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

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Abstract

To date, research on the long-term effects of childhood participation in voucher-assisted and public housing has been limited by the lack of suitable identification strategies and appropriate data. We create a new, national-level longitudinal data set on housing assistance and labor market earnings to explore how children's housing affects their later employment and earnings. While naïve estimates suggest there are substantial negative consequences to childhood participation in both types of subsidized housing, these relationships appear to be driven largely by negative selection into housing assistance programs. To mitigate this source of bias, we employ household fixed-effects specifications that use only within-household (across sibling) variation for identification. Compared to the naïve estimates, household fixed-effects estimates are attenuated for some demographic groups and positive and significant effects for others. Girls, in particular, appear to benefit from time spent in both public and voucher-assisted housing. (*JEL* H43; I31; I38; J38; J62).

1.   Introduction

In the year 2000, over 2.7 million children under the age of eighteen lived in voucher-supported or public housing, the two most popular subsidized housing programs run by the Department of Housing and Urban Development (HUD). Although large-scale assisted housing programs have been in place for some time, research on the long-term effects for resident children is scarce and hampered by methodological and data limitations.

This paper estimates the causal effect of participation in voucher-supported and public housing as a teenager on employment and earnings in early adulthood. To do so, we develop a novel data set that combines information on housing assistance, earnings, household structure, and neighborhood and demographic characteristics. By linking these different data sources together at the person level, we are able to track millions of children as they progress through voucher-supported, public, and unassisted housing as children, and into the labor market as adults.

There are a number of channels through which childhood participation in subsidized housing might impact adult outcomes. Both voucher and public housing provide a positive income effect for households. By expanding the budget set 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. This increase in human capital should be reflected in higher labor market earnings, suggesting that assisted housing residence in childhood would positively impact adult labor market outcomes.

However, other pathways would yield a negative relationship between subsidized housing participation in childhood and adult labor market performance. Oreopoulos (2003) raises the possibility that subsidized housing participation might impact outcomes through peer or neighborhood effects. If, as argued by Oreopoulos (2003) and Newman (1972), available subsidized housing units are located in worse neighborhoods—i.e. neighborhoods with higher crime rates and lower quality schools--than participants' counterfactual housing options, then public and voucher-assisted housing could have negative neighborhood and peer effects and therefore decrease adult earnings. Ex-ante, the sign of any neighborhood or peer effects, as well as the overall impact of subsidized housing participation, is unclear. Our results identify the net long-term effect on adult earnings of childhood participation in subsidized housing.

Implicitly assumed in the previous paragraphs was the idea that the impact of housing vouchers and public housing participation during childhood is the same. This need not be the case. In fact, the thought that the two programs might have different effects is one element underlying the shift in subsidized housing policy in the U.S. to provide housing choice through vouchers. The argument is that in the absence of discrimination on the part of potential landlords, voucher housing should offer households increased neighborhood choice. As such, the adverse consequences of public housing projects could be avoided while the positive income effect for households would still be present. The debate about housing vouchers vs. public housing has been the subject of previous research, but most of it has focused on the difference in short-term effects. For example, recent evidence indicates that female youth moving to lower-poverty neighborhoods experience improved mental and physical health (Sanbonmatsu et al. 2011). We contribute to this debate by examining the differences in long-term labor market outcomes between public and voucher-subsidized housing.

The core identification challenge facing all research on subsidized housing is how to overcome the selection problem associated with a household's decision to participate in the program. That is, households that decide to participate in public housing and voucher-assisted housing are different from those that do not. The growing literature that uses instrumental variables procedures, experimental evidence, or quasi-experimental evidence regularly finds that the impact of subsidized housing is more positive when unobserved heterogeneity is taken into account.[1]

We make use of the large sample size and longitudinal nature of the administrative data set available to us and employ a household fixed-effects specification that exploits variation in voucher-supported housing and public housing participation over time within households. This allows us to isolate the effect of each type of subsidized housing on labor market outcomes from observed and unobserved household-level heterogeneity that may impact both labor market outcomes and the program participation decision.

Our results confirm that selection into subsidized housing matters. Whereas OLS estimates show a substantial negative effect of housing subsidies when young on later adult earnings and employment outcomes, the household fixed-effects estimates are substantially less negative and, for many demographic groups, significantly positive. For example, for females, we

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[1] See the more detailed literature review below for relevant citations.

find that being in public housing as a teenager yields a 29 percent premium for young adult earnings, and voucher housing, a 14 percent premium. These positive effects for females are mostly driven by the estimated effects for Black non-Hispanic households. Our approach, while superior to naïve OLS estimates, still may be subject to time-varying unobserved characteristics related to both adult earnings and household subsidized housing participation. We include several sensitivity checks to address these concerns.

The remainder of the paper proceeds as follows. Section 2 provides background information on housing subsidies while Section 3 reviews a selection of studies on the effects of living in different types of subsidized housing. Section 4 describes the data and Section 5 the research design, hypotheses, and identification issues. Section 6 describes the study sample and Section 7 provides the empirical results. Section 8 concludes.

2.   Literature Review

2.1 Comparability of studies

There is a broad literature estimating the economic effects of housing subsidies, although studies of the long-run impacts on children are scarce. In the broad literature, conclusions about the effects of subsidized housing vary considerably. In part, the mixed results are likely a reflection of different study designs -- many of the studies estimate the impact of moving from one type of subsidy to another. While certainly an interesting and policy relevant parameter, these studies are unable to answer how the different subsidy types compare to receiving no subsidy. Others that do compare subsidized households against non-subsidized households do not distinguish among different subsidy types and thus miss potentially important distinctions among the different programs.[2] Studies that have been able to compare multiple subsidy types to private, unassisted households typically do so for a limited geographic area (a city or metropolitan area) and focus on short-term, rather than longer-term outcomes.[3]

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[2] For example, Olsen et al. (2005) used longitudinal HUD administrative data from 1995 to 2002 combined with data from other sources and a large, nationwide random sample to assess the employment results of multiple types of assistance. The authors found that each type of housing assistance has substantial negative effects on labor earnings that are somewhat smaller for tenant-based housing vouchers than for project-based assistance.

[3] For example, Bania et al. (2003) compares welfare leavers who received Section 8 housing vouchers or project-

As we discussed in the introduction, the biggest challenge is dealing with selection. Subsidized housing residents differ observably from non-subsidized housing residents, often having characteristics typically associated with worse employment and educational outcomes. This implies that extensive controls are needed if the identification approach uses a selection on observables approach. However, if public housing residents also have unobserved characteristics associated with worse labor market outcomes, then the estimated effects of housing on outcomes are likely biased (for a general discussion, see Shroder 2002). Previous research has employed a variety of approaches to deal with unobserved heterogeneity.

2.2 Identification in Previous Research

Some prior work relies on propensity score matching and other control variable-based methods to measure how outcomes differ among households in different public housing projects or programs. For example, Susin (2005) uses a rich set of controls from survey data to match households from project-based subsidized housing recipients and Section 8 Housing Choice voucher recipients with low-income non-recipient control households in the Survey of Income and Program Participation. He finds that housing subsidies reduce incentives to work and reduce earnings relative to control individuals but finds no difference in outcomes between voucher and project-based assistance recipients. However, he acknowledges some potential biases; for example, households with permanently low incomes may be matched to those with temporarily low incomes.

Recent work by Carlson et al. (2012a, 2012b) also uses a propensity score approach. As in this paper, the authors focus on employment and mobility outcomes for those receiving housing vouchers. The data come from administrative records in two databases maintained by the State of Wisconsin combined with Census Bureau public use microdata. Specifically, they draw a sample from the state's Client Assistance for Re-employment and Economic Support system, with 12,170 cases in the voucher group and 342,000 cases in the control group for up to 6 years after receipt of vouchers. Because the entire sample receives some sort of public assistance, their identification strategy is the equivalent of propensity score matching with a "receives public assistance" fixed effect. The results suggest that, 6 years after voucher receipt,

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based housing, with other welfare leavers. The study was limited to Cuyahoga County (Cleveland), Ohio, and followed residents from 1996 through 1997 using administrative data. They found no significant effect from the receipt of housing assistance, and no difference between voucher and project-based assistance recipients.

there is little effect on employment, but housing voucher recipients experience a negative effect on earnings that diminishes over time (Carlson et al. 2012a). Additional work indicates that voucher receipt resulted in both short- and long-term mobility and had little to no effect on four measures of neighborhood quality in the short term, but led to small long-term improvements in all four quality indicators (Carlson et al. 2012b).

The aforementioned studies rely on selection on observables identification approaches. With a rich set of controls, they are able to make progress on difficult selection issues but are subject to the concerns raised by Schroder (2002) and discussed above. As such, the literature has increasingly moved to alternative identification approaches to deal with these issues. Some researchers have used instrumental variables (IV) to identify the effect of public and assisted housing on outcomes. In one such study, Currie and Yelowitz (2000) identify a regulation in housing assignment that provides an extra bedroom to households with two children of different genders, as compared to those with two children of the same gender. They use this rule based variation as an instrument to estimate the effect of public housing on child outcomes. They find that households entitled to an extra room because of the gender composition of their children are 24 percent more likely to participate in public housing and their children are less likely to have been held back in school. This suggests public housing participation has a positive impact on children's educational outcomes.

Newman and Harkness (2000) also use an IV strategy to identify the effect of participation in public housing on children's educational attainment. With a sample of about 1,000 individuals from the Panel Study of Income Dynamics, they develop a county-level measure of public housing availability by regressing the number of assisted housing units per income-eligible family in each county on county characteristics, and use the regression residuals for each county as an instrument for public housing participation. The authors find that public housing has no effect on children's education.

Other research takes advantage of public initiatives that resulted in random assignment of program participation. For example, the Gautreaux project, which ended in 1998, involved the Chicago Housing Authority (CHA) distributing Section 8 housing vouchers to 7,100 African-American families on welfare. The vouchers were to be used to rent private market apartment units in either suburban or urban locations chosen at random by the CHA. Rosenbaum (1995) surveys 332 adults from the Gautreaux sample, and conducts detailed interviews with another 95.

He finds that adult suburban movers (voucher recipients) experienced higher employment but no change in wages or hours worked relative to control adults.

Inspired in part by the Gautreaux project, HUD's Moving to Opportunity (MTO) project randomly assigned 4,600 households living in public housing projects in five cities to receive Section 8 housing vouchers, either with no restrictions or only for use in areas with a poverty rate below 10 percent. Despite the fact that MTO generated persistent improvements in neighborhood conditions for treatment households, there was no significant effect on employment or earnings outcomes for adults or their grown children as reported by the parents (U.S. Department of Housing and Urban Development, 2011).

The Welfare-to-Work Voucher Program provided housing vouchers to 50,000 families receiving or eligible to receive welfare. Mills et al. (2006) use an 8,371-household sample from seven public housing agencies to evaluate the differences in outcomes between those receiving vouchers and those not receiving vouchers. They find that vouchers somewhat improve the neighborhoods in which extremely low-income families live, but over a 3½- year study period, vouchers had no impact on employment or earnings.

Jacob (2004) makes use of the schedule of public housing demolitions in Chicago, and the Chicago Housing Authority's (CHA) policy of providing residents of demolished projects with Section 8 housing vouchers, to generate plausibly exogenous variation in public housing and Section 8 voucher participation. After matching administrative data from the Chicago Public Schools containing places of residence and test scores for 94,000 students to public housing addresses, Jacob finds that children leaving public housing fared no better or worse than their peers who remained in public housing for longer.

Jacob and Ludwig (2012) evaluate a CHA program that randomly assigned applicant households to a position on a waiting list for housing vouchers. Of the 82,607 households who applied for Section 8 vouchers between 1997 and 2003, they focus on the 90 percent of applicants living in private-market housing. Thus, they are able to compare housing voucher recipients to households who do not participate in subsidized housing. They find that vouchers reduce quarterly employment rates and earnings and increase participation in the Temporary Assistance for Needy Families program.

Oreopoulos (2003) uses another quasi-experiment, the random initial assignment of households to heterogeneous housing projects in Toronto, to estimate neighborhood effects on

children. By matching earnings from Canadian tax data to historical information on parental residential location, he is able to reconstruct the childhood public housing experiences of adult workers. The results indicate that neighborhood conditions as a child have no effect on adult earnings or welfare participation.

3. Research Design, Hypotheses, and Identification Issues

Our primary goal is to identify the causal effect of living in subsidized rental housing as a teenager on eventual labor market success. To do so, we begin by specifying a linear, constant effects regression model for a particular labor market outcome (the inverse hyperbolic sine of total earnings from 2008 to 2010 in this paper), y, of teenager i as

$$y_{if} = \alpha + \beta'H_i + \phi'X_{if} + \gamma'Z_{if} + \epsilon_{if} \tag{1}$$

Where f indexes the household including child i in the year 2000. The outcome measures the teen's earnings as an adult while the explanatory variables pertain to the teen's unchanging characteristics or teen years; α is an intercept. The variables of interest, H_i, are dummy variables that measure participation in subsidized housing (public housing or housing voucher) as a teenager. The vector X_{if} includes observable child and household control variables, such as demographic characteristics. The vector Z_{if} contains a set of unobserved characteristics that may be related to y_{if}. Lastly, ϵ_{if} is an independent error term.

Further, suppose that Z_{if} and it's effect γ' can each be partitioned into two separate parts, $[Z_f, Z_i]$ and $[\gamma'_f, \gamma'_i]$. The first factor, Z_f is the composite of all observed and unobserved time-invariant characteristics for each household f that are common to all children $i \in f$ and γ_f is the associated effect. The remaining factor, Z_i, contains other unobserved characteristics that vary by child, such as behavioral characteristics or disability status.

Consider estimating equation (1) using Ordinary Least Squares (OLS) and, thereby, omitting the unobserved characteristics in Z_{if}. The estimated coefficient $\hat{\beta}_{OLS}$ will include both the true effect of subsidized housing participation and a term arising from omitted variable bias. The sign of the bias will depend on the effect of the omitted, household-specific characteristics on earnings (γ) and the covariance between participation in subsidized housing and the omitted characteristics. For example, if households with unobserved characteristics that tend to depress child outcomes are also more likely to enter public housing, then $\hat{\beta}_{OLS}$ will be biased downward.

Thus, a finding that subsidized housing depresses child outcomes may be spurious unless the specification controls for these potential biases. To account for the distinct possibility that estimates are contaminated by household-level heterogeneity, we propose an alternative identification strategy. To the extent the bias in OLS estimates is solely attributable to the omission of time-invariant heterogeneity at the household level that are correlated with both program participation and labor market outcomes, then conditioning on household fixed effects would eliminate the bias.

To that end, we specify a household fixed-effects regression that explores within-household variation in program participation across siblings to identify the impact of housing subsidies.

Griliches (1979) provides a summary of the early literature that makes use of sibling fixed effects and points out a number of potential issues. Recent studies include (1) Royer (2009) who used over 3,000 twin pairs and twin fixed effects to estimate the effect of birth weight on long-term outcomes, (2) Currie and Walker (2011), who used mother fixed effects to estimate the impact of the introduction of EZ-Pass in New Jersey and Pennsylvania on infant health outcomes, and (3) Currie et al. (2010), who employed sibling fixed effects to identify the relationship between early childhood health problems and outcomes in early adulthood. An especially relevant siblings study is Aaronson (1998), who estimated the effect of neighborhood on children's educational outcomes. Aaronson used the Panel Study of Income Dynamics to examine over 2,000 individuals in over 700 families and measures differences in exposure to high poverty neighborhoods across siblings. He found negative effects on high school graduation with and without the household fixed effects.

In our study, the household fixed-effects estimates control for time-constant, unobserved household-level heterogeneity (Z_f). The household fixed-effects (HFE) regression estimates the effect of subsidized housing participation on labor market outcomes using only variation in housing participation and outcomes across teenagers *within the same household*. In practice, we subtract out the household mean of the dependent and independent variables from each observation within a household.[4] Therefore, HFE only uses observations from household f to help identify $\hat{\beta}_{HFE}$ if there are at least two teenagers i and j aged 13-18 in the household in 2000 where $H_i \neq H_j$. For example, consider a household in the year 2000 with a 17 year-old and a 14

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[4] We also cluster standard errors at the household level.

year-old that does not enter HUD-subsidized housing until 2003. The older sibling, who leaves the household in 2002, would have $H_i = 0$ and the younger sibling would have $H_i = 1$.

The HFE model is written as:

$$y_{if} = \alpha + \beta_{HFE}'H_i + \phi'X_i + \gamma_f + \gamma'Z_i + \epsilon_{if} \tag{2}$$

where γ_f gives the fixed effect for all children in household f. The effects of observed characteristics common among all children in a household are not separately identified, but instead subsumed in γ_f, so only a subset of X_{if} remains. In practice, H_i is a vector containing measures of participation in both public housing and housing voucher programs as a teenager, X_i contains an indicator for whether the teenager is male, a set of age dummies, and, in some specifications, an interaction between whether the teenager is male and the race/ethnicity of the household. We also interact each of the subsidized housing measures with whether the teenager is male to allow for heterogeneous effects by teenager gender, and we estimate separate regressions for each race/ethnicity to allow all coefficients to vary. We estimate both a "dummy" version where the "treatment" H is a set of two binary indicator variables for whether an individual resided in each type of subsidized housing as a teenager and a "dose" version where treatment is the number of years an individual resided in each type of subsidized housing between ages 13 and 18.

The HFE estimation provides an unbiased estimate of the effect of teenage subsidized housing residence on labor market outcomes under much less stringent conditions than a typical conditional on observables approach (including propensity-score matching approaches, in which identification also hinges on controlling for all relevant observables that determines selection and impact outcomes). There are, however, two types of characteristics contained in the child-specific factor, Z_i, that could lead to bias in $\hat{\beta}_{HFE}$. First, any household-specific and *time-varying* characteristic that is correlated with both subsidized housing residence and labor market outcomes will lead to bias. For example, if families enter subsidized housing in response to negative economic shocks and under the assumption that these are also harmful to the subsequent labor market outcomes of the child, $\hat{\beta}_{HFE}$ would be a downward-biased estimate of the true effect.[5] In fact, HUD strongly prefers and in some cases requires that program households be

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[5] Job loss by a household member is an example of an economic shock, though it is unlikely that housing subsidies

below a certain income threshold. This suggests that if any bias from unobserved, time-changing heterogeneity is present, this bias is likely to be negative. To address this possibility, we also consider HFE specifications where we control for the parents' earnings while the teenager is between 13 and 18. This variable will capture differences in the household earnings across siblings that have different subsidized housing experiences.[6]

A second potentially confounding unobserved characteristic is any within-household, teenager-level heterogeneity that is correlated with both labor market outcomes and subsidized housing participation. In this case, the direction of the potential bias is less clear. However, we control for gender differences and it is rather implausible that this type of bias would contaminate the HFE estimates. The decision to move into subsidized housing is made at the household level. In effect, for this to be a concern, households would have to be making housing decisions in response to the characteristics of one teen but not the characteristics of the other teenage household members. Another factor that might mitigate concerns of correlation of housing treatment and child characteristics is the waiting periods typical for receipt of a housing subsidy. Such delays would tend to reduce any correlation of housing treatment and unobserved characteristics, which should attenuate any remaining bias. Indeed, waiting times are one reason that siblings may have different housing treatment experiences (in terms of dummy or dose), which is ideal variation for our analysis.

4.   Description of the Data

4.1 Siblings sample frame

The core data set brings together person- and household-level records from the 2000 Decennial Census and several different administrative files. To begin, we use the responses from the 2000 Census to construct a frame of over 1.8 million youth aged 13-18 and their households.[7] Because our focus is on employment outcomes from 2008 to 2010,[8] we require that children are

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are responsive to transitory events as the waiting lists are typically substantial. Another plausible scenario given eligibility requirements imposed by HUD is that households are more likely to be admitted into subsidized housing after a household member develops a disability. Again, under the assumption that exposure to this disability worsens potential labor market outcomes, this would lead to a downward-biased estimate.

[6] Aaronson (1998) also evaluated the validity of using across-sibling variation by examining whether moves into or out of high-poverty neighborhoods co-vary with other household characteristics, such as parents' income.

[7] Specifically, we use the Hundred Percent Edited Detail file.

[8] We recognize that 2008-2010 is a sluggish period for the national labor market, but our identification approaches are designed to exploit the cross-sectional variation. In future work we may consider whether the effects vary across the business cycle.

at least age 13 in 2000, meaning they will be at least 21 by 2008 and may be entering the labor force even if they attained some higher education. We cap the sample at age 18 and require that in 2000 the child was in a household with their parent(s). Including older youths would undermine the focus of the paper, and our identification approach relies on the assumption of parents making housing decisions for children.

Because our aim is to estimate the effect of childhood environmental factors on later life outcomes, we derive most of our demographic characteristics from the base year 2000 Census short form responses, when subjects are still children.[9] We retain responses for one or two parents as well as all youth between the ages of 13 and 18 and classify all respondents from the same address as a household.[10] We use time-invariant explanatory variables relating to the child such as date of birth, gender, race, and ethnicity, and characteristics of the household in the base year such as housing tenure (rent or own), number of people, number of children.[11] We also construct a household race/ethnicity variable to allocate households to race/ethnicity subsamples. Specifically, we define a household as Hispanic if any member reports being Hispanic, Black non-Hispanic (Black) if no member reports being Hispanic and at least one member reports being Black, White non-Hispanic (White) if no member reports being Hispanic or Black and at least one member reports being White, and Other non-Hispanic (Other) if no member reports being Hispanic, Black, or White.

Youth in the Census 2000 frame are then matched to administrative records on housing subsidies from the Department of Housing and Urban Development's HUD-PIC[12] file, place of residence from the Longitudinal Employer-Household Dynamics (LEHD) maintained Composite Person Record (CPR), and subsequent earnings from the LEHD[13] using a unique person

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[9] We chose to use all households in the U.S. rather than the 1-in-6 sample filling out the long form for the principal analysis in order to have a larger sample size. While the long form would allow us to include variables such as parent's education, such time-invariant explanatory factors will be subsumed into the household fixed effects in any case.

[10] We use the Master Address File ID (MAFID) to define a household as the set of responses collected from one address. MAFIDs, or addresses, constitute the residence frame for Census Bureau surveys. We define the head of household and the spouse of the head of household as the parents for each MAFID. In some cases these individuals may be grandparents, other relatives, or even unrelated adults.

[11] We exclude households including more than 15 residents or more than 10 teenagers.

[12] PIC refers to Public and Indian Housing Information Center. The data file contains an annual extract of recipients of voucher-supported housing and public housing, submitted by housing authorities and providers. For other research using the HUD-PIC extract file, see Lubell et al. (2003); Mills et al. (2006); Olsen et al. (2005); Shroder (2002); and Tatian and Snow (2005). We do not use the HUD-TRACS (Tenant Rental Assistance Certification System) since those data apply to project-based Section 8 subsidies.

[13] For a description of the LEHD infrastructure files and public statistics, see Abowd et al. (2004).

identifier. Person-level record matching is done by way of a Protected Identification Key (PIK), which is assigned to survey and administrative records based on personally identifying information. The 2000 Census has PIKs for over 89 percent of the person-records, while almost 98 percent of HUD records have a PIK, and all LEHD records have a PIK. We only retain households with a parent who has a PIK and at least two children aged 13 to 18 that have a PIK and non-missing basic characteristics.[14] From the full sample including records with no PIK, we estimate a logistic regression for whether or not a person response has a PIK, with explanatory variables including the number of persons in a household, the number of children, housing tenure as well as person age, gender, race, ethnicity and state fixed effects based on the year 2000 location.[15] To retain a representative sample of records with a PIK, we reweight them using the inverse of the probability of having a PIK, based on the model.

4.2 Housing subsidy

The HUD-PIC file provides detailed information on public housing and Housing Choice Voucher recipients during our study period from 1997 to 2005. As part of their housing occupancy verification process, local housing authorities provide HUD with the identities of residents, which HUD then compiles into an annual relational database. Table 1 presents characteristics of public and voucher supported housing participants from public use data derived from HUD-PIC. In 2000, households averaged approximately $10,000 in annual income, which was about a quarter of metropolitan area median income. A description of the major federal housing assistance programs that we consider appears in Appendix A. Table A-1 presents summary statistics of the HUD administrative rental subsidized housing data for the two major programs.

The person-level file used at the Census Bureau includes demographic and housing unit information, but this study only makes use of occupancy as an indicator of housing treatment.[16] We match PIKs from the decennial file to the HUD-PIC file and identify whether a child resided

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[14] For cases where a PIK has been assigned to multiple responses (less than 1 percent) we drop all cases, unless all observable characteristics (date of birth, race, ethnicity, gender, geographic location) are identical, in which case one record is retained.

[15] Characteristics highly associated with not having a PIK include race, ethnicity, age, and sex.

[16] Table B-1 in Appendix B presents the percentage of records with non-missing data in the PIC administrative file. Other tables there present some characteristics of the PIC sample.

in public or voucher housing in each year from 1997 to 2005. We consider a child to be a HUD-subsidized resident in a particular year if their PIK appears in the HUD administrative data *and* if that individual is still under the age of 18.[17] Thus, the maximum number of years a child could reside in HUD housing is 6 years before turning 18, which could occur for a 13-year-old first residing in subsidized housing in or before 2000. An 18-year-old in 2000 could only reside in HUD-subsidized housing for at most 4 years (beginning in 1997).

We construct an indicator variable for whether a teen resided in either public or voucher housing any time between 1997 and 2005. Our goal is to estimate the effect of this binary treatment variable on labor market outcomes. We also examine the effect of a treatment "dose" variable that could take on values from 0 to 6 for the count of (post-1996) years a child resides in voucher or public housing.

There are three exclusions we found necessary to avoid sample contamination due to possible measurement error. First, we exclude all households who respond as owning the home they reside in on the 2000 decennial census. While individuals in such households could end up in subsidized housing later in the decade, we decided they would not be representative of renter households eligible for subsidized housing. Second, we require that each teenager's parents earn less than 50 percent of HUD-specified Area Median Income (AMI) on average while the child is a teenager (see below).[18] Third, we excluded households who lived in the 119 counties participating in HUD's Moving to Work (MTW) demonstration (see Abravanel et al. 2004). Local housing authorities participating in the demonstration were permitted to stop reporting administrative data to HUD on participants.

4.3 Labor market outcomes

LEHD, a partnership between the Census Bureau and all 50 states and the District of Columbia, produces public use data tabulations that are widely used by state and local governments.[19] At its core are two administrative records files provided by states on a quarterly

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[17] We do not count individuals who are under 18 in 2000 but over 18 when we observe them in the HUD administrative data as being HUD residents.

[18] We use average annual total labor income from years where the child is between 13 and 18 years of age. To avoid dropping observations that do not match to the Composite Person Record (CPR) we use the 2000 census residence county to define AMI. HUD defines AMI using American Community Survey data; specified proportions of AMI are used as eligibility and priority criteria.

[19] LEHD data products include the Quarterly Workforce Indicators, the LEHD Origin-Destination Employment Statistics (LODES), and Job-to-Job Flows.

basis: (1) unemployment insurance (UI) wage records, giving the earnings of each worker at each employer, and (2) employer reports giving establishment-level data, also known as the Quarterly Census of Employment and Wages (QCEW), but often referred to as the 'ES-202' program. The coverage is roughly 96 percent of private non-farm wage and salary employment (Stevens 2007).[20]

The longitudinal LEHD data are based on quarterly earnings information for more than 130 million U.S. workers and their employers covered under state UI systems beginning in the mid-1990s and continuing to the present, essentially a universe of workers. The longitudinal data thus permit the measurement of complete employment "histories" beginning with a person's entrance into the labor force. This information includes earnings, employment status and industry, along with other work and home location information. Thus, LEHD wage data matched to the Census 2000 data enable us to track a large set of children into adulthood and measure earnings and employment outcomes. For our purposes, the national nature of the files and complete work histories enable one to compute outcome measures for individuals over any given horizon such as the number of quarters worked, cumulative number of jobs, the number of spells of joblessness, the durations of spells of joblessness, and the earnings levels and its growth within and between jobs.

For regression purposes, we use the inverse hyperbolic sine (IHS) of earnings[21] rather than the more traditional log of earnings because estimated coefficients can be interpreted in the same way as with a log transformed dependent variable but, unlike with the log of earnings, IHS is defined for zero earnings. The IHS is defined as $y_i^* = \log[y_i + (y_i^2+1)^{1/2}]$ where y_i is total earnings for individual i (see Burbidge et al. 1998).

4.4 *Other factors varying within households*

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[20] LEHD is in the process of integrating data on self-employed individuals and independent contractors who are not covered in the UI files but are available from the Census Bureau's Business Register which contains the universe of all businesses including all sole proprietorships on an annual basis (whether the sole proprietor has employees or is a non-employer). In addition, the LEHD project has acquired the personnel records from Office of Personnel Management (OPM) so that federal workers are now also tracked in the file system. This study does not yet make use of these new data sources, but may in future versions. For more information on the LEHD, see Abowd et al. (2004).

[21] Annual earnings are deflated to their 2000 purchasing power equivalent using the U.S. city average annual purchasing power for all urban consumers.

We introduce additional geographic data to address time-varying but spatially constant household factors. The LEHD program maintains an annual place of residence file composed of federal administrative data known as the Composite Person Record (CPR). LEHD uses CPR residences, which begin in 1999, for imputation models and for the residence component of public use data. We identify a residence census block for each child from 1999-2005 where available (approximately 10 percent of children are missing a CPR residence in each year). Where possible, we match the child residence to block group-level tabulations from Census 2000, giving neighborhood characteristics such as the poverty rate.

In addition to using LEHD earnings to construct outcome measures for the teenagers, we use parent's LEHD earnings to determine sample eligibility and to construct an annual measure of household income for 1997 to 2005 to use as a control variable. HUD defines eligibility for its assistance programs based on family income as a percentage of Area Median Income (AMI), which adjusts for area income and for family size.[22] For each teen, we calculate average parents' earnings (the sum of earnings for the head of household and the spouse of the head of household) while the child was a teenager (also transformed into the IHS of average income to match the dependent variable). Additionally, we use each household's location in 2000 and household size in 2000 matched to their average parents' LEHD earnings to identify AMI figures at the county level. We then create a ratio of parents' earnings to AMI in order to account for the differences in average earnings across regions, which can vary by almost $75,000 for metropolitan areas within the U.S. Since local housing authorities often require that a household earn less than 50 percent of AMI to be eligible for assistance, we retain only children in households with an parents' earnings-to-AMI measure below 0.5, so that the analysis sample includes only those widely eligible for the subsidized housing treatment. As with the labor market outcomes, some households may appear to have lower incomes because they do not work in UI-covered employment. In future work, we will assess the significance of such omissions for our sample composition.

We employ both the composite of neighborhood (at the Census block group-level) poverty and the IHS of average annual parents' earnings between the ages of 13-18 as control

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[22] Under most HUD programs, households pay 30 percent of their income for rent with HUD subsidizing the remainder to cover operating costs or up to a fixed local "Fair Market Rent". Actual program requirements vary by subsidy type, but generally require residents to earn less than 80 percent of AMI (low income), with additional requirements dictating the percentage of residents that must be "very low income" (at or below 50 percent of AMI) or "extremely low income" (at or below 30 percent of AMI).

variables in some specifications. Aaronson (1998) examined whether cross-sibling variation in household income is associated with moves across neighborhoods. Likewise, we acknowledge that changes in household income may be directly associated with moves into and out of subsidized housing. Controlling for the household income during the period each sibling is between 13-18 acts to control for such concerns. Controlling for changes in the poverty rate when each sibling is between 13-18 is designed to capture one of the mechanisms for the impact of subsidized housing. As such, we interpret adding each of these two longitudinal controls somewhat differently. We interpret specifications with controls for parents' earnings as a robustness check on the importance of unobserved, time-varying characteristics, and those with controls for block group percent poverty as a test of one potential causal mechanism.

5.   The Sample: Basic Facts

In sum, to be included in the estimation sample, we require that individuals have been between 13 and 18 years of age in the year 2000, have non-missing values for age, gender, ethnicity, treatment status, and residential location, have successfully been assigned a unique PIK based on the 2000 Census, and be from the same 2000 renter household as at least one other teenager. Finally, because not all households are eligible for subsidized housing, we limit our sample to teenagers from households more likely to qualify for housing assistance, with average annual earnings below 50 percent of local AMI (see above). Of the 2.8 million children aged 13-18 in the U.S. in 2000 meeting these criteria, we end up with a final sample size of 520,000 teenagers.

Table 2 presents summary statistics for this sample.[23] The first column presents summary statistics for the sample used in estimation – teenagers living with another sibling aged 13-18. This sample is subdivided further, into those who were in households not in subsidized housing anytime during the 1997-2005 study period (column 2), and those who were (column 3); the latter are then subdivided further, into those who never lived in subsidized housing as a teenager 13-18 (column 4), and those who did (column 5). The comparison between columns 4 and 5 is the raw differences analog to our main empirical results for the dummy treatment effect.

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

There are a few minor differences between the estimation sample (column 1) and the full sample of teenagers; that is, the sample including cases in which there is only one teenager in the household (not shown). Of course, since we require that the estimation sample have at least two teenagers aged 13-18, the average household size is bigger. In the estimation sample, the proportion which is non-Hispanic Black is slightly higher, the proportion in single-parent households is slightly lower, and the proportion receiving a housing subsidy is slightly higher. These differences relate to the generalizability of the study, but have no bearing on the identification approach.

Comparison of columns 2 and 3 foreshadow the likely findings from an OLS regression. There are substantial differences in the outcome variables examined – those in subsidized housing earned less during the 2008-2010 period ($30,000 versus $33,000 on average), they worked fewer quarters (6.540 versus 7.209 on average), and a lower percentage had any labor market earnings during the 2008-2010 period (80.6 percent versus 83.3 percent). Comparisons of columns 4 and 5 foreshadow the likely findings from a household fixed effects regression –there are few differences apparent from the comparison.[24] But unconditional differences are not likely to tell the whole story, for which we will turn to a regression analysis.

Figure 1 displays the distribution of within-household differences—each teenagers own subsidized housing participation net the household mean for all teenagers--we use to identify our regression model. The figure is based on the sample in Table 2, Column 3, but teenagers are also required to be from households with at least some within-household difference in subsidized housing participation among the household members aged 13-18.[25] This sub-sample included 41.7 percent of housing voucher participants and 69.3 percent of public housing participants. The distribution is unimodal and symmetric around zero, with an overwhelming majority of teenagers within two years from of the household mean participation.

Given the identification strategy we employ, an important question is what causes the observed within-household differences in subsidized housing participation?

We define treatment as teenage—i.e. between the ages of 13 and 18—participation in subsidized housing. However, for sample members who are 17 or 18 years of age in 2000, we are

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[24] Only 15 percent of children in the ever-subsidized household sample receive no subsidy between the ages 13-18. This might seem to be a small subset to serve as a "control" sample for the effect of a subsidy in the dummy treatment variable regressions. Note, however, that we also estimate models with a dose treatment variable, allowing for wider variation in subsidy receipt.

[25] The restriction that teenagers have some within-household variation is made for expositional purposes.

unable to observe their subsidized housing participation at age 13 (or age 14 for individuals aged 18 in 2000) because our administrative records begin in 1997. As a result, it is possible that some of the within-household variation results from this left-censoring of treatment. We test for the importance of censoring by limiting the sample to only teenagers aged 13-16 in 2000; that is, those teenagers with uncensored treatment.[26] We find no differences between our main estimates and the results run on the age-limited sample. Therefore, while some of the observed within-household variation may result from age censoring, this variation does not drive the empirical results we present later.

Similarly, measurement error in the administrative subsidized housing records could create within-household variation. For example, if for some reason HUD's enumeration of children in a household is incomplete in one year, we would incorrectly interpret the incomplete record as there being within-household heterogeneity in subsidized housing participation in that year. To account for this possibility, we present a robustness check that predicts subsidized housing participation for each teenager using just their age and the observed participation of their head of household from the 2000 census. This predicted treatment is not subject to differential measurement error within a household. We show both reduced form estimates that use predicted participation to define treatment and IV estimates that instrument for actual participation with the predicted participation. The results suggest that measurement error in the administrative records does not drive our estimates or explain the within-household differences we observe.

A third possibility, is that changes in parental income or earnings could alter household eligibility for different types of subsidized housing. This is potentially problematic for our identification strategy as household fixed effects do not account for this type of time-varying heterogeneity. As we discuss in more detail later in the paper, we develop a longitudinal, child-specific measure of parental earnings using the LEHD data and test whether our results are affected by its inclusion as a control variable. In short, the main estimates are unaffected. Given the extensive literature suggesting parents earnings have a strong positive relationship with child earnings (Chetty et al. 2014), this suggests that within-household (i.e. longitudinal) changes in parental income and earnings are unlikely to explain much of the within-household variation in subsidized housing participation.

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[26] Results available upon request.

In their research estimating the impact of the Earned Income Tax Credit (EITC) on earnings, Chetty et al. (2013) use changes in information about the existence and shape of the EITC benefit for identification. As with EITC, it is possible that eligible households are not aware of the location of public housing projects, their own eligibility for public or voucher-assisted housing, or how to apply to either program. If households acquire information about one or both programs while they have multiple teenage children, it could prompt them to apply for subsidized housing or to switch between the two programs. This would then create within-household variation in teenage subsidized housing participation. Further, this within-household variation would be unrelated to the potential outcomes of children, having been driven instead by the timing of an information shock to the household. Unfortunately, while this seems plausible and remains a likely explanation for within-household variation in treatment, we have no way to test empirically for the existence of such information shocks.

Finally, as pointed out by Jacob and Ludwig (2012) and others, subsidized housing programs are frequently oversubscribed, leading to lengthy lags between when households apply for a particular program and when they are allotted a voucher or public housing unit. Households that apply to an oversubscribed subsidized housing program may end up with children exposed to different amounts of the program purely as a result of their mandated wait time. Consider a household with one 13-year-old and one 12-year-old that applies for a public housing program, is placed on the waitlist for one year, and then remains in that project. In the absence of the wait time, both children would experience the same amount of teenage public housing participation: six years each. However, because of the one year wait, the 13-year-old will end up spending only five teenage years in public housing while the 12-year-old will spend six.

There also appear to be substantial wait times for both public and voucher-assisted housing in our sample. To illustrate these wait times, we use data on all subsidized housing participants from the year 2000. For most households, the data contain information on the date they entered a waitlist as well as the date they were granted admission to the program. In some cases the two dates are the same, indicating there was no wait for the program, but for most households there was a non-trivial wait between when they were placed on a waitlist and when they were admitted. Figure 2 displays the distribution of wait times for individuals in voucher and public housing who entered subsidized housing no earlier than 1995 and who were found in subsidized housing in 2000. We restrict the entrance date to be after 1995 because data quality

decreases in the early 1990s and because these waits are likely to be a better approximation to the waits experienced by the households in our sample. Figure 2 indicates that about 12% of public housing residents and 29% of housing voucher recipients faced wait times of one year or more. Clearly, many prospective subsidized housing participants face lengthy lags between when they apply and when they are admitted to programs. These lags offer another plausible explanation for the observed within-household differences in subsidized housing participation.

6.   Empirical Results

6.1 Samples and specifications

The key question we address is whether living in voucher-supported or public housing affects a teenager's labor market experiences as an adult. We compare the effects on earnings over the 2008-10 period of each of these two HUD housing types with nonsubsidized housing.[27]

Table 3 presents results for teenagers from all households while Tables 4, 5, and 6 present results for teenagers from non-Hispanic White households, non-Hispanic Black households, and Hispanic households, respectively. Each table presents results for a "dummy treatment," which consists of a binary measure of whether an individual ever participated in each type of subsidized housing as a teenager, and a "dose treatment," which is defined as the number of years an individual participated in each type of subsidized housing while under the age of 18. As described above, the dependent variable is the inverse hyperbolic sine of total earnings over the 2008-10 period, In addition to the treatment variables interacted with gender, unlisted controls include age, gender, age by gender, and household race/ethnicity by gender.[28] Table 7 presents the effect of each type of housing subsidy, separately for each sex and household race-ethnicity type, and it compares the estimated effect across gender and across the two subsidized housing types within each possible sex/household race-ethnicity combination.

In Tables 3 through 6, the first column presents OLS estimates of the specification described in equation (1). The coefficients capture the correlation between earnings and the two different types of subsidized housing participation after controlling for observed covariates, but

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[27] In unreported results, we have also used the total number of quarters worked over the 2008 to 2010 period and an indicator for whether the individual ever worked during the 2008 to 2010 period as dependent variables. The results are qualitatively consistent regardless of which measure of labor market performance is used.

[28] The complete regression results as well as the results for the other measures of labor market performance (cf. footnote 24) are available from the authors.

as discussed before, are susceptible to bias as a result of selection based on unobservable factors. The second column in each table presents estimates from the household fixed effects (HFE) specification, described in equation (2). By using only within-household variation, these estimates purge the treatment effects of all bias resulting from time-invariant, household-level unobserved characteristics. We believe these estimates better capture the causal effect of subsidized housing participation as a teenager on adult labor market earnings.

The third, fourth, and fifth columns in each table presents results from a HFE specification that, in addition to the controls in column (2), also includes, in column (3), a control for the average parents' earnings that each individual experienced between 13 and 18 and its interaction with a male dummy, in column (4) a control for average block group percent poverty that each child experienced between 13 and 18 years of age, and in column (5) controls for both parents' earnings and block group poverty. We interpret the estimates in Column 3 as a test for whether our household fixed effects are effectively ridding the treatment effects of bias from unobserved, time-varying heterogeneity. Specifically, if our treatment effects do not change after the inclusion of parents' earnings, then either the within-household differences in subsidized housing participation or the within-household differences in adult earnings (or both) are unrelated to within-household differences in parents' earnings. Similarly, the estimates in column 4 are an indicator of whether neighborhood quality, as proxied by block-group percent poverty, is a potential mechanism for the estimated treatment effects. Column 5 accounts for both factors.

6.2 Results for all households

We now turn to the coefficients of interest beginning with the estimates that pool across household race/ethnicity in Table 3. In column (1), for both the dummy and dose treatments, the OLS results show that there are significant negative effects on subsequent total earnings with larger negative effects for males. Significant negative relationships between the two types of subsidized housing participation and adult earnings also occur in each of the race/ethnicity groups (Tables 4-6) although magnitudes vary.

However, the HFE results, which control for all household level time-invariant heterogeneity, paint an entirely different picture; the HFE results for females and males are summarized in Table 7, Panel A. The negative effects from OLS are attenuated or reversed.

Housing voucher participation is not negatively related to adult earnings for females in the HFE specification. Both living in public housing and living in a housing voucher-subsidized unit lead to positive and significant effects on later earnings for females. The effect of voucher participation remains negative and statistically significant for males with the dummy treatment, and is not statistically different from zero for public housing. The effects estimated for the dose treatment (years) reinforce the findings of the dummy treatment. The effects for males are significantly more negative than the effects for females with both the dummy and the dose treatment. For the dummy treatment, public housing is more beneficial than housing vouchers for both females and males (no difference was found for the dose treatment).

The point estimate suggests that ever having lived in voucher-supported housing as a teenager increases early adult earnings by roughly 14 percent for females and reduces earnings by roughly 24 percent for males. The dose results indicate that each additional year of voucher participation increases adult earnings for females by about 6 percent and reduces adult earnings for males by 3 percent. For public housing, the relationship between participation and future earnings is not significantly different from zero for males, for both the dummy and dose treatments, but is positive and significant for females. The point estimate suggests that for females, ever having lived in public housing as a teenager increases adult earnings by roughly 29 percent. The dose results suggest that each additional year of voucher participation increases early adult earnings for females by 9 percent.

The results in columns 3 and 4, which add controls for average parents' earnings and average block group percent poverty, are essentially unchanged. In the following subsection, we find that columns 2 and 3 are similar even when allowing the results to differ for different race/ethnicity samples. We believe this indicates that the household fixed effects specification is effectively ridding the treatment effects of bias from unobserved, time-varying heterogeneity. Consequently, we report just the simple HFE results in the text (and Table 7) below.

6.2 Race/ethnicity samples

To help understand the results in Table 3, we investigate whether the results differ by household race-ethnicity. Tables 4 through 6 thus explore whether there is treatment effect heterogeneity by household race-ethnicity. We do this by estimating coefficients separately for non-Hispanic White households, non-Hispanic Black households, and Hispanic households,

respectively. Comparing results across these three subgroups (see Table 7 for a summary), we find important differences. For example, comparing the HFE results shows substantial positive effects of both living in voucher-supported housing and in public housing on young adult earnings for Black females, but not for Black males or for any subset of Hispanics or non-Hispanic Whites (we actually find a negative effect of housing vouchers on earnings for Hispanic males). The dose results generally echo the dummy results, except that we find a positive effect on later earnings for non-Hispanic Black males who lived in public housing. Clearly, there is important heterogeneity across race/ethnicity groups, affirming the importance of considering these groups separately.

The positive effects for non-Hispanic Black females suggest they receive an earnings premium of 15 percent from participating in the housing voucher program and 18 percent from living in public housing relative to not having participated in either program. The dose results indicate that each program increases earnings relative to non-participants by about 6 percent per year. Non-Hispanic Black males also see their adult earnings increase as a result of public housing participation, by about 7 percent per year of residence. The estimate for Hispanic males indicates that voucher housing decreases adult earnings by about 8 percent

Table 7, in addition to displaying the average partial effects of each type of subsidized housing separately by gender, also displays tests of whether the effects of each type of subsidized housing are equal. For example, we test whether the effect of voucher housing for females is the same as the effect of public housing for females. We conduct this test for each possible household race/sex combination, and for both the dummy and dose treatments. For the combined sample, we find that vouchers lead to lower male outcomes than public housing for the dummy but not the dose treatments. For the subsamples, this result is apparently driven by the result for non-Hispanic Black and Hispanic males; there was one significant dose treatment difference – for non-Hispanic Black females. When comparing the estimates for females to those for males, the results differ for non-Hispanic Blacks in housing vouchers, and for non-Hispanic Whites in public housing. Though there are differences between males and females for the combined sample, there are no statistically different effects when the samples are disaggregated by race/ethnicity.

7.   Extensions and robustness checks

We undertook three extensions in an attempt to understand these results in more detail. As much of the discussion of public housing in the popular media concerns high-rise projects primarily found in urban areas, we check whether the effect of living in a large public housing project is different from the overall results. That is, we allow for the effect of public housing participation to differ according to project size (population). To do so, we define person-weighted project size quartiles by considering all public housing projects over the period 1997-2005. On the basis of these quartiles, it was determined whether each individual in our sample who ever participated in public housing was also a resident of large public housing project (the top quartile). We then included either an indicator for whether each teenager in our sample ever lived in a large public housing project or a count of the number of years each teenager lived in a large public housing project in addition to the measures of housing voucher participation and general public housing participation included in previous specifications. The coefficient estimates from household fixed effects specifications for these large public housing measures capture any differential effect that large public housing residence as a teenager has on adult earnings. Table 8 presents these results. The estimated coefficients on the housing voucher and general public housing measures are almost identical to those from the more basic household fixed effects specification. This suggests the heterogeneity with respect to project size in the effect of public housing is not particularly important empirically. In no column is the differential of large public housing significantly different from zero for females or for males. We therefore find no evidence to support the idea that living in a large public housing project is particularly harmful for children's later earnings.[29]

Similarly, it might be the case that being assigned to a public housing project where households earn relatively low annual incomes has a differential impact on adult outcomes. Such a differential effect could exist as a result of role model effects (e.g. observing adults who supply more labor while a teenager increases labor supply as an adult) or if project level social networks enable individuals to find a job or a higher paying job more easily. To test for heterogeneity by project-level household income, we compute the person-weighted median household adjusted

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[29] One caveat with this finding is that we are limited to the set of metropolitan areas where housing authorities did not participate in Moving To Work (and thus, continued reporting housing status).

income for each project year.[30] Next, we create year-specific quartiles and assign each project-year to a quartile. Teenagers in our sample are then matched to the public housing project and the associated household income quartile for each year they participated in public housing. We define the lowest-income public projects as those that fall into the bottom quartile with respect to median household annual adjusted income. This match is used to create an indicator for whether each teenager ever resided in a lowest-income public housing project and a count of the number of years they resided there. These measures are then included, in addition to the housing voucher and general public housing measures, as discussed in the previous paragraph. Table 9 presents the household fixed effects estimates from these specifications. Allowing for the effect of public housing to differ by median household income has almost no impact on the main estimates. Further, there appears to be no additional impact of living in one of the lowest-income public housing projects for either males or females. Taken together, Tables 8 and 9 indicate little heterogeneity in the estimated treatment effect of public housing along project-type dimensions.

One additional robustness check we conducted deals with possible measurement error as well as endogenous changes in the structure of households. Specifically, in Table 10 we use predicted participation rather than the actual indicator of public and voucher housing participation. To construct the predicted value, we use the age of the children in the household and the actual information on whether the parent is in subsidized housing. That is, for any given year, if a parent is in subsidized housing and the child is in the 13-18 year-old age range, then the "predicted" participation measure indicates that the child is in subsidized housing in that year. Differences between actual and predicted measures of participation might arise for two reasons, both of which we would like to avoid. The first is measurement error. The second is that the child left the household while still aged 13-18. Such departures might reflect events (e.g. a child leaving to live with a member of the extended family such as a grandparent) that have an impact on later outcomes but are unrelated to the mechanisms we are seeking to identify. Using this approach, Table 10 reports results for using the actual treatment (the same as Table 3), using the predicted treatment instead of the actual treatment, and instrumenting the actual treatment with the predicted treatment. The results in Table 10 are strikingly similar when using any of the participation definitions. This suggests that measurement error and household

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[30] HUD computes adjusted annual income on the basis of household-type (elderly, disabled, family), the number of dependents in the household and income net of certain child care, medical and disability expenses. We use this HUD adjusted income to identify low income projects.

departures/dissolutions are not importantly affecting the estimated effects of subsidized housing participation.

8.   Concluding Comments

In spite of the policy relevance of having a sound understanding of the effects of subsidized rental housing on long-term outcomes, the existing literature is clearly lacking. In this paper, we report results from a project that fills important gaps in this literature by estimating the long-term causal effects of public housing and voucher assisted housing participation as a teenager on adult earnings.

Our use of national data on housing assistance, households, and earnings from administrative records, censuses, and surveys at the U.S. Census Bureau makes these contributions possible. The data permit us to identify households with children between the ages of 13-18 in the year 2000, follow those children across a variety of settings of assisted and unassisted rental housing, and then to investigate their employment and earnings up to 10 years later.

We recognize in that unobserved heterogeneity and the associated selection bias is an obstacle to estimating causal effects of housing. To overcome this issue, we exploit the very large sample size and longitudinal nature of the data and estimate household fixed effects models that identify the impact of assisted housing by exploiting variation within households. We also consider specifications that include time-varying household measures that may vary across children, including parent's income and average neighborhood poverty, but these controls do not affect our estimates. One main finding is that the substantial negative effects of subsidized housing often found in the literature may be largely attributable to the selection of households entering assisted housing. A second main finding is that having controlled for unobservable heterogeneity with household fixed effects, subsidized housing participation as a teenager yields a large positive effects on young adult earnings for females. For males we generally find no effect though in some cases the effect on earnings is appears slightly negative.

The point estimates suggest that young adult females earn 14 percent more if they ever resided in voucher housing and 29 percent more if they ever resided in public housing. The corresponding estimates from the dose treatment indicate that each additional year of voucher-

supported housing participation increases earnings by 6 percent for females while each additional year of public housing increases female earnings by 9 percent.

We disaggregate our sample by the race/ethnicity of households, to reflect the different contexts in which households select into assisted housing. We find results that differ considerably by race/ethnicity. In particular, non-Hispanic Blacks, and especially non-Hispanic Black females, benefit more than Hispanics or non-Hispanic Whites. Thus, the main finding of a large, positive effect for females is primarily driven by the impact on Black females. The findings for Black teenagers are in strong contrast to the findings for non-Hispanic White and Hispanic teenagers. There were no significant positive or negative findings for Non-Hispanic White male or female earnings as a young adult. We also find few significant results for Hispanic teenagers.

We also investigated heterogeneous treatment effects by type of public housing project (project size and project median income). We found no evidence that the effects of public housing on labor market outcomes varied along these dimensions. We also conducted robustness checks for measurement error and endogenous changes in family structure and found our results were robust to these concerns.

There remain a number of limitations of our analysis. First, our results apply to just two of the many subsidized housing programs, albeit the largest – public housing and housing vouchers. Second, our results might not be representative of all subsidized households (that is, households with younger children, and those with just one teenager). In this regard, our results pertain only to teenagers between the ages 13 and 18. While this is a formative period, other research on siblings has shown substantial effects for within household differences occurring early in childhood or even before birth. Future work should investigate whether exposure to subsidized housing during earlier periods of life has long-term implications as well.

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Figure 1: Distribution of Within−Household Differences among Households with Some Subsidized Housing Participation
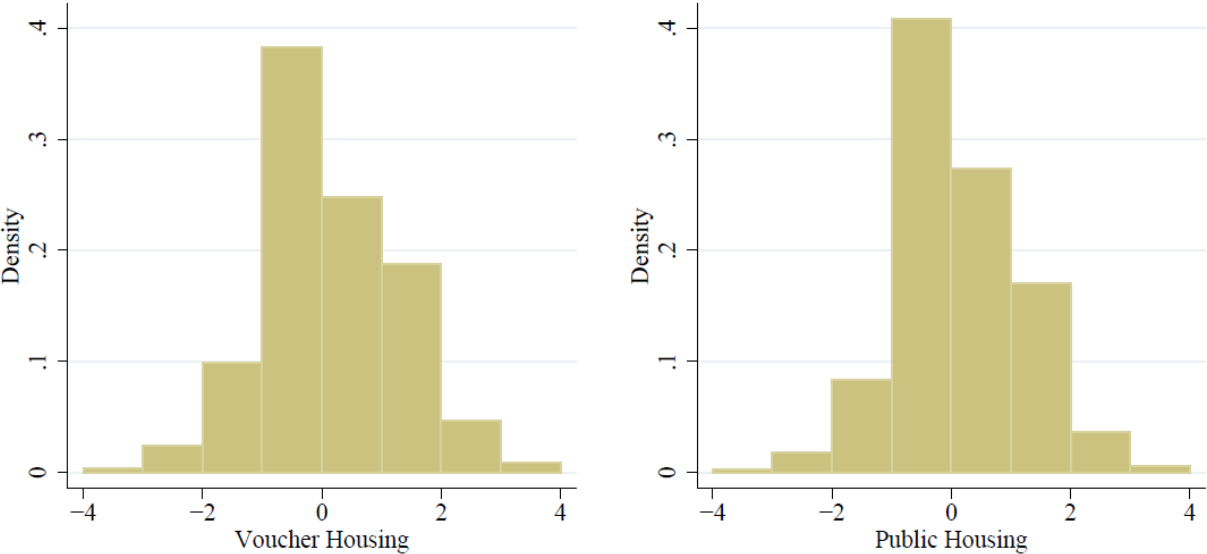
Note: Figure displays the distribution of within−household differences in public housing and housing voucher participation for teenagers in the main sample. Within−household differences are topcoded to have an absolute value no greater than four and individuals from households with no differences in program participation are omitted. Of individuals in households with some voucher housing participation, 0.417 have no within−household variation. Of individuals in households with some public housing participation, 0.693 have no within−household variation. Each bin represents a one year difference in program participation.

Figure 2: Days Spent on Waitlist Before Program Admission
By Subsidized Housing Type in 2000
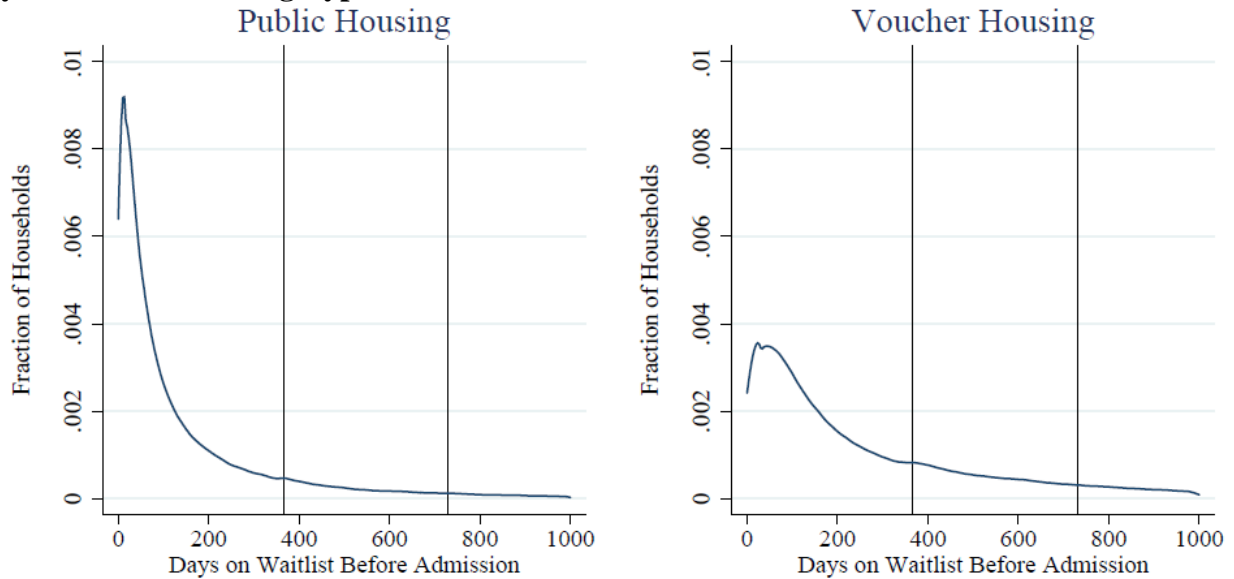
Note: Figure displays the distribution of days spent on the waiting list before admission for households found in both public and voucher housing in the year 2000. The sample is limited to households with non–missing admission and waitlist information who gained admission to their program no earlier than 1995. 0.116 of public housing households spent >1 year and 0.033 spent >2 years on a waitlist prior to admission. 0.287 of voucher housing households spent >1 year and 0.108 spent >2 years on a waitlist prior to admission.

Table 1: Characteristics of Households Receiving Federal Rental Subsidies in the Form of Public Housing or Vouchers, 2000

	Public Housing	Voucher-Supported Housing
Number of people per unit	2.3	2.7
Rent per month	$202	$226
Household income per year	$10,000	$10,600
Average months on waiting list	15	26
Average months since moved in	107	52
Percent of households where majority of income is derived from welfare	11%	12%
Percent of metropolitan area median income	25	23
Percent of households with children	45	61
Percent minority	69	61
Percent moved in past year	10	15
Percent with 0 or 1 bedrooms	48	25
Percent with 2 bedrooms	25	39
Percent with 3 or more bedrooms	27	35
Total Households	**1,282,099**	**1,817,360**

SOURCE: HUDUSER, HUD Public Use Data.

Table 2. Summary Statistics for Teenagers Aged 13-18 in 2000 in Renter Households Whose Parents Earned Less than Half of HUD-Specified Area Median Income in 2000

			Individuals Aged 13-18 in 2000 With at least One Other Sibling Aged 13-18 in 2000		
				In Households who received some housing subsidy 1997-2005	
	(1) Total	(2) In households not receiving any housing subsidy 1997-2005	(3) Total	(4) Teenagers who never lived in subsidized housing while aged 13-18	(5) Teenagers who lived in subsidized housing while aged 13-18
Household size in 2000	5.721	5.697	5.772	6.122	5.706
Age in 2000	15.381	15.424	15.287	15.537	15.240
Male	0.500	0.506	0.487	0.503	0.484
Black non-Hispanic household	0.312	0.232	0.482	0.463	0.486
Hispanic household	0.314	0.334	0.270	0.275	0.269
Other non-Hispanic household	0.080	0.086	0.068	0.065	0.068
White non-Hispanic household	0.294	0.348	0.180	0.196	0.177
Average block group percent poverty during teens	0.115	0.112	0.123	0.122	0.123
Average inverse hyperbolic sine of parents earnings during teens	8.006	8.219	7.546	7.662	7.524
Single-headed household	0.653	0.601	0.764	0.742	0.768
Public housing resident between ages 13-18	0.095	0.000	0.300	0.000	0.357
Housing voucher recipient between ages 13-18	0.182	0.000	0.573	0.000	0.681
Years in public housing between ages 13-18	0.311	0.000	0.979	0.000	1.163
Years in voucher-supported housing between ages 13-18	0.626	0.000	1.972	0.000	2.342
Total labor market earnings between 2008 and 2010	30000	33000	25000	25000	25000
Total number of quarters worked between 2008 and 2010	7.051	7.277	6.566	6.502	6.578
Proportion with any labor market earnings between 2008 and 2010	0.830	0.840	0.810	0.804	0.811
Number of observations (rounded)	520,000	358,000	162,000	25,000	137,000

SOURCE: Authors' tabulations of matched 2000 Census, HUD PIC, and LEHD files (see text).
NOTE: Excludes teenagers who lived in counties with at least one Public Housing Authority participating in HUD's Moving to Work Program.

Table 3. The Effect of Teenage Residence in HUD-Subsidized Housing on Total 2008-2010 Earnings

Panel A	Dummy Treatment				
	OLS	HFE	HFE with parents' earnings control	HFE with block group poverty control	HFE with both earnings and poverty controls
Living in a household receiving a housing voucher	-0.347***	0.135**	0.132**	0.135**	0.132**
	0.021	0.046	0.046	0.046	0.046
Male living in a household receiving a housing voucher	-0.349***	-0.371***	-0.366***	-0.371***	-0.366***
	0.032	0.040	0.040	0.040	0.040
Living in public housing	-0.290***	0.292***	0.290***	0.289***	0.288***
	0.027	0.059	0.059	0.059	0.059
Male living in public housing	-0.336***	-0.360***	-0.354***	-0.354***	-0.349***
	0.042	0.053	0.053	0.053	0.053
Male	-0.461***	-0.407***	-0.485***	-0.374***	-0.454***
	0.029	0.036	0.050	0.044	0.058
Natural log of average parents' earnings, 1997-2005			0.013		0.013
			0.016		0.016
Male, natural log of average parents' earnings, 1997-2005			0.009*		0.009*
			0.004		0.004
Average block group poverty, 2000				-0.127	-0.149
				0.468	0.468
Male, average block group poverty, 2000				-0.293	-0.249
				0.241	0.241
Constant	8.900***	8.722***	8.619***	8.737***	8.635***
	0.020	0.025	0.134	0.059	0.145
R-squared	0.009	0.006	0.006	0.006	0.006

Table 3, continued.

Panel B	Dose Treatment				
	OLS	HFE	HFE with parents' earnings control	HFE with block group poverty control	HFE with both earnings and poverty controls
Years living in a household receiving a housing voucher	-0.077***	0.062***	0.061***	0.062***	0.061***
	0.006	0.012	0.012	0.012	0.012
Male, years living in a household receiving a housing voucher	-0.087***	-0.089***	-0.087***	-0.089***	-0.087***
	0.008	0.010	0.010	0.010	0.010
Years living in public housing	-0.069***	0.088***	0.087***	0.087***	0.086***
	0.007	0.018	0.018	0.018	0.018
Male, years living in public housing	-0.079***	-0.086***	-0.084***	-0.084***	-0.083***
	0.012	0.014	0.014	0.014	0.014
Male	-0.472***	-0.422***	-0.502***	-0.387***	-0.469***
	0.029	0.036	0.050	0.044	0.058
Natural log of average parents' earnings, 1997-2005			0.013		0.013
			0.016		0.016
Male, natural log of average parents' earnings, 1997-2005			0.010*		0.009*
			0.004		0.004
Average block group poverty, 2000				-0.123	-0.145
				0.468	0.468
Male, average block group poverty, 2000				-0.313	-0.268
				0.241	0.241
Constant	8.881***	8.704***	8.601***	8.719***	8.617***
	0.020	0.025	0.134	0.059	0.145
R-squared	0.008	0.006	0.006	0.006	0.006

SOURCE: Authors' tabulations from matched Census 2000-LEHD-PIC file.
NOTES: OLS = Ordinary Least Squares. HFE = Household Fixed Effects. Number of observations = 520,000, rounded to the nearest thousand. See text for description of the sample. The dependent variable in each column is the inverse hyperbolic sine of total earnings between 2008 and 2010. All columns include controls for age, sex, age by sex, and household race by sex; race and ethnicity is determined by the reference person of the household as reported on the 2000 Census. Column 3 also includes a control for the inverse hyperbolic sine of average parents' annual earnings over the 1997-2005 period. Column 4 also includes a control for the average block group percent poverty experienced while a teenager. Column 5 includes both controls. Standard Errors below the estimates.
*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.10$.

Table 4. The Effect of Teenage Residence in HUD-Subsidized Housing on Total 2008-2010 Earnings, Non-Hispanic White Households

Panel A	Dummy Treatment				
	OLS	HFE	HFE with parents' earnings control	HFE with block group poverty control	HFE with both earnings and poverty controls
Living in a household receiving a housing voucher	-0.435***	0.005	0.002	0.009	0.006
	0.042	0.088	0.088	0.087	0.088
Male living in a household receiving a housing voucher	-0.040	-0.002	0.003	-0.011	-0.006
	0.061	0.076	0.076	0.076	0.076
Living in public housing	-0.312***	-0.082	-0.083	-0.075	-0.077
	0.067	0.125	0.126	0.126	0.126
Male living in public housing	0.298**	0.246*	0.250*	0.235	0.238
	0.098	0.122	0.122	0.122	0.122
Male	-0.224***	-0.185***	-0.256**	-0.266***	-0.338***
	0.045	0.055	0.079	0.074	0.094
Natural log of average parents' earnings, 1997-2005			-0.005		-0.005
			0.027		0.027
Male, natural log of average parents' earnings, 1997-2005			0.009		0.009
			0.007		0.007
Average block group poverty, 2000				-0.300	-0.294
				0.940	0.940
Male, average block group poverty, 2000				0.870	0.868
				0.561	0.561
Constant	8.813***	8.732***	8.773***	8.760***	8.800***
	0.031	0.037	0.219	0.097	0.237
R-squared	0.003	0.003	0.003	0.003	0.003

Table 4, continued.

Panel B	Dose Treatment				
	OLS	HFE	HFE with parents' earnings control	HFE with block group poverty control	HFE with both earnings and poverty controls
Years living in a household receiving a housing voucher	-0.117***	0.033	0.032	0.034	0.033
	0.012	0.026	0.026	0.026	0.026
Male, years living in a household receiving a housing voucher	-0.016	-0.008	-0.006	-0.010	-0.009
	0.017	0.021	0.021	0.021	0.021
Years living in public housing	-0.081***	0.016	0.015	0.018	0.017
	0.021	0.045	0.045	0.046	0.046
Male, years living in public housing	0.064*	0.050	0.051	0.047	0.048
	0.029	0.035	0.036	0.036	0.036
Male	-0.218***	-0.177**	-0.247**	-0.262***	-0.332***
	0.045	0.055	0.079	0.074	0.094
Natural log of average parents' earnings, 1997-2005			-0.005		-0.005
			0.027		0.027
Male, natural log of average parents' earnings, 1997-2005			0.009		0.009
			0.007		0.007
Average block group poverty, 2000				-0.312	-0.306
				0.940	0.940
Male, average block group poverty, 2000				0.903	0.901
				0.560	0.560
Constant	8.806***	8.713***	8.755***	8.742***	8.783***
	0.031	0.038	0.219	0.097	0.237
R-squared	0.003	0.003	0.003	0.003	0.003

SOURCES, NOTES: See Table 3. Number of observations = 197,000, rounded to the nearest thousand.

Table 5. The Effect of Teenage Residence in HUD-Subsidized Housing on Total 2008-2010 Earnings, Non-Hispanic Black Households

Panel A	Dummy Treatment				
	OLS	HFE	HFE with parents' earnings control	HFE with block group poverty control	HFE with both earnings and poverty controls
Living in a household receiving a housing voucher	-0.124***	0.145*	0.146*	0.140*	0.142*
	0.030	0.066	0.066	0.066	0.066
Male living in a household receiving a housing voucher	-0.174***	-0.229***	-0.232***	-0.218***	-0.223***
	0.049	0.061	0.061	0.061	0.061
Living in public housing	-0.176***	0.177*	0.178*	0.180*	0.181*
	0.035	0.078	0.078	0.078	0.078
Male living in public housing	-0.048	-0.050	-0.058	-0.060	-0.065
	0.057	0.073	0.073	0.073	0.073
Male	-1.356***	-1.253***	-1.101***	-1.361***	-1.206***
	0.052	0.065	0.088	0.082	0.107
Natural log of average parents' earnings, 1997-2005			0.066*		0.065*
			0.031		0.031
Male, natural log of average parents' earnings, 1997-2005			-0.019*		-0.017*
			0.007		0.007
Average block group poverty, 2000				-0.706	-0.632
				0.720	0.720
Male, average block group poverty, 2000				0.861*	0.706
				0.408	0.413
Constant	8.821***	8.618***	8.099***	8.707***	8.185***
	0.033	0.044	0.243	0.100	0.259
R-squared	0.029	0.033	0.033	0.033	0.033

Table 5, continued.

Panel B	Dose Treatment				
	OLS	HFE	HFE with parents' earnings control	HFE with block group poverty control	HFE with both earnings and poverty controls
Years living in a household receiving a housing voucher	-0.017*	0.061***	0.061***	0.060***	0.060***
	0.007	0.018	0.018	0.018	0.018
Male, years living in a household receiving a housing voucher	-0.030*	-0.033*	-0.033*	-0.030*	-0.031*
	0.012	0.015	0.015	0.015	0.015
Years living in public housing	-0.035***	0.058*	0.059*	0.060*	0.061*
	0.009	0.024	0.024	0.024	0.024
Male, years living in public housing	0.007	0.012	0.009	0.008	0.007
	0.015	0.019	0.019	0.020	0.020
Male	-1.385***	-1.300***	-1.151***	-1.410***	-1.259***
	0.052	0.064	0.088	0.081	0.106
Natural log of average parents' earnings, 1997-2005			0.066*		0.065*
			0.031		0.031
Male, natural log of average parents' earnings, 1997-2005			-0.019*		-0.016*
			0.007		0.007
Average block group poverty, 2000				-0.729	-0.658
				0.720	0.720
Male, average block group poverty, 2000				0.881*	0.731
				0.408	0.413
Constant	8.792***	8.592***	8.073***	8.683***	8.162***
	0.033	0.045	0.243	0.100	0.259
R-squared	0.029	0.033	0.033	0.033	0.033

SOURCES, NOTES: See Table 3. Number of observations = 162,000, rounded to the nearest thousand.

Table 6. The Effect of Teenage Residence in HUD-Subsidized Housing on Total 2008-2010 Earnings, Hispanic Households

Panel A	Dummy Treatment				
	OLS	HFE	HFE with parents' earnings control	HFE with block group poverty control	HFE with both earnings and poverty controls
Living in a household receiving a housing voucher	-0.840***	-0.159	-0.174	-0.159	-0.174
	0.049	0.101	0.101	0.101	0.101
Male living in a household receiving a housing voucher	-0.041	-0.025	0.006	-0.025	0.006
	0.071	0.082	0.083	0.082	0.083
Living in public housing	-0.693***	0.108	0.096	0.107	0.097
	0.063	0.133	0.133	0.133	0.133
Male living in public housing	0.027	-0.018	0.007	-0.017	0.004
	0.091	0.110	0.110	0.110	0.110
Male	-0.025	0.001	-0.241*	0.007	-0.251*
	0.058	0.072	0.103	0.087	0.116
Natural log of average parents' earnings, 1997-2005			-0.003		-0.003
			0.032		0.032
Male, natural log of average parents' earnings, 1997-2005			0.028***		0.028***
			0.008		0.008
Average block group poverty, 2000				0.031	-0.038
				0.840	0.841
Male, average block group poverty, 2000				-0.046	0.071
				0.396	0.396
Constant	9.118***	8.905***	8.933***	8.901***	8.939***
	0.040	0.049	0.266	0.118	0.288
R-squared	0.009	0.002	0.003	0.002	0.003

Table 6, continued.

Panel B	Dose Treatment				
	OLS	HFE	HFE with parents' earnings control	HFE with block group poverty control	HFE with both earnings and poverty controls
Years living in a household receiving a housing voucher	-0.194***	0.005	0.001	0.005	0.001
	0.013	0.026	0.026	0.026	0.026
Male, years living in a household receiving a housing voucher	-0.021	-0.019	-0.011	-0.019	-0.011
	0.018	0.021	0.021	0.021	0.022
Years living in public housing	-0.167***	0.033	0.030	0.033	0.030
	0.017	0.038	0.038	0.038	0.038
Male, years living in public housing	-0.000	-0.017	-0.011	-0.017	-0.012
	0.024	0.030	0.030	0.030	0.030
Male	-0.011	0.013	-0.222*	0.016	-0.235*
	0.058	0.071	0.102	0.087	0.115
Natural log of average parents' earnings, 1997-2005			-0.002		-0.002
			0.032		0.032
Male, natural log of average parents' earnings, 1997-2005			0.027**		0.027***
			0.008		0.008
Average block group poverty, 2000				0.010	-0.057
				0.840	0.841
Male, average block group poverty, 2000				-0.021	0.094
				0.396	0.397
Constant	9.086***	8.875***	8.897***	8.873***	8.905***
	0.040	0.050	0.266	0.118	0.288
R-squared	0.008	0.002	0.003	0.002	0.003

SOURCES, NOTES: See Table 3. Number of observations = 123,000, rounded to the nearest thousand.

Table 7. Summary of the Effect of Teenage Residence in HUD-Subsidized Housing on Total 2008-2010 Earnings, by Gender and Race/Ethnicity, Household Fixed Effects Specification

	Dummy				Dose		
	Housing Voucher (HV) Treatment Effect	*Public Housing (PH) Treatment Effect*	*Are Subsidy Effects Different? (HV vs. PH)*		*Housing Voucher (HV) Treatment Effect*	*Public Housing (PH) Treatment Effect*	*Are Subsidy Effects Different? (HV vs. PH))*
All Households							
Females (F)	0.135**	0.292***	Yes**		0.062***	0.088***	No
	0.046	0.059			0.012	0.018	
Males (M)	-0.236***	-0.068	Yes*		-0.027**	0.002	No
	0.048	0.064			0.013	0.019	
Are Subsidy Effects Different? (F vs. M)	Yes***	Yes***			Yes***	Yes***	
Non-Hispanic White Households							
Females	0.005	-0.082	No		0.033	0.016	No
	0.088	0.125			0.026	0.045	
Males	0.003	0.165	No		0.025	0.066	No
	0.090	0.136			0.026	0.048	
Are Subsidy Effects Different? (F vs. M)	No	Yes*			No	No	
Non-Hispanic Black Households							
Females	0.145*	0.177*	No		0.061***	0.058*	Yes*
	0.066	0.078			0.018	0.024	
Males	-0.084	0.126	Yes**		0.028	0.070***	No
	0.071	0.087			0.019	0.025	
Are Subsidy Effects Different? (F vs. M)	Yes***	No			No	No	
Hispanic Households							
Females	-0.159	0.108	No		0.005	0.033	No
	0.101	0.133			0.026	0.038	
Males	-0.184*	0.089	Yes*		-0.014	0.016	No
	0.104	0.137			0.026	0.039	
Are Subsidy Effects Different? (F vs. M)	No	No			No	No	

SOURCE: Tables 3-6.

NOTES: Estimates do not control for parents' earnings as a teenager or average block group percent poverty as a teenager. Observations rounded to the nearest thousand (All: 521,000; non-Hispanic White: 197,000; non-Hispanic Black: 162,000; Hispanic: 123,000). Standard errors are below the estimates.

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

Table 8. Subsidized Housing Residence and Adult Earnings by Household Race and Gender, Differentiating Large Public Housing

	Black Households		Hispanic Households		White Households		All Households	
	Dummy	Dose	Dummy	Dose	Dummy	Dose	Dummy	Dose
Living in a household receiving a housing voucher	0.145*	0.061***	-0.157	0.005	0.005	0.033	0.136**	0.062***
	0.066	0.018	0.101	0.026	0.088	0.026	0.046	0.012
Male living in a household receiving a housing voucher	-0.229***	-0.033*	-0.028	-0.019	-0.002	-0.008	-0.372***	-0.089***
	0.061	0.015	0.082	0.021	0.076	0.021	0.040	0.010
Living in public housing	0.172	0.076**	-0.005	0.019	-0.111	0.010	0.233***	0.093***
	0.088	0.028	0.154	0.044	0.130	0.047	0.066	0.021
Male living in public housing	-0.024	0.004	0.158	0.015	0.257*	0.063	-0.266***	-0.077***
	0.086	0.023	0.138	0.038	0.126	0.037	0.062	0.017
Living in a large public housing project	0.011	-0.057	0.280	0.032	0.289	0.071	0.192	-0.016
	0.147	0.051	0.257	0.079	0.425	0.166	0.120	0.041
Male living in a large public housing project	-0.077	0.023	-0.432*	-0.076	-0.093	-0.156	-0.285**	-0.028
	0.141	0.043	0.213	0.063	0.422	0.131	0.109	0.033
	8.618***	8.591***	8.905***	8.875***	8.732***	8.713***	8.722***	8.704***
Constant	0.044	0.045	0.049	0.049	0.037	0.038	0.025	0.025
Observations	162,000	162,000	123,000	123,000	197,000	197,000	520,000	520,000

SOURCE: Authors' tabulations from matched Census 2000-LEHD-PIC file.
NOTES: Coefficients from Household Fixed Effects regressions with the inverse hyperbolic sine of earnings between 2008 and 2010 as the dependent variable. In addition to the controls discussed in Tables 3-7, Table 8 allows for two different types of public housing defined by project total population. Large public housing projects represent the top quartile of projects with respect to population between 1997 and 2005.

Table 9. Subsidized Housing Residence and Adult Earnings by Household Race and Gender, Differentiating Lowest-Income Public Housing

	Black Households		Hispanic Households		White Households		All Households	
	Dummy	Dose	Dummy	Dose	Dummy	Dose	Dummy	Dose
Living in a household receiving a housing voucher	0.144*	0.061***	-0.158	0.005	0.005	0.033	0.133**	0.062***
	0.066	0.018	0.101	0.026	0.088	0.026	0.046	0.012
Male living in a household receiving a housing voucher	-0.227***	-0.032*	-0.026	-0.019	-0.002	-0.008	-0.368***	-0.088***
	0.061	0.015	0.082	0.021	0.076	0.021	0.040	0.010
Living in public housing	0.112	0.033	0.087	0.035	-0.144	0.006	0.176*	0.057**
	0.093	0.028	0.144	0.041	0.142	0.050	0.068	0.021
Male living in public housing	0.020	0.026	-0.032	-0.024	0.242	0.051	-0.229***	-0.058***
	0.090	0.023	0.118	0.031	0.140	0.039	0.062	0.016
Living in a lowest-income public housing project	0.153	0.088	0.099	-0.029	0.213	0.056	0.348***	0.144**
	0.125	0.051	0.283	0.124	0.241	0.134	0.103	0.044
Male living in a lowest-income public housing project	-0.174	-0.054	0.097	0.079	0.032	-0.004	-0.438***	-0.143***
	0.134	0.047	0.293	0.114	0.278	0.124	0.109	0.040
	8.619***	8.592***	8.906***	8.875***	8.732***	8.713***	8.724***	8.705***
Constant	0.044	0.045	0.049	0.049	0.037	0.038	0.025	0.025
Observations	162,000	162,000	123,000	123,000	197,000	197,000	520,000	520,000

SOURCE: Authors' tabulations from matched Census 2000-LEHD-PIC file.
NOTES: Coefficients from Household Fixed Effects regressions with the inverse hyperbolic sine of earnings between 2008 and 2010 as the dependent variable. In addition to the controls discussed in Tables 3-7, Table 9 allows for two different types of public housing defined by median household income per project. Lowest-income public housing projects represent the bottom quartile of projects with respect to person-weighted median household income in each year between 1997 and 2005.

Table 10. Subsidized Housing Residence and Adult Earnings Using Actual Participation, Predicted Participation, and Actual Instrumented by Predicted Participation

	Dummy			Dose		
	HFE	HFE Predicted Treatment	HFE IV (Predicted for Actual Treatment)	HFE	HFE Predicted Treatment	HFE IV (Predicted for Actual Treatment)
Living in a household receiving a housing voucher	0.135**	0.168*	0.194*	0.062***	0.045**	0.054*
	0.046	0.066	0.099	0.012	0.017	0.021
Male living in a household receiving a housing voucher	-0.371***	-0.403***	-0.474***	-0.089***	-0.090***	-0.108***
	0.040	0.040	0.047	0.010	0.010	0.012
Living in public housing	0.292***	0.217*	0.278	0.088***	0.043	0.052
	0.059	0.091	0.150	0.018	0.025	0.033
Male living in public housing	-0.360***	-0.383***	-0.460***	-0.086***	-0.088***	-0.105***
	0.053	0.054	0.065	0.014	0.014	0.017

SOURCE: Authors' tabulations from matched Census 2000-LEHD-PIC file.

NOTES: Number of observations = 520,000. Table 10 presents only the coefficients on the two housing subsidy measures and their interactions with a male indicator from six different specifications. The HFE columns repeats the estimates from our main household fixed effects specifications to simplify comparison. The HFE Predicted columns present household fixed effects regressions where we define participation in subsidized housing using the 2000 head of household's movements in and out of subsidized housing, as well as an individual's age, to define program participation, instead of observed participation from the teenager's administrative record. The HFE IV columns use the predicted treatment as an instrument for the teenager's actual treatment in a fixed effects instrumental variables specification. In both cases, the first stage F-statistics are well above conventional thresholds for weak instruments. In all columns the inverse hyperbolic sine of earnings between 2008 and 2010 is the dependent variable.

Appendix A: Major U.S. Subsidized Rental Housing Programs

There is a wide variety of subsidized housing programs. Table A-1 presents the major programs and the number of units subsidized.

Beginning in the 1930s, the U.S. government built public housing projects, and for decades, the program continued to be the primary means of federal assistance for rental housing. The Housing Act of 1949 introduced income limits and "Fair Market Rents" along with subsidies that would incentivize private development of low-cost housing and were further expanded in the late 1960s. In the 1980s, production was drastically reduced as housing assistance became a more decentralized effort, and no federal public housing has been built since 1981. A "regime change" in the mid-1980s additionally introduced even stricter requirements to focus assistance on the poorest households. There were about 1.4 million public housing units in 1990, falling to just under 1.3 million in 2000, and about 1.1 million in 2008. The reduction in these numbers reflects demolition of the worst-performing projects starting in the 1990s. In these cases, under the HOPE VI program, tenants are typically given housing vouchers to find housing elsewhere (Popkin et al. 2004). Today, over 3,000 Public Housing Authorities administer public housing projects, mostly for the very poor and typically neighborhoods that are predominantly low-income.

The Housing Choice Voucher Program (HCVP) provides direct rental assistance to housing tenants through vouchers. The Section 8 New Construction and Substantial Rehabilitation project-based subsidy program assists owners of housing units so that they may charge affordable rents; it accounted for almost 900,000 units in 2000. Note that these households are much smaller and live in smaller dwellings than their counterparts in residing public housing or receiving vouchers. This reflects in part the large share of elderly occupants.

While Section 8 subsidized housing began as project-based housing subsidy in 1974 and at that time was based on new construction, now much of the housing historically referred to as Section 8 housing is found in the tenant-based HCVP program. HCVP has developed more recently and is solely a demand-side, tenant-based subsidy program. Stemming from the ambitious Experimental Housing Allowance Program of the 1970s (see Friedman and Weinberg 1982, 1983) this program brings a different perspective to housing policy by separating itself from new production. Rather than choosing among specific subsidized housing locations, voucher recipients may live in any structurally adequate rental housing in a specified rent and size range, with the Federal subsidy making the unit affordable. Public Housing Authorities may to allocate up to 20 percent of their HCVP funds for project-based vouchers that are tied to specific private housing developments, rather than to the tenant. Tenant vouchers can be used by those wishing to live in Low Income Housing Tax Credit housing (described below) and thus there is the potential for multiple types of subsidies for a given unit. This program

provides anonymity and a choice of locations, although landlord willingness to participate limits its extent. There were about 1.1 million voucher households in 1990, growing dramatically to 1.8 million in 2000, and continuing to grow. Currently, over 30 percent of U.S. subsidized housing is provided by vouchers.

The Low Income Housing Tax Credit (LIHTC) program began with the 1986 Tax Reform Act, and was expanded by 40 percent in 2001. Unlike the "deep subsidies" provided by the other three programs discussed here, LIHTC provides "shallow subsidies" in that no ongoing operating costs are covered by the government. In this program, the U.S. government (through the Internal Revenue Service), provides tax credits to for-profit and non-profit developers to build income-restricted housing. In 1990, there were about 140,000 units, growing to almost 1 million in 2000, and growing further to almost 1.7 million units in 2008. While LIHTC housing has significant income limits for eligibility, this program does not provide housing for the very poor. Another concern raised about the LIHTC program is that it may crowd out nearby private investment in affordable rental housing, as Eriksen and Rosenthal (2010) find.

Table A-1. Total Subsidized Rental Dwelling Units, 1990, 2000, and 2008

	1990	2000	2008
Public Housing	1,404,870	1,282,099	1,155,557
Housing Choice Vouchers (previously Voucher-supported housing--Tenant-Based)	1,137,244	1,817,360	2,209,675
Voucher-supported housing--Moderate Rehabilitation	*	111,392	27,067
Voucher-supported housing--New Construction or Substantial Rehabilitation	822,962	877,830	1,116,250
Federal Housing Authority (FHA) Section 236 Projects	530,625	440,329	225,167
All Other Multifamily Assisted Properties with FHA Insurance or Department of Housing and Urban Development (HUD) Subsidy	*	352,337	329,355
All HUD-subsidized units	**4,515,000**	**4,881,081**	**5,063,071**
Low Income Housing Tax Credit (LIHTC)	139,094	945,347	1,672,239

SOURCE: Olsen (2003) for 1990; HUDUSER, U.S. Department of Housing and Urban Development (HUD), for 2000 and 2008.

* Data not readily available.

Appendix B: Characteristics of Data from HUD's Public and Indian Housing Information Center (PIC)
(SOURCE: Authors' tabulations)

Table B1: Rate of Occupants Having Non-missing Variables in 2000 HUD-PIC File

Variable	Percentage
Master Address File ID	75.0%
Protected Identification Key	97.8%
Date of Birth	99.6%
Gender	99.6%
Race	98.3%
Ethnicity	98.3%
Person type	99.6%

Table B2: Person Type of Occupants in 2000 HUD-PIC File

Person Type	Percentage
Head of Household/ Co-Head of Household/Spouse	44.8%
Youth	47.2%
Other	8.0%

Table B3: Age and Gender of Teenagers Aged 13-18 in 2000 HUD-PIC File

AGE	Male and Female	Male	Female
13	19.0%	19.2%	18.9%
14	17.8%	17.9%	17.7%
15	17.2%	17.3%	17.2%
16	16.2%	16.2%	16.1%
17	15.2%	15.1%	15.3%
18	14.6%	14.3%	14.8%

Table B4: Race and Ethnicity of Housing Occupants in 2000 HUD-PIC File

		Percentage
Race	White	46.6%
	Black	49.2%
	Other	4.2%
Ethnicity	Hispanic	19.8%
	Non-Hispanic	80.2%