence in each year), and the 2010 Decennial Census provides address data in 2010 for those not in the CPR. Fourth, we link a subset of the sample to the American Community Survey (ACS) to measure additional outcomes such as education and commute times. Fifth, we measure neighborhood characteristics using several publicly available data sources. Appendix B provides a detailed description of each data source and defines all variables used in the analysis.

Our sample construction begins by using the HUD-PIC records to identify children between the ages of 10 and 18 who lived in public housing between 1997 and 2001. The age range is chosen to allow us to observe earnings at age 26 for all children in the sample.[13] We choose to focus on age 26 earnings in our main results since most children will have completed their education by this date and work by Chetty et al. (2014) finds that outcomes measured at this age are strongly predictive of later-life measures of labor market success. We then attach data from the LEHD and 2010 Decennial Census to each record from the HUD-PIC data.[14] An analogous dataset is constructed using the householder, or reference person, of the children in the sample.

We construct a dataset of public housing projects that describes characteristics of the residents and the neighborhoods in which they are located. To identify the set of projects that received a HOPE VI demolition grant, we start from publicly available data that lists all 285 HOPE VI demolition grant awards.[15] We make several sample restrictions to the full list of projects to exclude those that are not well-suited for our study design (e.g., we exclude senior housing). These sample restrictions, described in Table A.1, reduce the analysis sample to about 160 projects that received HOPE VI demolitions awards.[16] Our sample also includes about 8,800 non-HOPE VI projects.[17]

To identify treated individuals, we need to determine who was living in the project just prior to a demolition award. However, identifying the timing of the demolition is complicated by the fact that the PHA may have started to move households out of a project prior to the physical demolition of the building. To address this possibility, we define the treated group as individuals

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[13] There is one cohort of children, 10-year-olds who appear in public housing in 2001, for whom we do not observe age 26 earnings because our earnings data are only available through 2016. For this cohort, we use observed earnings up through age 25 to impute their earnings at age 26.

[14] Individuals are identified by a "Protected Identification Key" (PIK) generated by the Census Bureau to personally identified information, allowing us to attach LEHD data to other data sources. PIKs are linked to approximately 98 percent of person records in the HUD-PIC member file for our study period and we drop the 2 percent of individuals that are not assigned a unique PIK.

[15] For the HOPE VI demolition grant list, see: HOPE VI DEMOLITION GRANTS: FY 1996 - 2003 (available at https://www.hud.gov/sites/documents/DOC_9890.PDF, dated October 2004).

[16] Throughout the paper we often report rounded numbers to limit risk of disclosure.

[17] Based on the restrictions defined in Table A.1, we apply the following sample restrictions to the non-HOPE VI projects: criteria 1, 5, 6, and 7. Data from HUD User indicate that in 1997 there were about 13,400 projects in the U.S. (excluding territories) and about 10,100 projects that were within our size range (between 15 and 3,000 occupied units) that were not senior citizen housing. Thus, even though we lack data on some PHAs that participated in the Moving To Work demonstration, our sample appears to cover most of the comparable public housing projects. We also drop projects that received a HOPE VI Revitalization grant but did not receive a HOPE VI Demolition grant, as households in these projects were treated by a different, but closely related, program.

who lived in the HOPE VI project two years prior to the award date.[18] We view this definition of timing as conservative as it minimizes the chance that our estimated treatment effects are contaminated by selection out of the project prior to the demolition while potentially underestimating the effect if the demolition does not occur until a later time. To contextualize this definition, Figure 1 presents changes in project size relative to the award date. The figure shows similar trends in project size for HOPE VI and non-HOPE VI projects prior to two years before the award, with HOPE VI projects declining in size for several years thereafter.

Also apparent from Figure 1 is that some of the projects were only partially demolished. While a substantial portion of the households in HOPE VI projects exited their dwellings within five years of the demolition, our sample does include households who resided in non-demolished units and remained in their original housing units. We include these households in the sample as our view is that they are still "treated" by the program since the demolition could have affected the people or characteristics of the neighborhood in which the HOPE VI project was located. Indeed, in Section 5 we find that the neighborhood in which the project was located is affected in important ways by the demolitions.

Our primary analysis dataset combines the project- and individual-level data to create a file in which an individual will appear in the sample for each year that they appear in public housing. We retain all individual-year observations for residents of non-HOPE VI projects.[19] For the HOPE VI projects we retain individuals who resided in the projects two years prior to the award date. Our sample contains 1,682,000 child-year and 1,023,000 householder-year observations.

## 4. Empirical Strategy

Our primary goal is to estimate the average effect of the HOPE VI Demolitions program on young adult (age 26) labor market outcomes for children affected by the program—the average treatment effect on the treated. The challenge is that, by design, the projects demolished under HOPE VI were systematically different from those that were not. This is readily apparent from Table 1, which presents the mean and standard deviations of baseline characteristics for projects and residents of HOPE VI and non-HOPE VI projects as well as the differences between two

[18] Because the HUD-PIC data start in 1997, any HOPE VI projects that have an award date prior to 1999 are assigned a demolition year of 1997. The decision to retain the early awardees is in part motivated by reports that there were longer delays between grant awards and demolitions for these projects (U.S. GAO, 2003). In Appendix C we show that our results are robust to how we treat projects that received HOPE VI grants prior to 1999.

[19] For the non-HOPE VI sample, we drop individuals who previously lived in a HOPE VI project. Thus, individuals who moved out of HOPE VI projects and into other public housing projects are excluded from the control group.

samples. Along almost every observable dimension, children growing up in HOPE VI projects are more disadvantaged. For example, HOPE VI projects are in census tracts with 52 percent higher poverty rates, the residents have 20 percent lower total annual household income, and are almost 50 percent less likely to have a married householder.

Given these pronounced observable differences and the lack of experimental variation, our empirical strategy aims to estimate causal impacts by accounting for observable baseline differences between HOPE VI and non-HOPE VI projects. We argue that this is a reasonable approach in our context because the number of distressed, eligible projects greatly exceeded the number of HOPE VI awardees, and our data allows us to observe and characterize the conditions in nearly all public housing projects in the U.S. Thus, there is a large sample of non-HOPE VI projects that are potentially informative of what would have happened to the residents of HOPE VI projects had there been no demolitions. To estimate the causal impacts of the Demolition program, we employ the stratification with regression estimator proposed by Rosenbaum and Rubin (1983, 1984) and discussed at length in Imbens and Rubin (2015) and Imbens (2015). The method combines features of both matching and regression in the following steps: (1) trim the sample with nearest-neighbor matching, (2) group similar observations into distinct strata based on an estimated propensity score, (3) use Ordinary Least Squares (OLS) regressions with controls within strata to estimate stratum-level treatment effects, and (4) calculate aggregate treatment effects as a weighted average of the stratum-level estimates.

There are three principal advantages of the stratification with regression estimator over the more traditional OLS estimator. First, trimming the sample and using the stratification structure helps us relax the linear functional form assumptions implicit in OLS. As a rule of thumb, linear regression techniques will be sensitive to the specification when the value of normalized differences between the treatment and control groups exceed one-quarter (Imbens and Woolridge 2009).[20] Table 1 demonstrates that many important baseline variables have normalized differences that exceed this threshold in the full sample.[21] Second, many choices on how to adjust for observable differences between HOPE VI and non-HOPE VI projects are governed by the data, which helps mitigate concerns that the choice of specification is influenced by ex-post analysis of

[20] Let $\bar{x}_d$ and $s_d$ be the mean and standard deviation of the variable x for the HOPE VI (d=1) and non-HOPE VI (d=0) samples, respectively. Then the normalized difference is defined as $(\bar{x}_1 - \bar{x}_0)/\sqrt{(s_1^2 + s_0^2)/2}$.

[21] The solid line in Figure A.2 makes a similar point by presenting the distribution of the normalized differences for all baseline variables calculated on the full sample.

results. Third, the stratification with regression method presents a number of ways in which we can evaluate the plausibility of the identifying assumptions, some of which are specific to the method and have no clear analogue under OLS. These are discussed in Section 5.3.

Construction of the strata is implemented in three steps. First, we trim the sample of non-HOPE VI projects to reduce the observable differences between the HOPE VI and non-HOPE VI samples. To do so, we start with a project-level dataset that includes all projects after imposing the restrictions mentioned in Section 3.[22] We use a project-level, as opposed to an individual-level dataset because the treatment is assigned at the project-level. For each HOPE VI project, we use nearest neighbor matching to identify and retain the five nearest neighbors among the non-HOPE VI projects. Matching is conducted with replacement; distance is measured using the Euclidean distance metric based on observable project and neighborhood characteristics (see Appendix B for list of variables used in matching); and we require exact matching on the PHA stratum (large or small). The resulting dataset, which we refer to as the matched sample, contains all 160 HOPE VI projects and a subsample of 570 matched non-HOPE VI projects, which we refer to as control projects (since we drop non-HOPE VI projects that are fundamentally different and unlikely to be informative of counterfactual outcomes for HOPE VI residents).

The right three columns of Table 1 present summary statistics and difference measures for HOPE VI and matched controls for a subset of important baseline variables. The differences are much smaller relative to those calculated in the full sample with nearly all smaller than one-quarter (the threshold mentioned above as being indicative of good balance). The dashed line in Figure A.2 makes a similar point by presenting the distribution of the normalized differences of all baseline variables in the matched sample. This step does not reduce the external validity of the estimates since we retain all HOPE VI projects, and our goal is to estimate the average treatment effect on the treated.

In the second step, we estimate a project-level propensity score defined as the probability that a project receives a HOPE VI Demolition grant, conditional on observable characteristics. To determine the covariates included in the propensity score model, we use a data-driven method described by Imbens and Rubin (2015). We start by estimating a logistic regression of receipt of HOPE VI on a set of covariates that we think are important for predicting treatment (average

[22] Project-level characteristics are measured two years prior to the award date for HOPE VI projects, whereas for non-HOPE VI projects they are equal to the average of observed values between 1997 and 2001.

household income and the proportion of householders who are black non-Hispanic). Next, we estimate a separate logistic regression for each baseline variable that we consider adding to the model and calculate the log likelihood for each logistic regression. If the value of the log likelihood ratio test statistic for a given set of covariates is larger than it is for the models with the other potential covariates and sufficiently greater than the initial log likelihood, then we include the covariate in the model.[23] We iteratively apply this procedure until no more covariates are selected. We then create interaction terms between all the selected covariates and repeat this process to determine which second-order terms to include in the model. This procedure is applied separately for projects located in large and small PHAs. Figure 2 plots the distribution of the linearized estimated propensity score for HOPE VI and control projects.[24] The figures display good overlap between the estimated propensity scores of the treated and control projects.

In the third step we group projects into distinct strata. We start by differentiating the projects into two strata based only on PHA size (small and large), which allows us to avoid comparisons between individuals who reside in fundamentally different economic environments (e.g., a comparison of someone living in a rural county to an individual living in major metropolitan area). We then use a stepwise data-driven method to expand the number of strata for each initial large and small stratum. At each step, the adequacy of the existing strata is assessed by calculating a t-statistic for each stratum based on a test of the null hypothesis that the average value of the estimated linearized propensity score is the same for the treated and control projects in that stratum. If the null hypothesis is rejected (i.e. the absolute value of the t-statistic exceeds 1.645), then the stratum is split into two new strata by grouping projects above and below the median linearized propensity score.[25] The newly generated strata are required to have at least 3 HOPE VI and control projects and 50 total projects in order to prevent issues related to small sample sizes in the analysis.[26] The step-wise process is then repeated until either the null hypothesis of no difference between treatment and control projects in the linearized propensity score is not rejected for any stratum, or splitting the stratum at the median treatment project's linearized propensity score would result in too few projects in one of the newly generated strata. This process divides

[23] We include additional first-order (second-order) terms only if the likelihood ratio statistic exceeds 2.50 (4.21).
[24] As a confidentiality protection measure, we Winsorize each distribution at the 5th and 95th percentiles. Thus, the figure understates the overlap at the tails of the distribution.
[25] Let $p$ denote the propensity score, then the linearized propensity score is defined as $\ln(p/(1-p))$.
[26] We require 50 total projects in each stratum because we cluster standard errors at the project level.

the sample into seven distinct strata. On average, each stratum contains 100 different projects with a population of about 18,000 unique children, 15 percent of whom reside in HOPE VI projects. The boundary points of the strata are depicted by the vertical lines in Figure 2 and Table A.2 presents the sample size within each stratum.

The procedure does an excellent job of eliminating differences in observable characteristics between control and treatment groups within each stratum. To demonstrate, we regress 92 different baseline variables on an indicator for HOPE VI within each of the seven strata and calculate a t-statistic to summarize the differences between control and treatment observations (standard errors are clustered at the project level). In Figure A.3 we plot the distribution of the absolute value of the resulting 644 t-statistics and compare it to the distribution one would expect from the absolute value of t-statistics from a standard normal distribution. The figure illustrates that, if anything, there is more balance within stratum than would be expected from random assignment. Table A.3 provides a more detailed view by presenting the proportion of test statistics that have a p-value of less than 0.10 for neighborhood-, project-, and individual-level characteristics. In a balanced sample, we would expect the share of significant test statistics to be approximately 10 percent.[27] For the most part, we find that this pattern applies. For example, column 6 of Table A.3 suggests that 12 percent of the 276 p-values calculated within the large PHA sample had a p-value of less than 0.10 and 6.2 percent of 368 p-values calculated within the small PHA sample had a p-value of less than 0.10.

An advantage of this methodology is that many of the choices about how to adjust for observable differences between HOPE VI and non-HOPE VI projects are data-driven. However, the method does depend on six tuning parameters, which must be defined by the researcher.[28] We chose the tuning parameter values that are most effective at eliminating baseline differences

[27] Without making any adjustments for multiple hypothesis testing, we should observe slightly more than 10 percent of tests rejected at the 10 percent level.

[28] These parameters are: (1) the number of matches to use when trimming the sample, (2) the threshold for the likelihood ratio test to include first-order terms for the estimation of the propensity score, (3) the threshold for the likelihood ratio test to include second-order terms for the estimation of the propensity score, (4) a threshold for the test statistic used to determine whether the estimated propensity scores of control and treated projects are sufficiently similar within strata, (5) the minimum number of control projects that must be included in each stratum and (6) the minimum number of treated projects that must be included in each stratum. We view the first three tuning parameters as both the most consequential, since they determine which projects serve as controls for each HOPE VI project, and the most likely to require values specific to applications that differ in number of observations and heterogeneity within the sample. Thus, we use standard values for the fourth through sixth tuning parameters but select "optimal" values for the first through third parameters.

between HOPE VI and control projects within strata.[29] There are two important features to note. First, the criteria used for selecting tuning parameters are only based on how well the method eliminates observable differences between HOPE VI and control projects and do not use the outcome variables. This helps reduce concerns about specification search. Second, our main findings are robust to alternative choices of tuning parameters (see Appendix C).

Using the stratification structure, we implement our estimator in two steps. First, we separately estimate the following OLS specifications within each of the stratum:

$$\{\text{Eq. 1}\} \quad y_{itps} = \alpha_b + D_p\delta_b + X_{itps}\beta_s + \varepsilon_{itps}$$

where $y$ is a labor market, neighborhood, or household outcome; $i$ is the individual; $t$ is the year in which that individual appears in public housing; $p$ is the project; $s$ is the stratum the project was assigned to in the first stage; $D$ is an indicator equal to one if the project received a HOPE VI demolition award; $X$ is a vector of observable individual-, household-, project-, and neighborhood-level characteristics; and $\varepsilon$ is an error term which we cluster at the project level.[30] Because the specifications are run *within* each stratum, all of the estimated coefficients are stratum-specific.

All specifications include controls for the year in which the individual appears in public housing (with the HOPE VI individuals only appearing in one year), and a standard set of project-level controls that include characteristics of the project (average total income of resident households, proportion of householders who are Black non-Hispanic, and proportion of householders who are female); area median income in 1990; characteristics of census tract in 1990 (proportion on public assistance, median income, and poverty rate); and the county-level unemployment rate in 1996.[31] The standard vector of individual-level covariates included in the specifications estimated on the child-level dataset includes the interaction between sex and

[29] To do this, we implement the stratification 33 different times using different values of the number of matches (3, 5 or 7) and different values of the second and third tuning parameter. (As a rule of thumb, Imbens and Rubin (2015) find 1.00 and 2.71 work well for the values of the second and third tuning parameters. We vary the value of the second tuning parameter from 1.0 to 6.0 and set the value of the third tuning parameter to 1.71 higher than the second.) We then create a score for each iteration based on the resulting balance of all baseline covariates across HOPE VI and control observations. We find balance is achieved most robustly when using five matches. Thus, we opt to use the specification that delivers the best balance of baseline covariates (lowest-ranked score) when using five matches.

[30] There are a small number of cases in which the outcome variable is missing. To avoid disclosure issues related to releasing results from multiple samples, we impute these missing values with the mean value in the control group and then include an interaction between an indicator for this imputation and treatment status in the regression. In this way imputed values do not contribute to the identification of the treatment effect. In unreported results we estimate all specifications with missing data without this imputation and confirm that the results are not materially different.

[31] The large number of individuals within each stratum allows us to include a large set of individual-level controls in our stratum-level regressions. Since the number of projects per stratum is more limited, we are careful to include a smaller number of project-level controls in the regression analysis.

mutually exclusive race/ethnicity categories (Black non-Hispanic, White non-Hispanic, Hispanic, and other race or Race not specified non-Hispanic); the number of dependents in the household; household size; an indicator for disability; a fixed effect for age at the time of appearing in public housing; an indicator for whether the householder has a disability; an indicator for whether the householder is female; the marital status of the householder; the age of the householder, and total household income.[32] While individuals from HOPE VI projects only appear once in the sample, individuals from control projects may appear multiple times in the sample with an observation for each year they appear in public housing between 1997 and 2001. Nearly all of these individuals appear in the same project and thus clustering standard errors at the project level allows us to take these "duplicate" observation into account when calculating standard errors with each stratum.[33]

The stratum-specific treatment effects are aggregated across strata, using the share of the total of treated individuals as weights. Let $N_{ts}$ be the number of treated individuals in stratum $s$ and $N_t$ be the total of treated individuals across all strata including both the large and small PHA groups. The weight for each stratum is given by $w_s = N_{ts}/N_t$, and the estimate of the average treatment effect on the treated, $\hat{\delta}^{att}$, and the corresponding standard error, $se(\hat{\delta}^{att})$, are given as:

$$\text{\{Eq. 2\}} \quad \hat{\delta}^{att} = \sum_{s=1}^{S}(\widehat{\delta_s} * w_s)$$

$$\text{\{Eq. 3\}} \quad se(\hat{\delta}^{att}) = \sqrt{\sum_{s=1}^{S}(se(\widehat{\delta_s}) * w_s)^2}$$

where the weighted averages are taken across all S strata (S=7 for the main specification).[34]

Our methodology will produce unbiased estimates of the average treatment effect on the treated under the Conditional Independence Assumption; that is, conditional on the covariates and stratification in the model, assignment of a HOPE VI demolition is as good as random. Our method successfully eliminates observable differences between HOPE VI and control projects, which provides some initial support for the Conditional Independence Assumption. After presenting the main results we discuss other checks intended to assess the validity of the empirical approach.

[32] The standard vector of individual-level covariates included in the specifications estimated on the householder-level dataset includes age, race, sex, number of dependents, household size, disability status, marital status, and total household income.
[33] Appendix C shows that our main results are robust to dropping all observations that appear in more than one project and shows that the standard errors are not significantly affected by the presence of these individuals.
[34] The implicit assumption needed to construct the standard errors is that observations across strata are independent. We argue that this is reasonable since no project appears in more than one stratum and standard errors are clustered at the project level.

## 5. Results

### *5.1 Long-Run Effects on Children*

Table 2 shows estimates from equation 1. HOPE VI led to substantial improvements in the long-run labor market outcomes of the children who resided in affected projects. Panel A presents estimates from the stratification with regression estimator for the full sample (i.e., both large and small PHAs). HOPE VI increased age 26 earnings by 15.3 percent, annual earnings by $622, number of quarters worked by 0.057, and probability of working all four quarters by 1.6 percentage points.[35]

While the overall impact of the program was positive, there is heterogeneity across housing environments. Panels B and C of Table 2 present results separately for large and small PHAs. The positive impacts are generally stronger in large PHAs, with differences that are often economically important in size. For example, the IHS earnings specification suggests a 21.2 percent increase in age 26 earnings for children in large PHAs and only a 4.6 percent increase for those in small PHAs.[36] We provide additional evidence below that there is meaningful heterogeneity by PHA size in the effect of the program.

We explore heterogenous effects by child age at the time of the demolition, race, and sex by estimating models in which the indicator for HOPE VI is interacted with the characteristics of interest. Table 3 presents the resulting estimates for large PHAs.[37] Column 1 indicates that the impacts of the program are no different for older and younger children.[38] Specifically, children exposed to HOPE VI when they were 10 years old experienced an earnings gain of 22.8 percent while this gain is 20.8 percent for 18-year-olds; a difference that is neither economically nor statistically significant. This offers some initial evidence that the impacts of the program may not

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[35] We use the inverse hyperbolic sine (IHS) of earnings rather than the log of earnings as the dependent variable because estimated coefficients can be interpreted in the same way as with a log-transformed dependent variable but, unlike with the log of earnings, IHS is defined for zero earnings. The IHS is defined as $\log[y_i+(1+y_i^2)^{0.5}]$, see Burbidge et al. (1988). We convert the IHS coefficients to percents using $\exp(\beta) - 1$ throughout the paper in terms of our discussion. Bellemare and Wichman (2020) show this is the appropriate transformation for categorical RHS variables.

[36] The long-run benefits found in large PHAs are robust to measuring earnings at alternative times. Figure A.4 in Appendix A presents estimates of the effect of HOPE VI on the IHS of earnings measured between ages 18 and 26. The effect of the program grows over time, starting around zero at age 18 and rising to about 0.2 by age 23, after which point the effects stabilize through age 26.

[37] Not surprisingly, we find little evidence of heterogeneous effects in small PHAs. The one exception is that there is some evidence that white non-Hispanic children benefited more than non-white children in small PHAs. See Appendix C for details.

[38] In unreported results we also find that the lack of heterogeneous effects by age is robust to estimating alternative specifications that employ project or household fixed effects.

be primarily driven by differences in human capital accumulation from exposure to neighborhoods of varying quality, at least through the exposure model typically considered in this literature (as in Chetty et al. 2016; Chetty and Hendren 2018). Column 2 indicates that males experience significantly larger earnings benefits and column 3 suggests that non-White children also benefit more. While we do not have enough power to estimate a model with the full set of interactions between race and sex, column 4 presents estimates from a specification in which we compare the effects for non-White males to all other children. We find that non-White males appear to be the primary beneficiaries of the program.

***5.2 Assessing the Validity of the Empirical Strategy***

While we argued above that the methodology does a good job eliminating observable differences between HOPE VI and control projects, it is possible that our results are biased by unobserved differences or functional form assumptions implicit in the stratum-level regressions. Of particular note is the possibility that HOPE VI Demolition grants were targeted towards metropolitan areas or neighborhoods where young adults from high poverty neighborhoods would have experienced increases in employment or earnings even in the absence of the program. To assess these potential threats further, in this section we implement three types of analyses: "pseudo treatment," "pseudo outcome," and "sensitivity/robustness" analyses.

First, we implement a pseudo treatment analysis in which we define a group of eligible projects that were not affected by HOPE VI as pseudo treatment projects. We then estimate pseudo treatment effects by re-implementing the full trimming and stratification with regression method with the pseudo treatment group in place of the true treatment group and omitting the true treatment group from the sample. Estimating null effects for projects that, a priori, should not have systematically different potential outcomes for resident children from comparable projects provides evidence that the methodology is able to adequately correct for baseline differences. We implement the pseudo treatment analysis using two different sets of projects: (1) projects that applied for but never received funding for the HOPE VI Demolition or Revitalization programs, and (2) observably similar projects located within the same PHA, which are selected using propensity score matching.[39] The first set of pseudo treatment projects are useful in that they are

[39] There were too few failed applicants identified in the public data for only the demolitions program, so we pooled applicants across the demolition and revitalization programs. Given that the two programs targeted a similar group of projects and that the projects look similar along observable characteristics at baseline, we argue that this is an informative exercise. Figure A.5 provides evidence to show that failed applicants had similar observable

observably similar to HOPE VI projects and their choice to apply suggests the PHA viewed them as being likely to benefit from HOPE VI funding. In contrast, the second set of pseudo treatment projects—the most similar projects found in the same PHAs as a treated project—help to assess the likelihood that HOPE VI grants were targeted towards metro areas that would have experienced improvements in economic outcomes for young adults, even in the absence of the program.

Table 4 presents the estimated pseudo treatment effects. The estimates are never statistically different from zero and have standard errors similar in size to those from our main results in Table 2. Table A.4 shows that OLS specifications that compare the children from the HOPE VI projects to those from the pseudo treatment projects yields results similar to our main findings: large benefits for children in large PHAs and no benefits for those in small PHAs. The lack of positive pseudo treatment estimates along with the OLS estimates from Table A.4 provide support for the idea that positive bias is not driving the main estimates from our matching estimator and bolsters our confidence in the identifying assumptions.[40]

Second, we implement pseudo outcomes analyses. These are designed to replace the true outcomes with characteristics likely to be predictive of the outcomes that are measured prior to and therefore not affected by the HOPE VI treatment. For each pseudo outcome measured prior to the demolition, we re-implement the trimming and stratification process after excluding any variable constructed from the pseudo outcome from being included in the matching or regression analysis. For example, if household income were the pseudo outcome, we would implement the matching and estimation of the propensity score without using average income at the project level in the matching or regression. We then use the stratification with regression estimator to estimate a pseudo outcome effect in which the pseudo outcome is the outcome variable, including the full set of controls after excluding variables constructed using the pseudo outcome. The results from these analyses are displayed in Table A.5. Each row presents results for one of the 18 pseudo

characteristics to the HOPE VI demolition awardees at baseline. Note that failed applicants were subject to the same set of restrictions as all other non-HOPE VI projects. For the second pseudo treatment group we exclude projects within one mile of a demolition project.

[40] If anything, there appears to be a negative pseudo treatment effect for the failed applicants, which could suggest that HOPE VI projects are negatively selected relative to counterfactual projects and our main estimates may provide lower bounds on the true effect of HOPE VI. Alternatively, these negative (not statistically significant) associations could be explained if the applicant projects were exposed to alternative, less effective programs in place of HOPE VI. The fact that they might have been exposed to other programs complicates the interpretation of the estimated effect of HOPE VI when the failed applicants are included in the set of controls. While we include the failed applicants in our set of potential controls, in practice they make up only small portion of the matched sample used to estimate the main results. Indeed, our results are robust to excluding failed applicants from the set of matched controls.

outcomes we consider, with columns 1-3 displaying estimates for the large, small, and pooled samples, respectively. The results largely confirm that the method is able remove differences between HOPE VI and control projects. 2 of the 18 pseudo outcome estimates are statistically significant when pooling across housing authority sizes. However, we reject the null of no pseudo outcome effect for household income. This potentially indicates that household income is a critical variable in the matching process for which there is not a close substitute, but it does not invalidate the identifying assumptions.

Third, we assess the robustness of the estimates to using alternative sets of control variables. Table A.6 presents estimates of the effect of HOPE VI for four different specifications that either (1) use the baseline stratification structure or simply define two strata by large and small PHAs and (2) do or do not include covariates, or control variables, in the model. Columns 3 and 4 use the baseline stratification structure but only column 4 also includes covariates in the model. For large PHAs, the estimated effect of HOPE VI on the IHS of earnings at age 26 is 0.157 with stratification but without controls compared to 0.195 when controls are added.[41] For small PHAs, estimates with and without controls are similarly small across the two specifications (0.005 and 0.045). Thus, once the stratification structure is implemented the main role of the covariates in the model is to increase precision. This finding suggests that the choice of which covariates are included in the stratum-level regressions and how they are included (functional form) are not driving the results. In addition, the similarity between the standard errors in column 2 and 4 mitigates concerns related to inadequate sample sizes for clustering standard errors at the project level within strata and to individuals in control projects appearing in multiple projects across distinct strata.

***5.3 Short- and Medium- Run Effects for Householders***

To better understand the mechanisms through which HOPE VI demolitions affected long-term labor market outcomes, we explore the short- and medium-term effects of the program. Column 1 of Table 5 shows that HOPE VI led to a 15 and 18 percentage point reduction in the probability that the householder lives in the same housing project five years after the demolition in large and small PHAs, respectively. Columns 2 and 3 indicate that HOPE VI pushed households

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[41] While the point estimate is smaller in column 3 (the specification that uses the stratification structure without covariate adjustment) relative to column 4 (the baseline specification), the estimate would be statistically significant if the standard error from the main specification were used to conduct the hypothesis test.

into both voucher housing and other public housing, with a slightly larger increase in voucher housing. Five years after the demolition, HOPE VI households in large PHAs are 9.8 percentage points more likely to be in voucher housing and 5.9 percentage points more likely to be in a different public housing project. The analogous figures in small housing authorities are 10.7 percentage points and 9 percentage points for voucher housing and different public housing projects, respectively. Thus, HOPE VI induced substantial movement of households into other public and voucher housing.

Column 4 of Table 5 illustrates that while there is evidence that households were displaced from assisted housing one year after the demolition in large PHAs, HOPE VI did not push households out of subsidized housing in the longer-run.[42] Many households in HOPE VI projects did end up leaving subsidized housing within a five-year period, but the rate at which they did so was similar in the control group —48.5 percent and 54.9 percent of control householders departed assisted housing within five years in large and small PHAs. This finding is consistent with other work that finds high rates of turnover in low-quality public housing projects (McClure, 2018).

In addition to altering the type of housing subsidy, HOPE VI also increased the likelihood of migration to new neighborhoods. Column 6 of Table 5 indicates that HOPE VI increased the probability of moving to a new census tract five years after the demolition by 13.0 and 17.2 percentage points in large and small PHAs, respectively. However, Column 5 shows that these moves to new neighborhoods typically occurred within counties.

While HOPE VI increased the probability of moving, it did not lead to large changes in many aspects of neighborhood environment. Table 6, presents estimates for the full sample of the effect of HOPE VI on the characteristics of the neighborhoods in which households resided between one and five years after the demolition.[43] Columns 1-5 show that the program did not have a statistically significant effect on neighborhood characteristics including school quality, the share of residents that were White non-Hispanic, the poverty rate, the change in poverty rate

[42] The category "other public" refers to individuals who appear in the HUD-PIC files but are not in the same project or in voucher housing. The vast majority of these individuals are actually in public housing but there may be a small percentage who participate in the Section 8 Moderate Rehabilitation Program, which is the other assisted housing program covered by the HUD-PIC files. The category "non-subsidized" refers to individuals who do not appear in the HUD-PIC files. The HUD-PIC files cover both the public housing and voucher programs, which are by far the largest programs subsidizing housing costs for renters. Thus, while there may some households in this group that participate in other subsidized housing programs not covered in the HUD-PIC data, the numbers are likely to be very small.

[43] We find similar patterns if we instead separately define outcomes for neighborhood characteristics 1, 3, and 5 years after a demolition as in Table 5.

relative to baseline, and a measure of upward mobility from the Opportunity Atlas—the expected income rank of children whose parents were at the 25th percentile of the income distribution. Though the estimates in column 4 are not statistically distinguishable from zero, the coefficients and control-group means make clear that households, regardless of HOPE VI status, lived in lower poverty neighborhoods than where they resided earlier in the study period. In large PHAs poverty rates declined by 12.3 and 10.4 percentage points for HOPE VI and non-HOPE VI households, respectively; in small PHAs, they declined by 7.3 and 6 percentage points for HOPE VI and non-HOPE VI households, respectively.[44]

The neighborhood poverty reductions found in Table 6 are small relative to both the control group means and the reductions in poverty studied in Chetty et al. (2016) and Chyn (2018). We do find evidence that HOPE VI households exposed to a full demolition—a demolition where there were no non-demolished units—saw larger declines in neighborhood poverty relative to non-HOPE VI households (7.2 percentage points in large PHAs). This result, however, is based on the comparison of outcomes for households exposed to full demolitions—a non-random subset of the treatment group—to all comparison households, and therefore should be interpreted with caution. Still, potential heterogeneity in the impact of HOPE VI on neighborhood poverty underscores the possibility that reductions in neighborhood poverty exposure could be a relevant mechanism for some of the children in our sample. Although, we find no evidence the effects on the adult earnings of children are larger for those who resided in the fully demolished projects.

The results in Column 6 of Table 6 indicate that, in large PHAs, the program led households to move to neighborhoods that had greater geographic accessibility to jobs. Relative to non-HOPE VI households, HOPE VI households in large PHAs lived in neighborhoods that scored 8.4 percentiles higher in terms of job accessibility between 1-5 years after a demolition. No similar improvement is observed for HOPE VI households in small PHAs.

We also find no evidence that HOPE VI affected labor market outcomes for householders. Table 7 shows that HOPE VI had no impact on the number of quarters worked and the IHS of annual earnings measured five and ten years after the demolition. Together, the results in this section provide mixed evidence for how the program impacted household and neighborhood environments. There were no improvements in householder labor market outcomes, no (average)

[44] The estimates in columns 3 and 4 of Table 6 are consistent with previous descriptive research, which finds that households displaced by HOPE VI moved to lower poverty neighborhoods (e.g., Kingsley et al., 2003).

changes in neighborhood poverty, no changes in neighborhood demographics, and no improvements in school quality or intergenerational mobility. The clearest evidence presented in Table 6 indicates that HOPE VI improved the accessibility of jobs in the neighborhoods which households resided 1-5 years after a demolition.

***5.4 Mechanisms***

What are the mechanisms through which HOPE VI affected long-run labor market outcomes? In existing research that finds long-term labor market benefits of exiting public housing when young, Chyn (2018) and Chetty et al. (2016) find evidence of an exposure model: neighborhood environment shapes the development of human capital and impacts are increasing in the duration of exposure. In Section 5.3, we show that there is limited evidence that HOPE VI improved childhood neighborhood environments. Furthermore, in Section 5.1 we find that impacts are not larger for children who were younger at the time of the demolition. The evidence therefore suggests that the children in our sample may have benefited from a mechanism other than the exposure model highlighted in existing literature. While it remains possible that exposure to lower poverty neighborhoods could have facilitated long-term earnings gains for some sample children, there are three reasons it may not have played as large of a role as in previous work. First, the households in our sample did not receive assistance or face requirements to move to low poverty neighborhoods. This contrasts with MTO, which provided assistance for households to facilitate moves to lower poverty neighborhoods and explicitly required moves to lower poverty neighborhoods in the experimental treatment arm. Second, our analysis focuses on older children—between the ages of 10-18 at the time of the demolition—for which prior research has found limited potential for exposure effects. Third, the public housing projects in our sample were in neighborhoods with substantially lower poverty rates than those included in Chyn (2018)—37.4 percent in our sample as compared to 78 percent in Chyn—likely making it more difficult to find lower-poverty alternative neighborhoods. Provided with similar incentives, similar alternative neighborhoods in which to search for housing, and support to move to lower poverty areas while children were young, it is plausible that HOPE VI households would have experienced similar reductions in neighborhood poverty and displayed the same age-gradient in long-term benefits.

Another possibility is that HOPE VI could have affected labor market outcomes by influencing where children lived as young adults. We investigate this possibility by studying residential outcomes of the children as measured in 2010.[45] As a starting point, we estimate specifications in which the outcome variable is an indicator equal to one if the distance between the project and the location of residence in 2010 exceeds a distance threshold. The results are presented in Figure 3. In both large and small PHAs, HOPE VI pushed children away from the neighborhoods in which their projects were located, but the resulting moves were quite local. About one-half of all children lived within five miles of their project in 2010, and HOPE VI increased the likelihood of moving to a new neighborhood within a five-mile radius of the project. There was no effect on the likelihood of moving farther away.

Table A.7 explores whether HOPE VI affected the probability that children lived in subsidized housing, with or near their parents as young adults, or the likelihood that children were in an adult correctional facility at the time of the 2010 Decennial Census. Columns 1-3 suggest that HOPE VI children were more likely to be participating in the voucher program, less likely to be in public housing (in large PHAs), but that there is no difference in the likelihood of being in any subsidized housing program. Columns 4 and 5 show that HOPE VI children were no more likely to be living in the same household or in the same census tract as their parents in 2010. Of note in column 4 is that more than half of all children were residing with their parents in 2010. This underscores the possibility that household moves that occurred just after a demolition could have had a persistent effect on the neighborhoods where children lived as young adults. Last, column 6 follows the methodology of Pollakowski et al. (2022) by linking individuals to the 2010 Decennial Census File to determine whether they reside in an adult correctional facility at the time of the survey. Table A.7 indicates that the effect of HOPE VI increased the probability of being incarcerated in 2010 by 0.001 and 0.005 in large and small PHAs. These effects are not economically meaningful or statistically distinguishable from zero.[46]

It is also possible that HOPE VI could have affected labor market outcomes by dispersing residents and breaking apart peer groups that negatively influenced young adult outcomes. To investigate, we use residential location in 2010 to measure the distance between adult children and

[45] We focus on 2010 because we are able to measure residential location by combining data from both the 2010 Decennial Census and the CPR. Children are between the ages of 19 and 31 in 2010 and thus these measures of residential location may not correspond exactly to where children are living when we measure their earnings at age 26. However, most children will be in their mid-twenties at this time and, as shown in Figure A.4, the effect of the program is relatively constant between ages 23-26. The longitude and latitude of the centroid of each census tract are from Census Bureau Gazetteer Files for 2010 geography.

[46] In unreported results, we show that this null result also holds when limiting the sample to males, who are at higher risk of being incarcerated.

each of their former public housing co-residents. We create four variables to measure dispersion: the average log distance to all former co-residents and the share of former residents who live within a 1-, 3-, and 5-mile radius. The results, presented in Table A.8, suggest that HOPE VI did not disperse residents geographically in large PHAs. While these are coarse measures, the results provide no evidence that HOPE VI disrupted peer groups that may have formed in public housing.

We do find, however, that HOPE VI led to changes in some of the characteristics of the neighborhoods where the children lived as young adults. We estimate the effect of HOPE VI on six characteristics of the census tract in which the children resided in 2010: the poverty rate, the employment rate, a measure of labor market networks (network isolation), and three measures of the geographic accessibility to jobs (the log of the ratio of jobs to people, the average commute time and a job proximity index that captures the "the accessibility of a given neighborhood as a function of its distance to all job locations within a [Core-Based Statistical Area]").[47] The results, presented in Table 8, largely confirm the patterns seen in Table 6 when defining outcomes for householders 1-5 years after a demolition. Within large PHAs HOPE VI improved the geographic accessibility of jobs along all three measures,[48] and we do not see an average effect on neighborhood poverty. There is also no evidence that HOPE VI children resided in neighborhoods with higher employment rates or lower levels of social network isolation. In small PHAs, there is no evidence that HOPE VI improved geographic proximity to jobs, and even some suggestion that it may have decreased job proximity. Conversely, HOPE VI children in small PHAs resided in lower poverty neighborhoods with higher employment rates in 2010.

Additional empirical support for the importance of the job accessibility channel comes from linking a subset of the sample to the 2008-2012 ACS data. Results for the survey-reported outcomes presented in columns 1 and 2 of Table 9 are consistent with our main results: HOPE VI increased employment and earnings in large PHAs. The outcome variables in columns 3 and 4 are

[47] For a description of the job proximity index see: http://hudgis-hud.opendata.arcgis.com/datasets/jobs-proximity-index. The underlying measure is the same as Shen (1998) and Wang (2007) and is similar to that in Andersson et al. (2018), though it uses distance for the impedance function rather than travel time. The values of this underlying measure are percentile ranked with values ranging from 0 to 100 and higher values indicates neighborhoods with better access to jobs. The job proximity index is constructed by HUD using data from LODES for 2014. The observed network isolation index measures, for employed residents of a tract, the share of their co-workers who are also neighbors, where high values of this variable could arise if information on job opportunities disseminate through local networks (see Hellerstein et al. 2011 and 2019).

[48] We find even larger effects on job proximity for the neighborhoods in which the householder lived in 1-5 years after the demolition (see Table 6, column 6). This short and medium-term effect provides evidence that households moved because of HOPE VI and that these moves affected where the children lived as adults.

equal to one if the individual is employed and has a commute below (column 3) or above (column 4) the control group median. The results indicate that the increase in employment in the large PHA sample is driven by individuals who have relatively short commutes. This is supported by column 5, which shows that HOPE VI reduced the average commute time for employed individuals by 2.1 minutes. Lastly, columns 6 and 7 show that HOPE VI had no impact on educational attainment (years of schooling) or on the monthly rent paid by the households where sample children reside.

Why did HOPE VI-induced moves generate the improvements in job proximity but not poverty? Figure 4 presents the average commute time, poverty rate, and population density in 1990 (before all demolitions) for housing projects by treatment status, PHA size, and distance to a sample project. In large PHAs HOPE VI neighborhoods stood out as outliers in terms of job proximity; thus, even moving short distances could still lead to large improvements in job proximity. HOPE VI neighborhoods also had significantly higher poverty rates, but we find little evidence that the program moved people to lower poverty neighborhoods. Appendix D presents evidence that suggests that housing in lower poverty neighborhoods is significantly more expensive while there is little relationship between measures of job proximity and housing costs. Thus, moves to neighborhoods with better job accessibility may have been financially feasible while higher housing prices may have made moves to lower poverty neighborhoods more difficult.

In addition to requiring households to move to new neighborhoods with higher job accessibility than the neighborhoods they left, HOPE VI could have also improved job accessibility by altering the characteristics of the original neighborhoods. To explore this, we measure the average job proximity index of census tracts within half-mile radius bands from zero to five miles around the project. We then attach these neighborhood-level measures to the child-level dataset and implement the stratification with regression methodology as before to estimate the effect of HOPE VI on the characteristics of these neighborhoods. The results for large and small PHAs are presented in Figure 5. We see no significant impacts on job proximity in small PHAs at any distance. In large PHAs, HOPE VI produced substantial improvements in the job proximity index for census tracts within half a mile of the HOPE VI project.[49] That the effects dissipate quickly with distance is reassuring since we would not expect the demolition of a public

[49] The finding that the neighborhood in which the project was located underwent large changes supports our choice to include all, and not just partial, demolitions in the analysis. Households in units that were not demolished were still treated by the program by changes in neighbors and changes in the existing neighborhood.

housing project to drastically transform the population or job density in more distant neighborhoods.[50]

To investigate the origins of the effect on these neighborhood-level measures of job proximity, we estimate the effect of HOPE VI on three characteristics of the census tract in which the project was located: the log of the ratio of jobs to people, the log of the density of jobs, and log of population density.[51] The results, presented in Table A.10, imply that, HOPE VI increased the ratio of jobs to people in large PHAs by 25 percent, and that this impact was driven primarily by a reduction in population density: HOPE VI reduced population density by 86 percent. HOPE VI neighborhoods also see a 4.6 percent increase in job density, but the difference is not statistically distinguishable from zero. A reduction in population density increases job accessibility by reducing the number of competing searchers in the local labor market as long as the number of jobs in the neighborhood does not decline. In the case of a public housing demolitions, the reduction is for a population likely to compete for a similar set of jobs (Lens, 2014; Lens et al., 2019). In small PHAs, we find no effect of HOPE VI on job or population density, potentially because the demolition of (smaller) public housing projects displaced fewer residents.

The preceding analyses suggest that HOPE VI improved geographic proximity to jobs in large PHAs both by transforming the neighborhood in which the project was located and by moving former residents to new neighborhoods with better accessibility. To investigate the quantitative importance of each channel, we estimate specifications that replace the true measure of job proximity with a counterfactual measure that discards all variation due to changes in the HOPE VI neighborhoods. To calculate this counterfactual measure, we use the stratification with regression method to estimate the effect of HOPE VI on the job proximity index, limiting the sample to census tracts within a half-mile radius of the original project; note that these are the areas where HOPE VI directly impacted job proximity, as shown in Figure 5. We obtain a predicted value of the job proximity index for HOPE VI neighborhoods in the absence of changes to the original neighborhood by setting all covariates to their true value except for the HOPE VI indicator, which is set to zero instead of one. The counterfactual measure of the job proximity index is equal

[50] The fact that HOPE VI affected both the census tract in which the project was located and census tracts within a half mile radius could reflect that projects may have been located in multiple census tracts though we assign each project to a unique census tract. Other research on HOPE VI has generally found that spillover effects of the demolitions dissipate within a mile (e.g., Sandler, 2017).

[51] Density is calculated by dividing the number of jobs (or population) by the land area of the census tract, so both measures use the same land area for normalization. Land area cancels out in the job/population ratio.

to this predicted value for all children who resided in HOPE VI projects and still lived within a half-mile of their project in 2010—i.e., children whose neighborhood job proximity was directly affected by the demolitions-induced changes—and is set to the true value of the job proximity index for all other children. Intuitively, we impute the job proximity for individuals from HOPE VI projects who remained within a half-mile of their original project (and therefore benefitted from changes in their original neighborhood) using the job proximity for individuals from observably similar control projects. Any estimated improvements using this counterfactual measure of job proximity will thus be entirely driven by HOPE VI-induced moves to new neighborhoods.

We then estimate the impact of HOPE VI on this counterfactual job proximity measure for large PHAs. The original estimates, presented in Table 7, indicate that HOPE VI increased the job proximity index by 2.11. When the counterfactual value of the job proximity index is used as the outcome variable, this estimated impact falls to 1.16, suggesting that the remainder, about 45 percent of the total impact on the job proximity index, is attributable to improvements in the neighborhood in which HOPE VI projects were located. This suggests that, within large PHAs, HOPE VI improved access to jobs by moving children to new neighborhoods and by improving job accessibility in HOPE VI neighborhoods, with both channels quantitatively important.

Improvements in job proximity could affect earnings by reducing job search and/or commuting costs and encouraging individuals on the margin between working and not working to participate in the labor market. Consistent with this hypothesis, we find that an important part of the earnings gains occurs through an extensive margin labor supply response. Using the estimates from Table 2, the control means from columns 1 and 4 indicate that the average working child from the control group earns $3,944 per quarter whereas column 1 indicates that HOPE VI increased quarters worked by 0.076. Using the effect on quarters worked and average earnings per quarter in control projects we calculate that the effect on annual earnings would be $300 (3,944*0.076=$300) if the entire effect were driven by an increase in labor force participation. This is about 57 percent of the estimated effect in column 3, suggesting that extensive margin labor supply responses are the main avenue through which the earnings impacts occur.

As discussed earlier, we find no effect of HOPE VI demolitions on earnings for the householders. Given that many of these householders are single parents who qualify for public support and have especially high opportunity costs for time supplied in the labor market, a likely explanation for this discrepancy is that the householders have higher reservation wages. Figure

A.6 presents the distribution of earnings for householders and the adult children. Consistent with the hypothesis that householders have a higher reservation wage, there is a hollowing out of the distribution of labor market earnings for householders relative to the adult earnings of the children in our main sample; householders are more likely to have zero earnings (48 percent compared to 35 percent) and less likely to have low levels of strictly positive earnings (10 percent of householders have earnings in the bottom quartile compared to 18 percent of the adult children).

In sum, there are four pieces of evidence that support the job accessibility mechanism as an important driver of our main results. First, we find systematic evidence of improvements in measures of job accessibility within large PHAs, where differences in job proximity should be larger and more meaningful. Second, the effect on earnings appears to have a substantial extensive margin component, which is consistent with the hypothesis that HOPE VI primarily affected the costs associated with finding a job and not the rewards from work. Third, the effects of HOPE VI on employment are driven by an increase in employment for individuals who commute short distances. Fourth, the difference in the effect of HOPE VI on earnings in large PHAs versus small PHAs is mirrored by the difference in the effects on the measures of job accessibility.

### *5.5 Reconciling Different Effects in Different Environments*

Why does HOPE VI produce substantial long-run labor market gains for children living in large but not small PHAs? One possible explanation is that the program interacted in important ways with local environments. In particular, poor geographic access to jobs might affect labor market outcomes more in the worst neighborhoods. Figure 6 presents kernel density plots of the average commute time, poverty rate, and population density in 1990 in the census tracts containing projects in the sample, separately by PHA size (large or small) and HOPE VI treatment status. The figure illustrates that prior to the demolitions, projects in large PHAs, regardless of whether they subsequently received a HOPE VI grant, had significantly higher average commute times, poverty rates, and population densities.[52]

Figure 6 also illustrates that there is substantial variation even within the large PHAs in terms of these baseline characteristics of neighborhoods. We make use of this variation by estimating three specifications in which we interact the indicator for HOPE VI with pre-demolition

[52] Komogorov-Smirnov equality-of-distribution tests confirm that the differences between HOPE VI projects in the large and small PHAs are statistically significant while the differences between the control and HOPE VI projects within large and small PHAs are not statistically different from one another.

measures of neighborhood average commute time, poverty, and population density. The results for large PHAs, presented in Table 10, suggest that demolitions had stronger effects for projects in neighborhoods that were more densely populated, where commutes were longer, and where the poverty rate was higher in 1990.[53] The heterogeneity is economically meaningful. For example, the results suggest that HOPE VI increased age 26 earnings by 44 percent for children in neighborhoods that had baseline poverty rates one standard deviation above the mean poverty rate among HOPE VI projects. In comparison, children in neighborhoods with poverty rates one standard deviation below the mean only experienced a 11 percent increase in earnings.

Together, the heterogeneity in the effect of HOPE VI both across and within large and small PHAs suggests that the program produced larger labor market gains for children originally residing in high-density, high-poverty neighborhoods, with limited job opportunities nearby. Within these communities, HOPE VI improved labor market outcomes both by moving children to neighborhoods with better job accessibility and by improving the job accessibility of the original neighborhoods. In contrast, the program offered fewer benefits to individuals residing in neighborhoods with better job accessibility, lower poverty, and lower population density prior to the demolition.

The treatment effect heterogeneity is also informative for interpreting findings from existing research. As previously discussed, Chyn (2018) and Chetty et al. (2016) both find long-term labor market benefits from exiting public housing when young, but only Chyn (2018) finds that these benefits extend to children older than 13. Our results offer an explanation for this discrepancy. Relative to MTO, the projects in Chyn (2018) were located in neighborhoods that were more disadvantaged and provided less job accessibility. Thus, moving older children out of these projects produced labor market gains, whereas no such gains occurred for older children in the context of Chetty et al. (2016). Relatedly, while Pollakowski et al. (2022) find that time spent in public and voucher housing when young produces long-term labor market benefits of similar magnitudes, our paper highlights the fact that these average effects mask substantial heterogeneity, and that children in the lowest quality public housing projects may benefit from changes in housing. More broadly, the results from our paper highlight how housing and neighborhood can

[53] Table A.11 presents the results for small PHAs. We find no evidence of meaningful interaction effects here, which is not surprising given that we find no significant effect of HOPE VI in this sample in general.

affect long-term outcomes through a multitude of channels that vary in importance with local context.

## 6. Conclusion

This paper uses administrative data on earnings and participation in subsidized housing to study how the HOPE VI demolitions program affected the long-run earnings of resident children. We find that exposure to the HOPE VI program increased earnings at age 26 by 15 percent. The benefits were largest for children who lived in projects run by larger Public Housing Authorities, and in neighborhoods that had greater population density, higher poverty rates, and were farther from jobs prior to the demolition. In terms of potential mechanisms, we find limited evidence that HOPE VI reduced neighborhood poverty for affected households, on average. There is also no evidence that the long-term impacts were larger for sample children who were younger at the time of a demolition, as would be predicted by a neighborhood exposure model. We find the strongest support for improvements in the proximity of job opportunities in the neighborhoods where HOPE VI children lived as young adults operating as the main mechanism. We show that these job accessibility gains resulted both from HOPE VI households moving to new neighborhoods with better access to jobs and from improvements in job accessibility in HOPE VI neighborhoods.

Over the past thirty years, federal housing policy has sought to move families living in subsidized housing out of especially disadvantaged neighborhoods. The results in this paper offer evidence that these moves can generate long-term labor market benefits for children. Interestingly, we find that these moves need not occur in early childhood to produce improvements in adult labor market outcomes, though existing research shows that inducing earlier moves to lower poverty neighborhoods can be more beneficial. Instead, our findings highlight the important and potentially immediate impact of reducing barriers to young adult employment through increasing the accessibility of formal market jobs. Neighborhoods can affect labor market outcomes through multiple channels, and severely distressed public housing projects can, in some cases, limit job accessibility and discourage labor force participation by creating densely populated neighborhoods with high rates of poverty and limited access to jobs.

Our results highlight the importance of accounting for the interaction between subsidized housing policies and local context. Much of the research on assisted housing has taken place in a limited set of large metropolitan areas. In the case of public housing demolitions, our results indicate that long-run labor market benefits for older children are specific to this setting, with few gains for children in smaller metropolitan areas. This result may be relevant to policy choices in less urban and disadvantaged environments. Research has convincingly documented that housing can have important long-run labor market implications but anticipating the effects of potential interventions requires a more complete understanding of the mechanisms. Future research should continue to focus on better understanding how the impacts of housing policies interact with the characteristics of local environments.

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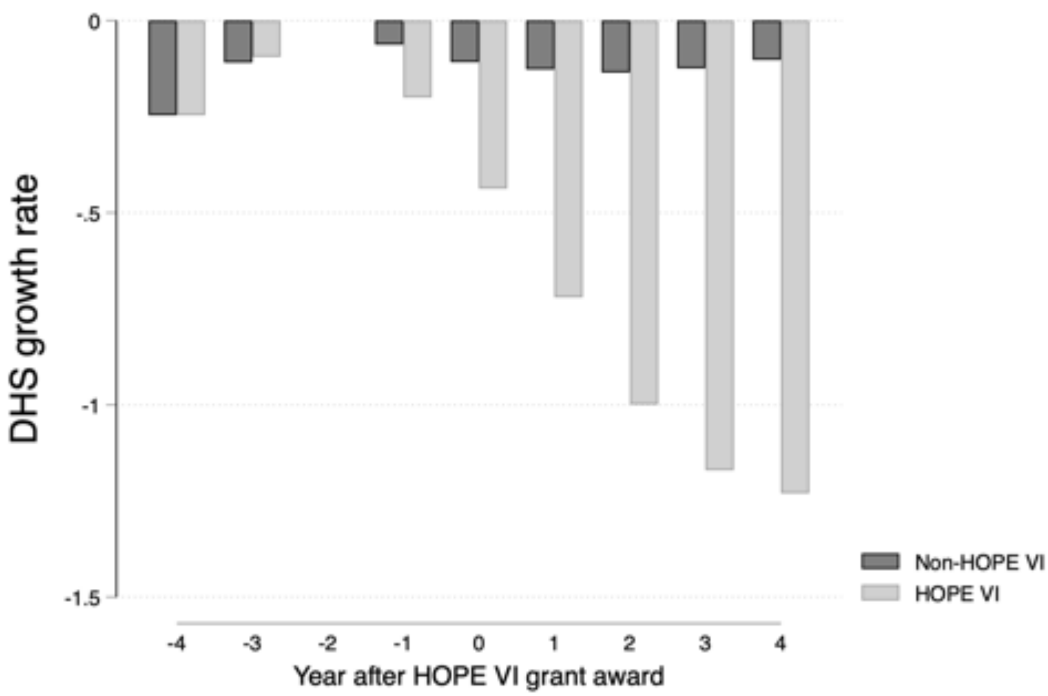
# 7 Figures

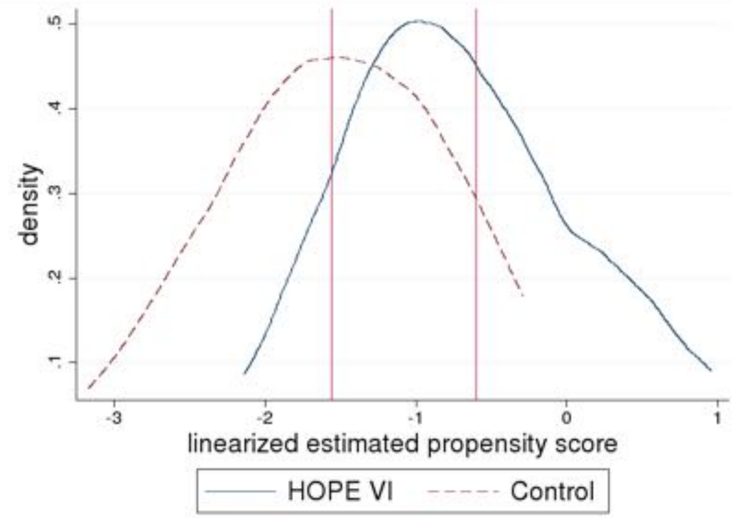
Figure 1: Changes in Project Size Relative to Two Years Before Award



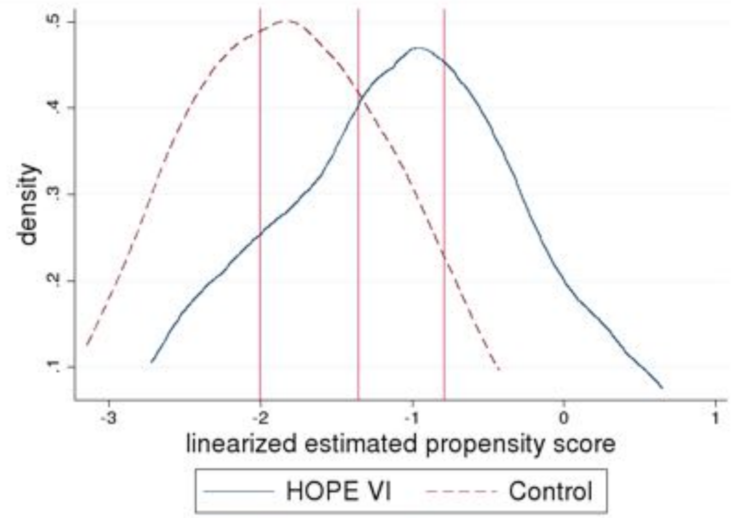

Notes: The figure plots the average DHS growth rate (Davis, Haltiwanger, and Schuh, 1996) in project size between the reference year and x years after the reference year, where x corresponds to the value on the horizontal axis. The growth rate in project size between year t ($y_t$) and year s ($y_s$) is defined as: $\frac{y_t - y_s}{\frac{1}{2}(y_t + y_s)}$. For HOPE VI projects, the reference year is defined as the greater of two years prior to the award year and 1997. Averages are weighted by project size.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

Figure 2: Distribution of Estimated Propensity Score in Matched Sample

(A) Large PHAs



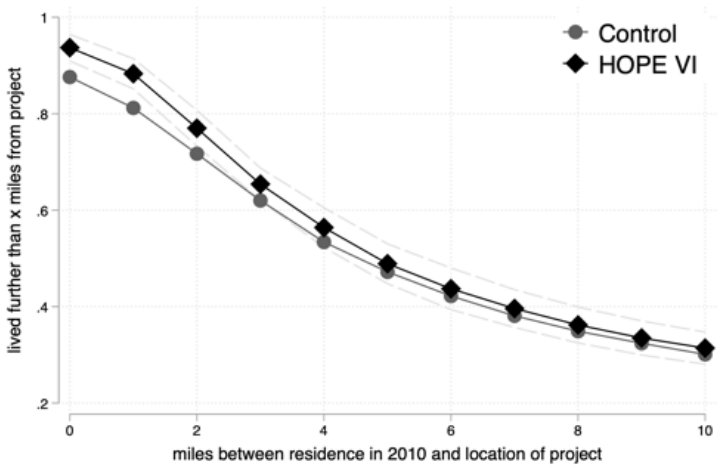

(B) Small PHAs



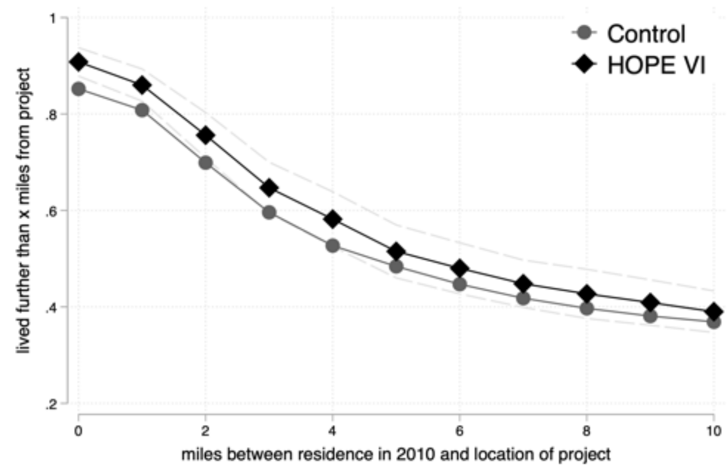

Notes: The figures present the kernel densities of the linearized estimated propensity score for large (panel A) and small (panel B) Public Housing Authorities (PHAs) for the matched sample. The estimated propensity score is the predicted value from a logistic regression of an indicator for HOPE VI on a vector of observable characteristics. The propensity score is estimated on project-level data and the covariates in the model include: average total household income of project residents; natural log of the number of occupied units in the project, the proportion of households in the PHA with a majority of income from wages or business income, average gross monthly rent in the PHA, and the first and second order terms (interactions terms too) of the proportion of householders in the project that are black non-Hispanic and married. The vertical lines indicate the boundaries of the strata. To pass disclosure review requirements, each of the four distributions are Winsorized at the 5th and 95th percentiles. Let $\hat{\rho}$ denote the estimated propensity score, then the linearzied estimated propensity score is, $ln(\frac{\hat{\rho}}{1-\hat{\rho}})$.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

Figure 3: Effect of HOPE VI on Distance between Project and Residence in 2010

(A) Large PHAs



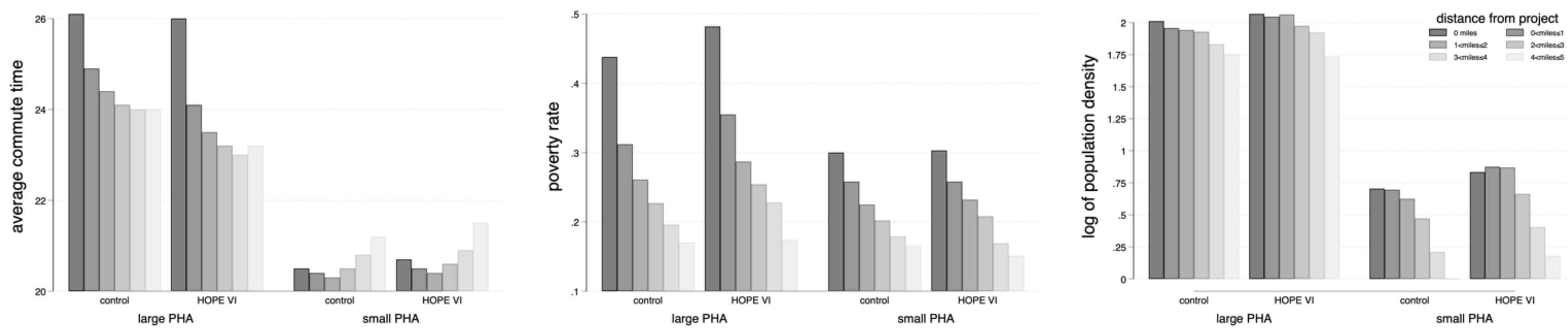

(B) Small PHAs




Notes: Panel A and B present results for large and small PHAs, respectively. The grey series with the circle markers plot the proportion of children from control projects for whom the distance between their place of residence in 2010 and the location of their project is strictly greater than the number of miles denoted on the horizontal axis (distance is calculated between the centroids of the census tracts). We use the stratification with regression estimator to estimate the effect of HOPE VI on living further than a given distance. The black series with the diamond markers depicts these results by plotting the control mean plus the estimated effect of HOPE VI. The grey lines denote the 95% confidence interval, where standard errors are clustered at the project-level.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

Figure 4: Characteristics of Surrounding Neighborhoods in 1990

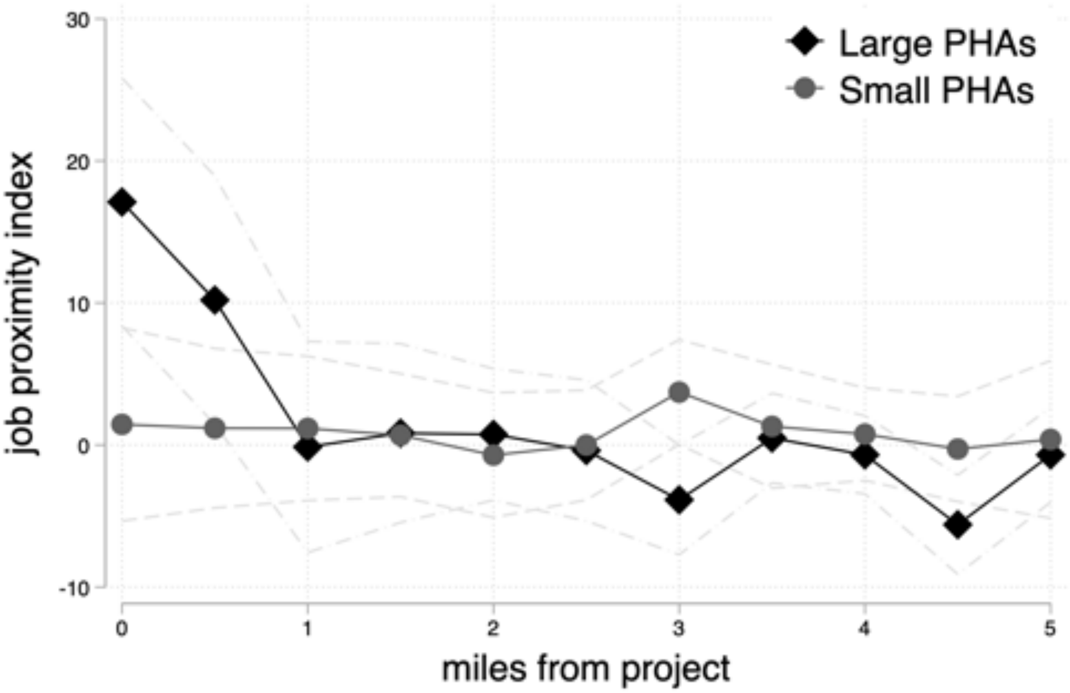
(A) Average Commute Time (B) Poverty Rate (C) Population Density




Notes: Panel A, B and C present the average commute time, poverty rate and log of the population density in the census tracts in 1990, respectively. We present characteristics of the neighborhoods surrounding the groups of projects separately by PHA size (large and small) and treatment status (HOPE VI and control). The bars present the average value of census tract characteristic within that group. The six shaded bars within each group summarize this information separately for the census tract in which the project was located as well as surrounding census tracts located within 5 miles of the project.

Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

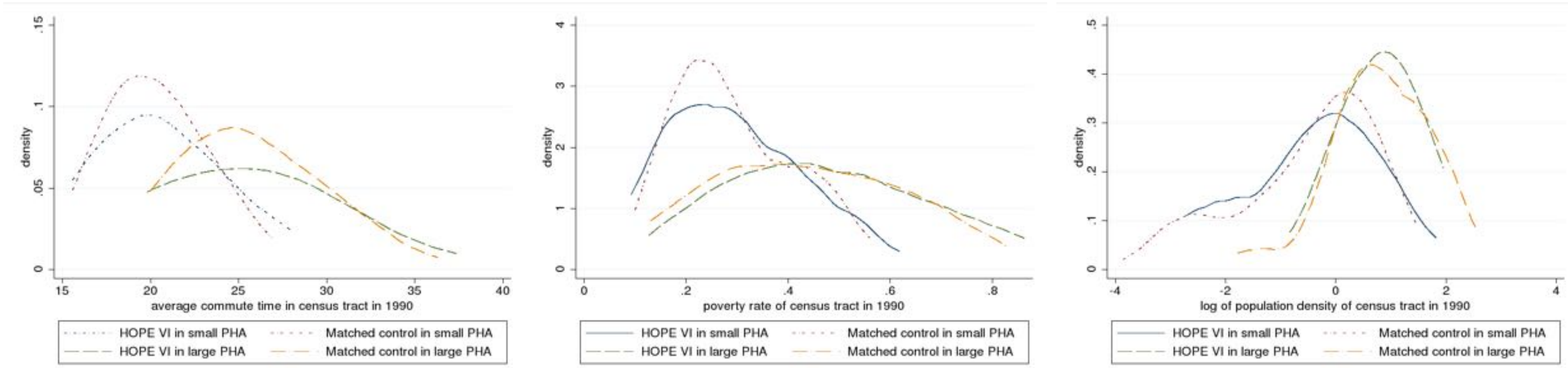
Figure 5: Effect of HOPE VI on Surrounding Neighborhoods




Notes: The black line with diamond markers and the grey line with circle markers plot the estimated effect of HOPE VI for large and small Public Housing Authorities (PHAs), respectively. Each point corresponds to results from a separate specification estimated via the stratification with regression methodology. The outcome for the points at the value of zero on the horizontal axis is the job proximity index (measured in 2010) for the census tract in which the project is located. The outcome for the remaining points correspond to the average job proximity index for other census tracts that are within the number of miles denoted on the horizontal axis (exclusive) and half a mile less than this value (inclusive). All stratum-level regressions are estimated on the child-year dataset and control for the base year in which the child appears in public housing, the year in which the child turns 26, and the standard vector of individual- and project- level covariates. Standard errors are clustered at the project level and are 95% confidence interval is depicted by the dashed light grey lines.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

Figure 6: Characteristics of Census Tracts in 1990

(A) Average Commute Time (B) Poverty Rate (C) Population Density




Notes: Panel A, B and C present kernel density plots of the average commute time, poverty rate and log of the population density in census tracts in 1990, respectively. Results are presented separately by groups defined by the interaction between PHA size (large and small) and treatment status (HOPE VI and control). All results are produced from a project-level dataset. To pass disclosure review requirements, each of the distributions are Winsorized at the 5th and 95th percentiles.

Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

# 8 Tables

Table 1: Summary Statistics of Baseline Characteristics

| | HOPE VI | All Non-HOPE VI | | | Control | | |
|---|---|---|---|---|---|---|---|
| | mean | mean | t-stat | Δ | mean | t-stat | Δ |
| **A. Neighborhood** | | | | | | | |
| median household income/1,000 | 22.0 | 27.9 | -6.68 | -0.520 | 22.9 | -0.936 | -0.085 |
| | [11.1] | [11.5] | | | [10.3] | | |
| poverty rate | 0.374 | 0.247 | 7.84 | 0.724 | 0.346 | 1.60 | 0.146 |
| | [.206] | [.14] | | | [.181] | | |
| log(population density) | 0.033 | -1.24 | 11.2 | 0.703 | -0.153 | 1.43 | 0.122 |
| | [1.42] | [2.13] | | | [1.63] | | |
| **B. Household Head** | | | | | | | |
| household income/1,000 | 9.00 | 11.3 | -6.93 | -0.515 | 9.65 | -1.77 | -0.174 |
| | [6.63] | [8.63] | | | [7.31] | | |
| age | 38.6 | 39.3 | -2.36 | -0.167 | 38.6 | -0.096 | -0.054 |
| | [10.1] | [10.1] | | | [10] | | |
| female | 0.904 | 0.869 | 2.46 | 0.409 | 0.899 | 0.338 | 0.044 |
| | [.294] | [.338] | | | [.301] | | |
| married | 0.078 | 0.133 | -4.52 | -0.436 | 0.081 | -0.241 | -0.014 |
| | [.268] | [.34] | | | [.273] | | |
| has disability | 0.113 | 0.121 | -1.64 | -0.063 | 0.111 | 0.284 | -0.003 |
| | [.316] | [.326] | | | [.314] | | |
| number of dependents | 2.76 | 2.54 | 4.24 | 0.454 | 2.63 | 2.27 | 0.273 |
| | [1.56] | [1.4] | | | [1.46] | | |
| white non-Hispanic | 0.064 | 0.207 | -12.0 | -0.709 | 0.079 | -1.16 | -0.034 |
| | [.244] | [.405] | | | [.27] | | |
| black non-Hispanic | 0.684 | 0.522 | 3.67 | 0.660 | 0.692 | -0.151 | 0.041 |
| | [.465] | [.5] | | | [.462] | | |
| Hispanic | 0.161 | 0.184 | -0.575 | -0.083 | 0.152 | 0.206 | -0.056 |
| | [.368] | [.387] | | | [.359] | | |
| **C. Children** | | | | | | | |
| age | 13.6 | 13.6 | -2.33 | -0.288 | 13.5 | 0.300 | -0.232 |
| | [2.58] | [2.57] | | | [2.58] | | |
| female | 0.509 | 0.507 | 0.431 | -0.137 | 0.512 | -0.631 | -0.124 |
| | [.5] | [.5] | | | [.5] | | |
| has disability | 0.020 | 0.027 | -2.79 | -0.077 | 0.020 | -0.011 | -0.020 |
| | [.14] | [.161] | | | [.14] | | |

Notes: This table presents summary statistics for the baseline variables listed in the rows. The variables in Panel A, B and C are characteristics of: (A) the census tract in which the projects were located measured in 1990, (B) the households or head of households and (C) the children. Column 1 presents the mean for the HOPE VI sample. Columns 2-4 (5-7) present statistics calculated from a sample that include all non-HOPE VI (control) projects. Columns 2 and 5 present the mean of the non-HOPE VI projects. Columns 3 and 6 present the t-statistic from a regression of the baseline variable in the row on an indicator for HOPE VI. Standard errors are clustered at the project level. Columns 4 and 7 present the normalized difference of the row variable between the HOPE VI and non-HOPE VI observation. Normalized differences are calcualted from data collapsed to the project level and are defined as $\Delta = (\bar{x}_1 - \bar{x}_0)/(\sqrt{(s_1^2 + s_0^2)/2})$, where $\bar{x}_d$ and $s_d$ is the sample average and variance for HOPE VI (d=1) and non-HOPE VI (d=0) observations, respectively. The standard deviation for each sample is presented in brackets below the mean.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

Table 2: Earnings Outcomes

| | qrtrs worked (1) | worked 4 qrtrs (2) | earnings / 1,000 (3) | IHS earnings (4) |
|---|---|---|---|---|
| **A. All PHAs** | | | | |
| HOPE VI | 0.057*** | 0.016*** | 0.622** | 0.142** |
| | (0.021) | (0.006) | (0.282) | (0.056) |
| control mean | 2.16 | 0.404 | 8.33 | 6.3 |
| | [1.73] | [0.482] | [34] | [4.53] |
| observations | 258,000 | 258,000 | 258,000 | 258,000 |
| **B. Large PHAs** | | | | |
| HOPE VI | 0.076*** | 0.019*** | 0.529* | 0.195*** |
| | (0.027) | (0.007) | (0.287) | (0.073) |
| control mean | 2.14 | 0.4 | 8.44 | 6.24 |
| | [1.73] | [0.481] | [40.5] | [4.55] |
| observations | 149,000 | 149,000 | 149,000 | 149,000 |
| **C. Small PHAs** | | | | |
| HOPE VI | 0.022 | 0.009 | 0.794 | 0.045 |
| | (0.035) | (0.009) | (0.601) | (0.087) |
| control mean | 2.2 | 0.41 | 8.12 | 6.4 |
| | [1.72] | [0.483] | [16.4] | [4.48] |
| observations | 109,000 | 109,000 | 109,000 | 109,000 |

Notes: Panels A, B, and C present estimates from the stratification with regression estimator for all, large, and small Public Housing Authorities (PHAs), respectively. All outcome variables are annual labor market outcomes measured in the year in which the child turns 26. In columns 1-4 the outcome variables are: the number of quarters worked, an indicator equal to one if the child had positive earnings for all four quarters, earnings/1,000 Winsorized at the $99^{th}$ percentile, and the inverse hyperbolic sine (IHS) of earnings. All stratum-level regressions control for the base year in which the child appears in public housing, the year in which the child turns 26, and the standard vector of individual- and project-level covariates. Standard errors are clustered at the project level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of stratum-level statistics, where the weights are proportional to the number of treated individuals in a strata.

Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

*** p≤0.01, ** p≤0.05, * p≤0.10.

Table 3: Heterogeneous Effects by Demographics, for Large PHAs

| | IHS of Earnings at Age 26 | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| HOPE VI | 0.189 | 0.071 | -0.194 | 0.059 |
| | (0.119) | (0.085) | (0.236) | (0.085) |
| HOPE VI×(18-age at demolition) | 0.002 | | | |
| | (0.020) | | | |
| HOPE VI×male | | 0.254** | | |
| | | (0.122) | | |
| HOPE VI×black | | | 0.425* | |
| | | | (0.252) | |
| HOPE VI×Hispanic | | | 0.422 | |
| | | | (0.326) | |
| HOPE VI×other | | | 0.354 | |
| | | | (0.295) | |
| HOPE VI×male×non-white | | | | 0.287** |
| | | | | (0.126) |
| observations | 149,000 | 149,000 | 149,000 | 149,000 |

Notes: The table presents estimates from the stratification with regression estimator for large Public Housing Authorities (PHAs) only. The outcome variable is the inverse hyperbolic sine (IHS) of annual earnings measured in the year the child turns 26. Each column presents results from a seperate regression in which the inidcator for HOPE VI is interacted with a different individual-level variable. Note that there are four mutually exclusive race/ethnicity categories, including: white (non-Hispanic), black (non-Hispanic), Hispanic, and other non-Hispanic. All stratum-level regressions control for the base year in which the child appears in public housing, the year in which the child turns 26 and the standard vector of individual- and project-level covariates. Standard errors are clustered at the project-level and are presented in parentheses.

Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

*** p≤0.01, ** p≤0.05, * p≤0.10.

Table 4: Pseudo Treatment

| | Similar Projects | | Failed Applicants | |
|---|---|---|---|---|
| | qrtrs worked (1) | IHS earnings (2) | qrtrs worked (3) | IHS earnings (4) |
| **A. Large PHAs** | | | | |
| pseudo treatment effect | 0.017 | 0.052 | -0.058 | -0.142 |
| | (0.032) | (0.084) | (0.037) | (0.091) |
| control mean | 2.170 | 6.330 | 2.200 | 6.410 |
| control s.d. | [1.730] | [4.530] | [1.720] | [4.490] |
| observations | 128,000 | 128,000 | 99,000 | 99,000 |
| **B. Small PHAs** | | | | |
| pseudo treatment effect | 0.001 | 0.053 | -0.028 | -0.070 |
| | (0.037) | (0.096) | (0.027) | (0.071) |
| control mean | 2.240 | 6.470 | 2.220 | 6.420 |
| control s.d. | [1.730] | [4.500] | [1.730] | [4.490] |
| observations | 89,000 | 89,000 | 124,000 | 124,000 |

Note: Panels A and B present estimates from large and small Public Housing Authorities (PHAs), respectively. In columns 1-2 the pseudo treatment group is the failed applicants, which are projects that applied for but never received HOPE VI funding (either the Revitalization or Demolition program). In columns 3-4 the pseudo treatment group includes similar projects in the same PHA, where similar projects are selected using the esitmated propensity score. The outcome variables in the odd and even numbered columns are annual labor market outcomes measured in the year in which the child turns 26 including number of quarters worked and the inverse hyperbolic sine (IHS) of earnings, respectively. All stratum-level regressions control for the base year in which the child appears in public housing, the year in which the child turns 26, and standard vector of individual- and project-level characteristics. Standard errors are clustered at the project-level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of stratum-level statistics, where the weights are proportional to the number of treated individuals in a strata.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.
*** p$\leq$0.01, ** p$\leq$0.05, * p$\leq$0.10.

Table 5: Householder Housing Outcomes

| | Housing Type | | | | Moved to New | |
|---|---|---|---|---|---|---|
| | same project (1) | voucher (2) | other public (3) | non-subsidized (4) | county (5) | tract (6) |
| **A. Large PHAs** | | | | | | |
| **A1. 1 year after** | | | | | | |
| HOPE VI | -0.115*** | 0.014*** | 0.023* | 0.077*** | | |
| | (0.028) | (0.005) | (0.013) | (0.024) | | |
| control mean | 0.754 | 0.026 | 0.035 | 0.185 | | |
| **A2. 3 years after** | | | | | | |
| HOPE VI | -0.135*** | 0.085*** | 0.062*** | -0.011 | -0.018** | 0.085*** |
| | (0.024) | (0.017) | (0.015) | (0.015) | (0.008) | (0.033) |
| control mean | 0.479 | 0.073 | 0.068 | 0.379 | 0.100 | 0.535 |
| **A3. 5 years after** | | | | | | |
| HOPE VI | -0.150*** | 0.098*** | 0.059*** | -0.007 | -0.029*** | 0.130*** |
| | (0.027) | (0.019) | (0.018) | (0.018) | (0.010) | (0.028) |
| control mean | 0.332 | 0.099 | 0.084 | 0.485 | 0.142 | 0.646 |
| observations | 87,000 | 87,000 | 87,000 | 87,000 | 87,000 | 87,000 |
| **B. Small PHAs** | | | | | | |
| **B1. 1 year after** | | | | | | |
| HOPE VI | -0.019 | 0.016*** | 0.017** | -0.014 | | |
| | (0.022) | (0.006) | (0.008) | (0.020) | | |
| control mean | 0.697 | 0.027 | 0.027 | 0.248 | | |
| **B2. 3 years after** | | | | | | |
| HOPE VI | -0.207*** | 0.092*** | 0.106*** | 0.009 | 0.016 | 0.125*** |
| | (0.023) | (0.018) | (0.021) | (0.019) | (0.011) | (0.030) |
| control mean | 0.416 | 0.071 | 0.056 | 0.458 | 0.134 | 0.523 |
| **B2. 5 years after** | | | | | | |
| HOPE VI | -0.184*** | 0.107*** | 0.090*** | -0.013 | 0.021 | 0.172*** |
| | (0.017) | (0.018) | (0.014) | (0.014) | (0.014) | (0.025) |
| control mean | 0.283 | 0.101 | 0.067 | 0.549 | 0.171 | 0.621 |
| observations | 66,000 | 66,000 | 66,000 | 66,000 | 66,000 | 66,000 |

Notes: Panels A and B present estimates from the stratification with regression estimator for large and small Public Housing Authorities (PHAs), respectively. Outcomes are measured: one year after the reference year in panels A1 and B1, three years after the reference year in panels A2 and B2, and five years after the reference year in panels A3 and B3. The outcomes in columns 1-4 are indicator variables with a value equal to one if the head of household appears in the same project, other public housing, voucher housing, or other housing after the reference year (categories are mutually exclusive). In columns 5-7 the outcomes are indicators equal to one if the head of household moved to a new state, county, and census tract, respectively. Each stratum-level regression contains a fixed effect for the base year in which the household appears in public housing as well as the standard set of project- and individual-level covariates. Standard errors are clustered at the project-level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of strata-level statistics, where the weights are proportional to the number of treated individuals in a strata.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.
*** p≤0.01, ** p≤0.05, * p≤0.10.

Table 6: Householder Neighborhood Outcomes

| | school proficiency index (1) | share white non-Hispanic (2) | poverty rate (3) | change in poverty rate (4) | upward mobility from Opportunity Atlas (5) | jobs proximity index (6) |
|---|---|---|---|---|---|---|
| **A. Large PHAs** | | | | | | |
| HOPE VI | 0.084 | 0.022 | -0.017 | -0.019 | -0.002 | 8.430*** |
| | (1.660) | (0.017) | (0.012) | (0.012) | (0.004) | (2.490) |
| control mean | 23.900 | 0.192 | 0.416 | -0.104 | 0.331 | 46.500 |
| | [16.900] | [0.235] | [0.181] | [0.196] | [0.054] | [19.400] |
| observations | 87,000 | 87,000 | 87,000 | 87,000 | 87,000 | 87,000 |
| **B. Small PHAs** | | | | | | |
| HOPE VI | -0.497 | 0.020 | -0.012 | -0.013 | 0.002 | 0.391 |
| | (2.130) | (0.022) | (0.011) | (0.011) | (0.004) | (1.560) |
| control mean | 30.500 | 0.367 | 0.289 | -0.060 | 0.351 | 52.900 |
| | [20.200] | [0.303] | [0.116] | [0.131] | [0.051] | [16.400] |
| observations | 66,000 | 66,000 | 66,000 | 66,000 | 66,000 | 66,000 |

Notes: Panels A and B present estimates from the stratification with regression estimator for large and small Public Housing Authorities (PHAs), respectively. All outcomes are an average characteristic of the census tracts in which the head of household resided in 1-5 years after the reference year. The characteristic in column 1 is the school proficiency index, which measures of the quality of the public schools in that area. In columns 2 and 3 the characteristics are the share of residents who are white non-Hispanic and shose income is below the poverty line, respectively. The outcome in column 4 is the difference between the poverty rate measured 1-5 years after and the baseline poverty rate. The outcome in column 5 is taken from the Opportunity Atlas and is the expected income rank of children whose parents are at the 25th percentile of the income distribution. The outcome in column 6 is the jobs proximity index created by HUD. Each stratum-level regression contains a fixed effect for the base year in which the household appears in public housing as well as the standard set of project- and individual-level covariates. Standard errors are clustered at the project-level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of strata-level statistics, where the weights are proportional to the number of treated individuals in a strata.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.
*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.10$.

Table 7: Householder Earnings Outcomes

| | 5 Years After | | 10 Years After | |
|---|---|---|---|---|
| | qrtrs worked (1) | IHS earnings (2) | qrtrs worked (3) | IHS earnings (4) |
| **A. Large PHAs** | | | | |
| HOPE VI | -0.001 | -0.065 | -0.043 | -0.134 |
| | (0.036) | (0.092) | (0.032) | (0.082) |
| control mean | 1.960 | 5.650 | 1.700 | 4.840 |
| | [1.810] | [4.800] | [1.840] | [4.950] |
| observations | 87,000 | 87,000 | 87,000 | 87,000 |
| **B. Small PHAs** | | | | |
| HOPE VI | -0.005 | 0.011 | 0.001 | 0.004 |
| | (0.039) | (0.099) | (0.041) | (0.109) |
| control mean | 2.070 | 5.880 | 1.790 | 5.050 |
| | [1.810] | [4.750] | [1.860] | [4.940] |
| observations | 66,000 | 66,000 | 66,000 | 66,000 |

Notes: Panels A and B present estimates from the stratification with regression estimator for large and small Public Housing Authorities (PHAs), respectively. All outcomes are annual labor market outcomes of the head of household measured 5 and 10 years after the reference year for columns 1-2 and 3-4, respectively. In odd and even numbered columns the outcome variables are the number of quarters worked and the inverse hyperbolic since of annual earnings, respectively. Each stratum-level regression contains a fixed effect for the base year in which the household appears in public housing as well as the standard vector of project- and individual-level covariates. Standard errors are clustered at the project-level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of strata-level statistics, where the weights are proportional to the number of treated individuals in a strata.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.
*** p≤0.01, ** p≤0.05, * p≤0.10.

Table 8: Neighborhood Outcomes

| | poverty (1) | employment (2) | social isolation index (3) | log(jobs/pop) (4) | avg. commute (5) | jobs proximity index (6) |
|---|---|---|---|---|---|---|
| **A. Large PHAs** | | | | | | |
| HOPE VI | 0.003 | -0.011** | -0.013 | 0.019** | -0.883** | 2.110** |
| | (0.007) | (0.006) | (0.03) | (0.009) | (0.388) | (0.859) |
| control mean | 0.296 | 0.516 | 1.03 | 0.285 | 27 | 48.1 |
| control sd | [0.179] | [0.122] | [0.951] | [0.347] | [6.51] | [21.2] |
| observations | 149,000 | 149,000 | 149,000 | 149,000 | 149,000 | 149,000 |
| **B. Small PHAs** | | | | | | |
| HOPE VI | -0.021*** | 0.017*** | -0.108 | 0.01 | 0.424 | -1.180* |
| | (0.007) | (0.006) | (0.084) | (0.011) | (0.351) | (0.703) |
| control mean | 0.273 | 0.509 | 1.7 | 0.318 | 24 | 51.7 |
| control sd | [0.148] | [0.112] | [1.35] | [0.317] | [5.55] | [20.2] |
| observations | 109,000 | 109,000 | 109,000 | 109,000 | 109,000 | 109,000 |

Notes: Panels A and B present estimates from the stratification with regression estimator for large and small Public Housing Authorities (PHAs), respectively. All outcomes are characteristics of the census tracts in which children resided in 2010. In columns 1-7 the outcome variable is: poverty rate, employment rate, social isolation index, log of the ratio of jobs to people, average commute time in minutes, and the jobs proximity index created by HUD. All stratum-level regressions control for the base year in which the child appears in public housing, the year in which the child turns 26, and the standard vector of individual- and project-level characteristics Standard errors are clustered at the project level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of stratum-level statistics, where the weights are proportional to the number of treated individuals in a strata.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.
*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.10$.

Table 9: Outcomes from the American Community Survey

| | IHS of wage income (1) | employed (2) | employed with: short commute (3) | employed with: long commute (4) | commute time (5) | years of education (6) | monthly rent (7) |
|---|---|---|---|---|---|---|---|
| **A. Large PHAs** | | | | | | | |
| HOPE VI | 0.315* | 0.035* | 0.028* | 0.01 | -2.120* | -0.131 | 1.41 |
| | (0.186) | (0.02) | (0.016) | (0.014) | (1.09) | (0.098) | (12) |
| control mean | 6.16 | 0.477 | 0.259 | 0.205 | 27 | 14 | 385 |
| control sd | [4.8] | [0.5] | [0.438] | [0.404] | [21.9] | [2.79] | [265] |
| observations | 17,000 | 17,000 | 17,000 | 17,000 | 8,500 | 12,000 | 11,000 |
| **B. Small PHAs** | | | | | | | |
| HOPE VI | -0.175 | -0.021 | -0.006 | -0.011 | -1.49 | 0.19 | 11.7 |
| | (0.193) | (0.023) | (0.02) | (0.021) | (1.47) | (0.145) | (16.3) |
| control mean | 6.39 | 0.502 | 0.237 | 0.253 | 24.7 | 14 | 340 |
| control sd | [4.71] | [0.5] | [0.425] | [0.435] | [25.5] | [2.92] | [235] |
| observations | 12,000 | 12,000 | 12,000 | 12,000 | 6,000 | 8,500 | 7,500 |

Notes: Panels A and B present estimates from the stratification with regression estimator for large and small Public Housing Authorities (PHAs), respectively. All outcomes are from individual-level responses to the American Community Survey. The outcomes include: (1) inverse hyperbolic sine (IHS) of total wage income; (2) an indicator equal to one if employed; (3-4) an indicator equal to one if employed and commute time is below or above the median in the control group; (5) commute time in minutes, measured for employed workers; (6) years of education, which is measured for individuals 25 and older; and (7) monthly rent, which is measured for individuals who live in rental housing. All stratum-level regressions control for the base year in which the child appears in public housing, the year in which the child turns 26, and the standard vector of individual- and project-level characteristics Standard errors are clustered at the project level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of stratum-level statistics, where the weights are proportional to the number of treated individuals in a strata.

Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.10$.

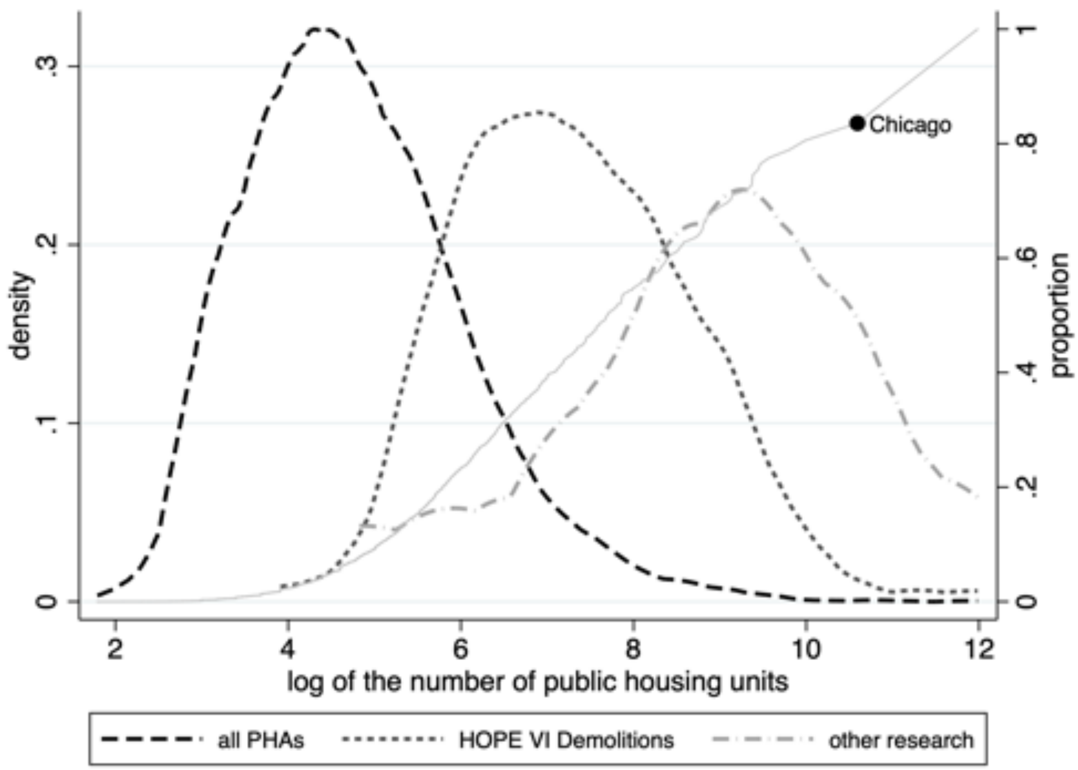
Table 10: Heterogeneous Effects by Neighborhood, for Large PHAs

| | IHS of Earnings at Age 26 | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| HOPE VI | 0.180** | 0.189*** | 0.233*** |
| | (0.072) | (0.065) | (0.071) |
| log population density | -0.110** | | |
| | (0.052) | | |
| HOPE VI × log population density | 0.192** | | |
| | (0.082) | | |
| average commute time | | 0.080 | |
| | | (0.079) | |
| HOPE VI × average commute time | | 0.146** | |
| | | (0.073) | |
| poverty rate | | | -0.011 |
| | | | (0.079) |
| HOPE VI × poverty rate | | | 0.132** |
| | | | (0.065) |
| observations | 149,000 | 149,000 | 149,000 |

Notes: The table presents estimates from the stratification with regression estimator for large Public Housing Authorities (PHAs) only. The outcome variable in all specifications is the inverse hyperbolic sine (IHS) of annual earnings measured at age 26. Columns 1-3 presents esitmates from models in which the indicator for HOPE VI is interacted with a characteristic of the census tract in which the project is located measured in 1990. For columns 1-3 these characteristics include the log of the popoulation density, the average commute time in minutes and the poverty rate, all three of which are normalized by substracting by the mean of the control group and dividing by the standard deviation of the control group. All stratum-level regressions control for the base year in which the child appears in public housing, the year in which the child turns 26, and the standard vector of individual- and project-level covariates. Standard errors are clustered at the project-level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of stratum-level statistics, where the weights are proportional to the number of treated individuals in a strata.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.
*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.10$.

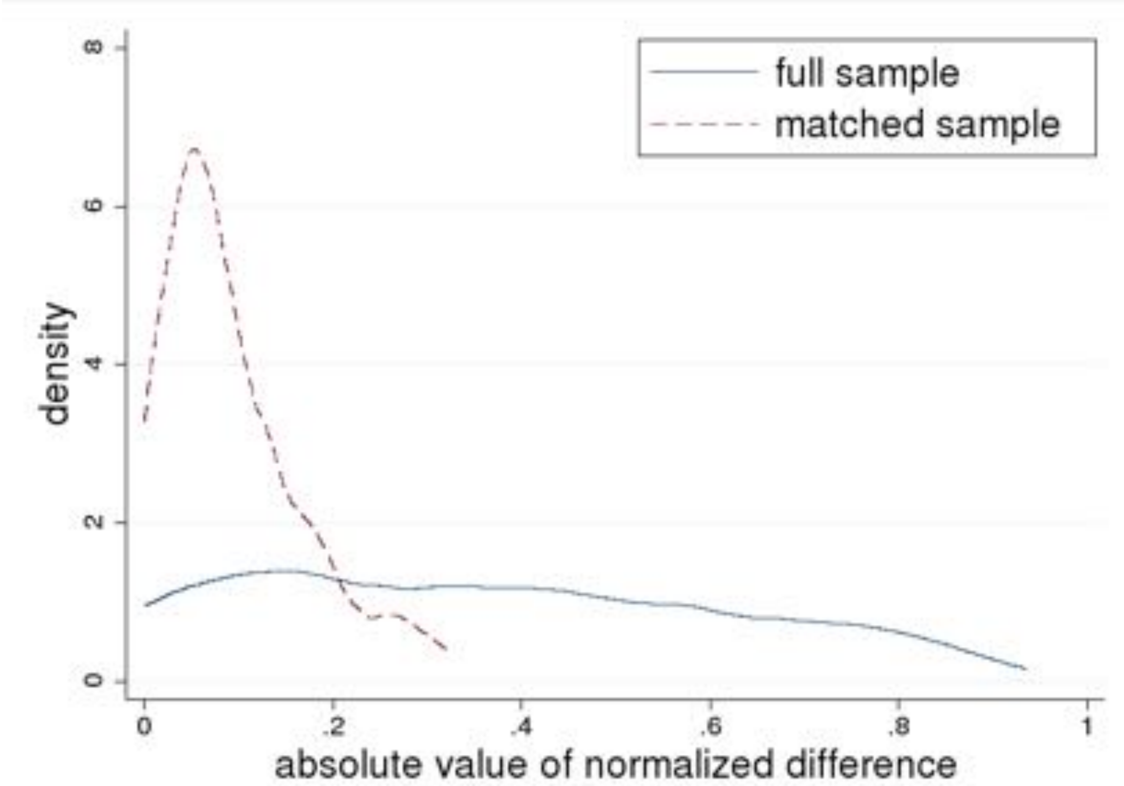
# Appendix A Additional Results

Figure A.1: PHA Size in Existing Research and HOPE VI Sample



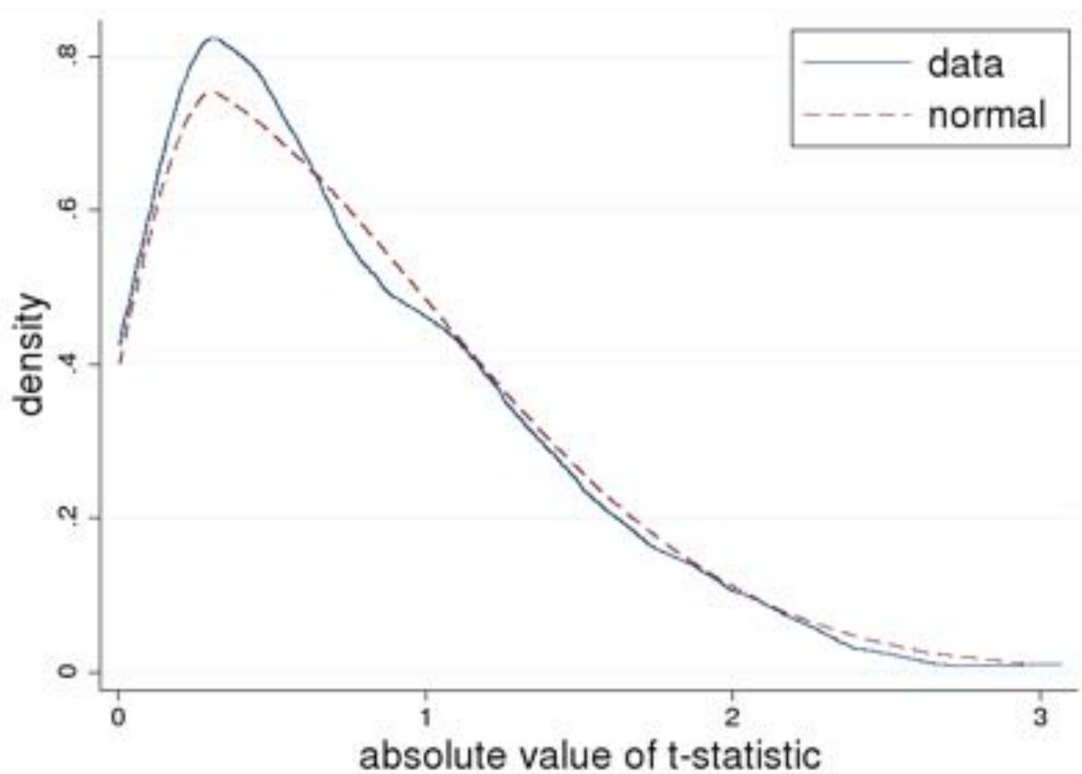

Notes: This figure plots the kernel density of the log of the total number of public housing units (measured in 1997) within each Public Housing Authority (PHA) in the U.S. The densities are plotted for three groups: all PHAs, PHAs that received a HOPE VI demolitions grant, and PHAs that participated in other randomized controlled studies. The PHAs from the other studies include: Baltimore MD, Boston MA, Chicago IL, Los Angeles CA, and New York NY from the Moving to Opportunity experiment (Ludwig et al. 2013); Chicago IL from the Gautreaux program (Rosenbaum 1995); and Atlanta GA, Augusta GA, Fresno CA, Houston TX, Los Angeles CA, and Spokane WA from the Effects of Housing Choice Voucher on Welfare Families project (Mills et al. 2006). The solid grey line presents the proportion of total public housing units in the U.S. that are located in a PHA with fewer than the number of housing units indicated on the horizontal axis. The black marker indicates the size of the Chicago PHA, which is the setting for Jacob (2004) and Chyn (2018).
Source: Authors' calculations based on data from HUD USER.

Figure A.2: Baseline Differences between HOPE VI and non-HOPE VI Observations



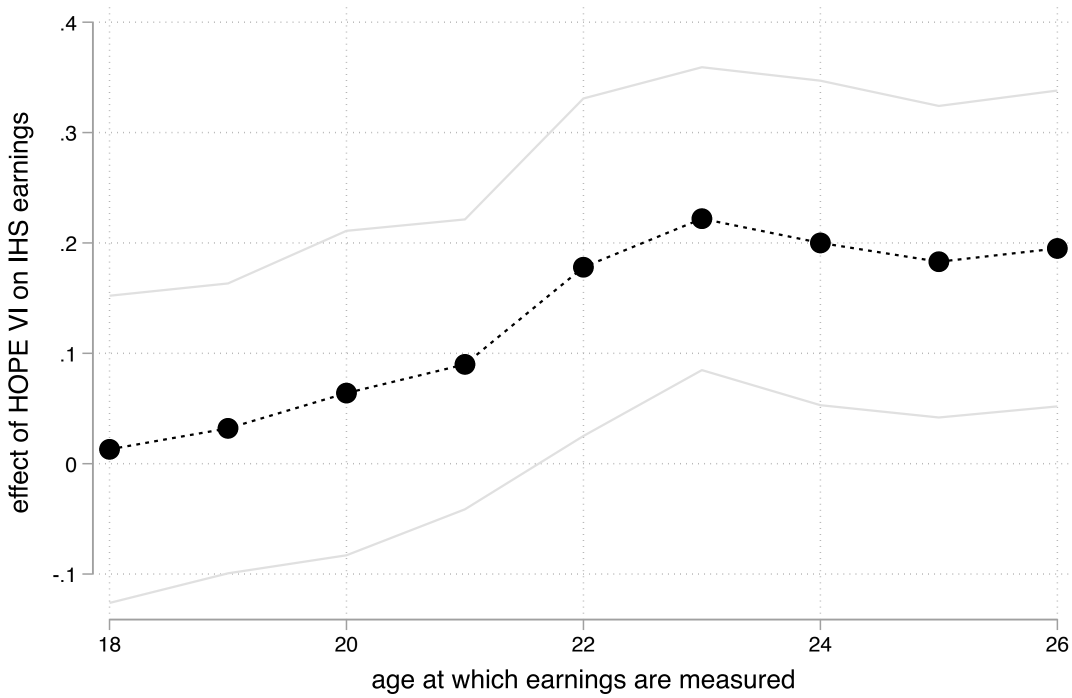

Notes: The figure presents kernel densities of normalized differences of all 92 baseline covariates calculated from the full and matched samples (see Appendix C for a description of the variables). To account for the clustered nature of the data within projects, we collapse data to the average value at the project-level to calculate the normalized difference for each variable. The normalized difference is defined as $(\bar{x}_1 - \bar{x}_0)/(\sqrt{(s_1^2 + s_0^2)/2})$, where $\bar{x}_d$ and $s_d^2$ is the sample average and variance for the HOPE VI ($d = 1$) and non-HOPE VI ($d = 0$) samples, respectively.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

Figure A.3: Baseline Differences within Strata



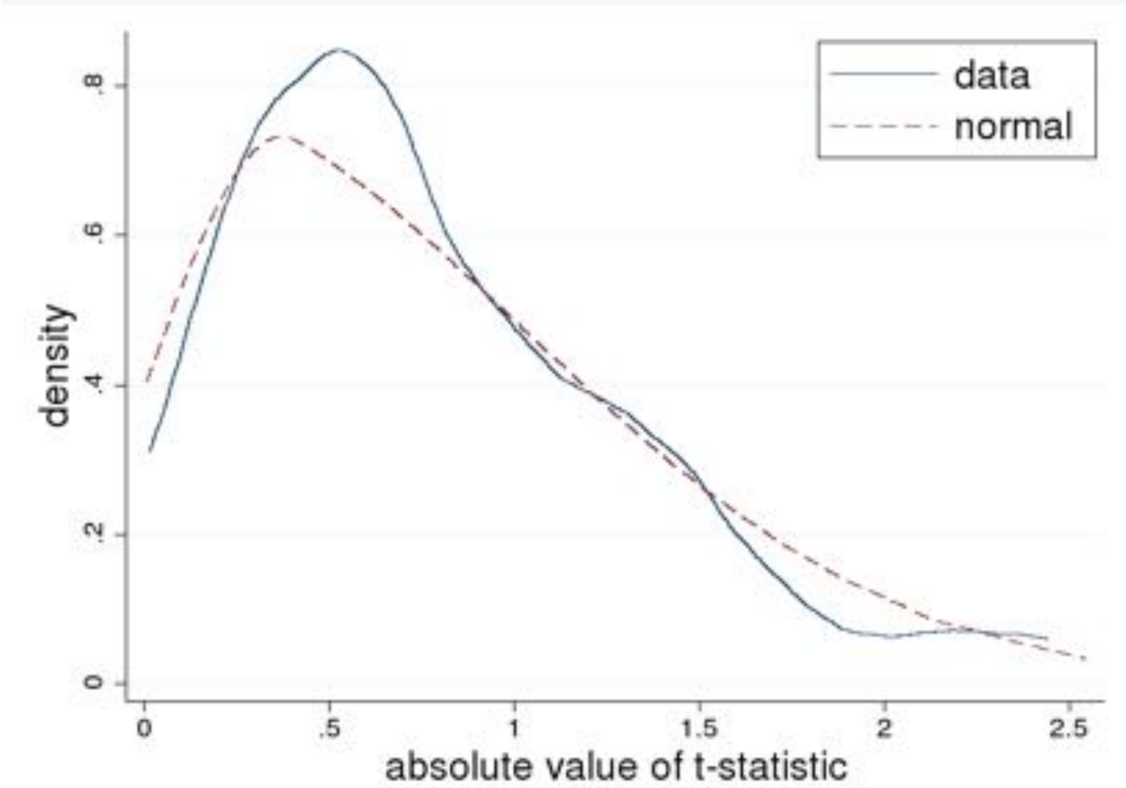

Notes: The figure presents the distribution of the absolute value of t-statistics obtained from regressing a baseline variable on an indicator for HOPE VI within each stratum. The t-statistics are calculated using the household-year dataset when the the baseline variable is measured at the household-year level and using the child-year dataset for all other variables. Standard errors are clustered at the project level. With 92 baseline variables (see Appendix C for a description of the variables) and 7 strata, the figure summarizes the distribution of 644 t-statistics. To aid interpretation, we also plot the distribution of the absolute value of t-statistics from a normal distribution. All statistics are calculated on the matched sample.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

Figure A.4: Evolution of Effect of HOPE VI on Earnings in Large PHAs




Notes: The figure presents estimates of the effect of HOPE VI on the inverse hyperbolic sine (IHS) of annual earnings measured in the year in which the child turns 18-26. Effects on earnings are estimated using the stratification with regression estimator where all stratum-level regressions control for the base year in which the child appears in public housing, the year in which earnings are measured and standard vector of individual- and project- level characteristics. Standard errors are clustered at the project level and the gray line indicates the 95% confidence interval. Estimates are for large PHAs only.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

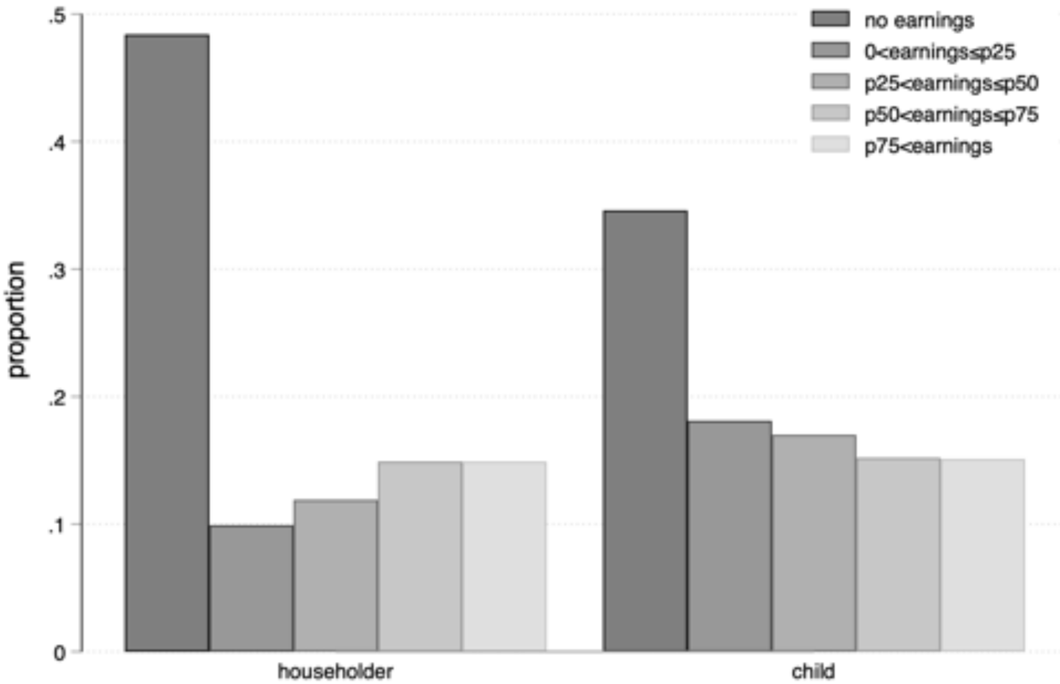
Figure A.5: Baseline Differences between HOPE VI and Failed Applicants




Notes: The figure presents the distribution of the absolute value of t-statistics obtained from regressing a baseline variable on an indicator for HOPE VI. Regressions are estimated separately for large and small PHAs and the set of non-HOPE VI projects includes only the failed applicants. The t-statistics are calculated using the household-year dataset when the the baseline variable is measured at the household-year level and using the child-year dataset for all other variables. Standard errors are clustered at the project level. With 92 baseline variables (see Appendix C for a description of the variables) and regressions with large and small PHAs, the figure summarizes the distribution of 184 t-statistics. To aid interpretation, we also plot the distribution of the absolute value of t-statistics from a normal distribution.

Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

Figure A.6: Distribution of Earnings for Householders and Children




Notes: The figure presents the proportion of householders and children whose earnings are zero or within a given quartile of the overall distribution of positive earnings. In the legend, the notation p25 denotes the 25th percentile. Parental earnings are measured 10 years after the reference year whereas the earnings of children are measured in the year they turn 26.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

Table A.1: Construction of the Sample of HOPE VI Projects

| | Criteria to Drop from Sample | Justification | Number of Projects: Dropped | Number of Projects: Remaining |
|---|---|---|---|---|
| 1 | located in U.S. territory | Our interest is in understanding how HOPE VI demolitions affected the long-term outcomes for children in the U.S. | 4 | 281 |
| 2 | defined as a scattered-site in HOPE VI award list | The majority of scattered-sites demolished under HOPE VI had fewer than 5 units demolished, although there were some scatter-sites with substantially more units demolished. In total, less than 2% of the total units demolished under the HOPE VI program were scattered sites. While there is no formal definition of scatter-site housing, the two key characteristics are low density and low concentration (Hogan 1996). | 16 | 265 |
| 3 | previously received a HOPE VI demolitions grant | Some projects received more than one grant and we limit our focus to the first awards. | 19 | 246 |
| 4 | unable to assign a project ID | The project ID is required to link records to the administrative data. | 3 | 243 |
| 5 | data irregularities due to MTW | HUD's Moving to Work (MTW) demonstration program exempted participating local housing authorities from HUD reporting requirements see Abravanel et al. (2004). | ≈50 | ≈190 |
| 6 | fewer than 15 occupied units or more than 3000 occupied units | The lower threshold limits the sample to larger public housing projects while the upper threshold ensures that we do not mistakenly group together spatially disparate projects. | ≈20 | ≈170 |
| 7 | senior housing | Given our focus on children, we drop projects if they are senior housing (over 80% of residents are above 55 years of age) or if they have no children ages 10-18 residing in them. | ≈10 | ≈160 |

Notes: This table describes the sample selection criteria that reduce the full set of 285 HOPE VI demolition awards to the (approximately) 160 awards studied in this paper. The columns present the restriction applied to the sample, the justification for imposing this restriction, the number of projects affected and the number of projects remaining. The symbol, ≈, denotes that the count is rounded according to disclosure avoidance rules of the U.S. Census Bureau.

Table A.2: Sample Size within Strata

| | PHA | | Project | | Households | | Children | |
|---|---|---|---|---|---|---|---|---|
| | HOPE VI | Control | HOPE VI | Control | HOPE VI | Control | HOPE VI | Control |
| **A. Large PHAs** | | | | | | | | |
| stratum I | $\leq 15$ | 20 | 20 | 40 | 1,500 | 7,000 | 2,500 | 13,000 |
| stratum II | $\leq 15$ | 50 | 20 | 100 | 1,500 | 17,000 | 2,000 | 32,000 |
| stratum III | 20 | 20 | 30 | 40 | 4,000 | 8,500 | 7,000 | 15,000 |
| total | 40 | 90 | 70 | 180 | 7,000 | 26,200 | 11,500 | 60,000 |
| **B. Small PHAs** | | | | | | | | |
| stratum IV | 40 | 60 | 50 | 60 | 2,500 | 4,000 | 4,000 | 7,000 |
| stratum V | 20 | 200 | 30 | 200 | 800 | 13,000 | 1,500 | 22,000 |
| stratum VI | $\leq 15$ | 40 | $\leq 15$ | 40 | 500 | 3,500 | 800 | 6,000 |
| stratum VII | $\leq 15$ | 80 | $\leq 15$ | 90 | 400 | 7,500 | 700 | 13,000 |
| total | 80 | 380 | 90 | 390 | 4,200 | 28,000 | 7,000 | 48,000 |

Notes: The table summarizes the sample size within each stratum. Projects in large and small Public Housing Authorities (PHAs) are divided into three and four distinct strata, respectively, resulting in a total of 7 strata. Panel A and B present counts for the large and small PHA's, respectively. The even and odd columns present the counts of HOPE VI and non-HOPE VI projects, respectively. Columns 1 and 2 present the unique number of PHAs, columns 3 and 4 present the unique number of projects, columns 5 and 6 present the unique number of households, and columns 7 and 8 present the unique number of children. Numbers below: 100 are rounded to the nearest 10, 999 are rounded to the nearest 50, 9,999 are rounded to the neaest 100 and 99,999 to the neaest 500. The total row is calculated using the numbers presented in the table and we use 10 to impute the count of cells with fewer the 15 observations (except for the count of HOPE VI projects in small PHAs, since we released the total count of HOPE VI projects as 160).

Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

Table A.3: Covariate Balance After Matching

| | Without Strata | | Across Strata | | Within Strata | |
|---|---|---|---|---|---|---|
| | number of tests (1) | $\lvert t\rvert > 1.645$ (2) | number of tests (3) | $\lvert t\rvert > 1.645$ (4) | number of tests (5) | $\lvert t\rvert > 1.645$ (6) |
| **Large PHAs** | | | | | | |
| Project | 16 | 0.000 | 16 | 0.000 | 48 | 0.146 |
| PHA | 19 | 0.105 | 19 | 0.105 | 57 | 0.140 |
| Neighborhood | 35 | 0.029 | 35 | 0.000 | 105 | 0.114 |
| Child | 11 | 0.091 | 11 | 0.091 | 33 | 0.091 |
| Householder | 11 | 0.091 | 11 | 0.091 | 33 | 0.061 |
| All Variables | 92 | 0.054 | 92 | 0.043 | 276 | 0.116 |
| **Small PHAs** | | | | | | |
| Project | 16 | 0.000 | 16 | 0.000 | 64 | 0.078 |
| PHA | 19 | 0.053 | 19 | 0.158 | 76 | 0.092 |
| Neighborhood | 35 | 0.029 | 35 | 0.057 | 140 | 0.043 |
| Child | 11 | 0.182 | 11 | 0.182 | 44 | 0.068 |
| Householder | 11 | 0.091 | 11 | 0.182 | 44 | 0.045 |
| All Variables | 92 | 0.055 | 92 | 0.098 | 368 | 0.062 |

Notes: Panel A and B present results for large and small Public Housing Authorities (PHAs), respectively. Each row summarizes a number of balance tests for the category of baseline variables defined by the row label (project, PHA, neighborhood, child and householder). Appendix C defines all 92 baseline variables that appear in the five categories. The even numbered columns summarize the proportion of t-statistics that are greater in absolute value than 1.645. The odd numbered columns present the number of t-statistics that contribute to calculating this proportion. All t-statistics are calculated by estimating a model in which the outcome is a baseline variable and the regoressor is an indicator for HOPE VI. The t-statistic is equal to the estimated coefficent divided by the standard error, where standard errors are clustered at the level of the project. Ordinary Least Squares (OLS) is used to estimate the t-statistics summarized in column 2. These regressions are estimated within the large and small PHA samples, but do not account for the stratification structure. The stratification with regression estimator is used to estimate the t-statistics summarized in column 4. Column 6 summarizes the t-statistics obtained from estimating OLS regressions within each of the strata. All estimates are calculated using a child-level dataset, except for the variables the correspond to the head of household household, which are estimated using the head of household-level dataset.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

Table A.4: Ordinary Least Squares Estimates

| | IHS of Earnings at Age 26 | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **A. Large PHAs** | | | | |
| HOPE VI | 0.12 | 0.146 | 0.088 | 0.288** |
| | (0.15) | (0.0991) | (0.134) | (0.111) |
| estimated with controls | no | yes | no | yes |
| non-HOPE VI sample | similar projects | similar projects | failed applicants | failed applicants |
| observations | 18,000 | 18,000 | 19,000 | 19,000 |
| **B. Small PHAs** | | | | |
| HOPE VI | -0.149 | 0.0276 | -0.037 | 0.087 |
| | (0.14) | (0.133) | (0.121) | (0.104) |
| estimated with controls | no | yes | no | yes |
| non-HOPE VI sample | similar projects | similar projects | failed applicants | failed applicants |
| observations | 11,000 | 11,000 | 13,000 | 13,000 |

Notes: Panels A and B present estimates from large and small Public Housing Authorities (PHAs), respectively. Each estimate is from a separate regression estimated by Ordinary Least Squares in which the dependent variable is the inverse hyperbolic sine (IHS) of annual earnings measured in the year the child turns 26 and the main independent variables is an indicator equal to one if the project received a HOPE VI grant. All regressions contain controls for the year in which the individual appears in public housing as well as the year in which the individual turns 26. The row below the point estimates indicates whether the standard set of additional project- and individual-level covariates are included in each specification. The sample of HOPE VI projects is identical across all specifications and the row above the observation counts indicates whether the set of non-HOPE VI projects includes observably similar projects in the same PHAs that were not awarded HOPE VI funding or projects that applied for but did not receive HOPE VI funding. Standard errors are clustered at the project-level and are presented in parentheses.

Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.10$.

Table A.5: Pseudo Outcomes Analysis

| | effect of HOPE VI on row variable | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| **Household-Level Variables** | | | |
| age | 0.242 | 0.493 | 0.333 |
| | (0.270) | (0.303) | (0.204) |
| black | 0.026 | 0.054 | 0.036 |
| | (0.042) | (0.040) | (0.030) |
| Hispanic | 0.026 | 0.004 | 0.018 |
| | (0.045) | (0.025) | (0.030) |
| white | 0.001 | -0.049** | -0.017 |
| | (0.014) | (0.024) | (0.013) |
| other non-Hispanic | -0.014 | 0.013 | -0.004 |
| | (0.021) | (0.012) | (0.014) |
| dependents | -0.001 | 0.018 | 0.006 |
| | (0.023) | (0.020) | (0.016) |
| disability | -0.005 | 0.006 | -0.001 |
| | (0.007) | (0.009) | (0.005) |
| household size | 0.037 | 0.013 | 0.028 |
| | (0.025) | (0.018) | (0.017) |
| female | 0.004 | -0.003 | 0.002 |
| | (0.007) | (0.005) | (0.005) |
| married | 0.008 | 0.002 | 0.006 |
| | (0.005) | (0.006) | (0.004) |
| income | -0.936*** | -0.790*** | -0.883*** |
| | (0.293) | (0.240) | (0.206) |
| **Child-Level Variables** | | | |
| age | 0.087** | 0.018 | 0.062** |
| | (0.039) | (0.042) | (0.029) |
| black | 0.035 | 0.057 | 0.043 |
| | (0.043) | (0.040) | (0.031) |
| Hispanic | 0.022 | 0.001 | 0.015 |
| | (0.038) | (0.026) | (0.026) |
| white | 0.001 | -0.050** | -0.017 |
| | (0.013) | (0.024) | (0.012) |
| other non-Hispanic | -0.024 | 0.015 | -0.010 |
| | (0.025) | (0.012) | (0.017) |
| disability | 0.003 | -0.005 | 0.000 |
| | (0.005) | (0.003) | (0.003) |
| female | 0.000 | -0.005** | -0.002 |
| | (0.002) | (0.002) | (0.001) |
| **Sample of PHAs** | large | small | all |

Notes: Columns 1-3 present estimates from the stratification with regression estimator for large, small, and all Public Housing Authorities (PHAs), respectively. Each row presents the results from a specification in which the variable listed in the row is the pseudo outcome. For each pseudo outcome, the entire matching procedure is implemented but the pseudo outcome (or any variable constructed using this variable) is omitted from the process. The results presented in the table are coefficients from a stratification with regression estimator, which regresses the pseudo outcome on an indicator for HOPE VI and the set of standard covariates (we omit the pseudo outcome from the covariates). Note that there are four mutually exclusive race/ethnicity categories, including: white (non-Hispanic), black (non-Hispanic), Hispanic, and other non-Hispanic. Standard errors are clustered at the project level and are presented in parentheses.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.
*** p$\leq$0.01, ** p$\leq$0.05, * p$\leq$0.10.

Table A.6: Sensitivity to Stratification and Covariates

| | IHS of Earnings at Age 26 | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **A. Large PHAs** | | | | |
| HOPE VI | 0.059 | 0.191** | 0.157 | 0.195*** |
| | (0.102) | (0.076) | (0.107) | (0.073) |
| stratification | | | X | X |
| covariates | | X | | X |
| observations | 149,000 | 149,000 | 149,000 | 149,000 |
| **B. Small PHAs** | | | | |
| HOPE VI | -0.090 | 0.015 | 0.005 | 0.045 |
| | (0.099) | (0.087) | (0.101) | (0.087) |
| stratification | | | X | X |
| covariates | | X | | X |
| observations | 109,000 | 109,000 | 109,000 | 109,000 |

Notes: Panels A and B present estimates from the stratification with regression estimator for large and small Public Housing Authorities (PHAs), respectively. The outcome variable is the inverse hyperbolic sine (IHS) of annual earnings at age 26. The rows below the point estimates indicate whether the stratification structure was used (if not, Ordinary Least Squares is used) and whether the standard vector of individual- and project-level controls are included in the regression. All specifications include a fixed effect for the base year in which the child appears in public housing as well as a fixed effect for the year in which the child turns 26. Standard errors are clustered at the project-level and are presented in parentheses.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.
*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.10$.

Table A.7: Housing Outcomes

| | Housing Subsidy Type | | | Co-Residence with Parents | | |
|---|---|---|---|---|---|---|
| | public<br>(1) | voucher<br>(2) | non-subsidized<br>(3) | census tract<br>(4) | household<br>(5) | incarcerated<br>(6) |
| **A. Large PHAs** | | | | | | |
| HOPE VI | -0.039** | 0.018* | 0.021 | -0.008 | -0.012 | 0.001 |
| | (0.02) | (0.01) | (0.019) | (0.007) | (0.007) | (0.003) |
| control mean | 0.683 | 0.089 | 0.228 | 0.523 | 0.519 | 0.047 |
| observations | 149,000 | 149,000 | 149,000 | 149,000 | 149,000 | 149,000 |
| **B. Small PHAs** | | | | | | |
| HOPE VI | -0.013 | 0.025*** | -0.012 | -0.013 | -0.006 | 0.005 |
| | (0.018) | (0.008) | (0.02) | (0.009) | (0.008) | (0.003) |
| control mean | 0.612 | 0.072 | 0.316 | 0.542 | 0.507 | 0.042 |
| observations | 109,000 | 109,000 | 109,000 | 109,000 | 109,000 | 109,000 |

Notes: Panels A and B present estimates from the stratification with regression estimator for large and small Public Housing Authorities (PHAs), respectively. All outcomes are measured in 2010. In columns 1-3 the outcome is an indicator equal to one if individual lived in public, voucher, or other housing, respectively (cateogries are mutually exclusive). In columns 4 and 5 the outcome is an indicator equal to one if the child lives in the same census tract or household as their parent, respectively. The outcome variable in column 6 is an indicator equal to one if the child was incaracerated in the 2010 census. All stratum-level regressions control for the base year in which the child appears in public housing, the year in which the child turns 26, and the standard vector of individual- and project-level characteristics Standard errors are clustered at the project-level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of stratum-level statistics, where the weights are proportional to the number of treated individuals in a strata.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.
*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.10$.

Table A.8: Dispersion of Project Co-Residents

| | share of former residents living within | | | avg. log |
|---|---|---|---|---|
| | one mile | three miles | five miles | distance |
| | (1) | (2) | (3) | (4) |
| **A. Large PHAs** | | | | |
| HOPE VI | -0.01 | -0.006 | -0.006 | 0.043 |
| | (0.006) | (0.012) | (0.016) | (0.086) |
| control mean | 0.065 | 0.193 | 0.315 | 3.29 |
| observations | 149,000 | 149,000 | 149,000 | 149,000 |
| **B. Small PHAs** | | | | |
| HOPE VI | -0.021*** | -0.02 | -0.016 | 0.047 |
| | (0.007) | (0.018) | (0.021) | (0.088) |
| control mean | 0.086 | 0.22 | 0.316 | 3.41 |
| observations | 109,000 | 109,000 | 109,000 | 109,000 |

Notes: Panels A and B present estimates from the stratification with regression estimator for large and small Public Housing Authorities (PHAs), respectively. In columns 1-3 the outcome is the share of former public housing residents that live within a one-, three- and five-mile radius, respectively. The outcome in column 4 is the average log distance (measured in miles) between the individual and each of the former public housing residents. The longitude and lattitude of residence correspond to the the centroid of the census tract in which the child resides in 2010. All stratum-level regressions control for the base year in which the child appears in public housing, the year in which the child turns 26, and the standard vector of individual- and project-level characteristics. Standard errors are clustered at the project-level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of stratum-level statistics, where the weights are proportional to the number of treated individuals in a strata.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.
*** p≤0.01, ** p≤0.05, * p≤0.10.

Table A.9: Relationship between Poverty, Job Proximity and Rent

| | 1990 Characteristics | | 2010 Characteristics | |
|---|---|---|---|---|
| | avg. commute | poverty | job proximity index | poverty |
| | (1) | (2) | (3) | (4) |
| **Panel A. Large PHAs** | | | | |
| median rent | -0.0189 | -0.5301*** | 0.0062 | -0.5616*** |
| | (0.0420) | (0.0534) | (0.0300) | (0.0310) |
| R-squared | 0.0003 | 0.1843 | 0.0000 | 0.3140 |
| observations | 13,934 | 12,865 | 13,834 | 13,834 |
| **Panel B. Small PHAs** | | | | |
| median rent | 0.1978*** | -0.4124*** | -0.1216*** | -0.5227*** |
| | (0.0716) | (0.0337) | (0.0337) | (0.0229) |
| R-squared | 0.0355 | 0.1085 | 0.0147 | 0.2722 |
| observations | 4,639 | 4,324 | 4,595 | 4,595 |

Notes: Panels A and B present results for large and small PHAs, respectively. The sample includes all census tracts located in counties that contain a HOPE VI project. Columns 1-2 present results in which the characteristics of the census tracts are measured in 1990. Column 3-4 present results in which the characteristics of the census tracts are measured in 2010. Each variable corresponds to a percentile rank that is calculated within counties and weighted by the census tract population. Within each panel each column presents results from a separate regression in which the outcome variable is listed in the column header and the independent variable is listed in the row. Standard errors are clustered at the level of the county.

Source: Authors' calculations based on publicly available data from the 1990 and 2010 Decennial Files and HUD.

*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.10$.

Table A.10: Neighborhood Job Density

| | log job density<br>(1) | log population density<br>(2) | log job/population<br>(3) |
|---|---|---|---|
| **A. Large PHAs** | | | |
| HOPE VI | 0.045 | -0.617*** | 0.220** |
| | (0.028) | (0.167) | (0.087) |
| control mean | 0.07 | -1.4 | 0.256 |
| observations | 149,000 | 149,000 | 149,000 |
| **B. Small PHAs** | | | |
| HOPE VI | -0.005 | 0.081 | -0.001 |
| | (0.014) | (0.171) | (0.056) |
| control mean | 0.053 | -2.77 | 0.354 |
| observations | 109,000 | 109,000 | 109,000 |

Notes: Panels A and B present estimates from the stratification with regression estimator for large and small Public Housing Authorities (PHAs), respectively. In columns 1-3 the outcome variable is a characteristic (measured in 2010) of the census tract in which the project was located, including: the log of the job density, the log of the population density and the log of the ratio of jobs to population, respecitvely. All stratum-level regressions control for the base year in which the child appears in public housing, the year in which the child turns 26, and the standard vector of individual- and project-level characteristics Standard errors are clustered at the project-level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of stratum-level statistics, where the weights are proportional to the number of treated individuals in a strata.
Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.
*** p≤0.01, ** p≤0.05, * p≤0.10.

Table A.11: Heterogeneous Effects by Neighborhood, for Small PHAs

| | IHS of Earnings at Age 26 | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| HOPE VI | 0.069 | 0.059 | 0.074 |
| | (0.096) | (0.096) | (0.083) |
| log population density | 0.050 | | |
| | (0.056) | | |
| HOPE VI × log population density | -0.166 | | |
| | (0.102) | | |
| average commute time | | 0.424*** | |
| | | (0.131) | |
| HOPE VI × average commute time | | -0.025 | |
| | | (0.084) | |
| poverty rate | | | -0.112** |
| | | | (0.131) |
| HOPE VI × poverty rate | | | -0.061 |
| | | | (0.079) |
| observations | 109,000 | 109,000 | 109,000 |

Notes: The table presents estimates from the stratification with regression estimator for small Public Housing Authorities (PHAs) only. The outcome variable in all specifications is the inverse hyperbolic sine (IHS) of annual earnings measured at age 26. Columns 1-3 presents esitmates from models in which the indicator for HOPE VI is interacted with a characteristic of the census tract in which the project is located measured in 1990. For columns 1-3 these characteristics include the log of the popoulation density, the average commute time in minutes and the poverty rate, all three of which are normalized by substracting by the mean of the control group and dividing by the standard deviation of the control group. All stratum-level regressions control for the base year in which the child appears in public housing, the year in which the child turns 26, and the standard vector of individual- and project-level covariates. Standard errors are clustered at the project-level and are presented in parentheses. The mean and standard deviation, presented in brackets, of the outcome for the control group are a weighted aggregate of stratum-level statistics, where the weights are proportional to the number of treated individuals in a strata.

Source: Authors' calculations from matched Longitudinal Employer-Household Dynamics, Department of Housing and Urban Development, and Decennial Census files.

*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.10$.

# Appendix B. Description of Data

### *B.1 Data Sources*

HUD-PIC tracks public housing and voucher recipients during our study period. As part of their housing occupancy verification process, PHAs provide HUD with the identities of residents, which HUD then compiles into an annual relational database. Absent the coverage limitations we discuss below, these files record every individual participating in public or voucher housing in each year between 1997 and 2010. Our analysis makes use of the individual- and household-level files, which include indicators of subsidy type (public or voucher), identifiers for housing authorities and projects, and some individual- and household-level demographic information. HUD provides a public use summary of these data through the HUD User web tool, which we use to calculate PHA-level characteristics.

Data from the LEHD program are based on two sources provided by states on a quarterly basis: (1) unemployment insurance (UI) wage records, providing the earnings of each worker at each employer, and (2) employer account reports providing establishment-level data, also known as the Quarterly Census of Employment and Wages (and formerly as the ES-202 program). The state-provided data cover more than 95 percent of wage and salary civilian jobs, including both private sector and state and local government workers. Some omissions remain, including the armed forces, self-employment, the postal service, federal workers, and some non-profit and agricultural workers (US BLS 1997, 2017). Nevertheless, the LEHD data enable us to track a large set of children into adulthood and measure their labor market outcomes as well as these outcomes for the parents of these children. The coverage extends from the beginning of state reporting through the last quarter of 2016.[1] See Abowd et al. (2004) for more detail on the LEHD.

Another strength of our data is our ability to track the residential location of households who leave subsidized housing. We do this using two sources. First, we use a measure of annual residential location from the Composite Person Record (CPR), a Census Bureau file created from

[1] We code earnings as missing if the state in which their project is located was not yet reporting in the LEHD. However, the vast majority of states are reporting to the LEHD by 2005, which is the earliest year in which we measure age 26 earnings for the children. For the small fraction of children who have missing age 26 earnings due to state-level data availability, we impute these values using earnings from later years. Specifically, we use a panel of non-missing earnings data for all children between ages 18 and 30 to estimate a regression of annual earnings on an individual fixed effect and a third-order polynomial in age interacted with sex. We use the estimates to impute missing earnings data at ages 18-26.

several federal administrative datasets, which begins in 1999 (Graham et al., 2017). We identify a residence census tract for each child and adult from 1999-2010 where available (approximately 10 percent of children are missing a CPR residence in each year). Second, we use responses from the 2010 Decennial Census to identify where individuals lived in April 2010. These responses provide an additional data source covering geographic residence of each individual, and also allow us to determine whether that individual is incarcerated in 2010.

We also draw on several publicly available data sources. Most importantly, we characterize the neighborhoods in which individuals live and projects are located using census tract-level characteristics drawn from the 1990, 2000, and 2010 Decennial Censuses and five-year-average data from the American Community Survey collected between 2008 and 2012;[2] county-level unemployment rates from the U.S. Bureau of Labor Statistics Local Area Unemployment Statistics program; area median income and characteristics of PHAs in 1997 from HUD User; the number of jobs per census tract in 2010, by workplace and residence, from the LEHD Origin-Destination Employment Statistics (LODES) dataset; school proficiency and jobs proximity indices constructed using data from 2013-2014 and provided through HUD Open Data (the job proximity index is based on LODES); intergenerational mobility measures drawn from the Opportunity Atlas (Chetty et al. 2018); land areas as well as crosswalks between various measures of geographies from the U.S. Census Bureau's Geography Relationship Files; and the Census Bureau Gazetteer files to measure the latitude and longitude (or internal point) of census tracts. We use the Consumer Price Index-Urban from the U.S. Bureau of Labor Statistics to convert all dollar amounts into 2000 dollars.

### *B.2 Description of Variables*

| Description | Unit of Measurement | Used in Nearest-Neighbor Matching | Used in Propensity Score Estimation |
|---|---|---|---|
| average total household income | public housing project | X | X |
| proportion of householders who are black non-Hispanic | public housing project | X | X |
| proportion of householders who are white non-Hispanic | public housing project | X | X |

[2] Tract-level data based on Census Bureau surveys are obtained from IPUMS (Ruggles et al., 2019).

| | | | |
|---|---|---|---|
| log of the number of occupied units | public housing project | X | X |
| proportion of householders who are disabled | public housing project | X | X |
| proportion of householders who are married | public housing project | X | X |
| proportion of householders who are female | public housing project | X | X |
| average count of individuals per housing unit | public housing project | | X |
| proportion of householders who are Hispanic | public housing project | | X |
| average age of householders | public housing project | | X |
| proportion of householders who are over 55 | public housing project | | |
| proportion of householders who are other race/ethnicity (not white, not black not Hispanic) | public housing project | | |
| average number of dependents per household | public housing project | | |
| proportion of children with disability | public housing project | | |
| proportion of residents who are children (under age 18) | public housing project | | |
| average gross household rent per month | public housing authority | X | X |
| proportion of households with majority of income from wages and or business income | public housing authority | X | X |
| log of the number of available units | public housing authority | | X |
| proportion of units that are occupied | public housing authority | | |
| proportion of household reporting | public housing authority | | |
| average household size | public housing authority | | |
| average federal spending per unit per month | public housing authority | | |
| average total household income | public housing authority | | |
| proportion of households with majority of income from welfare | public housing authority | | |
| average of household's income as a percent of local median income | public housing authority | | |
| proportion of householders (or spouse) who are under 25 years old | public housing authority | | |
| proportion of householders who are older than 62 years old | public housing authority | | |

| | | | |
|---|---|---|---|
| proportion minority | public housing authority | | |
| proportion black, non-Hispanic | public housing authority | | |
| proportion Hispanic | public housing authority | | |
| proportion householders (with children) married | public housing authority | | |
| proportion of householders (with children) who are single parents | public housing authority | | |
| proportion "over-housed" (with more bedrooms than people) | public housing authority | | |
| average assets | public housing authority | | |
| average number of months since manager reported on household | public housing authority | | |
| Median Family Income or (area median income), on which HUD bases income limits | county 1990 | X | X |
| unemployment rate in 1996 | county | X | X |
| average pay in 1996 | county | | X |
| unemployment rate in 1990 | county | | |
| average pay in 1990 | county | | |
| unemployment rate in 1991 | county | | |
| average pay in 1991 | county | | |
| unemployment rate in 1992 | county | | |
| average pay in 1992 | county | | |
| unemployment rate in 1993 | county | | |
| average pay in 1993 | county | | |
| unemployment rate in 1994 | county | | |
| average pay in 1994 | county | | |
| unemployment rate in 1995 | county | | |
| average pay in 1995 | county | | |
| poverty rate | 1990 census tract | X | X |
| proportion of households with wage or salary income | 1990 census tract | X | X |
| proportion of households with public assistance income | 1990 census tract | X | X |
| median rent | 1990 census tract | X | X |
| median year housing structure built | 1990 census tract | X | X |
| proportion of adults with high school education only | 1990 census tract | | X |
| proportion of housing units vacant | 1990 census tract | | X |
| proportion of population living in rural area | 1990 census tract | | X |

| | | | |
|---|---|---|---|
| proportion of households with social security income | 1990 census tract | | X |
| proportion of mothers who are single | 1990 census tract | | X |
| proportion black, non-Hispanic | 1990 census tract | | X |
| proportion Hispanic | 1990 census tract | | X |
| proportion born in the US | 1990 census tract | | |
| proportion of adults with graduate degree | 1990 census tract | | |
| median income | 1990 census tract | | |
| proportion of households with interest, dividend or net rental income | 1990 census tract | | |
| median gross rent as percent of household income | 1990 census tract | | |
| proportion white, non-Hispanic | 1990 census tract | | |
| proportion senior citizen | 1990 census tract | | |
| median home value | 1990 census tract | | |
| age | child | | |
| black, non-Hispanic | child | | |
| white, non-Hispanic | child | | |
| Hispanic | child | | |
| other race/ethnicity (not white, not black not Hispanic) | child | | |
| total household income | child | | |
| head of household is married | child | | |
| female | child | | |
| disabled | child | | |
| number of dependents in the household | child | | |
| total number of people living in the household | child | | |
| age | householder | | |
| black, non-Hispanic | householder | | |
| white, non-Hispanic | householder | | |
| Hispanic | householder | | |
| other race/ethnicity (not white, not black not Hispanic) | householder | | |
| total household income | householder | | |
| married | householder | | |
| female | householder | | |
| disabled | householder | | |
| number of dependents in the household | householder | | |
| total number of people living in the household | householder | | |

Notes: This table defines all of the 92 baseline variables mentioned throughout the paper. The third and fourth column indicate whether the variable was included in the matching specification used to trim the sample and considered in the propensity score estimation, respectively. Variables at the level of the public housing project are created using the microdata from the HUD-PIC files. For recipients of the HOPE VI demolition grants, the values correspond to the year of the demolition, for other projects the values correspond to the average values between 1997 and 2001. The variables at the level of the public housing authority are based on public use data made available through the HUD User web tool. Family median income is also based on public use data obtained through the HUD User web tool. The unemployment rate and average earnings are from public use data made available by the Bureau of Labor Statistics. The characteristics of the census tracts in 1990 are derived from the 1990 Decennial Census and are from public use data provided by IPUMS (see Ruggles et al., 2019). The variables at the child and householder level are from the HUD-PIC files.

### *B.3 References*

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# Appendix C. Additional Robustness Checks

**Revitalization Program:** While the focus of our paper is on the HOPE VI Demolitions program, there is some overlap with the HOPE VI Revitalizations program by which some projects received both Demolition and Revitalization grants. Typically, the award of the Revitalization grants and their implementation took place well after the Demolition grant, but it is possible that our estimates are affected by the Revitalizations program. To investigate this, we start by estimating two specifications. First, we estimate a specification in which we interact the indicator for the HOPE VI Demolitions award with an indicator for the Revitalizations program. Second, we estimate a specification in which we drop all projects that received a HOPE VI Revitalization grant.[1] We conduct both sets of analyses separately within large and small PHAs and the inverse hyperbolic sine of age 26 earnings is the outcome variable. For the first specification, the coefficient on the interaction term is statistically insignificant at the 10% level for both the large and small PHA samples. In other words, there is not a statistically significant difference between the treatment effects for projects awarded both a Revitalization and Demolition and projects that were only awarded a Demolitions grant. The results from both the first and second specifications indicate that the effect of the HOPE VI Demolition is positive and statistically significant at the 10% level in large PHAs and is positive but statistically insignificant in small PHAs. Taken together, these analyses suggest that our main estimates are not affected by the HOPE VI Revitalization program. Another important result in our paper is that the effect of HOPE VI is largest for projects that were located in neighborhoods characterized by high poverty rates and limited job accessibility (proxied by population density and commute time). To see if these results are sensitive to the presence of the Revitalization program we estimate a model in which we interact the HOPE VI indicator with both an indicator for the Revitalization program and baseline characteristics of the neighborhood. Our main findings with respect to heterogeneous effects by neighborhood characteristics are robust to controlling for the Revitalization program. Specifically, the interaction term between HOPE VI and the baseline characteristics is positive and statistically significant at the 10% level in large

[1] Note that in constructing the sample we already drop non-HOPE VI Demolition projects that receive a Revitalization grant. Thus, this restriction only drops projects that received both a Demolition and Revitalization grant.

PHAs for all three baseline characteristics, whereas this interaction term is never statistically significant in small PHAs.

**Early Demolitions:** We define the year of the demolition as two years prior to the award year. However, the microdata begins in 1997 and we therefore define the year of the demolition as 1997 for HOPE VI projects that were awarded funding before 1999. We conduct two sets of analyses to investigate whether the timing of these early demolitions impact our results. First, we estimate a specification in which we interact the HOPE VI indicator with an indicator equal to one if the award was made prior to 1999. Second, we estimate a specification in which we drop all HOPE VI projects that received funding before 1999. We conduct both sets of analyses separately within large and small PHAs and the inverse hyperbolic sine of age 26 earnings is the outcome variable. For the first specification, the coefficient on the interaction term is not statistically significant at the 10% level for both the large and small PHA samples. In other words, the treatment effect for HOPE VI projects awarded funding prior to 1999 is not statistically different from the treatment effect for HOPE VI projects awarded funding in 1999 or later. The results from both the first and second specifications indicate that the effect of HOPE VI is positive and statistically significant at the 5% level in large PHAs and is positive but statistically insignificant in small PHAs. Taken together, these results suggest that our main findings are not sensitive to how we treat the early demolitions that occur prior 1999.

**Tuning Parameters:** While our methodology is in some ways data-driven, we do select tuning parameters that govern this process. While we use standard values for most tuning parameters, we choose custom values for (1) the number of matched used in the trimming procedure and (2) the thresholds for selection of covariates to be included in the propensity score. The choice of tuning parameters was based on their ability to eliminate baseline differences between HOPE VI and non-HOPE VI projects within stratum. However, we also assess the sensitivity of our results to alternative choices of these parameters. First, we try implementing the entire stratification procedure using 3 through 8 matches (our baseline specification uses 5 matches) when creating the trimmed sample. Second, we implement the stratification procedure with alternative thresholds for the likelihood ratio test by using values for the threshold firs the first-order terms of 1.5 to 5 by intervals of 0.5 and setting the threshold value for the second-order terms to 1.71 plus the threshold for the first-order term (our main specification uses 2.5 and 4.21 for the tuning parameters related to the first-order and second-order terms, respectively). Using the resulting stratification structures, we then estimate the effect of HOPE VI on the inverse hyperbolic sine of age 26 earnings as the main outcome and do this separately for large and small PHAs. For small PHAs the effect of HOPE VI is never statistically significant at the 10% level across all fourteen specifications. For large PHAs, the point estimate is always positive and estimates in nine, twelve and thirteen of the fourteen estimates are statistically significant at the 1%, 5% and 10% level, respectively. Thus, for large PHAs only one of the fourteen estimates is not statistically significant at the 10% level. Thus, our main findings are quite robust to alternative choices of the tuning parameters.

**Duplicate Observations:** Control observations (individual-year observations) may appear in the data multiple times, as they are included for each year they appear in public housing. We cluster standard errors at the project level, which accounts for these duplicate observations within projects. However, if individuals in the control projects move to new projects, they will appear multiple times in the data and the clustering will not adequately account for the correlation in their outcomes. To assess the degree to which this is a problem we drop all individuals who appear in more than one project in our sample. We then re-estimate our main results focusing on the inverse hyperbolic sine of age 26 earnings. In large PHAs the effect of HOPE VI is positive and statistically significant at the 5% level and in small PHAs the effect of HOPE VI is positive but is not statistically significant. Thus, our main findings are not sensitive to how we account for individuals who appear in multiple projects within the sample period.

**Heterogeneous Effects:** Table 3 presents estimates of the effect of HOPE VI interacted with different individual characteristics for the large PHA sample. We replicate this analysis for the small PHA sample. With one exception, none of the interaction terms are statistically significant at the 10% level. The one exception is that, in the specification that interacts HOPE VI with race, the estimated coefficient on HOPE VI*black is negative and statistically significant at the 10% level whereas the estimated coefficient on HOPE VI is positive and statistically significant at the 10% level. Thus, there is some evidence that HOPE VI may have been beneficial for white non-Hispanic children in small PHAs.

## Appendix D. Job Accessibility, Poverty, and Rent

HOPE VI neighborhoods were outliers in terms of both job proximity and poverty. An important question then is: how the program could have induced moves to new neighborhoods that were better in terms of job accessibility but not poverty? One potential explanation, given the resource constraints faced by households participating in public housing, is that neighborhood poverty is more strongly (and negatively) associated with housing prices than job accessibility; indeed, column 7 of Table 9 suggests that HOPE VI did not lead individuals to move to neighborhoods with higher rents.

While a rigorous assessment of the rent-poverty and rent-accessibility relationships is beyond the scope of this paper, we briefly explore them using publicly available data from counties with a HOPE VI project. For each HOPE VI county we construct population-weighted percentile ranks of neighborhoods based on median rent, average commute time, and poverty rate as measured in 1990 and run bivariate OLS regressions of average commute time on median rent and neighborhood poverty. Columns 1-2 of Table A.9 present the results. Poverty and median rent are strongly negatively correlated: neighborhoods with a one percentile higher rank in terms of median rent have a 0.53 and 0.41 percentile lower poverty rate rank in large and small PHAs, respectively. There is, however, no evidence that neighborhoods with higher levels of job proximity are more expensive: in large PHAs the R-squared from a regression of the average commute time rank on the median rent rank is 0.0003, and the point estimate is small, negative (-0.0189), and not statistically distinguishable from zero.[1] The lack of a relationship between housing costs and job proximity is robust to using the job proximity index constructed by HUD, which is available only in 2010 (see columns 3-4 of Table A.9). The cross-sectional associations therefore support the idea that while moves to neighborhoods with better job accessibility may have been financially feasible for HOPE VI-affected households, higher housing prices may have made moves to lower poverty neighborhoods more difficult.

[1] In small PHAs there appears to be evidence that job proximity is negatively related to housing costs.