Childhood Housing and Adult Outcomes: A Between-Siblings Analysis of Housing Vouchers and Public Housing

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We create a national-level longitudinal dataset to analyze how children's participation in public and voucher-assisted housing affects age 26 earnings and adult incarceration. Naïve OLS estimates suggest that returns to subsidized housing participation are negative, but that relationship is driven by household selection into assisted housing. Household fixed-effects estimates indicate that additional years of public housing increase earnings by 6.2% for females and 6.1% for males, while voucher-assisted housing increases earnings by 4.8% for females and 2.7% for males. Childhood participation in assisted housing also reduces the likelihood of adult incarceration for all household race/ethnicity groups. (JEL H43; I31; I38; J38; J62).

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In recent decades, millions of children have grown up in Housing Choice Voucher (HCV)-supported or public housing, two of the largest subsidized housing programs run by the U.S. Department of Housing and Urban Development (HUD). For these children, residential location, neighborhood amenities, peer composition, and the availability of household resources were shaped by their participation in subsidized housing. Given the mounting body of evidence that early characteristics and experiences can have lasting consequences for a range of adult outcomes,[1] exposure to HCV-assisted or public housing during childhood could have important implications for adult well-being. Yet, research on the long-term economic effects of assisted housing for resident children has been hampered by data and methodological limitations.

In this paper, we estimate the long-term effects of teenage participation in HCV-supported and public housing on young adult (age 26) earnings and incarceration. To do so, we develop a national dataset that combines 2000 and 2010 Decennial Census information with comprehensive longitudinal administrative data on housing assistance, residential location, and earnings. The integrated data permit us to identify nearly the universe of individuals aged 13-18 in 2000 and to observe demographic information, household structure, housing assistance, neighborhood characteristics, and parents' earnings throughout their teenage years. These longitudinal data enable us to follow teenagers into adulthood, where we observe their quarterly labor market earnings and whether they were incarcerated in April 2010. To our knowledge, this is the first paper to estimate the long-term economic effects of both public and HCV-supported housing for a large nationally representative sample. We contrast the impacts of both programs with each other as well as to unsubsidized housing.

We employ a household fixed-effects (HFE) specification that exploits variation in children's exposure to HCV-supported and public housing participation within households. Exposure differences are a product of moves into and out of housing along with sibling age differences at the time of moves. This between-siblings approach allows us to isolate the effect of an additional year of each type of subsidized housing, relative to a year in unsubsidized housing, on adult outcomes from observed and unobserved household-level heterogeneity that may affect both program participation and adult outcomes. We observe 13-18-year-old assisted housing

[1] See Aaronson 1998; Almond and Mazumder 2005; Black et al. 2007; Almond, Edlund, and Palme 2009; Akee et al. 2010; Chetty et al. 2014; Aizer et al. 2016; Chetty, Hendren, and Katz 2016; Chetty, Hendren, and Katz 2016; Hoynes, Schanzenbach, and Almond 2016; Chetty and Hendren 2018a; Chetty and Hendren 2018b; Chyn 2018; Hoynes and Schanzenbach 2018; Brown, Kowalski, and Lurie 2019.

participation for about 282,000 teenagers—24% of the full sample of 1.172 million; 96,000 have within household variation in public housing and 190,000 have within household variation in HCV-assisted housing. The differences in participation are generated by almost equal numbers of teenagers in households with entries and exits from the two programs. We therefore observe similar numbers of older siblings and younger siblings with more subsidized housing participation.

Our results confirm that selection into subsidized housing matters. Whereas Ordinary Least Squares (OLS) estimates indicate a substantial negative effect of housing subsidies when young on later adult earnings, the HFE estimates are generally positive and statistically significant. For females, we find that each additional year spent in public housing as a teenager generates a 6.2% premium for young adult earnings. The corresponding estimate for HCV-assisted housing is 4.8% per year of participation. For males, the estimates are 6.1% and 2.7% for public and HCV-assisted housing, respectively. We find the largest impacts on earnings for females from Black non-Hispanic households in HCV-assisted housing and for females from Hispanic households in public housing, who earn an additional 7.3% and 9.2% from additional years of participation. Translating the primary effects on age 26 earnings into expected changes in lifetime earnings suggests that each additional year of teenage public housing participation increases total discounted lifetime pre-tax earnings by $21,900 for females and $21,500 for males, while each additional year of teenage HCV-assisted housing increases total discounted lifetime pre-tax earnings by $16,900 for females and $9,500 for males.[2]

The results for 2010 incarceration closely follow our earnings estimates, both overall and for the sex by race/ethnicity subgroups. For teenagers from Black non-Hispanic households, each additional year of HCV-assisted housing reduces the likelihood of being incarcerated in April 2010 by 0.3 percentage points for males and 0.7 percentage points for females. These estimates contrast sharply with the OLS estimates which suggest a positive association between subsidized housing participation and incarceration, especially for Black non-Hispanic males.[3] The estimates for public housing suggest similarly sized reductions in the likelihood of 2010 incarceration for children from Black non-Hispanic households: 0.5 percentage points for males and 0.7 percentage points for females.

[2] All dollar figures are in 2000 US$. The increase in the Consumer Price Index (CPI-U) from 2000 to 2017 was 42.1%.

[3] Previous research using the HCV lottery in Chicago found no significant effect of childhood HCV receipt on adult criminal outcomes (Jacob et al. 2015).

Though assisted housing programs in the United States have existed since the 1930s, researchers have only recently found convincing strategies to deal with the non-random selection of households into subsidized housing. A series of evaluations of HUD's Moving to Opportunity (MTO) program and research using administrative records from Chicago together with experimental and quasi-experimental variation in participation provide the most convincing evidence on the impacts of subsidized housing (Jacob 2004; Kling et al. 2005, 2007; Jacob and Ludwig 2012; Ludwig et al. 2012, 2013; Jacob et al. 2013, 2015; Chetty, Hendren, and Katz 2016; Chyn 2018).[4] With the exception of Chetty, Hendren, and Katz (2016) and Chyn (2018), these papers identify, at most, modest differences in short- and long-term outcomes such as physical and mental health, criminal behavior, and adult employment and earnings.[5] However, much of this research estimates differences between the outcomes of children from households in public housing and the outcomes of children from households that received tenant-based housing vouchers (HCV) and thus does not permit inference about the effects of assisted housing programs relative to unsubsidized housing. Notable exceptions are the papers examining the Chicago HCV lottery, which use data from one city to compare HCV-assisted housing to unsubsidized housing. Our results show the importance of being able to contrast both programs to each other as well as to unsubsidized housing. Comparing public to HCV-assisted housing our earnings effects are consistent with the findings of Chetty, Hendren, and Katz (2016) for children older than 13 when MTO began—slightly larger benefits of public housing relative to HCV-assisted housing—but we also show that both programs generate earnings benefits relative to time spent in unsubsidized housing.

While the MTO and Chicago HCV lottery papers identify internally valid estimates, their limited geographic coverage along with MTO's experiment-specific features (households in the experimental group were required to move to low-poverty census tracts and they received counseling support to help them find an apartment and adapt to their new circumstances) lessen the external validity of the results. Public and HCV-assisted housing opportunities are not uniform

[4] Earlier work estimates the effect of either public housing or HCV-assisted housing on short- and long-term outcomes by employing instrumental variables strategies (Currie and Yelowitz 2000; Newman and Harkness 2000), propensity score matching (Carlson et al. 2012a, 2012b), or panel data methods (Olsen et al. 2005).

[5] Chetty, Hendren, and Katz et al. (2016) link MTO data to administrative tax records on college attendance, earnings, and adult residential locations. They find that voucher recipients randomly assigned to be offered HCVs that could only be used in low-poverty neighborhoods initially, on average, live in better neighborhoods, are more likely to have attended some college, and have higher earnings as adults, but only if the MTO-driven moves occurred prior to age 13.

across public housing authorities (PHAs): they vary with respect to structure type, physical proximity to amenities, ease of availability (i.e., waitlist times and area median income thresholds), tightness of the rental housing market, access to lower-poverty neighborhoods, and the characteristics of participating households. Furthermore, while the five metropolitan areas included in the MTO experiment[6] account for an important share of assisted housing participants, the households residing in public or HCV-assisted housing in these cities are observably different from public and HCV-assisted housing participants in the United States as a whole. We show there is suggestive evidence of heterogeneity in the impact of public and HCV-assisted housing in especially high-poverty neighborhoods in large metro areas, where there are higher returns to HCV-assisted housing and lower returns to public housing for males. This highlights the importance of including assisted housing variation from a broad range of neighborhoods and metro areas to better understand the benefits of the two programs.

Our HFE approach, while addressing several sources of bias that are problematic for OLS estimates, is still vulnerable to unobserved time-varying household-level heterogeneity. We confirm that our main results are robust to a series of checks to assess the likelihood that different potential sources of time-varying heterogeneity are affecting the estimates. Semi-parametric regressions of age 26 earnings on age first and last observed in assisted housing that use sharp variation across age in the timing of moves and relax the linearity and symmetry assumptions in the main specifications suggest similar increases in adult earnings to the main results; likewise, versions of the HFE specifications that relax the linearity assumption leave the main conclusions unchanged. Estimates are unaffected when we restrict the sample to areas with long average wait times where selection due to unobserved time-varying changes should be attenuated by lags between desired participation and actual participation. Impacts are no different for households that move into assisted housing—where younger children participate for longer—and households that move out—where older children participate for longer. We find no evidence that results are driven by changes in household structure: results are similar when limiting the sample to children from households where the marital status of the household head and the identity of their spouse do not change during the study period. Overall, the robustness checks support the existence of approximately linear, dose-response-type relationships between age 26 earnings and 13-18-year-

[6] The five PHAs included in the MTO experiment are Baltimore, Boston, Chicago, Los Angeles, and New York City.

old participation in public and HCV-assisted housing. Further analysis of heterogeneous effects by household and place-based characteristics points towards the primary mechanism being an income effect for recipient households.

The remainder of the paper proceeds as follows. Section I describes the subsidized housing programs we study and discusses how they might affect labor market earnings and incarceration. Section II presents our research design. Section III discusses the data infrastructure and describes the study sample. Section IV presents the primary empirical results, Sections V and VI assess robustness and search for heterogeneity in the results. Section VII converts the estimates into expected differences in lifetime earnings and conducts a cost-benefit analysis. Section VIII compares our findings to those in the existing literature, and Section IX concludes.

I. Background

A. Subsidized and Unsubsidized Housing in the United States

The federal public housing program began with the enactment of the United States Housing Act of 1937.[7] Initially the program consisted of federal subsidies for construction and ongoing management and operations performed by local PHAs. Because construction subsidies were not sufficient to cover maintenance, the federal government instituted an operating subsidy (in 1974) and imposed a rent ceiling—the maximum rent that each family could be charged—currently set at 30 percent of family income. In 1970 there were approximately 1 million units of public housing and construction continued slowly thereafter with the program reaching a peak of 1.4 million units in operation in 1994. Since 1994, participation in public housing has declined to just under 1.3 million in 2000 and about 1.1 million in 2013. This reflects, in part, the demolition of severely distressed projects under the HOPE VI program.

Enacted in 1974, the Housing Choice Voucher program provides rental assistance for low-income households through vouchers that prospective tenants take to private-sector landlords of approved rental units. The vouchers allow the landlords to receive the full rental price, up to a "Fair Market Rent," while still capping household contributions to 30% of income in most cases.

[7] We thank David Hardiman and Todd Richardson of HUD for providing substantive clarifications for this section.

The HCV program has grown rapidly over the past two decades, with participation rising from 1.1 million households in 1990 to 1.8 million in 2000 and to nearly 2.4 million in 2013.

HUD rental assistance is not an entitlement and serves only a fraction of the households that meet the basic income requirements. HUD estimates that in 2013, at least 7.72 million unassisted very low-income (and therefore "eligible") households paid more than 50 percent of their income in rent (Steffen et al. 2015).

B. Pathways from child housing to adult outcomes

There are a number of channels through which childhood participation in subsidized housing might affect later adult outcomes. Both HCV and public housing provide a positive income effect for households. By relaxing the budget constraint faced by participating households, these programs may enable parents to devote more time and financial resources to develop the human capital of children residing in the household (Dahl and Lochner 2012; Aizer et al. 2014; Jacob, Kapustin, and Ludwig 2015). This increase in human capital would suggest that childhood residence in assisted housing should improve adult labor market outcomes and decrease adult incarceration.

Beyond direct income effects, assisted housing may help households avoid some of the most acute consequences of housing unaffordability, including eviction and homelessness. Desmond Gershenson, and Kiviat (2015), examining renters in Milwaukee, find higher rates of forced moves for low-income households, including formal and informal eviction, landlord foreclosure, and building condemnations. They find that these relocations account for roughly a quarter of all moves and can result in moves to substandard housing or cause further relocations.

However, other pathways might yield a negative relationship between subsidized housing participation in childhood and adult well-being. Newman (1972) argues that the design of some public housing projects is not conducive to community watchfulness and leads to isolation and crime. Schill (1993) documents the distressed state of public housing with a backlog of unmet maintenance and modernization needs that could create a harmful living environment for children. Both building structure-related mechanisms predict a negative relationship between childhood participation in public housing and adult outcomes.

Oreopoulos (2003) proposes that public housing participation might affect outcomes through peer or neighborhood effects. If, as he argues, assisted housing units are in worse neighborhoods (e.g., neighborhoods with higher crime rates and lower-quality schools) than participants' counterfactual housing options, then public and HCV-assisted housing could have negative neighborhood and peer effects and therefore decrease adult well-being.

The impact of HCV-assisted and public housing participation during childhood need not be the same. Indeed, the perception that distressed public housing might have especially deleterious effects partly motivated the shift in subsidized housing policy in the U.S. to providing housing choice through vouchers. In the absence of discrimination on the part of potential landlords, HCV-assisted housing could offer households increased neighborhood choice, though encouraging moves to better neighborhoods may require considerable effort from policy-makers (Collinson and Ganong 2018; Bergman et al. 2019). As such, the potential adverse consequences of public housing projects (e.g., negative peer effects) might be avoided while the positive income effects would be preserved.

Alternatively, public housing projects may offer increased stability for residents. Whereas HCV recipients and private-market households are forced to search for open rental units, public housing residents receive housing at pre-determined prices (subject to adjustment for household income) in known locations. Further increasing the search costs faced by HCV-assisted households is the possibility that some landlords may prefer not to rent units to households using HCVs. Consistent with this, a significant fraction of families that are offered a voucher are unable to successfully locate housing on which to use it (Finkel and Buron 2001). Public housing participants, with PHAs as their landlords, do not face this type of discrimination or search cost.

There may also be heterogeneous impacts of assisted housing for different demographic groups. For instance, some MTO research uncovers larger benefits of the program for girls than boys (Kling, Ludwig, and Katz 2005; Ludwig et al. 2013) and more recent work suggests there may be more positive effects of HCV-assisted housing for children that were younger when their household first received a voucher (Chetty, Hendren, and Katz 2016; Chyn 2018). To account for the possibility that treatment effects differ across demographic groups, in our empirical analysis we allow the effects of assisted housing to vary by sex and household race/ethnicity. We also explore whether there is any heterogeneity by household income, metro area size, neighborhood poverty, and whether younger or older children were likely to have been exposed to assisted housing for longer.

In sum, there is no clear prediction as to how subsidized housing participation while young will affect long-term outcomes. Nor is there a strong prediction about which type of subsidized housing will have more advantageous or harmful effects.

II. Research Design

A. Household fixed effects model

Our goal is to identify the causal effect of living in subsidized housing between the ages of 13 and 18 on adult earnings and incarceration, relative to time spent in unsubsidized housing. To do so, we specify a linear, constant HFE regression model for each outcome, *y*, of child *i* as:

$$(1) \qquad y_{if} = \gamma_f + \boldsymbol{H}'_{if}\boldsymbol{\beta}_{\boldsymbol{HFE}} + \boldsymbol{X}'_{if}\boldsymbol{\phi} + \epsilon_{if}$$

where *f* indexes the household including child *i* in the year 2000. The outcome y_{if} is either the inverse hyperbolic sine (IHS) of total earnings at age 26[8] or an indicator for whether child *i* is incarcerated in 2010. The explanatory variables of interest, $\boldsymbol{H}_{if}$, are separate measures of the number of years spent in public housing and HCV-assisted housing between 13-18, and the HFE estimates of their impact on the outcomes, relative to years spent in unsubsidized housing, are represented by $\boldsymbol{\beta}_{\boldsymbol{HFE}}$.

The vector $\boldsymbol{X}_{if}$ includes child-specific control variables, including an indicator for whether the child is male, a full set of age dummies, an interaction between whether the child is male and the age dummies, and when pooling across household races, an interaction between household race/ethnicity and whether the child is male. We also interact each of the subsidized housing measures with the male indicator to allow for heterogeneous effects by child sex, and we estimate separate regressions for each race/ethnicity to allow all coefficients to vary along this dimension. In some specifications we also include controls for parents' earnings and the block group poverty rate between the ages of 13 and 18. All time-invariant household-level characteristics are subsumed in the household fixed effects, γ_f. Lastly, ϵ_{if} is a zero-mean error term that we cluster by household.

[8]We use the inverse hyperbolic sine (IHS) of earnings rather than the more traditional log of earnings. The coefficients can be interpreted in the same way as with a log-transformed dependent variable but, unlike with the log of earnings, IHS is defined for zero earnings. The IHS is defined as $\log[y_i + (1 + y_i^2)^{0.5})]$ where y_i is total earnings for individual i (see Burbidge, Magee, and Robb 1988). A more general form of the IHS also includes a scaling factor θ (see Pence 1996). Our results are robust to using different scaling factors.

The error term in equation (1) includes all unobserved time-varying household-level characteristics and all unobserved child-level characteristics that affect the outcomes. If, after conditioning on household fixed effects, these factors are correlated with subsidized housing participation and adult outcomes, then our estimates of the impact of subsidized housing may be biased. We explore a variety of robustness checks in our analysis to alleviate these concerns.

The HFE regression estimates the effect of subsidized housing participation on labor market outcomes and adult incarceration using only variation in housing participation and outcomes across teenagers *within the same household*. In practice, each dependent and independent variable is de-meaned at the household level and ordinary least squares (OLS) is run using the de-meaned values for everyone in the sample. While we retain individuals from households with no between-sibling difference in assisted housing participation, the variation used for identification comes from households with some across-sibling difference in participation. As we document in the next section, there is ample within-household variation in assisted-housing exposure to help identify the effect of interest.

Griliches (1979) provides a summary of the early literature making use of sibling fixed effects and points out potential issues. More recent applications employ household fixed effects to explore questions related to infant health (Currie and Walker 2011), educational attainment (Aaronson 1998; Miller, Shenhav, and Grosz 2019), adult health (Currie et al. 2010), and adult labor market outcomes (Page and Solon 2003; Vartanian and Buck 2005; Levine and Mazumder 2007; Royer 2009; Chetty and Hendren 2018a). Miller, Shenhav, and Grosz (2019) also extend the discussion in Griliches (1979), pointing out that "selection into identification"—treatment effect heterogeneity for the identifying sample—can generate a difference between the average treatment effects (ATEs) for the full sample and the sub-population with within household variation.

We also implement two less parametric variants of equation (1) to explore whether findings are driven by assumptions implicit in the linear HFE specification. The first replaces the counts of the number of years spent in HCV-assisted and public housing between ages 13-18 with a set of indicator variables for whether child i from household f participated in $\{1, \ldots, 6\}$ years of each program, with no participation serving as the omitted category. The remainder of this less parametric specification is identical to the main equation described above.

The second variant drops the household fixed effects from equation (1) and instead implements semi-parametric regressions of the IHS of earnings at age 26 on the first and last age individuals are observed in HCV-assisted or public housing. Age in 2000, age by male, male by household race/ethnicity, and state by age fixed effects are included as controls.[9] We run two separate specifications—one for public housing and one for HCV-assisted housing—and limit the sample for each program to individuals first and last observed between the ages of 12 and 19. The age restrictions ensure that variation comes from differences in the timing of the start and end of adolescent exposure to assisted housing, rather than comparing individuals who never (or always) participate to others who experience a change during these years, and that the variation occurs at similar ages to the variation used in the main results. The age range allows for one adjacent cohort on each end of the 13 to 18-year-old range without including sparse indicators that generate disclosure issues (e.g., there are few sample members last observed in assisted housing at ages 8-11). The age 12 indicators serve as the omitted category in all regressions.[10]

The semi-parametric specifications relax the linearity assumption and do not require that the impact of additional assisted housing participation be symmetric when generated by starting participation one year earlier or continuing participation until one year later. The resulting figures offer a visible way to inspect how average earnings differ between cohorts that were first observed in assisted housing one year earlier or last observed one year later.

III. Data

A. Sample Construction

We use confidential microdata from the U.S. Census Bureau, including administrative files and censuses, as well as linked public use data to construct a dataset of children eligible for assisted housing, their housing exposure, and their long-run outcomes (see Appendix A for a detailed exposition of the data sources). We characterize households using responses to the 2000 Census short form, including geographic location, housing tenure (rent or own), and, for each person, household relationship types and demographics (age, sex, race, and ethnicity). Assisted housing

[9] The inclusion of household fixed effects, age in 2000 indicators, and age first and last observed indicators yields less precise point estimates, but the main findings are the same.

[10] The adjacent ages allow us to capture transitions relevant for the main results. Individuals last observed in a program at age 12 will have no 13-18-year-old participation and serve as the comparison category for those last observed at ages 13-19. Given the semi-parametric set-up, the coefficients on the age 19 indicators are distinct estimates and do not affect those for ages 13-18, though we report them for completeness. Restricting the sample to individuals last observed no later than age 19 is especially important since some of our sample may move back into assisted housing themselves after departing their 2000 Census household. Given the income eligibility requirements for assisted housing participation these individuals are likely to be negatively selected.

participation from 1997 to 2005 is taken from HUD's Public and Indian Housing Information Center (PIC) file, an annual administrative record submitted by housing authorities for the housing occupancy verification process that lists households by authority, subsidy type, and project, and includes application and move-in dates as well as identifying information and demographics for each participant. Earnings from wage and salary jobs come from the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) Infrastructure Files, an employer-employee matched dataset assembled from quarterly earnings records from states and from similar records for federal workers. The jobs data, which begin for most states in the 1990s, cover approximately 96 percent of private non-farm wage and salary employment (Stevens 2007). Jobs data for federal workers are included beginning in 2011. Adult incarceration is defined using the Group Quarters reporting in the 2010 Census. Finally, we identify location from an annual place of residence file assembled by the LEHD program, which – when administrative records are available – gives a census block of residence from 1999 onwards.

Starting with the 2000 Census households, we integrate the files to create a sample of children from households that are likely eligible for housing assistance. We identify households with two or more children aged 13 to 18 on April 1, 2000, the reference date for the 2000 Census. These children will turn 26 years old between 2008 and 2013, a period spanned by the LEHD earnings data and including the 2010 Census.[11] The Census Bureau uses personal identifying information to link respondents to unique identifiers, with about 89 percent successfully linked. We use these identifiers to link the children, as well as Person 1 (on the Census) and any spouse, to LEHD earnings records, housing assistance records, the annual place of residence data, and the 2010 Census. To limit the sample to likely eligible households, we require that while a child is 13-18, the average annual total of LEHD reported earnings for the adults (Person 1 and spouse) be no greater than 50% of the Area Median Income (AMI); below 50% AMI households are classified by HUD as being "very low income" and are typically considered eligible for both public housing and HCV-assisted housing.[12] We retain all households between 50% and 100% AMI for use in a robustness check though they are not included in the main sample. Although we have no wealth

[11] Age 26 earnings are used as an outcome in some previous studies of intergenerational economic mobility (e.g., Chetty and Hendren 2015). It is likely that most individuals will have completed their education by that age.

[12] We base the AMI calculation on the household size and response location in 2000. According to HUDUSER, in the year 2000 87% of households in public housing and 95% of HCV-assisted housing residents were below 50% AMI; in 2005 these figures had increased to 91% and 97%, respectively (HUD 2000).

information, we further restrict the sample to require that households be renters in 2000. We also drop households residing in 42 counties that were served by housing authorities that participated in HUD's Moving to Work (MTW) demonstration program.[13] This sample restriction is made because MTW relaxed reporting requirements for PHAs, so retaining these households would compromise our ability to link persons with housing assistance.

Having started with 2.8 million children aged 13-18 in 2000, our data integration requirements and restrictions give us a sample of 1.172 million children in sibling households (and 672,000 for our sample linked to the 2010 Census). All subsequent estimates are weighted to the enumerated population of households with at least two children aged 13 to 18 in 2000 with non-missing values for covariates, although our results are not sensitive to the application of weights (see Appendix A for a full description of the sample construction as well as our methodology for weighting).

For this sample, we use the linked files to define our dependent and explanatory variables as well as to allocate households to subsamples. We construct household level race/ethnicity indicators for whether the household is White non-Hispanic (White), Black non-Hispanic (Black), other race non-Hispanic (Other), or Hispanic. We measure participation in both public housing and HCV-assisted housing as the count of years a child resided in each program between ages 13-18. For children aged 17 or 18 in 2000, who we do not observe in HUD-PIC data when they are 13 or 14 (in 1995 and 1996), we impute participation in those years based on a household's move-in date in 1997 (if they participated then) and add those years to the treatment count.[14] Using this count, we generate indicator variables for all sample members to identify how many years they spent in each program. We also use the data to identify the first and last age that sample members are observed in public and HCV-assisted housing and generate a set of indicator variables using this information.

We use LEHD to calculate household income as the inverse hyperbolic sine of average parents' annual earnings while a child is 13-18. We link individuals to the annual place of residence file (where available), and by census block and census tract, to the 2000 Census summary file, to calculate average observed neighborhood poverty rate (measured at the census tract or block group level) while a child is 13-18. Lastly, we use LEHD to calculate the inverse hyperbolic sine of

[13] Chicago is in one of the counties included in the MTW Demonstration, so we cannot do any direct estimation of results for Chicago to use in comparisons with those studies that concentrate on Chicago. See Online Appendix D for a complete listing of counties dropped from the data.

[14] The age restriction for program participation (13-18) is made to ensure we observe all years of potential participation directly or indirectly. The results are robust to omitting the imputation and to removing 17-18-year-olds entirely.

earnings for each child in the year they turn age 26, generate an indicator for whether they had any LEHD earnings in the year they turn 26, and create an indicator for whether they are observed in a correctional facility on April 1, 2010.

B. Summary Statistics

Our analysis sample closely resembles housing-subsidy eligible households in the United States. The exclusion of residents (based on the 2000 Census residential location) in counties containing PHAs that entered MTW during the study period does not distort the representativeness of the sample. Appendix Tables A1 and A2 indicate that the housing authorities located in the 3,025 non-MTW counties have similar characteristics to public and HCV program participants overall. For the year 2000, our non-MTW areas cover 91% of public housing residents and 93% of HCV residents. As mentioned in the introduction, this coverage is substantially higher than is available for existing studies, such as MTO, which covers PHAs where 20% and 12% of public housing and HCV households reside, respectively.

Table 1 presents summary statistics for housing subsidy-eligible teenagers from the counties included in our sample.[15] The first column presents summary statistics for the sample used in estimation – youth aged 13-18 in 2000 living with another sibling aged 13-18 in 2000 whose parents earned less than 50% of AMI. This sample is subdivided further, into those that lived in households not in subsidized housing anytime during the 1997-2005 study period (column 2), and those that lived in households receiving a subsidy at some point during this period (column 3); the latter are then subdivided further into those children that themselves never lived in subsidized housing while aged 13-18 (column 4) and those that did (column 5).

A comparison of columns 2 and 3 shows that there are substantial differences in age 26 earnings, with those in subsidized households earning 24% less at age 26. Black non-Hispanics make up a larger portion of the subsidized sample (47% versus 22%), parents' earnings are lower in the subsidized sample, and a higher portion of the subsidized sample lived in single-parent households (75% versus 57%). In contrast, the comparison between columns 4 and 5 uncovers small differences.

[15] Confidentiality restrictions preclude us from releasing summary statistics for the entire sample of 13-18-year-old children from the 2000 census.

Table 1 also presents the fraction of our sample observed in the 2010 Census and the incarceration rate among those found in 2010; Appendix Table A3 shows summary statistics for the study outcomes by household race/ethnicity and sex. 76.5% of sample children were linked to a 2010 Census response. Of these, 3.3% were observed in an adult correctional facility in 2010. This figure rises to 5.9% for children in Black non-Hispanic households, though this masks important heterogeneity by sex: 0.8% of Black non-Hispanic females and 12.3% of Black non-Hispanic males were in an adult correctional facility in 2010. The latter figure underscores the high prevalence of adult incarceration for our study sample.

To help introduce the within-household variation in subsidized housing participation, Figure 1 displays the distribution of within-household differences; that is, each individual's own subsidized housing participation net of the household mean among all 13-18-year-olds (in 2000). The figure is based on the sample described in Table 1, Column 3, so individuals from households where no sample member participates in the program between ages 13 and 18 are dropped to ease exposition.[16] The distribution is unimodal and symmetric around zero, with an overwhelming majority of teenagers within 2 years of the household mean participation.

Appendix Table A4 shows more details on the within-household variation in assisted housing we use for identification. Entries and exits from assisted housing are approximately equally likely: similar numbers of households in assisted housing and not in assisted housing in the first year of our data have within-household variation in both programs. The balance in moving patterns ensures there is no relationship between child age or birth order and the within-household differences in assisted housing. The within-household differences in assisted housing are meaningful in size. For households that move into or out of assisted housing during our study period, we find that children, on average, have about 0.95 years more (or less) exposure than the mean participation for their household. Thus, for a household that moves during the study period, on average, there is a difference of just under two years between the minimum and maximum 13-18-year-old participation among sample members.

[16] The left panel of Figure 1 includes the 234,000 sample observations with any 13-18-year-old HCV-assisted participation in their household while the right panel of Figure 1 includes the 118,000 sample observations with any 13-18-year-old public housing participation in their household.

IV. Empirical Results

A. Pooled Estimates

Table 2 presents the main results pooling across household race/ethnicities. The OLS results in column 1 of Table 2 suggest there is a statistically significant negative relationship between both public and HCV-assisted housing participation and age 26 earnings. A one-year increase in 13 to 18-year-old HCV-assisted housing participation is associated with a reduction in the IHS of earnings at age 26 by 0.062 for females and 0.076 for males. The corresponding estimates for public housing are similar in size, suggesting each additional year of public housing participation reduces the IHS of earnings at age 26 by 0.077 for females and 0.065 for males.

The HFE results in column 2 paint a dramatically different picture. The negative relationships in the OLS specification completely disappear. The estimates indicate that additional time in public housing and HCV-assisted housing, relative to unsubsidized housing, lead to positive and statistically significant effects on age 26 earnings for both males and females. The "All Households" coefficients in Table 3 display the implied treatment effects along with p-values from tests of the null hypotheses of no difference in the treatment effects by sex or by assisted housing program, pooling across household race/ethnicities. The estimates imply that each additional year of voucher housing increases age 26 earnings by 4.8% for females and 2.7% for males, while an additional year of public housing increases age 26 earnings for females and males by 6.2% and 6.1%, respectively.[17] We can reject the null hypothesis of no difference between the effects of voucher housing for males and females at the 10% level, with females receiving larger benefits than males.[18]

To facilitate the interpretation of these treatment effects, we calculate the implied differences in age 26 earnings in 2000 US$, evaluated at the mean age 26 earnings for teenagers who spent some time in subsidized housing as a teenager ($9,718) (see Appendix Table B1 for details). The results suggest that each additional year of HCV participation increases age 26 earnings for females by

[17] Throughout the draft we convert point estimates into semi-elasticities using the approximation $\hat{E} = \exp(\hat{\beta}) - 1$ using the values of $\hat{\beta}$ shown in the relevant table (i.e., rounded to three decimal places). Simulations in Bellemare and Wichman (2020) suggest this method is appropriate for dependent variables with means greater than 10 (the mean of untransformed earnings is $11,820 for the sample).

[18] We check for heteroscedasticity in our main HFE specifications by regressing the squared predicted error term on the treatment indicators and the same set of controls included in the main regressions. The relationship between the assisted housing indicators and the squared error terms is always small in magnitude, below 0.03 in absolute value, and only statistically significant for one of the four assisted housing by male groups: years in voucher housing interacted with the male indicator. We conclude that heteroscedasticity is unlikely to produce bias in the treatment effects. Results are available upon request.

about $457 and increases age 26 earnings for males by $262. For public housing, the point estimates suggest that an additional year of public housing participation increases age 26 earnings for females by $583; for males, the corresponding increase is $573. We note that an overwhelming majority of teenagers fall within 2 years of the household mean participation and therefore appropriate caution must be taken in extrapolating these estimates beyond the within-sample range of variation.

Figure 2 plots the point estimates and 95% confidence intervals from the semi-parametric regressions of the IHS of earnings at age 26 on indicators for the ages that sample members were first and last observed in each program. As described above, while the household fixed effects are dropped, the sample is limited to individuals first and last observed in a program between the ages of 12 and 19. Variation is thus driven by sharp differences in the start or end of participation rather than comparisons between individuals who participated in the program and those that did not. All coefficients capture the expected change in IHS age 26 earnings relative to sample members who were first or last observed at age 12. Linear best fit lines and the slope across the coefficients for ages 12 to 18 are shown in each sub-figure.

For both programs, a later age first observed is associated with lower age 26 earnings and a later age last observed is associated with higher age 26 earnings. In the left panels, which display differences by age first observed, the coefficients are precisely estimated, they begin around zero, and decline roughly linearly to -0.533 (for public housing) and -0.514 (for HCV-assisted housing). The slopes are -0.066 for public housing and -0.058 for HCV-assisted housing indicating that, on average, first participating in public or HCV-assisted housing one year earlier increases age 26 earnings by 6.8% and 6.0%.

Similarly, the right panels show coefficients on age last observed that are increasing in age, albeit less linearly than figures on the left. For public housing, all ages older than 12 predict higher age 26 earnings than having a last age observed in public housing of 12. Subsequent pairwise comparisons of adjacent coefficients generally, but not always, indicate that being last observed in public housing at a later age leads to higher age 26 earnings. The slope from the best fit linear line through the age-specific point estimates is 0.088 so that, on average, being last observed in public housing one year later is predicted to increase age 26 earnings by 9.2%. For HCV-assisted housing, the coefficients on age last observed first fall below zero before rapidly increasing and settling at 0.21 for sample members last observed at age 19. The best fit line has a slope of 0.032 so that, on average, being last observed in HCV-assisted housing one year later predicts a 3.3% increase in age 26 earnings. The estimates in Figure 2 are remarkably similar, though slightly larger, than the main estimates shown in Table 3 (discussed above). However, being first observed one year earlier or last observed one year later does not perfectly predict a one-year increase in total program participation. We therefore should expect the slopes to be slightly larger in absolute value than our main estimates.

We also explore relaxing the linearity assumption in the main results by replacing the count of years spent in HCV-assisted and public housing with a full set of indicators for whether each individual spent {1,2,3,4,5,6} years in each program with 0 serving as the omitted category. The household fixed effects and remainder of the specification are identical to equation (1). The results (see Appendix Figure B1)—which are shown separately by sex and assisted housing program—appear broadly linear with coefficients increasing in the number of years in a program. All adjacent comparisons, excluding the 0 to 1 difference for HCV-assisted housing, suggest more time in assisted housing is better for age 26 earnings. The male HCV-assisted housing coefficients are negative until four years of participation, but this appears to be driven by males with zero years having unusually high age 26 earnings.

The results in Figures 2 and B1 reinforce the main HFE findings. Figure 2 suggests that relying only on sharp variation in the timing of moves across households yields similar conclusions to using only within-household variation in exposure to assisted housing. This is true regardless of whether the sharp variation comes from a later move out or an earlier move into assisted housing. The combination of results, which are consistent with 13-18-year-old assisted housing participation having a roughly linear dose-response relationship with age 26 earnings, are hard to reconcile with alternative explanations, and support the interpretation that effects relate to the extent of exposure to assisted housing.

B. Race/ethnicity subsamples

To further explore the results in Table 2, we investigate how the estimates differ by household race/ethnicity. That is, we estimate equation (1) separately for White non-Hispanic households, Black non-Hispanic households, and Hispanic households. The control variables are unchanged from Table 2, but the indicators for household race/ethnicity by sex are now absorbed by the male dummy variable. The estimated treatment effects are summarized in Table 3 with complete results reported in Appendix Table B2.

Comparing results across the three subgroups in Table 3, we find either a positive effect or no effect of public and voucher assisted housing on age 26 earnings for all possible sex by household race/ethnicity groupings. We do, however, observe some important differences. The positive effects for females in Table 2 are driven by females in Black non-Hispanic and Hispanic households, whereas males have positive effects in all three race/ethnicity samples. The point estimates for Black non-Hispanic females imply they receive an IHS earnings premium of about 0.062 per year in HCV housing and 0.072 per year in public housing. These translate to increases in age 26 earnings of 6.4% and 7.2%, respectively. Black non-Hispanic males also see their earnings at age 26 increase, by about 3.1% and 5.9% per year of residence in HCV-assisted and public housing; White non-Hispanic males have age 26 earnings that are 3.5% and 7.5% higher for additional years of HCV-assisted and public housing. The estimates for White non-Hispanic females are small and not significantly different from zero for either program. Finally, age 26 earnings increase for Hispanic males by 3.0% and 5.8%, and for Hispanic females by 4.5% and 9.2%, from additional years of HCV and public housing, respectively.

For no household race/ethnicity/sex cell are the effects of HCVs and public housing statistically significantly different from one another at the 5% level. Given the prevailing view of public housing in the United States, this lack of significant differences is perhaps somewhat surprising. It must be noted, however, that public housing programs differ greatly across geographic areas and frequently are quite unlike the oft-cited worst-case scenarios. We find that public housing residence in childhood has substantial positive benefits for age 26 earnings for all but one race/ethnicity/sex group, with White non-Hispanic females being the lone exception.

There is also heterogeneity in the effects by sex: females benefit more than males from HCVs in the pooled and Black non-Hispanic samples, and males benefit more than females from HCVs and public housing in the White non-Hispanic sample.

We estimate HFE specifications by household race/ethnicity with an indicator for whether individuals worked at all during their age 26 year as the outcome. Appendix Table B3 presents these results. The results closely match those presented in Table 2 for Age 26 earnings. The HFE treatment effects indicate that additional time spent in the HCV program while a teenager increases the likelihood of being employed at age 26 by 0.8 percentage points for Black non-Hispanic females and 0.4 percentage points for Black non-Hispanic males. The corresponding estimates for public housing are a 0.6 percentage point increase for females and a 0.4 percentage point increase for males in Black non-Hispanic households. Patterns are similar in magnitude and direction for Hispanic females and males but somewhat attenuated for White non-Hispanic females, as was the case with the IHS of earnings at age 26 outcome.

C. Adult incarceration

Table 4 presents linear probability model estimates of the effect of subsidized housing on adult incarceration, using the same controls as our main HFE results in Table 2. For the full sample, we find that an additional year of HCV housing reduces the likelihood of 2010 incarceration for females and males by 0.4 and 0.1 percentage points, respectively. Similarly, a year of public housing reduces incarceration for females and males by 0.5 and 0.3 percentage points. In columns 2, 3, and 4, we find similar patterns for each of the household race/ethnicity subsamples. The largest reductions in adult incarceration are found for females in Black non-Hispanic households: 0.7 percentage points for additional years of HCV-assisted and public housing. The effects for males from Black non-Hispanic households are 0.3 and 0.4 percentage point decreases in adult incarceration from public and HCV-supported housing. Appendix Table B4 shows estimated effects on 2010 incarceration using alternative functional forms for binary dependent variables. Both a logit with Chamberlain-Mundlak controls (Mundlak 1978; Chamberlain 1980) and the two-step logit proposed by Miller, Shenhav, and Grosz (2019) suggest that additional time in assisted housing reduces the likelihood of 2010 incarceration, albeit with average marginal effects smaller than those suggested by the linear probability models with HFE.

Although the effects of youth subsidized housing participation on adult incarceration presented in Table 4 are independently important, they are also likely to be closely related to the earnings effects shown in Tables 2 and 3.[19] Adult incarceration could lead to decreases in expected adult earnings because of incapacitation effects, recidivism, reduced self-sufficiency, or because formerly incarcerated individuals receive a negative earnings premium from disinvestment in human capital or from having a criminal record (Mueller-Smith 2014). However, the direction of

[19] Note that for this sample, the indicator for 2010 incarceration may be measured at ages 23 to 28, spanning the age 26 earnings outcome. Since we do not have information on the duration or frequency of incarceration, we do not attempt to disentangle the sequence of earnings changes and incarceration episodes.

causality is not obvious. A reduction in potential earnings could also increase the likelihood of incarceration through various behavioral and environmental pathways. Kling, Ludwig, and Katz (2005) present evidence on how neighborhood poverty affects youth criminal behavior.

While a comprehensive analysis of the relationship between adult incarceration and adult earnings is outside the scope of this paper, we do attempt to gauge how important the observed association between youth subsidized housing and incarceration may be for explaining our earnings results. In Appendix Table B5, for each sex by race/ethnicity group, we calculate the share of the observed effect of youth subsidized housing participation on age 26 earnings that can be potentially explained by the incarceration effects presented in Table 4. To obtain these figures, we multiply the effect of a year of additional subsidized housing participation on the likelihood of 2010 incarceration by the average association between 2010 incarceration and age 26 earnings based on HFE regressions by sex and household race/ethnicity with the same controls as equation (1). We then divide this expected earnings difference by the direct effect of housing on earnings (the estimates shown in Table 3) and multiply the resulting fraction by 100 to get an estimate of the percent of the earnings that could be explained by the observed differences in incarceration. Appendix Table B5 does not report results when the earnings effect was not statistically significant, instead displaying "(0)."

Appendix Table B5 suggests that reductions in incarceration can account for approximately a quarter of the positive effect of subsidized housing on earnings. This pathway has the greatest explanatory potential for Black non-Hispanic households: explaining roughly 35.8% and 40.0% for female and males in HCV-assisted housing and 40.4% and 29.0% for females and males in public housing. We remain agnostic about whether the associations between subsidized housing participation and incarceration are causes, or consequences of the main earnings effects, but the alignment and strength of the associations suggests that subsidized housing may have multi-dimensional long-term benefits for children.

V. Robustness

The HFE specifications are still susceptible to bias from time-varying shocks or unobserved individual-level characteristics. If present, strict income eligibility requirements imply we would expect to find a negative association between household income and assisted housing participation; reductions in income will increase, and increases in income will reduce, the likelihood of eligibility for assisted housing. If longer or earlier exposure to changes in economic conditions have larger impacts on potential adult outcomes (Cunha and Heckman 2007; Duncan, Morris, and Rodrigues 2011; Hoynes, Schanzenbach, and Almond 2016; Aizer et al. 2016; Hoynes and Schanzenbach 2018; Brown, Kowalski, and Lurie 2019), then this would bias treatment effects downwards as we would incorrectly attribute effects of changing economic conditions to changes in assisted housing. Non-economic changes could also potentially lead to positive or negative bias in the HFE estimates. We conduct several robustness checks to assess the likelihood that the HFE estimates are affected by these potential confounders.

If anything, income eligibility requirements should result in children with more time in assisted housing also having longer exposure to worse economic circumstances. However, whether the association is meaningful in size and whether it persists after conditioning on household fixed effects are unclear. In Appendix C, we describe an event study specification to measure the relationship between parents' earnings, which could vary across children between the ages of 13 and 18, and changes in assisted housing participation over time. Consistent with the income eligibility story mentioned above, Appendix Figure B2 shows that parents' earnings decrease prior to entry into public and HCV-assisted housing and increase prior to exits from HCV-assisted housing. We find no evidence of pre-move changes in parents' earnings prior to an exit from public housing. If the only impacts on the subsequent earnings of children operate through observed changes in parents' earnings, the event study estimates suggest that the HFE estimates will be downward biased. We note that the potential for the event study differences to affect the HFE estimates is limited by the fact that the siblings in our sample share exposure to household economic conditions through the vast majority of their early lives.[20]

If the observed changes in parents' earnings are accompanied by unobserved changes in other determinants of future success, HFE specifications could still be biased in an indeterminate direction. Changes in household structure are one factor that may have positive or negative consequences with potentially opposite effects of observed changes in household earnings. For

[20] To empirically gauge the potential importance of these differences, we conduct back-of-the-envelope calculations using the event study coefficients and estimates of the long-term impact of family income in childhood on the future adult income of children from research on the earned income tax credit (EITC) (Bastian and Michelmore 2018). The results reinforce that the biases in HFE estimates due to changes in parents' earnings are likely small in magnitude and negative in sign, with little impact on the main conclusions. See Appendix Figure B3 and discussion in Appendix C for details. The event study results suggest that the main estimates are likely to be underestimated by no more than 0.0048 (for moves into HCV-assisted housing). The main HCV estimates would at most increase from 0.027 to 0.032 for males and from 0.047 to 0.052 for females; public housing estimates would at most increase from 0.059 to 0.063 for males and from 0.06 to 0.064 for females.

example, consider the removal or entrance of an abusive or criminal parent.[21] Recent work finds evidence that parental incarceration may reduce adult incarceration and improve neighborhood quality for children (Norris, Pecenco, and Weaver 2021). If the incarcerated adult participated in the formal labor market, we would observe a negative correlation between parents' earnings and future outcomes for children, and a positive correlation between eligibility for assisted housing and the same outcomes; as a result, this could produce positive bias in the HFE estimates.

To assess whether this is problematic for the main results, we re-estimate the HFE specifications on a sub-sample of children from "stable" households. For these children, either the household was single-headed in 2000 and remained single-headed in 2010, or the head of household and their spouse remained members of the same household in 2000 and 2010. Children in the stable sample are less likely to benefit or suffer from changes in household structure because, by construction, they experience no changes in parental marital status or parental identity from 2000 to 2010. As a result, any bias from the entrance or removal of an abusive or criminal spouse should be diminished. Column 1 of Table 5 presents the results. The implied increases in age 26 earnings from additional years of public or HCV-assisted housing remain large, positive, and statistically distinguishable from zero for males and females. If anything, the implied treatment effects are slightly larger for this sample. We conclude that positive bias from changes in household structure are unlikely to be driving our main findings.

Other unobserved time- or child-varying factors that are correlated with both within-household differences in outcomes and within-household differences in assisted housing participation could also be problematic. Birth order, pre-teen participation in assisted housing, or disciplinary removals from assisted housing could lead to bias.[22] If relevant, these factors predict treatment effects that differ between households that move into assisted housing (where younger children will have more exposure) and households that move out of assisted housing (where older children will have more exposure). For moves out, older children, who will have more—unobserved to us—early-life assisted housing participation, higher non-cognitive skills and education, or less exposure to discipline-inducing behavior problems, would also have more participation in assisted housing, potentially creating positive bias relative to moves into assisted housing. Conversely,

[21] We thank an anonymous referee for pointing this out.

[22] Previous research suggests that lower birth order (i.e., earlier-born children) is predictive of superior non-cognitive skills (Black, Gronqvist and Ockert 2018) and greater educational attainment (Black, Devereux, and Salvanes 2005; Bagger et al. 2021).

increases in parental effort should be more strongly associated with moves into assisted housing, as entry costs (e.g., paperwork, eligibility screenings, apartment search) are larger than the costs of maintaining eligibility (lease compliance and income verification). As a result, bias from changes in parental effort should be larger for households moving into assisted housing.

To explore the relevance of these potential issues for the HFE specifications we conduct three robustness checks. First, we interact 13 to 18-year-old assisted housing with an indicator for whether households were in either program in the first year of our data (1997) to check whether treatment effects are different for households likely to have moved out and households likely to have moved into assisted housing. All the potential confounders mentioned above predict impacts that are different for these two groups. Second, we limit the variation in assisted housing used to identify the treatment effects and only consider ages 14-17 (as opposed to 13-18 in the main results). Exploring whether the restricted estimates differ from the main results is informative for whether our focus on 13 to 18-year-old variation is likely to affect the estimates because of unobserved participation at other ages. Third, we estimate treatment effects separately for households that resided in a county in 2000 with assisted housing wait times above 9 months (approximately the median county-level wait time) and for those in counties with wait times below 9 months. In areas where households are quickly able to adjust participation, the association between unobserved time-varying heterogeneity and within-household differences in assisted housing is likely to be stronger and the estimates may be more susceptible to bias. Figure 3 indicates that about 12% of public housing residents and 29% of housing voucher recipients faced wait times of 1 year or more. Observing no differences in the public and HCV-assisted housing coefficients between households moving in and moving out, when limiting the variation to ages 14-17, and between short and long wait time areas offers support for the identifying assumptions and increases our confidence in the validity of the main estimates.

Columns 2-5 of Table 5 present the results. The estimates are remarkably stable across specifications. An additional year in HCV-assisted housing increasing the IHS of earnings at age 26 by between 0.044 and 0.055 for females and between 0.026 and 0.032 for males; the corresponding ranges for the public housing are between 0.059 and 0.077 for females and 0.055 and 0.078 for males. In all cases, additional time spent in either program predicts a meaningful increase in age 26 earnings. The interactions with participation in 1997 are all small—suggesting the impacts are not different for households moving in and those moving out—and we can never reject the null of no difference between low and high wait time areas. That we do not find a differential impact for "move-in" and "move-out" households, and that we see similar effect sizes and magnitudes in the semi-parametric specifications that rely on sharp variation in age first and last observed—which are not plagued by the same concerns—indicate that the above listed confounders are not affecting the impacts for 13-18-year-old participation.

In addition to the robustness checks in Table 5, the main estimates are unaffected by adding controls for birth order fixed effects, whether we use survey weights,[23] and whether we include child-specific controls for parents' earnings or block group poverty rates (Appendix Table B7).[24] Adjusting inference for multiple hypothesis testing when we explore impacts separately by household race/ethnicity and sex by controlling for the false discovery rate with sharpened q-values (Anderson 2008) similarly has no impact on the results (see Appendix Table B8); pooled results are strong enough that they survive even overly conservative adjustments (e.g. Bonferroni corrections). Relaxing the age restrictions on participation and using a simple count of the number of years between 1997 and 2005 yields slightly smaller but still meaningful and statistically significant impacts of both programs (Appendix Table B9); including households up to 100% AMI in the estimation sample also has no impact on the main results (Appendix Table B10).

VI. Heterogeneity

Housing subsidy programs are implemented by local housing authorities across a diverse set of areas where they serve a uniformly low-income but still heterogeneous population. These household, neighborhood, and metro area characteristics may predict program impacts. In addition to household race/ethnicity, we explore how the treatment effects vary with metropolitan statistical area (MSA) size, neighborhood poverty, and household percent of AMI—calculated as the minimum (across siblings) average parents' LEHD earnings between ages of 13 and 18.

Table 6 presents results that allow for the effect of assisted housing to differ for households in high-poverty tracts (1990 poverty rate greater than 40%) in large MSAs (>2.5 million inhabitants in 2000); for public housing residents, this classification parallels the eligibility requirements for participation in the MTO experiment. Households not residing in MTO-like, high-poverty tracts

[23] See Appendix C for further discussion. Birth order results are shown in Appendix Table B6 and weight-related results are available upon request.

[24] These results must be interpreted with caution given that the controls include some post-participation outcomes that may be impacted by participation in subsidized housing. It is nevertheless instructive that the main results are not impacted by inclusion of these controls.

in large MSAs experience benefits similar to those seen for the full-sample. We are unable to reject that there is no effect of either program for households in high-poverty tracts from large MSAs. However, for this sub-sample the male HCV-housing point estimate is roughly three times larger, and the public housing point estimate is around one-third of the size of the corresponding estimates for the full sample.

Table 7 presents results that allow for heterogeneity by household earnings defined as the percent of AMI for each household based on their 2000 place of residence (there is no within household variation in this measure). Results at the mean household percent of AMI for the sample (the non-interacted coefficients) are comparable, though somewhat smaller than the main results. Both the interactions with household percent of AMI are meaningful in size and statistically distinguishable from zero. For both programs, a 10-percentage point decrease in household percent of AMI leads to a per-year impact of assisted housing that is 0.02 (2%) higher for females and males; lower-income households therefore benefit more from access to assisted housing. This could be driven by lower-income households having counterfactual housing options that are of especially low quality, or housing subsidies enabling lower-income households to increase expenditure on goods or allocate time to behaviors that are especially high return for the future outcomes of children.[25]

Last, we calculate the HFE estimates separately for different geographic areas—by county for those with more than 6,000 individuals in the data and by state (for smaller counties)—and compute county or state-level values of the prevalence of crime, income inequality, unemployment, poverty, median household income, racial segregation, intergenerational mobility, and school quality.[26] We then estimate OLS regressions of the public and HCV-assisted housing treatment effects on the county or state-level characteristics, including a control for whether the observation corresponds to a large county or a state-level residual. The results are presented in Appendix figures B4 and B5. For HCV-assisted housing we find little evidence of associations between the observable characteristics and the program impact. For public housing the regressions

25 We find little heterogeneity in public housing project average income and size (Appendix Table B11), metro area housing affordability (column 2 of Appendix Table B12), or household size for the 95% of households with two or three 13 to 18-year-olds in 2000 (Appendix Table B13); there is some evidence that HCV-assisted housing is more beneficial for households with a longer average duration in the program (column 1 of Appendix Table B12) and that public housing is more beneficial for households in non-metro areas (Appendix Table B14).

26 County and state-level characteristics are converted to Z-scores by de-meaning and scaling by the sample standard deviation. Crime data from UCR (2000); income inequality, unemployment, poverty median household income, racial segregation from Ruggles et al. (2017); intergenerational mobility data from the Opportunity Atlas (Chetty et al. 2018c); school quality data from HUD (2016).

suggest that the program may be more beneficial in counties that are higher-poverty, lower-income, and that have lower school quality.

The public and HCV-assisted housing correlations in Figures B4 and B5, along with the evidence presented in Table 7, offer support for the idea that the primary mechanism for the treatment effects is an income effect for recipient households. The effects of assisted housing are largest for lower-income households and impacts are no larger in areas with better schools, lower crime, or higher-income neighbors. Alternative narratives, where housing assistance is a complement to either household earnings or place-based amenities are not supported by the data. Appendix Table B15 offers additional evidence that changes in neighborhood characteristics are unlikely to drive the main impacts; HFE regressions of the average census tract or county-level characteristics between ages 13-18 on assisted housing participation show, if anything, that public housing participation is associated with exposure to slightly lower-income, higher-unemployment, and lower-mobility neighborhoods. However, the associations are always small in magnitude, underscoring that sample households are likely to reside in disadvantaged neighborhoods even when they are not participating in assisted housing.

VII. Lifetime Earnings and Cost-Benefit Analysis

While our main estimates suggest there is a statistically significant and economically meaningful increase in age 26 earnings from additional years spent in public and HCV-assisted housing as a teenager, this captures only a small fraction of the expected increase in lifetime earnings. We therefore follow Chetty, Hendren, and Katz (2016) and Chyn (2018) and use the main treatment effects for age 26 earnings to estimate the expected benefit for lifetime earnings from an additional year of public or voucher-assisted housing. To do so, we make four key assumptions: (1) the HFE treatment effects shown in Table 3 are constant over the lifecycle for males and females; (2) the trajectory of lifetime earnings for the children in our sample follows the pattern suggested by the U.S. population average; (3) real earnings growth is 0.5% per year; and (4) the discount rate is 3%, slightly below the highest 30-year Treasury bond rate observed in 2019.

To estimate the trajectory of lifetime earnings at the U.S. population average, we tabulate the sum of wages, salary, commissions, bonuses, and tips across all jobs by age using the 2000 Census 5% sample microdata from the University of Minnesota's Integrated Public Use Microdata Series (Ruggles et al. 2017). Adding together annual earnings from 19 to 65—after applying a real wage growth rate of 0.5%—gives an undiscounted sum of lifetime earnings of $1.51 million in 2000 US$ for the average worker, and a present discounted value *at age 12* of $618,000.[27] Under the above assumptions and using the main treatment effects for age 26 earnings shown in Table 3, each additional year of 13-18-year-old public housing participation increases undiscounted total pre-tax lifetime earnings by $53,600 for females and $52,700 for males; an additional year of teenage voucher-assisted housing increases undiscounted total pre-tax lifetime earnings by $41,500 for females and $23,300 for males.[28] The analogous estimates for discounted total pre-tax lifetime earnings are $21,900 (females) and $21,500 (males) for public housing and $16,900 (females) and $9,500 (males) for HCV-assisted housing.

For a hypothetical household with two children, in expectation, this suggests that each additional year of public housing participation while children are 13-18 increases total discounted pre-tax lifetime income for the children by between $43,000 and $43,800 and each additional year of voucher-assisted participation increases total discounted pre-tax lifetime income for the children by between $19,000 and $33,800, depending on the sex of the children. Clearly, childhood participation in assisted housing produces meaningful long-term economic returns by increasing expected adult earnings for children in beneficiary households.

With these estimates, we conduct a cost-benefit calculation from the perspective of the U.S. federal government. In doing so, we emphasize that we only consider one of potentially many benefits of subsidized housing, and therefore the estimated benefits should be interpreted as a lower bound on the true lifetime benefits of assisted housing participation.[29] We assume that the children in our sample fall into the bottom federal income tax bracket, file their taxes as singles or as married individuals filing separately, that they elect to take the standard deduction for their filing status, and that they earn above the standard deduction even in the absence of any teenage subsidized housing participation. As a result, the federal government receives 10% of the expected increase in lifetime earnings. For a household with two children, this implies an expected

[27] Note that we discount earnings to age 12 rather than birth because we only use program exposure that occurs after this age.

[28] The mean age 26 total earnings for the individuals in our sample who never participate in subsidized housing while a teenager is 57% of the U.S. population average. We therefore calculate the estimated impacts on undiscounted lifetime earnings of an additional year of teenage participation in subsidized housing as being equal to TE x 0.57 x $1.51m, where TE is the relevant treatment effect from Table 3 (e.g., the estimated impact of an additional year of public housing for females: 0.062). The corresponding estimates for discounted lifetime earnings are calculated as TE x 0.57 x $618,000.

[29] Important omitted benefits include the immediate utility from the income transfer received by the participating household, any increased housing stability or positive long-term health effects generated by the subsidy, and any decrease in government costs related to lower incarceration rates. See Desmond et al. (2015) for a detailed discussion of the value placed on stability for poor households.

discounted benefit to the federal government of between $4,300 and $4,380 for each additional year of public housing and between $1,900 and $3,380 for each additional year of HCV participation while both children are teenagers.

To calculate the cost to the federal government we use publicly available "Picture of Subsidized Households" data from HUDUSER for the year 2000 (HUD 2000). We compute the national average federal spending per unit per month for both public housing and the HCV program. For public housing, this is the sum of the operating subsidy and the capital improvement cost scaled by the number of occupied units; for the HCV program, it represents the total housing assistance payment (payment to landlords) plus an administrative cost divided by the total number of reported participating households. The results suggest that the average cost per year for a unit of public housing is $5,112 and the average cost per year for one year of HCV-assistance is $5,124. Even considering only the intergenerational financial benefit to the federal government from providing housing assistance to households, the expected increase in tax revenue covers 84%-86% of the cost of supplying public housing and 37%-66% of the cost of supplying HCV-assisted housing.

VIII. Comparison of Results to the Literature

Research using variation in assisted housing participation generated by MTO, the Chicago HCV lottery, and public housing project demolitions in Chicago generated rigorous evidence on the long-term impacts of public housing and HCV-assisted housing. Here, we compare our findings with the results from four studies that present estimates on long-term earnings outcomes for affected children: Chetty, Hendren, and Katz (2016), Sanbonmatsu et al. (2011), Jacob, Kapustin, Ludwig (2015), and Chyn (2018). To make the comparison, we use per-year HFE estimates for different race/ethnicity by sex cells from Appendix Table B1 of this paper to calculate the expected difference in age 26 earnings from a year spent in HCV-housing relative to a year spent in public housing or a year spent in HCV-housing relative to a year spent in unsubsidized housing. We use the sample characteristics (e.g., the share Black non-Hispanic, the share male) in the existing studies as weights. When relevant, we also convert intent-to-treat (ITT) estimates into local average treatment effects (LATE) intended to measure expected differences in earnings from one additional year of program participation. Details on the calculations can be found in Appendix E.

Sanbonmatsu et al. (2011) and Chetty, Hendren, and Katz (2016) use experimental variation in HCV-assisted and public housing generated by the MTO program. Our sample is, on average, 15.42 years old, so we focus on the Chetty, Hendren, and Katz (2016) results for children 13 and older and the Sanbonmatsu et al. (2011) results for grown children under 18 at baseline and over age 20 in 2007 (average age 12 at baseline). Jacob, Kapustin, and Ludwig (2015) use variation from the Chicago HCV lottery to provide estimates of the long-term return to HCV-assisted housing relative to private housing. We use their instrumental variables (IV) estimates for annual adult earnings for children 6-18 at the age of random assignment. Chyn (2018) uses variation from public housing project demolitions in Chicago to calculate Two-Stage Least Squares estimates of the change in earnings from additional years of public housing (relative to HCV-assisted housing).

For the MTO studies, we aggregate the estimates across the experimental and HCV treatment arms, using the share of observations in each arm as weights. We then convert them to per year effects by dividing by the expected difference in years spent in HCV-assisted instead of public housing through age 18 because of the program: 19 minus the average age at baseline for the MTO sample, multiplied by the take-up gap. For Jacob, Kapustin, and Ludwig (2015) we convert the IV estimates to per year effects by dividing by the expected difference in HCV-assisted housing participation using 12 as the average age for 6 to 18-year-old sample. No conversion is required for Chyn (2018) other than multiplying by -1 so that the estimates capture the effect of an additional year of HCV housing relative to public housing.

We adjust our Table 3 estimates to match the sex and household race/ethnicity distributions in the four studies, using the share of study respondents that were male, Black non-Hispanic, Hispanic, and White non-Hispanic to aggregate our household race/ethnicity and sex-specific comparisons. For Chyn (2018), we assume the race/ethnicity distribution is the same as in Jacob (2004), for which 100% of children were Black non-Hispanic.

Table 8 presents the results. Column 1 shows the comparison being made (e.g., HCV to public housing), Column 2 displays the sex of the sample observations, Column 3 presents the estimand, Column 4 presents the estimated treatment effects—averaging across the experimental and HCV arms for MTO—Column 5 shows the per year LATE estimates in 2000 US$, and Column 6 presents our per year treatment effect aggregated as described in the previous paragraph.

Our results are most similar to the MTO findings, especially those in Chetty, Hendren, and Katz (2016) which allow us to compare impacts for children of similar ages (15.1 years relative to 15.4 in our data). We estimate an age 26 earnings increase of 164.8 US$ from an additional year of public housing relative to HCV-assisted housing. The corresponding estimate from Chetty, Hendren, and Katz is 132.9 US$ per year. Both papers suggest that older children benefit more from public housing, though the difference is not statistically significant in either paper. The similarity is striking, particularly given that the MTO results are pooled across treatment households that were required to move to low poverty neighborhoods and others that received conventional HCVs with no neighborhood poverty restrictions.

Chetty, Hendren, and Katz find a larger HCV-PH difference in adult outcomes for children who were younger at the time of randomization. They argue that this discrepancy could be driven by disruption effects, whereby only younger children have enough exposure to lower-poverty neighborhoods to overcome the negative disruption effects from moving. Disruption effects could be pertinent for our sample. However, all individuals in our sample with within-household variation in assisted housing must have some exposure to a move,[30] and the comparatively narrow range of ages in our sample implies that disruptions occur at similar ages; this may make it more likely that disruptions have similar impacts on future adult earnings for everyone and is partly why we focus on the results for the MTO sub-sample older than 12 at the time of randomization.

We are also able to compare both programs to unsubsidized housing. Doing so using the same weights as above suggests that the average (HCV-PH) difference in age 26 earnings is approximately a third of the size of the benefit from HCV-assisted housing relative to time spent in unsubsidized housing: 440.0 US$ per year.

The results in Sanbonmatsu et al. (2011), which do not disaggregate effects by age, suggest that each additional year of HCV-assisted housing increases adult earnings by 80 US$ for females and decreases adult earnings by 403.4 US$ for males, relative to remaining in public housing; our results suggest an 83.3 US$ decrease in age 26 earnings for females and a 261.5 US$ decrease for males. While the effects are similar, there is a more pronounced divergence by sex in the MTO results. Pooling across sex, the results in Sanbonmatsu et al. predict a 199.7 US$ decrease in annual earnings from an additional year in HCV housing relative to public housing. Our estimates predict a 173.9 US$ decrease per year. In sum, the differences between HCV-assisted and public housing in our data track the MTO findings closely, particularly when using children of similar ages.

Our results are less similar to those in Jacob, Kapustin, and Ludwig (2015) and Chyn (2018) which are based on data from Chicago. Jacob, Kapustin, and Ludwig find no difference in adult

[30] In comparison, treatment MTO households are more likely to move than control households.

earnings between children in households exposed to additional time in HCV-assisted housing while we find important gains from HCV-assisted housing: 657.1 US$ per year for females and 301.6 US$ per year for males. Chyn (2018) suggests that each additional year of HCV-assisted housing increases adult earnings by roughly 200 US$, relative to time spent in public housing. Ignoring differences in samples and treatment effect heterogeneity the combined results indicate that—in Chicago—assisted housing is not beneficial for adult earnings: there are no gains from HCV-assisted housing and potentially adverse effects from public housing. We find the opposite pattern in our data.

While Chicago is one of the MTW cities not included in our data, publicly available data from HUD can highlight some contextual differences between Chicago and the United States as a whole (HUD 2000). On average, public housing residents reside in census tracts with higher poverty rates everywhere, but the gap is much larger in Chicago, and larger still for the public housing projects in Chyn (2018). In Chicago, mean census tract poverty rates in 1999 were 19.3% overall, 42% for households in public housing, and 28% for households in HCV-assisted housing. The corresponding figures for the United States were 12.9%, 20%, and 29%, respectively. These are less disparate than the Chicago-specific figures and substantially lower in levels. The pre-demolition poverty rates in Chyn (2018) were even higher, with an 82% tract poverty rate, on average (Jacob 2004). Conversely, the unsubsidized households in the Chicago HCV Lottery had census tract poverty rates comparable to the city-wide average for HCV recipients (30%) and the intervention had no impact on neighborhood poverty. The public housing residents in Chyn (2018) therefore may have had more room to benefit from changes in neighborhood than public housing participants more broadly given their position in the neighborhood poverty distribution and the likely locations for HCV-assisted housing in Chicago. For unsubsidized households in Jacob, Kapustin, and Ludwig (2015) finding housing in lower-poverty neighborhoods may have been more difficult as they resided in neighborhoods close to the city-wide HCV average neighborhood poverty prior to the program. If, as MTO and other work from Chetty et al. (2018a, 2018b) find, effects of place and neighborhood poverty in particular are important predictors of long-term outcomes for children, then we should expect smaller benefits from public housing in Chicago where neighborhood poverty rates for public housing projects are particularly high. Likewise, we should see smaller returns to HCV-assisted housing for the households in Jacob, Kapustin, and

Ludwig (2015) who resided in neighborhoods with similar poverty rates to HCV-participants city-wide prior to the program.

While understanding the effect of encouraging neighborhood mobility through HCVs in contexts with extreme poverty is of critical importance, it is not necessarily informative about their effectiveness in more diverse settings. That is a key strength of our approach, which allows us to estimate impacts of both assisted housing programs for a population that closely resembles the full set of participants, projects, and neighborhoods across the United States.

IX. Conclusion

Despite the exposure of millions of children in low-earning households to subsidized rental housing and the potential for these programs to have effects on long-term outcomes, the existing literature lacks a well-identified comparison of public housing, HCV-assisted housing, and private-market housing. This study estimates the long-term effects of living in public and HCV-assisted housing as an adolescent on age 26 earnings and incarceration, enabling the direct comparison of both programs to each other and to private-market housing.

We estimate household fixed effects models that identify the impact of assisted housing exploiting only variation within households. This between-siblings approach allows us to isolate the effect of each type of subsidized housing on adult outcomes from household-level differences that may affect both program participation decisions and adult outcomes. We find that the substantial negative effects of subsidized housing in naïve OLS specifications are entirely attributable to the selection of households into assisted housing. After accounting for this selection, assisted housing participation as a teenager yields substantial positive effects on age 26 earnings for both females and males relative to unsubsidized housing. Less parametric OLS regressions of IHS earnings at age 26 on the age each individual is first and last observed in public or HCV-assisted housing produce estimates for moving in a year earlier or departing a year later that are in line with the main HFE estimates.

We convert the age 26 earnings into expected differences in lifetime earnings. The results suggest that additional participation in public and HCV-assisted housing leads to substantial increases in lifetime earnings. Increasing 13 to 18-year-old public housing participation by two years increases total discounted pre-tax lifetime earnings in 2000 US$ by $43,800 for females and $43,000 for males, while two additional years of teenage HCV participation increase total discounted pre-tax lifetime earnings by $33,800 for females and $19,000 for males. The increase in lifetime earnings is sufficiently large that assisted housing pays for between 37.1% and 85.7% of the cost of providing it, even when only considering the implied increase in tax revenue from intergenerational increases in earnings. We also find important reductions in the likelihood of adult incarceration, particularly for children from Black non-Hispanic households.

There remain limitations to our analysis. First, our results apply to just two of the largest HUD-subsidized housing programs—public and housing choice voucher-assisted housing. The project-based housing voucher program, which serves a somewhat higher proportion of elderly households and a lower proportion of families with children, is not considered, nor is the Low-Income Housing Tax Credit program. Second, our results may not be representative of all subsidized households. We exclude households with only younger children, and those with just one teenager. However, the sub-population for which we estimate treatment effects—households with two or more teenagers born within a 6-year range—represent a large and important fraction of subsidy-eligible households. While this is a formative period, other research has suggested that early childhood circumstances may be even more important predictors of long-term outcomes. Future work should investigate whether exposure to subsidized housing during earlier periods of life has long-term implications as well.

Public and HCV-assisted housing participation while a teenager has meaningful and beneficial effects on both age 26 earnings and incarceration. Though the increased neighborhood choice afforded to participating households suggests there could be higher returns to HCV-assisted housing than public housing, we find no evidence that children who grow up in HCV-assisted housing do better than children who grow up in public housing as adults. One possibility is that, without financial incentives or intensive counseling, households in the HCV program are unlikely to move to appreciably better neighborhoods. Future research should explore how the local rental housing market as well as the physical and social characteristics of public housing projects affect the long-term benefits of HCV-assisted and public housing for children.

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Figures and Tables

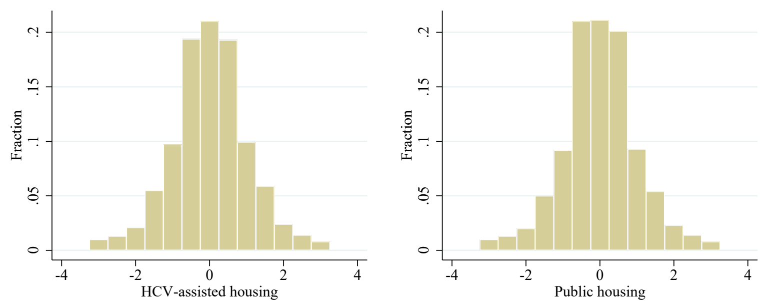

FIGURE 1. WITHIN-HOUSEHOLD DIFFERENCES IN ASSISTED HOUSING PARTICIPATION

Notes:
HCV is Housing Choice Voucher-assisted housing. Figure displays the distribution of within-household differences in public housing and HCV-assisted housing participation between the ages of 13 and 18 for the individuals in the main sample in households with at least some participation in public housing (for the public housing figure) or HCV-assisted housing (for the HCV figure). Main sample includes individuals aged 13-18 in 2000 from households below 50% area median income in non-owner occupied housing. Bars display the fraction of sample individuals in each one-half year width bin, with the middle bin spanning -0.25 to 0.25. The top and bottom bins include all individuals with a within-household difference greater than 2.75 and less than -2.75, respectively. Note that individuals with no household participation in either program are included in the main sample but are omitted from the sample used to produce the figure in order to facilitate the presentation of the results. Based on authors' calculations from matched 2000 Census, HUD-PIC, and LEHD files.'

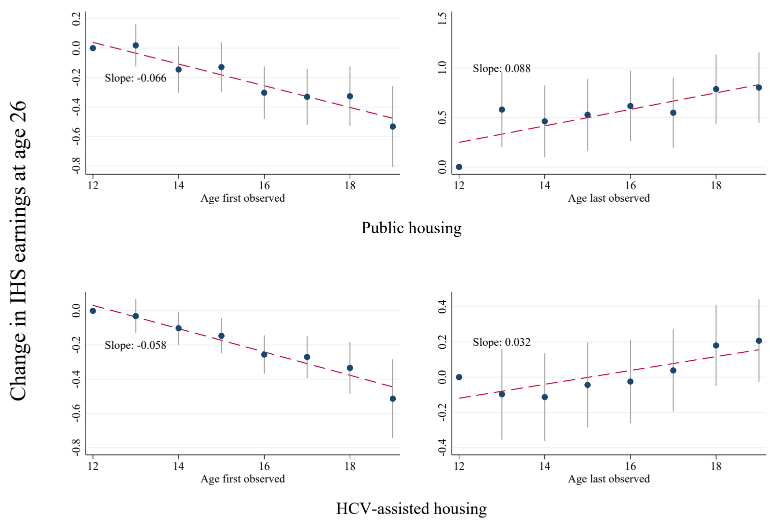

FIGURE 2. AGE FIRST AND LAST OBSERVED IN ASSISTED HOUSING AND IHS EARNINGS AT AGE 26

Notes:
HCV is Housing Choice Voucher-assisted housing. Figure displays the coefficient and 95% confidence interval for being first and last observed in Public or HCV-assisted housing at different ages between 12 and 19 based on a regression of the inverse hyperbolic sine (IHS) of earnings at age 26 on indicators for the first age observed in a program, the last age observed in a program, age by male fixed effects, male by household race/ethnicity fixed effects, and state by age in 2000 fixed effects. In all cases, the coefficients capture the expected change in the inverse hyperbolic sine of age 26 total labor market earnings relative to individuals who were first or last observed in the program at age 12. The dashed line displays the linear prediction from a regression of the point estimates against the age first and last observed in the two assisted housing programs using just the age indicators for 12 to 18. The sample is limited to individuals from the main sample who are first and last observed in public housing or HCV-assisted housing between the ages of 12 and 19 to avoid including participation that occurs after having departed their Census 2000 household, to more closely match the variation we use in the main specifications that is focused on participation between 13 and 18, and so as not to compare movers to children who never experienced a change in assisted housing participation in our study period. The main sample includes individuals aged 13-18 in 2000 from households below 50% area median income in non-owner occupied housing. 95% confidence intervals based on robust standard errors clustered at the household level. Based on authors' calculations from matched 2000 Census, HUD-PIC, and LEHD files.

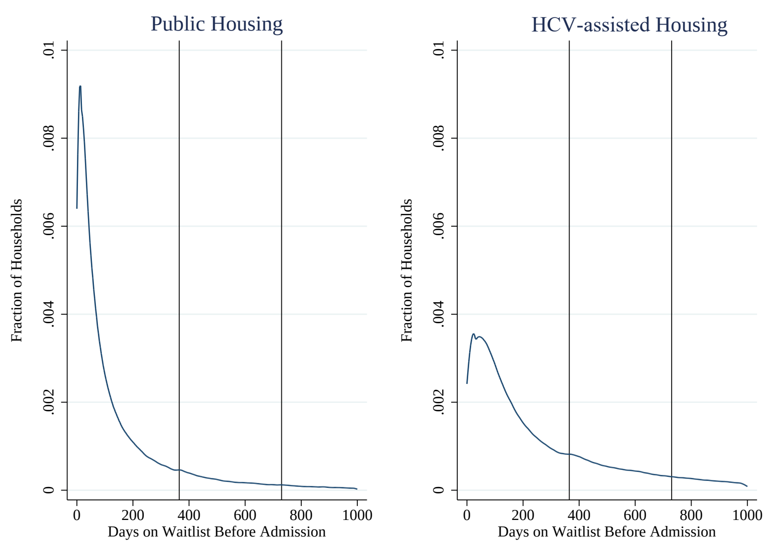

FIGURE 3. DAYS ON PUBLIC OR HCV-ASSISTED HOUSING WAITLIST PRIOR TO PROGRAM ADMISSION IN 2000

Notes:
Figure displays the distribution of days spent on the waiting list before admission for households found in both public and voucher housing in the year 2000. The sample is limited to households with non-missing admission and waitlist information who gained admission to their program no earlier than 1995. 11.6% of public housing households spent >1 year and 3.3% spent >2 years on a waitlist prior to admission. 28.7% of voucher housing households spent >1 year and 10.8% spent >2 years on a waitlist prior to admission.

TABLE 1—CHARACTERISTICS OF HOUSEHOLDS IN PUBLIC OR HCV-ASSISTED HOUSING IN 2000

	13-18 in 2000 with at least one other sibling 13-18				
			In a household that received a subsidy		
	Total (1)	In households not receiving any housing subsidy (2)	Total (3)	Never lived in subsidized housing while 13-18 (4)	Lived in subsidized housing while 13-18 (5)
Household size in 2000	5.355	5.331	5.414	5.669	5.368
Age in 2000	15.42	15.46	15.31	15.53	15.28
Male	0.499	0.504	0.487	0.507	0.483
White non-Hispanic household	0.345	0.402	0.203	0.230[a]	0.199
Black non-Hispanic household	0.289	0.216	0.471	0.449	0.475
Hispanic household	0.284	0.295	0.256	0.255	0.256
Other non-Hispanic household	0.082	0.087	0.069	0.065[a]	0.070[a]
Block group poverty rate while 13-18	0.113	0.109	0.121	0.120	0.122
Inverse hyperbolic sine parents' earnings	7.893	8.071	7.452	7.594	7.426
Total parents' earnings while 16-18 (US$)	36,080	39,640	27,230	29,130	26,900
Single-parent household	0.621	0.569	0.750	0.722	0.756
Public housing resident while 13-18	0.084	0.000	0.295	0.000	0.347
HCV recipient while 13-18	0.168	0.000	0.585	0.000	0.690
Years public housing while 13-18	0.292	0.000	1.018	0.000	1.199
Years HCV housing while 13-18	0.593	0.000	2.069	0.000	2.438
Labor market earnings 2008-2013 (US$)	69,580	74,700	56,850	55,800	57,040
Age 26 labor market earnings (US$)	11,820	12,680	9,674	9,427	9,718
Number of years worked 2008-2013	4.242	4.311	4.071	3.999	4.083
Observed in 2010 Census	0.765	0.778	0.730	0.721	0.731
Incarcerated in 2010	0.033	0.026	0.049	0.054[a]	0.045[a]
Observations	1,172,000	839,000	332,000	50,000	282,000
2010 Census Observations	672,000	502,000	171,000	25,000	146,000

Notes:
HCV is Housing Choice Voucher-assisted housing. Table displays summary statistics for the main empirical sample (Column 1) as well as for four different sub-samples: individuals from households where no 13-18 year-old in 2000 is ever observed in public or HCV-assisted housing while between the ages of 13 and 18 (Column 2), individuals from households where at least one 13-18 year-old in 2000 is observed between the ages of 13 and 18 in public or HCV-assisted housing (Column 3), the subset of Column 3 individuals who themselves are never observed in public or HCV-assisted housing while between the ages of 13 and 18 (Column 4), and the subset of Column 3 individuals who are observed in public or HCV-assisted housing while between the ages of 13 and 18 (Column 5). Race and ethnicity is assigned at the household level using information from the 2000 Census. The full sample includes individuals aged 13-18 in 2000 from households below 50% area median income in non-owner occupied housing.Based on authors' calculations from matched 2000 Census, HUD-PIC, and LEHD files. Number of observations rounded to the nearest thousand. [a] denote statistics from a previous version of the paper that were not updated due to disclosure restrictions because of a slight change in the sample used to estimate sample means.

TABLE 2—THE EFFECT OF 13-18-YEAR-OLD RESIDENCE IN ASSISTED HOUSING ON AGE 26 EARNINGS, ALL HOUSEHOLD RACE/ETHNICITIES

	Ordinary Least Squares (OLS) (1)	Household Fixed Effects (HFE) (2)
HCV Housing	-0.062	0.047
	(0.004)	(0.010)
HCV Housing*Male	-0.014	-0.021
	(0.006)	(0.008)
Public Housing	-0.077	0.060
	(0.006)	(0.014)
Public Housing*Male	0.012	-0.001
	(0.008)	(0.011)
Male	0.364	0.454
	(0.024)	(0.031)
Observations	1,172,000	1,172,000
Demographic controls	yes	yes
Household Fixed Effects	no	yes

Notes:
HCV is Housing Choice Voucher-assisted housing. Table displays estimates of the impact of years spent in public or HCV-assisted housing on the inverse hyperbolic sine (IHS) of total earnings at age 26. Column 1 presents ordinary least squares estimates while column 2 displays household fixed effects estimates. Years spent in public housing and years spent in HCV-assisted housing are counts of the number of years each individual was observed in the two programs between ages 13 and 18, which can take on values between 0 and 6. The assisted housing participation measures are interacted with whether the individual is male to allow the impacts to vary by sex. All columns include controls for male by age and male by household race fixed effects. Race and ethnicity is assigned at the household level using information from the 2000 Census. The sample includes individuals aged 13-18 in 2000 from households below 50% area median income in non-owner occupied housing. Standard errors are clustered at the household level and shown beneath each estimate in parentheses. Based on authors' calculations from matched 2000 Census, HUD-PIC, and LEHD files. Number of observations rounded to the nearest thousand.

TABLE 3—ASSISTED HOUSING AND AGE 26 EARNING BY SEX, SUBSIDY TYPE, AND RACE/ETHNICITY

	HCV-assisted housing (HCV) (1)	Public Housing (PH) (2)	P-value: (HCV)-(PH)=0 (3)
All households			
Females (F)	0.047	0.060	0.414
	(0.010)	(0.014)	
Males (M)	0.027	0.059	0.050
	(0.010)	(0.014)	
P-value: F-M=0	0.008	0.894	
Observations	1,172,000		
Non-Hispanic White households			
Females (F)	0.006	0.004	0.965
	(0.020)	(0.035)	
Males (M)	0.034	0.072	0.353
	(0.020)	(0.036)	
P-value: F-M=0	0.061	0.017	
Observations	464,000		
Non-Hispanic Black households			
Females (F)	0.070	0.062	0.704
	(0.014)	(0.018)	
Males (M)	0.031	0.057	0.242
	(0.014)	(0.019)	
P-value: F-M=0	0.001	0.735	
Observations	336,000		
Hispanic households			
Females (F)	0.044	0.088	0.203
	(0.021)	(0.028)	
Males (M)	0.030	0.056	0.443
	(0.021)	(0.029)	
P-value: F-M=0	0.370	0.134	
Observations	279,000		

Notes:
All columns present household fixed effects estimates of the impact of years spent in public or HCV-assisted housing between the ages of 13 and 18 on the inverse hyperbolic sine (IHS) of total earnings at age 26 for females and males in all households, and separately by household race/ethnicity. Years spent in public housing and years spent in HCV-assisted housing are counts of the number of years each individual was observed in the two programs between ages 13 and 18, which can take on values between 0 and 6. All columns include controls for male by age and male by household race fixed effects. Race and ethnicity is assigned at the household level using information from the 2000 Census. P-values from Column 3 displays p-values from Wald tests of the null that there is no difference between the effect of an additional year of HCV-assisted housing and an additional year of public housing for the sex (female or male) and household race/ethnicity represented in each row. At the base of each household race/ethnicity block, p-values are presented from Wald tests of the null that there is no difference between the impact for females and males of an additional year spent in public or HCV-assisted housing for that household race/ethnicity group. Standard errors are clustered at the household level and shown beneath each estimate in parentheses. The sample includes individuals aged 13-18 in 2000 from households below 50% area median income in non-owner occupied housing. Based on authors' calculations from matched 2000 Census, HUD-PIC, and LEHD files. Number of observations rounded to the nearest thousand.

TABLE 4—ASSISTED HOUSING AND ADULT INCARCERATION

	2010 Incarceration			
	All households	White households	Black households	Hispanic households
	(1)	(2)	(3)	(4)
HCV Housing	-0.004	-0.002	-0.007	-0.003
	(0.001)	(0.001)	(0.001)	(0.001)
HCV Housing*Male	0.003	0.002	0.004	0.003
	(0.001)[a]	(0.001)	(0.001)	(0.001)
Public Housing	-0.005	-0.004	-0.007	-0.004
	(0.001)	(0.002)	(0.001)	(0.001)
Public Housing*Male	0.002	0.002	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	672,000	291,000	160,000	167,000
Demographic controls	yes	yes	yes	yes
Household Fixed Effects	no	yes	yes	yes

Notes:
HCV is Housing Choice Voucher-assisted housing. Table displays household fixed effects estimates of the impact of years spent in public or HCV-assisted housing on the likelihood of being incarcerated at the time of the 2010 Census. Years spent in public housing and years spent in HCV-assisted housing are counts of the number of years each individual was observed in the two programs between ages 13 and 18, which can take on values between 0 and 6. The assisted housing participation measures are interacted with whether the individual is male to allow the impacts to vary by sex. All columns include controls for male by age and male by household race/ethnicity fixed effects. To be included in the sample individuals must be in households that have no attrition between the 2000 and 2010 Censuses in addition to being aged 13-18 in 2000 and from a household below 50% area median income in non-owner occupied housing in 2000.To adjust for the non-random attrition, we re-weight observations by the inverse of the predicted probability that the household had no attrition between 2000 and 2010 based on a household-level probit of whether there is no attrition on household race/ethnicity, household size, the number of 13-18-year olds in the household, whether the household paid rent in 2000, and state fixed effects. Race/ethnicity is assigned at the household level using information from the 2000 Census. White households are White non-Hispanic and Black households are Black non-Hispanic. Standard errors are clustered at the household level and shown beneath each estimate in parentheses. Based on authors' calculations from matched 2000 Census, HUD-PIC, and LEHD files. Number of observations rounded to the nearest thousand. [a] denote standard errors that round down to 0.000.

TABLE 5—ROBUSTNESS OF THE RELATIONSHIP BETWEEN ASSISTED HOUSING AND AGE 26 EARNINGS

	Stable households (1)	In 1997 Interaction (2)	Ages 14-17 (3)	Wait time: <=9mo (4)	Wait time: >9mo (5)
HCV Housing	0.061	0.044	0.055	0.047	0.053
	(0.013)	(0.015)	(0.012)	(0.014)	(0.014)
HCV Housing*Male	-0.034	-0.016	-0.031	-0.015	-0.027
	(0.010)	(0.013)	(0.011)	(0.011)	(0.011)
Public Housing	0.082	0.059	0.066	0.059	0.077
	(0.019)	(0.022)	(0.018)	(0.017)	(0.023)
Public Housing*Male	-0.002	0.007	-0.000	-0.005	0.001
	(0.014)	(0.018)	(0.015)	(0.014)	(0.018)
HCV housing*In assisted housing 1997		-0.000			
		(0.020)			
HCV housing*In assisted housing 1997*Male		0.003			
		(0.019)			
Public housing*In assisted housing 1997		-0.003			
		(0.028)			
Public housing*In assisted housing 1997*Male		-0.003			
		(0.025)			
P-values: No difference in treatment effects by wait time					
Females in HCV-assisted housing				0.79	
Females in Public housing				0.573	
Males in HCV-assisted housing				0.707	
Males in Public housing				0.496	
Observations	582,000	1,172,000	1,172,000	609,000	563,000
Demographic controls	yes	yes	yes	yes	yes
Household Fixed Effects	yes	yes	yes	yes	yes

Notes:
HCV is Housing Choice Voucher-assisted housing. Table displays household fixed effects estimates of the impact of years spent in public or HCV-assisted housing on the inverse hyperbolic sine (IHS) of earnings at age 26. Column 1 replicates the main results in Table 2 but restricts the sample to observations from households where the marital status and (if present) the spouse of the head of household did not change between the 2000 and 2010 Census (Stable households). Column 2 adds interactions between the assisted housing participation measures and an indicator for whether any members of the household in the empirical sample was observed in assisted housing in 1997, the first year of our data. Interactions between the indicator for participation in 1997 and whether the individual is male, as well as the two triple interactions are also included. Column 3 replaces the main measures of assisted housing participation (a count of the number of years between 13 and 18 that each individual is observed in public or HCV-assisted housing) with the count of years between 14 and 17 that individuals are observed in each program thereby ignoring any variation that occurs at age 13 or age 18. The results in Columns 4 and 5 are run separately by the average wait time for public and HCV-assisted housing in a county. Average wait time is calculated as the weighted housing authority average of the mean days spent on a waitlist prior to admission in each program with the number of teenagers observed in each housing authority-program type-county cell in the year 2000 used as weights. The overall average county-level wait time is then computed as the arithmetic mean of the public housing and HCV-assisted housing county-level average wait times. Counties are classified as having a wait time above nine months if this average is greater than 273 days and below nine months if it is less than or equal to 273 days. The bottom panel displays p-values from tests of whether the effect is the same in counties with long (>9 months) and short (<=9 months) wait times. All specifications include controls for male by age and male by household race fixed effects. Race and ethnicity is assigned at the household level using information from the 2000 Census. Standard errors are clustered at the household level and shown beneath each estimate in parentheses. Sample includes individuals aged 13-18 in 2000 and from a household below 50% area median income in non-owner occupied housing in 2000. Based on authors' calculations from matched 2000 Census, HUD-PIC, and LEHD files. Number of observations rounded to the nearest thousand.

TABLE 6—ASSISTED HOUSING AND AGE 26 EARNINGS, DIFFERENTIATING LARGE METRO AREA, HIGH POVERTY CENSUS TRACTS

	All other areas (1)	Large MSA high poverty tract (2)	P-value: no diff. (3)
Females			
HCV housing	0.048	0.037	0.841
	(0.010)	(0.051)	
Public housing	0.060	0.057	0.936
	(0.014)	(0.041)	
P-value: HCV-PH difference equal in (1) and (2)			0.915
Males			
HCV housing	0.025	0.075	0.328
	(0.010)	(0.051)	
Public housing	0.066	0.023	0.343
	(0.015)	(0.042)	
P-value: HCV-PH difference equal in (1) and (2)			0.158
Observations	1,172,000		
Demographic controls	yes	yes	
Household Fixed Effects	yes	yes	

Notes:
HCV is Housing Choice Voucher-assisted housing; MSA is metropolitan statistical area. Table displays household fixed effects estimates of the impact of additional years spent in public or HCV-assisted housing on the inverse hyperbolic sine (IHS) of earnings at age 26. The assisted housing participation measures and the male indicator are interacted with an indicator for whether individuals resided in a census tract in 2000 in an MSA with >2.5 million residents in 2000 and with a census tract poverty rate >40% in the 1990 Census (Large MSA high poverty tract). The coefficients and standard errors correspond to the implied treatment effects and standard errors for large MSA high poverty census tracts (Column 2) and not large MSA high poverty census tracts (Column 1) areas. Column 3 presents p-values from F-tests of whether there is no difference between the treatment effects in Columns 1 and 2. P-values are also shown from tests of whether the difference in differences between HCV and public housing treatment effects in columns 1 and 2 is equal to zero. All columns include controls for male by age and male by household race/ethnicity fixed effects. Race and ethnicity is assigned at the household level using information from the 2000 Census. Standard errors are clustered at the household level and shown beneath each estimate in parentheses. Main sample includes individuals aged 13-18 in 2000 and from a household below 50% area median income in non-owner occupied housing in 2000. Based on authors' calculations from matched 2000 Census, HUD-PIC, and LEHD files. Number of observations rounded to the nearest thousand.

TABLE 7—AGE 26 EARNINGS AND HETEROGENEITY BY HOUSEHOLD % OF AREA MEDIAN INCOME

	Interactions with Household % AMI (1)
HCV housing	0.038
	(0.01)
HCV housing*Male	-0.016
	(0.008)
Public housing	0.049
	(0.014)
Public housing*Male	0
	(0.011)
HCV housing*%AMI	-0.002
	(0.001)
HCV housing*Male*%AMI	0
	(0.001)
Public housing*%AMI	-0.002
	(0.001)
Public housing*Male*%AMI	0
	(0.001)
Observations	1,172,000
Demographic controls	yes
Household Fixed Effects	yes

Notes: HCV is Housing Choice Voucher-assisted housing; AMI is area median income. Table displays household fixed effects estimates of the impact of years spent in public or HCV-assisted housing on the inverse hyperbolic sine (IHS) of earnings at age 26, allowing for the effect of additional years in each program to vary with household percent of area median income (AMI). Household %AMI is calculated by averaging parents' LEHD earnings for each individual between the ages of 13 and 18 and then identifying the minimum value across all 13 to 18-year-olds in the household in 2000. This value is then scaled by AMI based on household size and the place of residence in the 2000 Census, multiplied by 100 (so it varies between 0 and 50), and de-meaned for the full empirical sample prior to interacting with the assisted housing participation measures. All specifications include controls for male by age and male by household race/ethnicity fixed effects. Race and ethnicity is assigned at the household level using information from the 2000 Census. Standard errors are clustered at the household level and shown beneath each estimate in parentheses. Sample includes individuals aged 13-18 in 2000 and from a household below 50% area median income in non-owner occupied housing in 2000. Based on authors' calculations from matched 2000 Census, HUD-PIC, and LEHD files. Number of observations rounded to the nearest thousand.

TABLE 8—MAPPING ESTIMATES TO PREVIOUS LITERATURE

Comparison	Gender	Estimand	Earnings Effect	Per Year LATE (2000 US$)	Anderson et al. (2000 US$)
Chetty, Hendren, and Katz (2016)					
HCV-PH	Females and Males	ITT	-320.39	-132.90	-164.80
Sanbonmatsu et al. (2013)					
HCV-PH	Females	LATE	628.04	80.02	-83.34
HCV-PH	Males	LATE	-3,166.47	-403.43	-261.53
HCV-PH	Females and Males	LATE	-1,567.75	-199.74	-173.86
Jacob, Kapustin, and Ludwig (2015)					
HCV-Unsubsidized	Females	LATE	-145.43	-15.36	657.11
HCV-Unsubsidized	Males	LATE	91.73	9.69	301.59
Chyn (2018)					
HCV-PH	Females and Males	LATE	277.36	208.03	-83.83

Notes: Table presents estimates from the four most relevant recent papers on public and HCV-assisted housing alongside estimates from this paper. Sanbonmatsu et al (2013) and Chetty, Hendren, and Katz (2016) use variation in public and HCV assisted housing generated by MTO to estimate the impacts of an experimental treatment arm (HCV+ residential counseling+requirement that households move to a low poverty neighborhood for one year) or an HCV treatment arm. MTO sample is drawn from a population living in public housing projects located in census tracts with 1990 poverty rates>40% in Baltimore, Boston, Chicago, Los Angeles, or New York. We average the intent-to-treat effects (ITT) on age 26 earnings for children 13 and older at the start of MTO (in Chetty, Hendren, and Katz (2016)) or on 2009 earnings for adult children (in Sanbonmatsu et al. (2013)) across the experimental and HCV groups, using the shares of treated children in each arm as weights. ITT effects are then converted to per year LATE estimates by scaling them by the expected difference in years of exposure to HCV-assisted housing (relative to public housing): 19-average sample member age multiplied by the group-control take-up gap. We calculate the difference between HCV-assisted housing and public housing (from Table B1) for all household race/ethnicity by gender cells and aggregate these using the shares of the MTO samples that were male, Black non-Hispanic, Hispanic, or White non-Hispanic. The Jacob, Kapustin, and Ludwig (2016) IV estimates based on the Chicago HCV lottery, capture the impact of an additional year of HCV-assisted housing relative to unsubsidized housing. These are converted to per year estimates in the same way but without adjusting for the take-up gap as it is already accounted for by the IV estimates. Chyn presents 2SLS estimates of the effect of an additional year of public housing relative to HCV-assisted housing, on adult labor market earnings using Chicago public housing project demolitions; we simply take the negative of these results to arrive at the HCV-PH LATE estimates. We aggregate our household race/ethnicity by gender-specific differences (HCV-PH) using the share of the Chyn sample that is male and assuming that the Chyn sample is 100% Black non-Hispanic (as in Jacob (2004)). The Comparison column shows the programs being contrasted, Gender identifies the sex the estimates pertain to, Estimand identifies if the estimates capture local average treatment effects for compliers (LATE) or ITT effects, Earnings Effect shows the paper estimates (aggregating across experimental and HCV arms for MTO studies), Per Year LATE shows the implied per year effect, and Anderson et al. (2000 USD) shows the effects from this paper averaged to match the sample from the other paper.