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

June 2024

John C. Haltiwanger, University of Maryland
Mark J. Kutzbach, Federal Deposit Insurance Corporation
Giordano Palloni, Consumer Financial Protection Bureau
Henry O. Pollakowski, Harvard University
Matthew Staiger, Opportunity Insights and Harvard University
Daniel H. Weinberg, U.S. Census Bureau (retired)

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 greatest for demolitions in high-poverty neighborhoods in large cities, the context for most prior research on HOPE VI. However, most HOPE VI projects were in smaller cities where we find weaker effects that are not statistically significant. 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 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, CBDRB-FY20-CED0060029, and CBDRB-FY2023-CES010-018). 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. We link administrative data on participation in subsidized housing with earnings data, household surveys, and data on neighborhoods. Because of limitations on when earnings data become available, we focus on children aged 10 to 18 at the time of exposure to a demolition, which allows us to estimate the effect of HOPE VI on earnings at age 26. 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

[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.

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 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. Our estimates of the long-term effects of the demolitions—which use a more nationally representative sample—are similar in magnitude. However, we find weaker evidence of earnings gains for children residing in small PHAs, suggesting that the program may have been less beneficial in this context (which is not represented in most existing research).

[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.

Several results support a causal interpretation of our estimates. Our most convincing evidence comes from a series of placebo analyses in which we study projects that are similar to the HOPE VI awardees but did not themselves receive an award. Specifically, we use three alternative sets of projects: (1) projects that applied for but never received a HOPE VI grant, (2) observably similar projects located within the same PHA as HOPE VI recipients, and (3) projects that are located within one mile of a HOPE VI project. As in our main analysis, we use the stratification with regression estimator to compare children in these three sets of "pseudo treated" projects to children in other observably similar non-HOPE VI projects and we find no difference in long-run earnings for these groups. As none of the "pseudo treated" projects were actually exposed to a HOPE VI Demolition, we interpret this as evidence that our stratification estimator successfully selects valid comparison projects. Furthermore, we find earnings gains similar to our main estimates when we compare children in the HOPE VI projects to children in projects from these three other "pseudo treated" 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). We also find no evidence of larger long-term impacts for children who were

[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.

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 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 the HOPE VI demolitions did not induce 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 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 encourage individuals on the margin between working and not working to participate in the labor market (Smith and Zenou, 2003).[5] The lack of an earnings response for parents—largely single and predominantly female—is consistent with their having a higher reservation wage than their children do as young adults. 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 far away (primarily by reducing population density).

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

[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 that 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).

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 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,

[7] On Oct. 6, 1992, P.L. 102-389 Title II funded the Homeownership and Opportunity for People Everywhere grants, listing HOPE VI (the sixth item) as the Urban Revitalization Demonstration Program.

[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

HUD awarded $392 million through 285 HOPE VI grants for the demolition of more than 57,000 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.

---

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.

[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.

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 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 (i.e., displacement costs attributable to the disruption of established routines or the separation of children from extended family and friends) 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

[12] Quotes are from NCSDPH (1992).

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 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 up through 2016 (when all children in the sample will have reached age 26) 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 for the parents of the children in the sample.[15]

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.[16] 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). We also drop approximately 50 HOPE VI projects located in PHAs that participate in HUD's Moving to Work (MTW) demonstration program, as participation in this program exempted PHAs from HUD reporting requirements (Abravanel et al., 2004). The three largest PHAs that received HOPE VI funding and participated in MTW include Chicago, Philadelphia, and Baltimore. The sample

[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] The HUD-PIC data identify the householder or reference person for each household. For simplicity, in the case of households with children aged 10 to 18, we define this individual as the parent although they may not be the legal or biological parent.
[16] 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).

restrictions, described in Table A.1, reduce the analysis sample to about 160 projects that received HOPE VI demolitions awards.[17] Our sample also includes about 8,800 non-HOPE VI projects.[18]

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 who lived in the HOPE VI project two years prior to the award date.[19] 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 A.2 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 A.2 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.

[17] Throughout the paper we often report rounded numbers to limit risk of disclosure.
[18] 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.
[19] 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.

We retain all individual-year observations for residents of non-HOPE VI projects.[20] 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 parent-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 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 parent.

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 United States. 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.

As a starting point, we use Ordinary Least Squares (OLS) regressions to compare the IHS earnings at age 26 of children living in HOPE VI projects to the earnings of those living in all non-demolished projects.[21] The estimates are presented in Table A.2. There are two key takeaways. First, controlling for baseline differences has a large impact on the estimated effect of HOPE VI: in the full sample the coefficient on HOPE VI increases from -0.24 to 0.06 when adding covariates.

[20] 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.

[21] 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 $\log[y_i+(1+y_i\text{^}2\ )\text{^}0.5\ )]$, see Burbidge et al. (1988).

Second, there is evidence of substantial heterogeneity by PHA size: when controlling for baseline differences, the coefficient on HOPE VI in small and large PHAs is -0.02 and 0.18, respectively. These OLS specifications are consistent with our main findings of gains in large PHAs and no effects in small PHAs. However, the results also highlight the importance of accounting for baseline differences.

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 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).[22] Table 1 demonstrates that many important baseline variables have normalized differences that exceed this threshold in the full sample.[23] 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 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 5.2.

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

[22] 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 $(\bar{x}_1 - \bar{x}_0)/\sqrt{(s_1^2 + s_0^2)/2}$.

[23] The solid line in Figure A.3 makes a similar point by presenting the distribution of the normalized differences for all baseline variables calculated on the full sample.

restrictions mentioned in Section 3.[24] 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, or matches, 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.3 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 household income and the proportion of parents 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

[24] 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.

the model.[25] 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 1 plots the distribution of the linearized estimated propensity score for HOPE VI and control projects.[26] 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.[27] 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.[28] 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 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 in terms of linearized propensity score are depicted by the vertical lines in Figure 1 and Table A.3 presents the sample size within each stratum. In Appendix Section

---

[25] We include additional first-order (second-order) terms only if the likelihood ratio statistic exceeds 2.50 (4.21).
[26] 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.
[27] Let p denote the propensity score, then the linearized propensity score is defined as $\ln(p/(1-p))$.
[28] We require 50 total projects in each stratum because we cluster standard errors at the project level.

B.2, we document that our sample size, built from linked microdata, is in line with publicly available data aggregations produced by HUD.

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 2 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.4 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.[29] For the most part, we find that this pattern applies. For example, column 6 of Table A.4 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.[30] We chose the tuning parameter values that are most effective at eliminating baseline differences between HOPE VI and control projects within strata.[31] There are two important features to note.

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

[30] 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.

[31] 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

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 ε is an error term which we cluster at the project level.[32] 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 parents who are Black non-Hispanic, and proportion of parents 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.[33] The standard vector of individual-level covariates included in the specifications estimated on the child-level dataset includes the interaction between sex and 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

---

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.

[32] 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.

[33] 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.

for disability; a fixed effect for age at the time of appearing in public housing; an indicator for whether the parent has a disability; an indicator for whether the parent is female; the marital status of the parent; the age of the parent, and total household income.[34] 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.[35]

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).[36]

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

[34] The standard vector of individual-level covariates included in the specifications estimated on the parent-level dataset includes age, race, sex, number of dependents, household size, disability status, marital status, and total household income.

[35] 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.

[36] We follow the methodology described by Imbens and Rubin (2015) to estimate standard errors, but there are two potential issues worth mentioning. First, an 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. Second, the standard errors do not account for uncertainty in the estimated propensity score, which affect the stratum to which observations are assigned. There is not a consensus for how to account for this source of uncertainty (e.g., Bodory et al., 2020; Abadie and Spiess, 2022); particularly for the stratification with regression estimator. As we discuss below, we find similar point estimates and standard errors when we define the stratum based on the size of the PHA only, and not on the estimated propensity score. This suggests that the standard errors in our main specification are unlikely to be severely biased.

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.

5. Results

5.1 Long-Run Effects on Children

Table 2 shows estimates from equation 1. HOPE VI led to 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) and all outcomes are annual measures at age 26. HOPE VI increased the number of quarters worked by 0.057, the probability of working all four quarters by 1.6 percentage points, and earnings by 15.3 percent.[37] We estimate that children from households affected by HOPE VI earned $622 more at age 26, relative to mean annual earnings of $8,330 for children in matched control households. As a point of reference, in 2010, the maximum Earned Income Tax Credit was $457 (in current dollars) for a household with no children and $3,050 for a household with one child (Congressional Research Service 2022).

An important part of the earnings gains appears to be driven by an extensive margin labor supply response. Using the estimates from Table 2, the control means from columns 1 and 3 indicate that the average working child from the control group earns $3,856 per quarter whereas column 1 indicates that HOPE VI increased quarters worked by 0.057. Combining these estimates we would expect the observed increase in quarters worked to increase annual earnings by $220 (3,856*0.057=$220) if the entire effect were driven by an increase in labor supply. This is about 35 percent of the estimated effect of HOPE VI on annual earnings in column 3, suggesting that intensive margin earnings gains contributed about twice as much as extensive-margin labor supply responses to overall earnings impacts.

While the overall impact of the program was positive, there appears to be heterogeneity across housing environments. Panels B and C of Table 2 present results separately for large and small PHAs. The positive impacts are generally greater in large PHAs, with differences that are often economically important in size. For example, column 4 indicates that HOPE VI increased earnings at age 26 by 21.1 percent in large PHAs but only 4.6 percent in small PHAs.[38] For each

[37] 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.

[38] The long-run benefits found in large PHAs are robust to measuring earnings at alternative times. Figure A.4 presents estimates of the effect of HOPE VI on the IHS of earnings measured between ages 18 and 26. The effect of the program

of the four outcomes we find a positive and statistically significant effect in large PHAs and a statistically insignificant effect in small PHAs. Using the extensive margin calculation (above), increased labor supply would account for 57 percent of earnings gains in large PHAs and 10 percent in small PHAs. We acknowledge that the differences between the coefficient estimates for large and small PHAs is not statistically significant at the 10 percent level for any of the outcomes (the p-value for this difference when the outcome is IHS earnings is 0.18). However, 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.[39] Column 1 indicates that the impacts of the program are no different for older and younger children.[40] 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 level effect is inconsistent with differences in human capital accumulation through the neighborhood 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

We argued above that the methodology does a good job eliminating observable differences between HOPE VI and control projects but it is possible that our results are biased by unobserved differences or functional form assumptions implicit in the stratum-level regressions. For example, HOPE VI Demolition grants may have targeted metropolitan areas or neighborhoods where young

---

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.

[39] 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.

[40] 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.

adults would have experienced gains in employment or earnings even in the absence of the program. This section assesses potential threats to identification and implements 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 three different sets of projects: (1) projects that applied for but never received funding for the HOPE VI Demolition or Revitalization programs; (2) observably similar projects located within the same PHA but further than one mile from a HOPE VI project, which are selected using propensity score matching; and (3) nearby projects that are not demolished but are located within one mile of a HOPE VI project.[41] The first set of pseudo treatment projects are useful in that they are 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. The second and third set of pseudo treatment projects help to assess the likelihood that HOPE VI grants were targeted towards metro areas or neighborhoods that would have experienced improvements in economic outcomes for young adults, even in the absence of the program.

Table 4 presents the estimates from the pseudo treatment analysis. Columns 1, 4, and 7 of Table 4 present estimates from OLS regressions of the IHS of earnings at age 26 on the pseudo treatment indicators, controlling for the year in which the individual appears in public housing and the year in which earnings are measured; the regressions are estimated on a sample that includes the pseudo treatment groups and all other non-HOPE VI projects. With the one exception of similar projects in small PHAs, these estimates are all large and negative, suggesting that children who

[41] 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 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.

reside in pseudo treated projects earn significantly less in adulthood relative to children in typical, non-HOPE VI public housing projects. Columns 2, 5, and 8 present the estimated pseudo treatment effects based on the full trimming and stratification with regression method. The estimates are never statistically different from zero and have standard errors similar in size to those from our main results in column 4 of Table 2. In other words, children who lived in the pseudo treated projects have similar earnings to those that lived in other, observably similar projects.[42] A caveat is that due to statistical imprecision, the 95 percent confidence intervals for the pseudo treatment effects when using similar projects or nearby projects in large PHAs contain the estimated effect of HOPE VI from Table 2. We do, however, reject the null of no difference between the HOPE VI treatment effect and the pseudo treatment effect based on failed applicants (p-value <0.01). Given the pre-award similarities between the pseudo treated projects and the HOPE VI projects, we also assess whether our main results are robust to using the pseudo treated projects as the control group. Columns 3, 6, and 9 present estimates from OLS specifications that compare the children from the HOPE VI projects to those from the pseudo treatment projects. These comparisons yield results similar to our main findings: large benefits for children in large PHAs and no benefits for those in small PHAs.[43] While the estimates based on the comparison to similar projects in the same PHAs are not statistically significant for the large PHA sample, they are qualitatively similar to the estimates from the stratification estimator found in column 4 of Table 2. Furthermore, Table A.5 shows that, within large PHAs, the OLS estimates are statistically significant for the other three outcome variables considered in Table 2. The lack of positive pseudo treatment estimates along with the OLS estimates 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.[44]

[42] Table A.6 presents estimated pseudo treatment effects for the other outcomes shown in Table 2.

[43] Within large and small PHAs, none of the differences between the estimates in columns 3, 6, and, 9 in Table 4 and those in column 4 of Table 2 are statistically significant at the 10 percent level, and the largest p-value corresponding to these differences is 0.48.

[44] 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.

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.7. Each row presents results for one of the 18 pseudo 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.8 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.[45] 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

[45] 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.

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 Effect on Environment During Childhood

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. However, Section 5.1 shows that impacts are not larger for children who were younger at the time of the demolition, which suggests that the children in our sample may have benefited from a mechanism other than the exposure model highlighted in existing literature. This section shows that HOPE VI led to limited improvements in environment during childhood, which explains why our results are not explained by the exposure model.

We begin by showing that HOPE VI led many families to move. Column 1 of Table 5 shows that HOPE VI led to a 15 and 18 percentage point reduction in the probability that the parent 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 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.[46] Many households in HOPE VI projects

[46] 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

ended 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 parents 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.[47] 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 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. We include the share White non-Hispanic as an outcome as prior work has investigated why families with subsidized housing often live and racially segregated neighborhoods and how these environments have shaped the adult outcomes of children (e.g., Chyn et al., 2023). 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.[48]

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.

[47] 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.

[48] 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).

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 the parents. 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. Why do we find impacts on the earnings of children but not adults? Given that many of the parents in our sample 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 parents have higher reservation wages. Figure A.6 presents the distribution of earnings for parents and the adult children. Consistent with the hypothesis that parents have a higher reservation wage, there is a hollowing out of the distribution of labor market earnings for parents relative to the adult earnings of the children in our main sample; parents 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 parents have earnings in the bottom quartile compared to 18 percent of the adult children).

Together, the results in this section provide mixed evidence for how the program impacted household and neighborhood environments. There were no improvements in parental labor market outcomes, no 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.

While it is possible that exposure to lower poverty neighborhoods could have facilitated long-term earnings gains for some children in our sample, there are two reasons it may not have played as large of a role as in previous work. First, 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. Second, on average, HOPE VI did not lead households to move to neighborhoods with drastically lower poverty rates. There are a several reasons why MTO and the demolitions in Chicago studied by Chyn (2018) find a reduction in exposure to neighborhoods poverty while we find no effect. Namely, the households in our sample did not receive assistance or face requirements to move to low poverty neighborhoods; whereas MTO provided assistance for households to facilitate moves to lower poverty neighborhoods and explicitly required moves to lower poverty neighborhoods in the experimental treatment arm. And 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.

5.4 Effect on Neighborhood Characteristics in Early Adulthood

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, when the children are between the ages of 19 and 31.[49] Over 50% of children live in the same household as a parent in 2010, so HOPE VI-induced moves might be expected to influence where the children live in early adulthood. We focus on six characteristics of the census tract of residence: the poverty rate, the employment rate, a measure of labor market networks (social isolation index), 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]").[50]

---

[49] 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.

[50] 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 social isolation index (or 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).

Table 8 presents results for residential outcomes in 2010. Within large PHAs HOPE VI improved the geographic accessibility of jobs along all three measures (columns 1, 2, and 3),[51] and reduced population density (column 4).[52] We do not find any evidence that HOPE VI children resided in neighborhoods with lower poverty rates, higher employment rates, or higher levels of social network isolation. Note that poverty rate, employment rate, and average commute time (computed from the ACS five-year estimates for 2008-2012) are for the Census tract population as a whole and, due to selection into neighborhoods along multiple dimensions (e.g. access to services), more accessible tracts may not have higher employment rates and lower poverty rates overall. 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. Whether the HOPE VI children prefer the program-induced changes in neighborhood conditions will depend on how they value the different neighborhoods attributes (e.g., poverty rates and job proximity).

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 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. However, we note that the difference between the coefficient estimates for large and small PHAs for the short and long commute outcomes is not statistically significant at the 10 percent level (p-value of 0.20). Conditional on employment, HOPE VI children had shorter commute times by 2.1 minutes (column 5), which could be due to nearby jobs facilitating employment as well as selection into

[51] We find even larger effects on job proximity for the neighborhoods in which the parent 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.

[52] Why did HOPE VI-induced moves generate the improvements in job proximity but not poverty in large PHAs? HOPE VI neighborhoods had higher levels of poverty and lower levels of job proximity compared to other neighborhoods in the city before the demolitions. Housing prices are negatively correlated with poverty rates but largely uncorrelated with job proximity. Thus, those forced out of the HOPE VI neighborhoods tended to move to neighborhoods with greater job accessibility but were priced out of neighborhood with lower poverty rates. See Appendix C for details.

closer jobs. 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. Table A.9 presents the pooled results for all PHAs.

HOPE VI could have improved job accessibility by leading households to move to new neighborhoods with higher job accessibility or by improving job accessibility in the original neighborhoods for those that remained close to the project. Figure A.7 shows that approximately 10% of individuals from HOPE VI projects lived within one mile of the project in 2010.[53] Thus, if HOPE VI had a large impact on targeted neighborhoods this could translate into effects on individuals.

To explore whether the effects on neighborhoods translated to effects on individuals, we begin by measuring 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 3. 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.[54] That the effects dissipate quickly with distance is reassuring since we would not expect the demolition of a public housing project to drastically transform the population or job density in more distant neighborhoods.[55]

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

[53] Figure A.7 also shows that, relative to the matched control group, children from the HOPE VI projects are less likely to live in the original HOPE VI neighborhoods, more likely to live outside the original neighborhood but within 5 miles, and no more likely to live further than 5 miles away. These results provide some evidence against the possibility that HOPE VI generated employment gains by leading households to make longer distance moves out of the city center and closer to jobs in the suburbs as in Miller (2022). As discussed in Appendix C, we also find no evidence that the effect of HOPE VI is larger for projects located near the city center.

[54] 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.

[55] 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).

of population density.[56] 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 (see Raphael 1998 and Andersson et al. 2018). 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. Prior to demolition, the neighborhoods of HOPE VI projects had higher job density than surrounding neighborhoods (on average) up to four miles away (see Appendix Figure A.8 and Appendix C for discussion). Reducing the population of co-located job seekers, who likely have similar patterns of spatial job search, would be expected to have an outsized effect of reducing competition for available jobs, especially for a population with weaker attachment to the labor market who may only choose to work when commute costs are sufficiently low.

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 3. 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

[56] 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. Appendix D provides a more detailed discussion of the methodology and shows that if there are no individuals who remain living near the HOPE VI neighborhoods but would have moved away absent the award, then this exercise quantifies the extent to which the effects on individuals are driven by changes in the characteristics of the HOPE VI neighborhoods. We view this assumption as plausible given the fact that the demolitions increased the probability of moving.

We then estimate the impact of HOPE VI on this counterfactual job proximity measure for large PHAs. The original estimates, presented in Table 8, 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.

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 effects of HOPE VI on employment are driven by an increase in employment for individuals who commute short distances. Third, 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.

While the evidence above suggests that improvements in job accessibility play an important role in explaining the impacts on earnings, there are other possible channels through which neighborhoods might have a contemporaneous effect of labor market outcomes. For example, employers might discriminate against individuals living in large public housing projects. Alternatively, HOPE VI could have disrupted peer groups that negatively influenced young adult outcomes, reduced exposure to crime, or moved people to neighborhoods with stronger social networks. Arguing against these mechanisms, Appendix C discusses results that show that HOPE VI had no effect on the probability of being incarcerated in 2010 and did not lead people to live farther away from their original neighbors. These measures are admittedly coarse, and we cannot rule out the possibility that other mechanisms play some role. However, none of the alternative mechanisms offer an obvious explanation for all the findings presented in this section. In particular, the findings in Table 9, which show that HOPE VI reduced commute times, are best explained by improvements in job proximity.

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 A.9 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.[57]

Figure A.9 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 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.[58] The heterogeneity is economically meaningful. For example,

[57] 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.

[58] 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.

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 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.

References

Abadie, Alberto, and Jann Spiess. "Robust post-matching inference." *Journal of the American Statistical Association* 117, no. 538 (2022): 983-995.

Andersson, Fredrik, John Haltiwanger, Mark Kutzbach, Henry Pollakowski, and Daniel Weinberg. 2018. "Job Displacement and the Duration of Joblessness: The Role of Spatial Mismatch." *The Review of Economics and Statistics* 100(2): 203-218.

Arellano-Bover, Jamie. 2020. "The Effect of Labor Market Conditions at Entry on Workers' Long-Term Skills." *The Review of Economics and Statistics* 1-45.

Bellemare, Marc and Casey Wichman. 2020. "Elasticities and the Inverse Hyperbolic Sine Transformation", *Oxford Bulletin of Economics and Statistics*, 82(1), 50-61.

Bodory, Hugo, Lorenzo Camponovo, Martin Huber, and Michael Lechner. "The finite sample performance of inference methods for propensity score matching and weighting estimators." *Journal of Business & Economic Statistics* 38, no. 1 (2020): 183-200.

Burbidge, John B., Lonnie Magee, and A. Leslie Robb. 1988. "Alternative Transformations to Handle Extreme Values of the Dependent Variable" *Journal of the American Statistical Association* 83(401): 123-127.

Chetty, Raj and Nathaniel Hendren. 2018. "The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects." *The Quarterly Journal of Economics* 133(3): 1107–1162.

Chetty, Raj, Nathaniel Hendren, Patrick Kline, and Emmanuel Saez. 2014. "Where is the land of Opportunity? The Geography of Intergenerational Mobility in the United States." *The Quarterly Journal of Economics* 129(4): 1553-1623.

Chetty, Raj, Nathaniel Hendren, and Lawrence Katz. 2016. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment." *American Economic Review* 106(4): 855-902.

Chyn, Eric. 2018. "Moved to opportunity: The long-run effects of public housing demolition on children." *American Economic Review 108*(10): 3028-3056.

Chyn, Eric, Robert Collinson, and Danielle Sandler. 2023. "The Long-Run Effects of Residential Racial Desegregation Programs: Evidence from Gautreaux." Manuscript.

Congressional Research Service. 2022. "The Earned Income Tax Credit (EITC): Legislative History" CRS Report R44825, Updated April 28, 2022.

Davis, Steven, John Haltiwanger, and Scott Schuh. 1996. "Small business and job creation: Dissecting the myth and reassessing the facts." *Small Business Economics* 8(4): 297-315.

Gallagher, Megan, and Beata Bajaj. 2007. "Moving on: Benefits and challenges of HOPE VI for children." *HOPE VI: Where do we go from here*.

Goetz, E. G. 2010. "Desegregation in 3D: Displacement, dispersal and development in American public housing. Housing Studies" 25(2): 137-158.

Hellerstein, Judith K., Melissa McInerney, and David Neumark. 2011. "Neighbors and Co-Workers: The Importance of Residential Labor Market Networks." *Journal of Labor Economics* 29(4): 659-695.

Hellerstein, Judith, Mark Kutzbach, and David Neumark. 2019. "Labor market networks and recovery from mass layoffs: Evidence from the Great Recession period," *Journal of Urban Economics* 113.

Imbens G. 2015. "Matching Methods in Practice." *Journal of Human Resources* 50(2): 373-419.

Imbens, Guido, and Donald Rubin. 2015. *Causal Inference in Statistics, Social, and Biomedical Sciences*. New York: Cambridge University Press.

Imbens, Guido W., and Jeffrey M. Wooldridge. 2009. "Recent Developments in the Econometrics of Program Evaluation." *Journal of Economic Literature* 47(1): 5-86.

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

Jacob, Brian A., Jens Ludwig, and Douglas L. Miller. 2013. “The effects of housing and neighborhood conditions on child mortality.” *Journal of Health Economics* 32(1): 195-206.

Jones, Roderick W., and Derek J. Paulsen. 2011. “HOPE VI Resident Displacement: Using HOPE VI Program Goals to Evaluate Neighborhood Outcomes.” *Cityscape: A Journal of Policy Development and Research* 13(3): 85-102.

Kain, John F. 1968. “Housing Segregation, Negro Employment, and Metropolitan Decentralization.” *Quarterly Journal of Economics* 82: 32-59.

Kingsley, Thomas G., Jennifer Johnson, and Kathryn Pettit. 2003. “Patterns of Section 8 Relocation in the HOPE VI Program.” *Journal of Urban Affairs* 25(4): 427–47.

Lens, Michael. 2014. “Employment Accessibility Among Housing Subsidy Recipients.” *Housing Policy Debate* 24(4): 671-691.

Lens, Michael, Kirk McClure, and Brent Mast. 2019. "Does Jobs Proximity Matter in the Housing Choice Voucher Program?" *Cityscape* 21(1): 145-162.

Ludwig, Jens, Greg Duncan, Lisa Gennetian, Lawrence Katz, Ronald Kessler, Jeffrey Kling, and Lisa Sanbonmatsu. 2013. “Long-term Neighborhood Effects on Low-Income Families: Evidence from Moving to Opportunity.” *American Economic Review* 103(3): 226-231.

McCarty, Maggie. 2005. "HOPE VI: background, funding, and issues. Report No. RL32236, Congressional Research Service." *The Library of Congress, Washington, DC*.

McClure, Kirk. 2018. "Length of Stay in Assisted Housing." *Cityscape* 20(1): 11-38.

Miller, C., 2022. “When work moves: Job suburbanization and black employment.” *Review of Economics and Statistics*, pp.1-45.

Mills, Gregory, Daniel Gubits, Larry Orr, David Long, Judie Feins, Bulbul Kaul, Michelle Wood, Amy Jones & Associates, Cloudburst Consulting, and the QED Group. 2006. *The Effects of Housing Vouchers on Welfare Families.* Washington, DC: U.S. Department of Housing and Urban Development, Office of Policy Development and Research.

Murphy, LaShonia. 2012. “The Allocation of Funds with HOPE VI: Applicants and Recipients.”

National Commission on Severely Distressed Public Housing (NCSDPH). 1992. *The Final Report*: *A Report to the Secretary of Housing and Urban Development*. Washington D.C. August.

Oreopoulos, Philip. 2003. “The Long-Run Consequences of Living in a Poor Neighborhood.” *Quarterly Journal of Economics* 118(4): 1533-1575.

Pollakowski, Henry O., Daniel H. Weinberg, Fredrik Andersson, John C. Haltiwanger, Giordano Palloni, and Mark J. Kutzbach. 2022. “Childhood Housing and Adult Earnings: A Between-Siblings Analysis of Housing Vouchers and Public Housing.” *American Economic Journal: Economic Policy* 14(3): 325-72.

Popkin, Susan J., Diane K. Levy, Laura E. Harris, Jennifer Comey, Mary K. Cunningham, and Larry F. Buron. 2004. “The HOPE VI Program: What about the Residents?” *Housing Policy Debate* 15: 385-414.

Popkin, Susan J., Diane K. Levy, and Larry F. Buron. 2009. “Has HOPE VI Transformed Residents' Lives? New Evidence from the HOPE VI Panel Study.” *Housing Studies* 24(4): 477-502.

Raphael, Steven. 1998 “Intervening Opportunities, Competing Searchers, and the Intra-Metropolitan Flow of Male Youth Labor.” *Journal of Regional Science* 38(1): 43-59.

Rosenbaum, James E. 1995. “Changing the Geography of Opportunity by Expanding Residential Choice: Lessons from the Gautreaux Program.” *Housing Policy Debate* 6(1): 231-269.

Rosenbaum, Paul, and D. Rubin. 1983. “The Central Role of the Propensity Score in Observational Studies for Causal Effects.” *Biometrika* 70: 41-55.

Rosenbaum, Paul, and D. Rubin. 1984. “Reducing the Bias in Observational Studies Using Subclassification on the Propensity Score.” *Journal of the American Statistical Association* 79: 516-524.

Sandler, Danielle H. 2017. “Externalities of Public Housing: The Effect of Public Housing Demolitions on Local Crime.” *Regional Science and Urban Economics* 62: 24-35.

Schwandt, Hannes and Till von Wachter. 2019. “Unlucky Cohorts: Estimating the Long-Term Effects of Entering the Labor Market in a Recession in Large Cross-Sectional Data Sets.” *Journal of Labor Economics* 37(S1): S161-S198.

Shen, Qing. 1998. “Location Characteristics of Inner-City Neighborhoods and Employment Accessibility of Low-Income Workers,” Environment and Planning B: Planning and Design 25(3): 345–365.

Smith, Tony E. and Yves Zenou. 2003. “Spatial mismatch, search effort, and urban spatial structure.” *Journal of Urban Economics* 54(1): 129-156.

Turner, Margery Austin, Mark Woolley, G. Thomas Kingsley, Susan J. Popkin, Diane Levy, and Elizabeth Cove. 2007. "Severely Distressed Public Housing: The Costs of Inaction." *Washington, DC: The Urban Institute.*

U.S. Department of Housing and Urban Development (HUD). 2007. HOPE VI Program Authority and Funding History. https://www.hud.gov/sites/documents/DOC_9838. (accessed 19 September 2019).

U.S. General Accounting Office (GAO). 2003. *Public Housing: HUD's Oversight of HOPE VI Sites Needs to be More Consistent*.

Wang, Fahui. 2007. “Job Access in Disadvantaged Neighborhoods in Cleveland, 1980—2000: Implications for Spatial Mismatch and Association With Crime Patterns.” *Cityscape* 9(3): 95-121.