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

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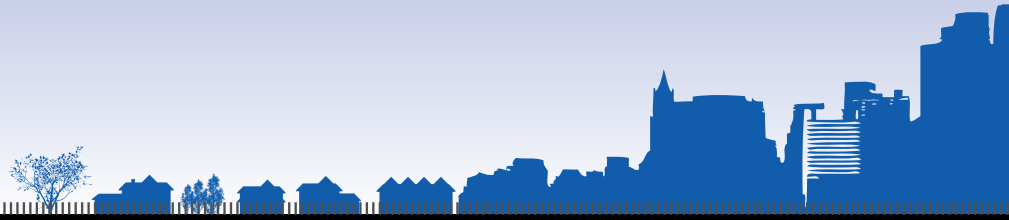

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Childhood Housing and Adult Earnings:
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Abstract

To date, research on the long-term effects of childhood participation in voucher-assisted and public housing has been limited by the lack of data and suitable identification strategies. We create a national-level longitudinal data set that enables us to analyze how children's housing experiences affect adult earnings and incarceration rates. While naive estimates suggest there are substantial negative consequences to childhood participation in voucher-assisted and public housing, this result appears to be driven largely by selection of households 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 naive specifications, household fixed-effects estimates for earnings are universally more positive, and they suggest that there are positive and statistically significant benefits from childhood residence in assisted housing on young adult earnings for nearly all demographic groups. Childhood participation in assisted housing also reduces the likelihood of incarceration across all household race/ethnicity groups. Time spent in voucher-assisted or public housing is especially beneficial for females from non-Hispanic Black households, who experience substantial increases in expected earnings and lower incarceration rates.
(*JEL* H43; I31; I38; J38; J62).

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1. Introduction

Over the last several decades, millions of children have lived in Housing Choice Voucher-supported or public housing, two of the largest subsidized housing programs run by the U.S. Department of Housing and Urban Development (HUD).[1] For these children, residential location, neighborhood amenities, peer composition, and the availability of household resources have been shaped by their households' participation in subsidized housing. Given the mounting body of evidence that early characteristics and experiences can have lasting consequences for a range of adult outcomes (Almond and Mazumder 2005; Black et al. 2007; Almond et al. 2009; Akee et al. 2010), exposure to voucher-supported or public housing early in life could have important implications for adult well-being. Yet, research on the long-term economic effects of assisted housing for resident children has been hampered by data and methodological limitations.

Though public and voucher-assisted housing programs in the United States have existed for some time—the first public housing projects were built in the 1930s and voucher-assisted housing began in the early 1970s—researchers have only recently found convincing strategies to deal with the non-random selection of households into subsidized housing. A series of evaluations of the U.S. Department of Housing and Urban Development's (HUD) Moving to Opportunity (MTO) program and research using administrative records from Chicago together with experimental and quasi-experimental variation in participation provide the most convincing evidence on the impacts of subsidized housing (Jacob 2004; Kling et al. 2005; Jacob and Ludwig 2012; Ludwig et al. 2012; Jacob et al. 2013; Ludwig et al. 2013; Jacob et al. 2015; Chetty et al. 2016; Chyn 2016).[2] With the exception of Chetty et al. (2016), these papers identify, at most, modest differences in short- and long-term such as physical and mental health, criminal behavior, and adult labor market outcomes.[3]

[1] In this paper, we focus on the cohort of children who were 13-18 in 2000 with participation in either public housing or housing choice voucher housing in the 1997-2005 period. This permits us to focus on long term outcomes young adult earnings at age 26 and incarceration in the 2010. In 2000, there were approximately 3 million children under 18 in public housing or housing choice voucher housing.

[2] Earlier work by Currie and Yelowitz (2000), Newman and Harkness (2000,2002), and Carlson et al. (2012a,2012b) estimate the effect of either public housing or voucher-assisted housing on short- and long-term outcomes by employing instrumental variables strategies (Currie and Yelowitz 2000; Newman and Harkness 2000) or propensity score matching (Carlson et al. 2012a, 2012b).

[3] Chetty et al. (2016) link MTO data to administrative tax records on college attendance, earnings, and adult residential locations. They find that voucher recipients who had agreed to move to considerably lower poverty neighborhoods, on average, live in better neighborhoods, are more likely to have attended some college, and have

However, the majority of this research estimates differences between the outcomes of children from households in project-based subsidized housing and the outcomes of children from households that received tenant-based housing vouchers, and thus does not permit inference about how either assisted housing program compares to unsubsidized housing. Notable exceptions are the projects examining the Chicago housing voucher lottery, but these studies use data from just one city and they are only able to contrast voucher-assisted housing with unsubsidized housing.[4]

Furthermore, while the MTO and Chicago Housing Voucher Lottery papers arguably identify internally valid estimates, their limited geographic coverage along with MTO's experiment-specific features—households in the experimental group were required to move to low poverty census tracts and they received counseling support to help them find an apartment and adapt to their new circumstances—suggest that the results may not be generalizable to other contexts.[5] Public and voucher-assisted housing opportunities are not uniform across housing authorities. They vary considerably with respect to structure type, physical proximity to amenities, ease of availability (i.e., waitlist times and area median income thresholds), tightness of the rental housing market, and characteristics of participating households.[6] And while the five metropolitan areas included in the MTO experiment account for an important share of assisted housing participants, the households residing in public or voucher-assisted housing in these cities are observably different from public and voucher-assisted housing participants in the United States as a whole.[7]

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higher earnings as adults, but only if the MTO-driven moves occurred prior to age 13.

[4] See Jacob and Ludwig (2012), Jacob et al. (2015), and Chyn (2016). These papers use administrative data from the city of Chicago along with a housing voucher lottery to compare tenant-based housing vouchers to unsubsidized housing. The first two papers test for effects on short-term outcomes (child mortality and adult labor supply), the third explores how long-term health, schooling, and criminal behavior are affected, and the fourth examines long-term labor market outcomes.

[5] Chyn (2016) raises another generalizability related concern with MTO and the Chicago Housing Voucher Lottery: because all participating households had to apply for the program, the returns they experience may not be representative of the expected return to voucher housing for eligible households. He presents results that indicate that households who elected to participate in the Chicago Housing Voucher Lottery may be negatively selected on the expected return to voucher housing.

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

[7] See columns 1 and 3 of Tables A2 and A3. Relative to the full set of public and voucher-assisted housing participants in the United States, households participating through MTO housing authorities are more likely to be Black non-Hispanic, they face substantially longer waitlist times, and they reside in neighborhoods with higher poverty rates when residing in public housing.

In this paper, we estimate the long-term effects of teenage participation in voucher-supported and public housing on adult earnings and incarceration. To do so, we develop a dataset that combines 2000 and 2010 Census information with comprehensive longitudinal administrative data on housing assistance and earnings. The integrated data permit us to identify nearly the universe of youth aged 13-18 in 2000, and to observe demographic information, household structure, housing assistance, neighborhood characteristics, and parents' earnings over their teenage years. The longitudinal nature of the data then enables us to follow these teenagers into adulthood, where we observe their quarterly labor market earnings and whether they are incarcerated in April of 2010.[8] To our knowledge, this is the first paper to estimate the long-term economic effects of both public and voucher-supported housing for a nationally-representative sample. We are able to contrast the impacts of both programs with each other as well as with unsubsidized housing.

We employ a household fixed-effects specification that exploits variation in children's exposure to voucher-supported and public housing participation within households. This between-siblings approach allows us to isolate the effect of each type of subsidized housing on adult outcomes from observed and unobserved household-level heterogeneity that may affect both program participation decisions and adult outcomes. We focus on earnings and incarceration as indicators of adult well-being, with earnings observed at age 26, when individuals are old enough that it is unlikely they are enrolled full time in higher education.

Our results confirm that selection into subsidized housing matters. Whereas Ordinary Least Squares (OLS) estimates indicate a substantial negative effect of housing subsidies when young on later adult earnings, the household fixed-effects estimates are generally positive and statistically significant. For example, for females we find that each additional year spent in public housing as a teenager generates a $488 (in 2000 dollars) annual premium for young adult earnings.[9] The corresponding estimate for voucher-assisted housing is $468 per year of participation. For males, the corresponding estimates are $508 and $256, respectively. The largest effects on earnings we estimate are for females from non-Hispanic Black households,

[8] We do not include observations from several counties because their local housing authorities participated in HUD's Moving to Work (MTW) program during our study period and hence faced relaxed reporting standards which sometimes display inconsistent data quality. Nonetheless, columns 1 and 2 of Tables A2 and A3 suggest the households participating in subsidized housing in our estimation sample closely resemble the national population residing in subsidized housing across a wide range of characteristics.

[9] Evaluated at the mean for young adults who spent some time in public housing as a teenager.

who earn an extra $704 for each additional year spent in voucher housing, and females from Hispanic households, who earn an extra $704 for each additional year spent in public housing.

We also test whether assisted housing participation while young has any effect on adult incarceration, as measured by group quarters information from the 2010 Census. Incarceration is both a potential mechanism through which assisted housing residence while a child could affect adult earnings, and an indicator of social and behavioral well-being not completely captured by earnings. Previous research using a housing voucher lottery in Chicago found no significant effect of childhood housing voucher receipt on adult criminal outcomes (Jacob et al. 2015). We find incarceration results that closely follow the earnings estimates, both overall and for the gender by race/ethnicity subgroups. For teenagers from non-Hispanic Black households, each additional year of voucher-assisted housing reduces the likelihood of being incarcerated in April 2010 by 0.3 percentage points for males and 0.7 percentage points for females. Additional years spent in public housing have a similar effect on the likelihood of being incarcerated in 2010: a 0.3 percentage point decrease for Black non-Hispanic males and a 0.6 percentage point decrease for Black non-Hispanic females. The point estimates represent a reduction of roughly 10% (for males) and 20% (for females) of the mean incarceration rate for the overall sample. These findings contrast sharply with the OLS estimates which suggest a positive association between subsidized housing participation and incarceration – especially for Black non-Hispanic males.

Our household fixed-effects approach, while addressing important sources of bias inherent in the OLS estimates, still could be affected by time-varying unobserved events related to both adult outcomes and household subsidized housing participation. The rich comprehensive longitudinal earnings data permit us to control for the most likely concern – changes in the economic circumstances of the household that may vary across siblings. We find that our results are robust to controlling for time-varying measures of parents' earnings. The results are also robust to a household fixed-effects instrumental variables strategy that uses the age differences between siblings and the observed subsidized housing participation of the head of household in 2000 to predict public housing and voucher assisted housing participation for each child. We conduct a variety of additional sensitivity checks, all of which support our main results.

The remainder of the paper proceeds as follows. Section 2 describes the subsidized housing programs we study and discusses how they might affect labor market earnings and incarceration. Section 3 presents our research design. Section 4 discusses the data infrastructure

and describes the study sample. Section 5 presents the primary empirical results, Section 6 explores extensions and robustness checks, and Section 7 concludes.

2. Background on Subsidized Housing and Expected Impacts

2.1 Subsidized Housing in the United States[10]

The federal Public Housing program began with the New Deal era enactment of the United States Housing Act of 1937. Initially the program consisted of subsidies for construction provided by the federal government and ongoing management and operations performed by local government public housing agencies. By 1970, there were approximately 1 million units in the public housing program and construction continued slowly thereafter with the program reaching a peak of 1.4 million units in operation in 1994. Because construction subsidies were not sufficient for the maintenance of public housing, the federal government instituted operating subsidies (in 1974) and imposed a rent ceiling—the maximum amount of rent that each family could be charged—which was initially set at 25 percent of family income but later raised to 30 percent of family income (in 1981). Since 1994, participation in public housing has steadily declined, to just under 1.3 million in 2000, and to about 1.1 million in 2013. The reduction in the number of available public housing units reflects, in part, the demolition of severely distressed projects starting in the 1990s, largely under the HOPE VI program. In these cases, some tenants were given housing vouchers to find housing elsewhere, while others tenants received units in different public housing projects (Popkin et al. 2004).

Enacted in 1974, the Housing Choice Voucher (HCV) program provides rental assistance for low-income households through vouchers that prospective tenants take to private sector landlords of approved rental units; the vouchers allow the landlords to receive the full rental price, up to a "Fair Market Rent" (FMR). The HCV subsidy covers the difference between the rental cost of the unit (up to the FMR) and the household's rent contribution (30 percent of its income). Households also have the option of paying a higher portion of their income for rent for units that charge rents above the FMR. The HCV program has grown rapidly over the past two decades. In 1990 there were about 1.1 million voucher households. This figure rose to 1.8 million in 2000, and to nearly 2.4 million in 2013 (over 45 percent of U.S. subsidized housing.

In this paper, we do not consider other HUD rental assistance programs, the most prominent of which, the Section 8 Project-Based Rental Assistance program, also began in 1974

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

and provides an additional 1.2 million units of affordable housing. This program serves a somewhat higher proportion of older households and a lower proportion of households with children.[11] Appendix Table A1 presents the major subsidized housing programs and the number of households and units subsidized through each program over time. In 2000, there were nearly 5 million subsidized households, with 1.3 million in public housing and 1.8 million in voucher housing.

2.2 Unassisted housing

We briefly describe conditions in private rental housing, the alternative for lower earning households eligible for housing subsidies. This is highly relevant because HUD rental assistance is not an entitlement and serves only a fraction of the households that meet the basic income requirements. As a point of comparison, both the public housing and HCV programs use a general rule that households pay 30 percent of their incomes for rent. For unassisted very low income households, HUD estimates that in 2013 at least 7.72 million of these households were paying more than 50 percent of their income in rent (Steffin, et al. 2015). Quigley and Raphael (2004) note that among all renters, the overall share of income paid for rent rose from 19 percent in 1960 to 26 percent in 2000. Over the same time period, the rental share for households in the first income quintile rose from 47 percent to 55 percent, with 79 percent of those households spending more than 30 percent on rent in 2000. One consequence of high housing expenditure and constrained liquidity is a high incidence of eviction and homelessness. Desmond et al. (2015), examining renters in Milwaukee, find higher rates of forced moves for low income households, including formal and informal eviction, landlord foreclosure, and building condemnations. These relocations account for roughly a quarter of all moves and can result in moves to substandard housing and cause further relocations. Burt (2001), examining a nationally representative sample of homeless people who use homeless assistance programs, finds that the most cited reason for homelessness is difficulties with paying rent. Due to the uneven geographical wealth distribution, residential segregation, and limited affordable housing in

[11] 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 with this number growing to almost 2 million units in 2010. While LIHTC housing has significant income limits for eligibility, this program often does not provide housing for the very poor.

higher cost neighborhoods, low earning households are likely to live in high poverty neighborhoods.

2.2 Potential pathways from child housing subsidies to adult outcomes

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

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

Oreopoulos (2003), proposed that public housing participation might impact outcomes through peer or neighborhood effects. If, as argued by Oreopoulos (2003), 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 well-being.

The impact of housing vouchers and public housing participation during childhood may not be the same. Indeed, the perception that public housing might have especially deleterious effects partly motivated the shift in subsidized housing policy in the U.S. to providing housing choice through vouchers. 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 potential adverse consequences of public housing projects (e.g. negative peer effects) might be avoided while the positive income effect for households would still be present.

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

Furthermore, the impacts of assisted housing may not be the same for different demographic groups. For instance, Chetty et al. (2016) show that boys in families in the lowest income quintile, in contrast to the broader population, are less likely to work than girls. To account for the possibility that treatment effects are heterogeneous across demographic groups, in our empirical analysis we allow the effects of assisted housing to vary by gender and race/ethnicity.

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

3. Research Design

3.1 Naive model

Our goal is to identify the causal effect of living in subsidized rental housing as a teenager on adult earnings and incarceration. To do so, we begin by specifying a linear, constant effects regression model for each outcome, *y*, of a child *i* as

$$y_{if} = \alpha + \beta' H_i + \phi' X_{if} + \gamma' Z_{if} + \epsilon_{if}, \qquad (1)$$

where *f* indexes the household including child *i* in the year 2000. The outcome y_{if} is either the inverse hyperbolic sine of total age 26 earnings or an indicator for whether child *i* is incarcerated in 2010. The explanatory variables, H_i are separate measures the number of years spent in public housing and the number of years spent in voucher housing while individual *i* was a teenager. Throughout the text we focus our analysis on this "dose" version of subsidized housing

treatment.[12] The vector X_{if} includes observable child and household control variables, such as race/ethnicity, sex, and age indicators; α is an intercept. The vector Z_{if} contains a set of unobserved characteristics that are related to y_{if}. Lastly, ϵ_{if} is an error term.

Suppose that Z_{if} and its 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 the overall economic circumstances of the household when that child was a teenager (beyond what we can measure based on available earnings data on parents).

Consider first estimating equation (1) using OLS and, thereby, omitting the unobserved characteristics in Z_{if}. The estimated coefficient $\hat{\beta}_{OLS}$ on each type of housing subsidy will reflect 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 the outcome (γ) and the covariance between participation in the type of subsidized housing and the omitted characteristics. For example, if households that possess unobserved unfavorable characteristics (e.g., a lack of savings, poor health, or a social network limited to other poor households) which adversely impact children's subsequent adult outcomes are also more likely to enter public housing, then $\hat{\beta}_{OLS}$ for public housing will be biased downward. Thus, a finding that subsidized housing depresses adult outcomes may be spurious unless the specification controls for these potential biases. To account for the possibility that estimates are contaminated by household-level heterogeneity, we employ an alternative identification strategy.

3.2 Household fixed effects model

To the extent the bias in OLS estimates is solely attributable to the omission of time-invariant heterogeneity at the household level that is correlated with both program participation and adult outcomes, conditioning on household fixed effects will eliminate the bias. To that end, we employ a household fixed-effects regression that explores within-household variation in

[12] In unreported results, we also estimate a dummy version of our models, where the treatments are indicators for whether the individual ever resided in each program while a teenager. The results from these dummy specifications closely mirror the dose versions.

program participation across siblings to identify the impact of having lived in types of subsidized housing while young.

Griliches (1979) provides a summary of the early literature that makes use of sibling fixed effects and points out a number of potential issues. More recent studies include Royer (2009), who used over 3,000 twin pairs and twin fixed effects to estimate the effect of birth weight on long-term outcomes; 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 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 characteristics on children's educational outcomes. In addition, a number of studies have used a between-siblings methodology to study intergenerational economic mobility (e.g., Chetty and Hendren 2015; Levine and Mazumder 2007; Page and Solon 2003; Vartanian and Buck 2005). These studies have the same motivation for employing a household fixed-effects strategy as we do: to abstract from unobserved time invariant family attributes. As emphasized in many of these studies, we recognize that within-household variation in factors such as changes in family economic circumstances across siblings may bias the household fixed effects results. We address these concerns with a number of different identification strategies, as discussed below.

In our study, the household fixed-effects estimates control for time-invariant, unobserved household-level heterogeneity (Z_f). The household fixed-effects (HFE) regression estimates the effect of subsidized housing participation on labor market outcomes and adult incarceration using only variation in housing participation and outcomes across teenagers *within the same household*. In practice, we subtract out the household mean of the dependent and independent variables from each observation within a household.[13] Therefore, HFE only uses observations from household f to help identify $\hat{\beta}_{HFE}$ if there are at least two individuals 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 year-old which 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 $H_j = 3$ and therefore this household would contribute to the identification of $\hat{\beta}_{HFE}$.

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

Fortunately, as we document in the next section, there is ample within-household variation in assisted-housing exposure to help identify the effect of interest.

The HFE model is written as:

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

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 $\gamma_{f,}$ so only a subset of X_{if} remains.

In practice, y_{if} is either the inverse hyperbolic sine of earnings at age 26 or an indicator for whether the individual is incarcerated in 2010.[14] The treatment, H_i, is a vector containing the count of years participating in public housing and the count of years participating in the housing voucher program while aged 13-18, X_i contains an indicator for whether the child is male, a set of age dummies, an interaction between whether the child is male and the set of age dummies, and, when pooling across household races, an interaction between the household race/ethnicity and whether the child is male. We also interact each of the subsidized housing measures with whether the teenager is male to allow for heterogeneous effects by child gender, and we estimate separate regressions for each race/ethnicity to allow all coefficients to vary along this dimension.

3.3 Threats to identification

The HFE estimation provides an unbiased estimate of the effect of youth subsidized housing residence on incarceration and labor market outcomes under less stringent conditions than a typical conditional-on-observables approach. That said, it still requires that we make assumptions about how and when households elect whether to enter assisted housing. First, there must be no unobserved time-varying household-level factors that are correlated with both assisted housing participation while a teenager and adult outcomes. A potential violation of this assumption would be if families enter subsidized housing in response to negative economic shocks. Under the assumption that these negative economic shocks are also harmful to the subsequent adult outcomes of children, $\hat{\beta}_{HFE}$ would be a downward-biased estimate (upward-

[14] We use the inverse hyperbolic sine (IHS) of earnings 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 $\log[y_i + (1 + y_i^2)^{0.5})]$ where y_i is total earnings for individual i (see Burbidge et al. 1998). Annual earnings are deflated to their year 2000 purchasing power equivalent using the U.S. city average annual purchasing power for all urban consumers (CPI-U).

biased in the case of incarceration) of the true effect of subsidized housing.[15] In fact, as HUD requires that participating households be below certain income thresholds, this bias (if present) from unobserved, time-varying heterogeneity is likely to be negative, since harmful household-level shocks both reduce potential adult outcomes for children and increase the chances that households are eligible for housing assistance. To address this possibility, we consider HFE specifications that control for the parents' earnings while the child is between the ages of 13 and 18.[16]

A second potential threat to our household fixed effects identification strategy is unobserved individual-level heterogeneity. For example, if teenagers depart households in assisted housing early for reasons that are correlated with later outcomes (e.g. post-secondary education, juvenile incarceration), these early departures could bias our HFE estimates. We would only observe the reduced time spent in subsidized housing and the subsequently affected adult outcomes, with the direction of the bias depending on the sign of the relationship between the omitted factor and the outcome of interest. In the event of education, this bias is likely to be negative (i.e., we would underestimate the impact of public or voucher-supported housing) while in the case of juvenile incarceration the bias is likely to be positive. Though we have no direct way to observe whether these potential sources of bias are relevant, we employ a household fixed-effects instrumental variables (HFE-IV) strategy intended to discard exactly this type of variation. To do so, we use the observed public and voucher-supported housing participation of the head of household (as defined in the 2000 Census) and the ages of each child in the household to generate predicted public housing and voucher-supported housing participation measures. These predicted participation measures serve as the instruments in the HFE-IV specification. As predicted participation for each program will be unaffected by either the early departure for post-secondary school or the incarceration of a teenaged household member, the HFE-IV estimates will be purged of these potential sources of bias.

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

[16] Aaronson (1998) did something similar as he 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.

Another potentially confounding unobserved characteristic is between-sibling differences in pre-teen exposure to subsidized housing. While data limitations prevent us from controlling for precise measures of the amount of pre-teenage exposure, we confirm that our main results are robust to controlling for whether the household was in subsidized housing as of the beginning of the sample period. [17]

There are of course a number of plausibly exogenous factors that could drive between-sibling variation in exposure to housing subsidy exposure, and it is this type of variation that we want to exploit. The strategies outlined above seek to eliminate the problematic within-household variation while preserving the plausibly exogenous variation. But we also pursue strategies that highlight or isolate plausibly exogenous variation. One such source of between-sibling variation is the waiting periods typical for receipt of a housing subsidy. We use information on waiting times to estimate our results for locations where waiting times are above average to highlight such variation.

4. Data

4.1 Data Sources

This project draws from several sources of confidential microdata at the Census Bureau as well as a number of public use files. From households responding to the 2000 Census of Population and Housing, we select the set of teenagers aged 13 to 18 on April 1, 2000.[18] The frame for the Census is the set of all addresses. A household, which we use in our fixed effects analysis, is the set of persons responding at an address.[19] Each household lists the relationship of all respondents to Person 1[20], and we use these relationships to characterize family structure. The 2000 Census data also provide a geographic location, reported housing tenure (rent or own) and reported demographics (age, sex, race, and ethnicity) for each member of the household.[21]

[17] Concerns of such omitted variable bias is also mitigated in that it is not immediately obvious that we should expect *differences* in teenage exposure to subsidized housing across siblings to be systematically correlated with differences in pre-teenage exposure to subsidized housing, since the expected sign of the correlation largely depends on whether older or younger siblings have more teenage exposures. If so, there is no reason to expect bias in the parameter estimates for teenage exposure even if pre-teenage exposure is omitted from the exposure.

[18] Responses to the Census "short-form" are assembled in the Hundred-Percent Census Edited File (HCEF).

[19] 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.

[20] Throughout the paper and tables, we refer to this Person 1 as the Head of Household and the spouse of Person 1 as the spouse of the head of household.

[21] We chose to use all households in the U.S. rather than the 1-in-6 sample (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 are eliminated by a household fixed effects approach.

The HUD-PIC files provide detailed information on public housing and housing voucher recipients during our study period from 1997 to 2005.[22] 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. HUD-PIC identifies the members within each household and includes fields for when a household applied for housing and when they moved in. The most significant reporting shortfalls are for pre-1997 data, and for housing authorities participating in HUD's Moving to Work (MTW) demonstration (see Abravanel et al. 2004). MTW relaxed reporting requirements for participating housing authorities, resulting in inconsistent data quality for some authorities during our study.

Table 1 presents summary statistics for public housing and voucher-subsidized residents in 2000. The summary statistics are generated using publicly available data derived from the HUD-PIC records. Households in public housing have substantially longer housing tenures, are more likely to have members who are age 62 or older, and are less likely to have children than households in voucher-supported housing.

The Census Bureau produces the Longitudinal Employer-Household Dynamics (LEHD) Infrastructure Files, an employer-employee matched dataset, which it develops in partnership with state data providers.[23] At its core are two administrative records files provided by states on a quarterly 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. The coverage is roughly 96 percent of private non-farm wage and salary employment (Stevens 2007). The data series of most states begin in the 1990s and there are approximately 130 million workers for all states and D.C. in 2010. We also include earnings records for federal workers, based on information from the U.S. Office of Personnel Management.[24] While the longitudinal data support the measurement of job

[22] 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 tenants in projects receiving project-based Section 8 subsidies. HUD-PIC was formerly known as Multifamily Tenant Characteristics System (MTCS).

[23] For a description of the LEHD Infrastructure Files and public statistics, see Abowd et al. (2004).

[24] 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). This study does not make use of these new data. LEHD also excludes earnings from those in the military and those in the U.S. Postal Service. Federal earnings data begin in 2011.

histories, turnover measures, and employment status, this analysis focuses on annual earnings for parents and adult earnings for children.

To complement our analysis of adult earnings, we use information from the 2010 Census to examine adult incarceration. Specifically, we make use of Group Quarters reporting information to observe whether children in our sample were found in an adult correctional facility in April 2010. Raphael (2005) finds a strong relationship between the institutionalization totals from the 2000 Census group quarters data and separate calculations conducted by the U.S. Bureau of Justice Statistics.

We introduce additional geographic data to address time-varying but spatially constant household factors. The LEHD program makes use of 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 use CPR geocodes to characterize a household's time varying residence location. For this analysis, the most precise neighborhood definition we use is a census block group, which has a target population of 600 to 3,000 people (most census tracts have three or four block groups). These geocodes may be linked with spatially constant neighborhood information, such as the poverty rate in 2000 (available from Census 2000's Summary File 3).

4.2 Data Integration

We first use the responses from the 2000 Census to construct a frame of children aged 13 to 18 and their households. Because our focus is on employment outcomes at age 26 and incarceration in 2010, we require that children be at least age 13 in 2000, meaning they will be at least 26 by 2013. The 18 year-olds in 2000 will be 26 in 2008. By age 26, young adults are likely to have entered the labor force even if they attained some higher education.[25] We cap the sample at age 18 and require that in 2000 the child be in a household with adults, who may be their parents, grandparents, or other caregivers (we refer to these adults as parents).[26] Based on the 2000 Census county of residence, we also exclude residents of 119 counties participating in MTW, where a link with the HUD-PIC file would be expected to fail due to non-reporting.

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 and HUD-PIC files have a PIK for approximately 89 percent and

[25] Age 26 earnings are used in some studies of intergenerational economic mobility (Chetty and Hendren 2015).
[26] Specifically, we limit the adults to Person 1 and the spouse of Person 1, should there be a spouse.

98 percent of person-records, respectively. All LEHD records have a PIK value, though a small share of them are not valid. We only retain Census 2000 households with a parent who has a PIK and at least two children aged 13 to 18 who have a PIK, are renters (see below), and have non-missing basic characteristics.[27] To obtain a representative sample from the Census 2000, we reweight the sample.[28] We use PIKs to link both parents and children to HUD-PIC, LEHD earnings records, and the CPR residence information (as well as the 2010 Census, described later).

In addition to using LEHD earnings to construct outcome measures for the youth in our study, we use parents' 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.[29] For each child, we calculate the inverse hyperbolic sine of average parents' earnings (the sum of earnings for the head of household and the spouse of the head of household in each year while the child was aged 13-18).

We take several steps to select a sample of teenagers from households likely to be eligible for housing assistance. 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.[30] We therefore use each household's county of residence in 2000 and household size in 2000 matched to their average parents' LEHD earnings to create a ratio of parents' earnings to AMI; this ratio accounts for the differences in average earnings across metropolitan areas within the U.S. [31] Since local housing authorities typically require that a

[27] We exclude households including more than 15 residents or more than 10 teenagers. For cases where a PIK has been assigned to multiple individuals (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.

[28] From the full sample of households with at least two children aged 13 to 18 in 2000, including records with no PIK, we estimate a logistic regression for whether or not that household also has at least two children with a non-missing 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. We then reweight the records using the inverse of the probability of having a PIK, based on the model estimates. Our results are robust to excluding these weights.

[29] We require that for the time period in which each child is between 13-18 that we observe at least one year of earnings in the LEHD data infrastructure. This restriction eliminates teenagers in states that are not part of the LEHD program (e.g., Massachusetts) in our national sample. Not all states have data back to 1997 so there are some limitations for this control.

[30] 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 FMR. Actual program requirements vary by subsidy type, but generally require residents to earn at or below 80 percent of AMI (low income), with additional requirement 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).

[31] We use average annual total labor income from years where the child is between 13 and 18 years of age. To avoid

household earn less than 50 percent of AMI to be eligible for assistance, we retain only children in households with a parents' earnings-to-AMI measure below 0.5. This provides us with an analysis sample that includes only those widely eligible for the subsidized housing treatment. In addition, based on the housing tenure question from Census 2000, we require that the household be renters at that time. Given that we have no household wealth information, renter status helps to characterize a household as having limited assets and is also consistent with the housing assistance programs we consider, which are rental arrangements.

Of the 2.8 million children in our sample aged 13-18 in the U.S. in 2000, we end up with a final sample size of 1.17 million children in sibling households with parents who earned less than 50 percent of HUD's AMI, 28 percent of whom were in households that resided in subsidized housing at some point between 1997 and 2005. This is the main sample for our analysis of the impact on earnings. Because not all of the children in our main sample are found in the 2010 Census (for example, they were not assigned a PIK in 2010 and therefore cannot be linked), we limit the estimation of effects on incarceration to the sibling groups (as defined by our 2000 households) where we can find all siblings in both 2000 and 2010. This longitudinal restriction reduces the sample size substantially, from 1,172,000 to 673,000.[32]

4.3 Variables

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. We describe children using age on April 1, 2000, gender, race, ethnicity, and household size. We also construct a household-level race/ethnicity variable to allocate households to race/ethnicity subsamples as follows. We decompose the sample into mutually exclusive groups, as follows: we define a household as

dropping observations that do not match to the Composite Person Record (CPR) we use the 2000 census residence county to define AMI. After 2005, HUD defines AMI using American Community Survey data; specified proportions of AMI are used as eligibility and priority criteria.

[32] The fraction of the sample remaining, 0.574, roughly corresponds with what one might expect given the 0.764 share of the 2000 sample being observed in the 2010 Decennial (Table 2). Considering a two-child household, the expected retention rate if retention of each child was independent would be 0.584. We re-weight observations by the inverse probability that a household would be fully accounted for in 2010, where this probability is predicted using household race/ethnicity indicators, the number of people in the household in 2000, the number of teenagers in the household in 2000, an indicator for whether the household rented their home in 2000, an indicator for whether the household lived rent free but did not own their home in 2000, and a set of state fixed effects. We note that we don't have this attrition problem for the main sample since LEHD has virtual universal coverage of employment and earnings outcomes with all workers having a PIK.

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 or African American, 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.

We generate a treatment "dose" variable that counts the years a child resides in voucher or public housing (based on the PIK match to the HUD-PIC annual files from 1997 to 2005).[33] 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 no older than 18.[34] The maximum would be 6 years in HUD housing, which would be for a 13-year-old first residing in subsidized housing in or before 2000. Our goal is to estimate the effect of these treatment measures on labor market and incarceration outcomes.

One possible spurious source of between-sibling variation is simple censoring of the subsidized housing treatment. We define treatment only for individuals between the ages of 13 and 18. However, for sample members who are 17 or 18 years of age in 2000, we are unable to observe their subsidized housing participation at age 13 (or age 14 for individuals aged 18 in 2000) because we use HUD administrative records beginning in 1997 (earlier records are less complete). As a result, it is possible that some of the within-household variation results from this left-censoring of treatment. Therefore, for those children who were 17 or 18 in 2000 and whose household resided in public housing in 1997, we impute housing treatment in the censored years based on the move-in date reported by that household in the HUD-PIC data. All reported results are for the treatment measures including the imputations for 17 and 18 year olds, but we obtain similar estimates without the imputed treatment and when we completely remove 17 and 18 year olds from the sample.

We use the average annual parents' earnings between the ages of 13-18 to control for differences in household economic circumstances across siblings. As we discussed above, changes in household income may be directly associated with moves into and out of subsidized

[33] The PIKs for the head of household and the spouse of the head of household for each child in our sample are also matched to the HUD-PIC file. We use this match, in tandem with the age of each child, to define an alternative subsidized housing participation measure which is discussed in more detail later in the paper.
[34] 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.

housing. We therefore interpret the specifications with controls for parents' earnings as addressing possible unobserved, time-varying characteristics.

We also consider additional within-household variation in some specifications. We use the mean of neighborhood poverty (measured at the census block group level) between the ages of 13-18 as a control variable in some specifications. Controlling for the average poverty rate when each sibling is between 13-18 is designed to capture one of the possible mechanisms for the impact of subsidized housing. 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). When possible, we match the child residence to block group-level tabulations from Census 2000, giving neighborhood characteristics such as the poverty rate.[35]

4.4 Summary Statistics

Our analysis sample closely resembles the characteristics of housing-subsidy eligible households in the United States. As mentioned, the only geographic exclusions (based on the 2000 Census residential location) are the counties containing housing authorities that entered MTW during the study period (and may have incomplete reporting). Appendix Tables A2 and A3 indicate that the housing authorities located in these 3,025 non-MTW counties are representative of public and voucher housing programs overall. For the year 2000, our non-MTW areas cover 91 percent of public housing residents and 93 percent voucher housing residents. As mentioned in the introduction, this coverage is substantially higher than is available for existing experimental studies, such as MTO, where 20 and 12 percent of public housing and voucher housing households reside, respectively. The MTO studies also cover housing authorities that have a higher share of black non-Hispanic residents, a higher share of households with income mostly from welfare programs, higher neighborhood poverty when residing in public housing, and longer wait lists.

Table 2 presents summary statistics for housing subsidy-eligible teenagers from the counties included in our sample.[36] The first column presents summary statistics for the sample used in estimation – youth aged 13-18 in 2000 living with another sibling aged 13-18 in 2000 whose parents earned less that 50 percent of AMI. This sample is subdivided further, into those who lived in households not in subsidized housing anytime during the 1997-2005 study period

[35] We use the county-level average as a fallback for a small share of records.

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

(column 2), and those who lived in households receiving a subsidy at some point during this time period (column 3); the latter are then subdivided further into those who themselves never lived in subsidized housing while of age 13-18 (column 4) and those who did (column 5).

A comparison of columns 2 and 3 shows that there are substantial differences in the outcome variables – those in subsidized housing earned less at age 26, with children from unsubsidized households earning $12,681 and those from subsidized households earning $9,673. In addition, Black non-Hispanics make up a larger portion of the subsidized sample (47 percent versus 22 percent), parents' earnings are lower in the subsidized sample, and a higher portion of the subsidized sample lived in single-parent households (77 percent versus 60 percent). In contrast, the comparison between columns 4 and 5 uncovers only small differences.[37] This similarity suggests, unsurprisingly, that children who never participated in subsidized housing themselves but who come from households where at least one child did participate are much more similar to subsidized housing participants.

To help introduce the within-household variation in subsidized housing participation, Figure 1 displays the distribution of within-household differences; that is, each youth's own subsidized housing participation net of the household mean among all 13 to 18 year olds (in 2000), that we use to identify our regression model. The figure is based on the sample in Table 2, Column 3, but youth 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.[38] The subsample displayed therefore excludes the 41.7 percent of housing voucher participants and 69.3 percent of public housing participants with no between-sibling variation. The distribution is unimodal and symmetric around zero, with an overwhelming majority of teenagers within 2 years of the household mean participation. This latter observation turns out to be of importance in interpreting our results.

Table 2 also presents the fraction of our sample observed in the 2010 Census as well as the incarceration rate among those found in 2010. Those not receiving a housing subsidy had the lowest incarceration rate, just as they had higher adult earnings. In households ever receiving a housing subsidy, 73 percent of children were linked to a 2010 Census response. Of these, 5.4 percent of the children who did not reside in subsidized housing while a teenager were identified

[37] Only 15 percent of children in the ever-subsidized household sample receive no subsidy between the ages 13-18. We focus on estimates of a dose treatment variable, allowing for variation in the number of years of subsidy receipt among those children from households that were ever subsidized.

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

in an adult correctional facility in 2010, while only 4.5 percent of those who did reside in some form of subsidized housing while a teenager were found in an adult correctional facility in 2010. These rates rise to 7.9 and 6.6 percent for children in Black non-Hispanic households, underscoring the high prevalence of adult incarceration for our study sample as well as the potential for housing to explain these differences.

5. Empirical Results

5.1 Samples and specifications

The key question we address is whether living in voucher-supported or public housing affects a youth's labor market experiences and incarceration as an adult. We compare the effects that residence in each of these two HUD housing programs have on earnings at age 26 and incarceration in 2010, relative to unsubsidized housing. Table 3 presents results for teenagers from all households.

In Table 3 the first column presents OLS estimates of the specification described in equation (1). The coefficients capture the relationship between age 26 earnings and the two different types of subsidized housing after controlling for observed covariates. As discussed earlier, these estimates are susceptible to bias as a result of selection based on both time-varying and time-invariant 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 between siblings, these estimates purge the treatment effects of all bias resulting from time-invariant, household-level unobserved characteristics. As discussed above, 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 HFE specifications that, in addition to the controls in column 2, also include a control for the average parents' earnings that each individual experienced between 13 and 18 and its interaction with a male dummy variable (in column 3), a control for average block group percent poverty that each child experienced between 13 and 18 years of age and its interaction with a male dummy (in column 4), and controls for both parents' earnings and block group poverty and their interactions with a male dummy (column 5). 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 with the inclusion of average 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 approximated by block-group percent poverty, is a potential mechanism for the estimated treatment effects.

5.2 All households

The OLS results in column 1 of Table 3 suggest that there is a statistically significant negative relationship between both public and voucher assisted housing participation and age 26 earnings. Further, the point estimates are large and negative for both males and females. Female age 26 earnings decline by 6.2% for each additional year of voucher housing and 8.1% for each additional year of public housing. The corresponding figures for males are a 7.7% decline from voucher housing and a 6.6% decline from public housing.

However, the HFE (between-siblings) results, which control for all household level time-invariant heterogeneity, paint a dramatically different picture. The negative relationships in the OLS specification completely disappear. Both living in public housing and living in a housing voucher-subsidized unit lead to positive and significant effects on age 26 earnings for males and females. The estimates imply that each additional year of voucher housing increases female and male earnings by 4.7 and 2.6 percent, respectively, while an additional year of public housing increases female and male earnings by 4.9 and 5.1 percent, respectively. We are also able to reject the null hypothesis of no difference between the effect of voucher housing for males and the effect of voucher housing for females, with females receiving significantly larger benefits than males.

Since we include individuals with zero earnings (through our use of the inverse hyperbolic sine transformation), assigning a dollar amount to the estimated coefficients depends on where in the distribution the estimated effects are evaluated. In what follows, we use the mean of the total earnings at age 26 for those young adults who lived in households that received some housing subsidy while they were a teenager. That is, we evaluate predictions using the mean $9,716 (in 2000 dollars).[39]

The results indicate that each additional year of voucher participation increases age 26 earnings for females by about $468 and increases age 26 earnings for males by $256. For public housing, the point estimates suggest that each additional year of public housing participation

[39] We use the same figure for all groups when we make these calculations.

increases age 26 earnings for females by $488. For males the corresponding increase is $508. As mentioned above, an overwhelming majority of teenagers fall within 2 years of the household mean participation. Thus appropriate caution must be taken in extrapolating these estimates beyond the within sample range of variation.

The specifications presented in column 3 add controls for parents' earnings while children were between the ages of 13 and 18. For siblings of different ages, this is likely to differ. We find that the sibling who experiences higher average parental income has significantly higher earnings at age 26. However, controlling for changes in the household's economic circumstances yields essentially no change on the impact of either subsidized housing program—the subsidized housing coefficients in Columns 2 and 3 are nearly identical. This suggests that the HFE estimates are unlikely to be biased by time-varying household level shocks, which themselves seem likely to be strongly correlated with total parents' earnings.

Column 4 adds controls for differences in block group poverty across siblings. For females, block group poverty has a negative effect on age 26 earnings, while for males the effect is not statistically different from zero. However, as with parents' earnings, adding block group poverty has little impact on the estimated effect of subsidized housing. Consider what this implies for the potential mechanism linking subsidized housing residence to changes in adult earnings. For voucher-assisted housing in particular, where households are in principle able to move to lower poverty neighborhoods by using their housing choice voucher, the Column 4 results suggest that either voucher households do not typically move to substantially lower poverty neighborhoods, or that these moves to lower poverty neighborhoods do not generate earnings benefits for the children who reside in them for longer durations. Given the dense literature on the long-term effects of growing up in better neighborhoods (Aaronson 1998; Kling et al. 2005; Ludwig et al. 2012; Ludwig et al. 2013; Chetty et al. 2014; Chetty and Hendren 2015; Chetty et al. 2015; Chetty et al. 2016) and related work by Collinson and Ganong (2015) which suggests that inducing voucher recipients to move into better neighborhoods requires that they receive financial incentives for doing so, we are inclined to believe the former explanation: without financial incentives or counseling assistance to help them move to lower poverty neighborhoods, many housing voucher recipients will remain in neighborhoods similar to where they lived prior to receiving their housing voucher

Column 5 of Table 3 adds both of these time-varying within-household controls. Again, the main effects of these longitudinal controls indicate that block group poverty and parents'

earnings are predictive of adult earnings, but their inclusion causes almost no change in the estimated effects of subsidized housing.

5.3 Race/Ethnicity Subsamples

To further explore the results in Table 3, we investigate how the estimates differ by household race/ethnicity. That is, we estimate equation (2) separately for non-Hispanic White households, non-Hispanic Black households, and Hispanic households in Appendix C Tables C1, C2, and C3, respectively.[40] The control variables are unchanged from Table 3, but the indicators for household race/ethnicity by gender are now subsumed by the male dummy variable.

Comparing results across the three subgroups (see Table 4 for a summary of the estimated treatment effects), we find either a positive effect or no effect of public and voucher assisted housing on age 26 earnings for all household race-ethnicities. We do, however, observe some important differences. The positive effects for girls in Table 3 are driven by females in Black non-Hispanic and Hispanic households, whereas boys appear to benefit in all three race/ethnicity samples. The positive effects for non-Hispanic Black females suggest they receive an earnings premium of about $704 ($549) per year in voucher (public) housing. Non-Hispanic Black males also see their adult earnings increase as a result of assisted housing participation, by about $296 and $508 per year of residence in voucher and public housing. Non-Hispanic White males have similar earnings premiums for vouchers and public housing as non-Hispanic Black males. The estimates for White non-Hispanic females are insignificant for both programs. For Hispanic males, the results are also quite similar to those for Black non-Hispanic males and White non-Hispanic males, although the voucher effect is imprecise and therefore statistically indistinguishable from zero. Finally, for Hispanic females, we find effects that are statistically significant and broadly similar in magnitude to those found for Black non-Hispanic females.

For no household race/ethnicity/gender cell are the effects of housing vouchers and public housing statistically significantly different from one another. Given the popular perceptions on subsidized housing programs, this lack of significant differences is perhaps somewhat surprising. It must be noted, however, that public housing programs differ greatly across geographic areas and frequently are quite unlike the oft-cited worst-case scenarios. We find that public housing residence in childhood has substantial positive benefits for age 26 earnings for all but one race/ethnicity/gender group (White non-Hispanic females).

[40] The sample size did not provide robust results for Other race non-Hispanics.

There is, however, some heterogeneity in the effects by gender: females benefit more than males from housing vouchers in the pooled and Black non-Hispanic samples, and males benefit more than females from housing vouchers and public housing in the White non-Hispanic sample.

5.4 Adult incarceration

While we believe adult labor market earnings are a useful proxy for overall adult well-being, we acknowledge that they are an imperfect measure. Though our use of extensive administrative data helps us avoid many of the issues that plague existing research on the long-term effects of subsidized housing, we are unable to explore how a number of other, potentially meaningful measures of adult well-being (physical and mental health, educational attainment, mortality) are affected by teenage participation in public and voucher-supported housing. However, by linking our data to group quarters records from the 2010 Census, we are able to examine how one additional dimension of adult well-being is impacted by childhood participation in subsidized housing: the likelihood of being incarcerated as an adult.[41] Specifically, we re-estimate our models with an outcome indicating whether each child in our sample was listed as residing in a correctional facility for adults on April 1, 2010.

Table 5 presents estimates of the effect of subsidized housing on adult incarceration, using the same controls as our main HFE results in Table 3. For the full sample, we find that an additional year of voucher housing reduces the likelihood of 2010 incarceration for females and males by 0.4 and 0.1 percentage points, respectively. Based on the overall mean adult incarceration rate for children from households with some subsidized housing participation, these correspond to roughly a 9 percent and 2 percent reduction in adult incarceration. Similarly, a year of public housing reduces incarceration for females and males by 0.5 and 0.3 percentage points (11 and 6.5 percent), respectively. In columns 2, 3, and 4, we find similar patterns for each of the household race/ethnicity subsamples. The largest reductions in adult incarceration are found for females in Black non-Hispanic households: 0.7 and 0.6 percentage points for an additional year of voucher and public housing. The effect for males from Black non-Hispanic households is a 0.3 percentage point decrease in adult incarceration for both public and voucher-supported housing.

[41] The survey and administrative data available to this project did not allow for examining the wide array of outcomes explored in the MTO studies. Thus, incarceration is chosen from among the potentially interesting set of health and well-being outcomes included in those studies based on its availability for our national sample.

To help put the Table 5 results in perspective, we also estimated (in unreported results) OLS versions with the same controls as in Table 3 but without household fixed effects. The OLS specifications with controls for observable factors suggest a positive and statistically significant relationship between time spent in subsidized housing as a teenager and incarceration for both public and voucher housing. The effects are especially large for Black, non-Hispanic males. Thus, as with Table 3, the HFE results in Table 5 seem to eliminate substantial bias relative to the OLS estimates. Strikingly, when we eliminate bias stemming from time-invariant household-level heterogeneity, we find that rather than being associated with an increase in incarceration rates, more time in subsidized housing decreases incarceration rates for our sample.

Although the effects of youth subsidized housing participation on adult incarceration presented in Table 5 are independently important, they are also likely to be closely related to the earnings effects shown in Tables 3 and 4.[42] Adult incarceration could lead to decreases in expected adult earnings because of incapacitation effects, recidivism, reduced self-sufficiency, or because formerly incarcerated individuals receive a negative earnings premium from disinvestment in human capital or from having a criminal record (Mueller-Smith 2014). However, the direction of causality is not so obvious. A reduction in potential earnings could also increase the likelihood of incarceration through various behavioral and environmental pathways. Kling et al. (2005) present evidence on how neighborhood poverty affects youth criminal behavior.

While a comprehensive analysis of the relationship between adult incarceration and adult earnings is outside the scope of this paper, we do attempt to gauge how important the observed association between youth subsidized housing and incarceration may be for explaining our earnings results. In Table 6, for each gender by race/ethnicity group, we calculate the share of the observed effect of youth subsidized housing participation on age 26 earnings that can be explained by the incarceration effects presented in Table 5. To obtain these figures, we multiply the effect of a year of additional subsidized housing participation on the likelihood of 2010 incarceration by the average association between 2010 incarceration and age 26 earnings. This latter association is based on household fixed effects specifications with the inverse hyperbolic sine of age 26 earnings as the dependent variable and an indicator for 2010 incarceration as the

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

explanatory variable of interest.[43] We then divide this expected earnings difference by the direct effect of housing on earnings (the estimates shown in Table 4), and multiply the resulting fraction by 100 to get an estimate of the percent of the earnings estimates that could be explained by the observed differences in incarceration.[44] Table 6 does not report results for gender by race/ethnicity groups where the earnings effect was not statistically significant, instead displaying a (0).

Table 6 suggests that reductions in incarceration can account for approximately a quarter of the positive effect of subsidized housing on earnings. This pathway has the greatest explanatory potential for females from Black non-Hispanic households: explaining roughly 60 percent for voucher-supported housing and 29 percent for public housing. Given the discussion in the previous two paragraphs, we remain agnostic about whether the associations between subsidized housing participation and incarceration are a potential cause or a consequence of the main earnings effects, but the alignment and strength of the associations suggests that subsidized housing may have multi-dimensional long-term benefits for children, especially those in Black non-Hispanic households.

6. Robustness

6.1 Potential Sources of Bias

While the household fixed-effects specifications eliminate unobserved time invariant household specific heterogeneity, they are still possibly susceptible to bias from at least two sources: time-varying economic shocks and the early departure of children from households (e.g., juvenile incarceration, early post-secondary school attendance). And while we believe the parents' earnings controls are a good proxy for most of the time-varying economic shocks that could generate bias, we recognize that the lack of movement in the treatment effects when we control for parents' earnings is not entirely sufficient to rule out either of these two possible sources of bias. We therefore undertake several additional robustness checks, intended to further explore whether time-varying economic shocks, the early departure of children from households, or unobserved early childhood differences in subsidized housing participation could be driving our results.

[43] These specifications also include a full set of age by male and household race/ethnicity by male fixed effects.
[44] As with the incarceration outcomes, we restrict this sample to children in households with no attrition from 2000 to 2010. We note that these earnings effects are similar to the main sample estimates in Table 4, but are somewhat attenuated for several subgroups, particularly for boys in voucher housing.

6.2 Wait times and selection into housing

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. Figure 2 indicates that about 12 percent of public housing residents and 29 percent of housing voucher recipients faced wait times of 1 year or more.

In Appendix B Table B1 we present estimates for two subsamples that differ by whether the household resided in a county in 2000 with average subsidized housing wait times of less than or greater than 9 months (approximately the median county-level wait time). The HFE estimates are similar to the results in Table 3 for households in both low and high wait time areas. In no case can we reject the hypotheses that the estimated treatment effects are the same in the two samples. If time-varying economic shocks were driving the results, we would expect the results to be driven by low wait time areas, where households are able to more quickly respond by shifting into subsidized housing. That we never estimate different treatment effects for low and high wait time areas provides further evidence that our main results are not driven by unobserved, time-varying economic shocks.

6.3 Within-household selection into housing[45]

The early departure of teenagers from households could also potentially bias our HFE results. To address these concerns, we implement a HFE instrumental variables specification (HFE IV) that uses the observed participation in public and voucher assisted housing of the head of household from the 2000 Census, along with the birth dates of the teenagers in our sample, to define a predicted measure of teenage participation in both public and voucher assisted housing. We then use these predicted participation measures as instruments for the observed participation

[45] We also estimate the two-stage least squares approach proposed by Newman and Harkness (2000, 2002), with and without household fixed effects. This approach generates instruments for public housing participation using variation in public housing supply after conditioning on a number of county-level characteristics that are predictive of public housing participation. The instruments should reflect public housing supply driven variation in public housing participation at the county-year level. In results available upon request, we find that both the IV and HFE IV specifications suggest a similar pattern of results to our main estimates: positive effects of public housing for both males and females, with males predicted to benefit less per year of participation. However, the estimates are an order of magnitude larger than our main estimates and highly sensitive to the way we specify the earnings outcome.

of the teenager (see Appendix B for a more detailed description of our approach to predicting participation).

Table B2 reports household fixed-effects results using the actual treatment (also found in column 2 of Table 3), using the predicted treatment instead of the actual treatment, and household fixed effects instrumental variables estimates which instrument for the actual treatment with the predicted treatment. In all columns we transform the earnings variable into a distributional measure, giving the earnings percentile of each child in their age 26 year among all children in the sample. We do so to ensure that the outcome is more robust to outliers and less sensitive to small within-household differences in earnings which may be particularly troublesome as the HFE IV estimates use only a fraction of the total within-household variation in subsidized housing. The HFE estimates follow the same pattern as those displayed in Table 3: the effect of public housing and voucher housing on age 26 earnings is positive, with larger effects of voucher housing for females than males and slightly larger effects of public housing for males than females.

Turning to the estimates that use the predicted treatment measures (HFE PRED), there is little movement in the housing voucher estimates relative to the HFE estimates. As expected given the strong first stage (shown at the bottom of Table C2), the HFE IV estimates closely track both the HFE and HFE PRED results. The effect of housing vouchers for females remains large, positive, and statistically significant, while the male interaction is negative but small enough that males are still expected to receive an earnings premium from time spent in voucher housing. While the HFE IV female public housing estimate remains positively signed, it is imprecisely estimated and not significantly different from zero. That said, we are unable to reject that the effect of public housing for females is equal when using HFE (the observed participation measure) and the HFE IV strategy. In fact, The HFE IV estimates are never significantly different from either the HFE estimates which use observed participation or the HFE PRED estimates. The HFE IV estimates therefore confirm that the early departure of children from subsidized households is not driving our main results.[46]

6.4 Pre-teenage exposure

[46] We have also replicated Table C2 using the 2010 incarceration measure as the dependent variable. As with age 26 earnings, the HFE-IV specifications closely follow the pattern of HFE results when 2010 incarceration is the dependent variable.

While some of the MTO studies focus on effects for children who experienced subsidized housing at a young age, due to data limitations this study examines only treatment during the teenage years. Our estimates of the impact of teenage exposure to subsidized housing could potentially be contaminated by the omission of pre-teenage exposure to subsidized housing. While we are unable to directly control for the amount of pre-teenage exposure, we can at least partially test the robustness of our results by controlling for whether the household was in subsidized housing at the start of the sample period. In Appendix B Table B3, we present results that add an interaction between each of the subsidized housing measures and whether the child's household participated in voucher or public housing in 1997, the first year of available data. We find that the effects for children who entered housing as teenagers are similar to our primary results. In addition, the interactions for having received a housing subsidy in 1997 are small in magnitude and statistically insignificant. It therefore appears unlikely that pre-1997 differences in housing participation are biasing our main results.

6.5 Heterogeneity by public housing characteristics

Given that housing subsidy programs are implemented by local housing authorities, the programs can vary considerably across geographic areas. As discussed in the introduction, previous research has identified potential concerns with large and low-income public housing projects, some of which is classified as severely distressed housing. Although we do not have information on the overall quality, upkeep, or crime rates in housing projects, we can examine these hypotheses indirectly by identifying especially large or especially low-income housing projects. In Appendix B Tables B4 and B5, we examine whether public housing projects in the upper quartile of size or the lower quartile of resident income have differential effects on children (we do not consider characteristics of voucher housing, where subsidies are tied to the recipient and may be transferred across locations).

The results provide little evidence that large or low-income projects are worse for individuals who reside in them as teenagers. In the pooled sample, these public housing projects do not have any differential effect on age 26 earnings relative to projects in the bottom three quartiles of size or the top three quartiles of household income for either males or females. In the race/ethnicity sub-samples, there is some weak evidence that especially large public housing projects are less beneficial for Hispanic males and low-income public housing projects are less beneficial for Hispanic females. That said, if

anything, White non-Hispanic and Hispanic males seem to benefit more from particularly low-income public housing projects. Together, we find little evidence to support the theory that especially large or low-income public housing projects have differential effects on the age 26 earnings of individuals who reside in them as teenagers. We note, however, that these measures are only general and indirect characterizations of project quality and that using improved measures of public housing project quality could yield different results.

7. Conclusion

Despite the exposure of millions of children in low-earning households to subsidized rental housing and the potential for these programs to have effects on long-term outcomes, the existing literature lacks a well-identified comparison of public housing, voucher-assisted housing, and private market housing. In this study, we estimate the long-term effects of public housing and voucher-assisted housing as a teenager on adult earnings and incarceration, enabling the direct comparison of both programs to each other and to private market housing.

We create a confidential national data set on housing assistance, household structure, earnings, and incarceration by merging administrative records and the 2000 and 2010 censuses. The data permit us to identify households with children between the ages of 13-18 in the year 2000, to follow those children through a variety of assisted and unassisted rental housing experiences, and to measure their earnings and incarceration up to 13 years later. We are able to observe all 13 to 18 year-olds in the sample at age 26, and therefore focus on the implications for age 26 earnings.

We address potential unobserved heterogeneity and selection bias by estimating household fixed effects models that identify the impact of assisted housing by exploiting only variation within households (between siblings). This between-siblings approach allows us to isolate the effect of each type of subsidized housing on adult outcomes from observed and unobserved household-level differences that may affect both program participation decisions and adult outcomes. We find that the substantial negative effects of subsidized housing in OLS specifications are attributable to the selection of households into assisted housing. After accounting for this household-level selection, we find that subsidized housing participation as a teenager yields substantial positive effects on young adult earnings for both females and males. The point estimates suggest that non-Hispanic Blacks and Hispanics receive the largest benefits from subsidized housing participation as a teenager. For example, non-Hispanic Black females

earn an estimated $704 more annually for each additional year of voucher-supported housing participation and $549 more annually for each additional year of public housing participation. For non-Hispanic Black males, the corresponding estimates are $296 and $508.

We also estimate substantial reductions in the likelihood of adult incarceration, particularly for non-Hispanic Black males and females. Pooling across household race/ethnicities, we find that each additional year of public housing participation reduces 2010 incarceration by 0.5 percentage points for females and 0.2 percentage points for males. The corresponding figures for voucher housing are 0.4 percentage points and 0.1 percentage points, respectively.

We address the possibility that time-variant unobserved heterogeneity or early departure from households may be biasing our household fixed-effects estimates by estimating specifications that include time-varying controls for the economic circumstances of the household across siblings, exploring heterogeneity in the effects by county-level average wait times, and employing a household fixed effects instrumental variables strategy. Our main results remain unchanged by any of these robustness checks.

There remain a number of limitations of our analysis. First, our results apply to just two, albeit two of the largest, HUD subsidized housing programs – public housing and housing vouchers. The project-based housing voucher program, which serves a somewhat higher proportion of elderly households and a lower proportion of families with children, is not considered, nor is the Low Income Housing Tax Credit program. Second, our results may not be representative of all subsidized households (that is, we exclude from our estimates households with only younger children, and those with just one teenager). However, the sub-population for which we are able to estimate treatment effects—households with two or more children born within a 6-year range—represent a large and important fraction of subsidy-eligible households. Third, the results are relevant only for teenagers between the ages 13 and 18. While this is a formative period, other research has suggested that early childhood circumstances may be even more important predictors of long-term outcomes. Future work should investigate whether exposure to subsidized housing during earlier periods of life has long-term implications as well.

Public or voucher assisted housing participation while a teenager has meaningful and beneficial effects on two different adult outcomes: age 26 earnings and incarceration. Though the increased neighborhood choice afforded to participating households suggests there could be higher returns to voucher-assisted housing than public housing, we find no evidence that children

who grow up in voucher housing do better than children who group in public housing as adults. One possibility is that, without financial incentives or intensive counseling, households that enter the housing voucher program are unlikely to actually move to better neighborhoods. Future research should explore how the local rental housing market as well as the physical and social characteristics of public housing projects affect the long-term effects of both voucher-assisted and public housing.

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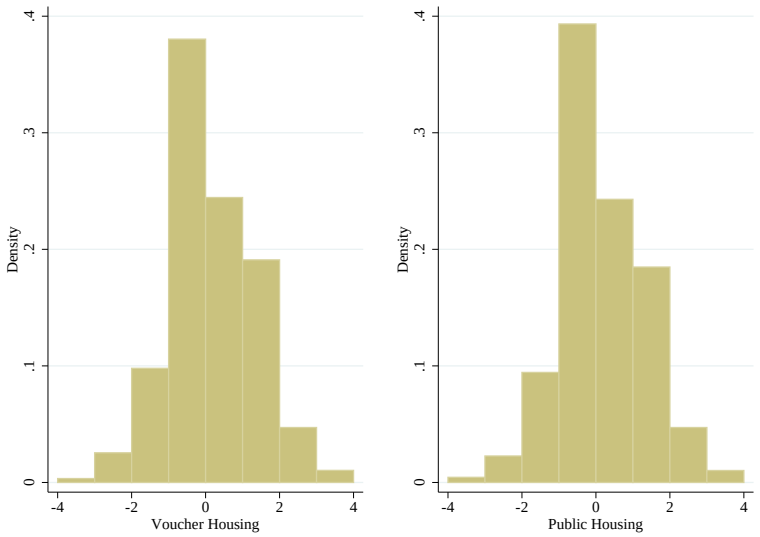
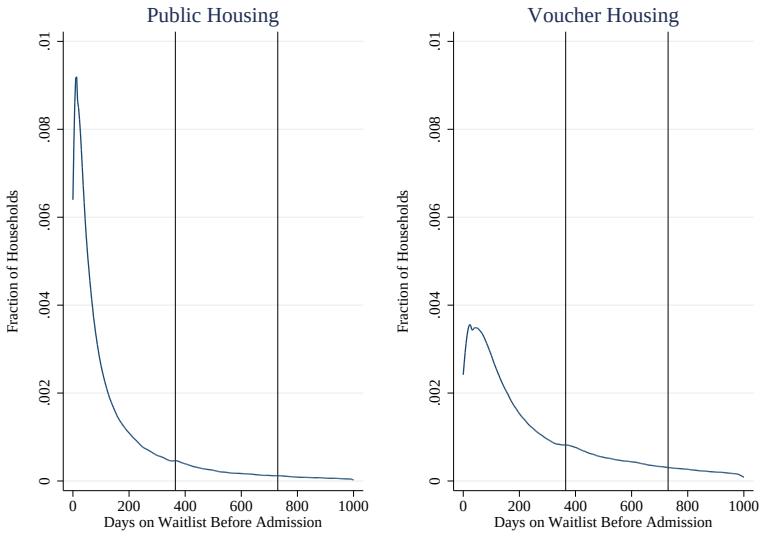
Figure 1: Within-Household Differences in Subsidized Housing Participation



Notes:
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, 0.428 have no within-household variation. Of individuals in households with some public housing participation, 0.699 have no within-household variation. Each bin represents a one year difference in program participation.

Figure 2: Days on a Waitlis Prior to Program Admission in 2000



Notes:
Figure displays the distribution of days spent on the waiting list before admission for households found in both public and voucher housing in the year 2000. The sample is limited to households with non-missing admission and waitlist information who gained admission to their program no earlier than 1995. 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 in Public or Voucher Supported Housing in the Year 2000

	Public Housing (1)	Voucher Supported Housing (2)
Number of people per unit	2.260	2.653
	(0.459)	(0.371)
Monthly tenant payment (rent+utilities)	210.118	227.348
	(54.390)	(59.291)
Household income per year (USD)	10,333.221	10,666.891
	(2,555.877)	(2,021.567)
Months on waiting list	14.844	28.067
	(12.801)	(16.534)
Months since moved in	105.777	52.223
	(57.661)	(23.882)
% of households with most income from welfare	11.017	12.172
	(6.593)	(7.406)
% area median income	25.046	23.020
	(6.041)	(3.571)
% households with children	43.971	60.962
	(15.513)	(11.439)
% minority	68.299	60.235
	(32.858)	(30.963)
% 62 or older	32.277	17.183
	(14.819)	(8.594)
% with 0 or 1 bedrooms	52.263	25.990
	(26.771)	(12.443)
% with 2 bedrooms	22.925	39.516
	(13.845)	(7.348)
% with 3 bedrooms	24.779	34.516
	(15.813)	(11.380)
Total households	1,080,359	1,447,688

Notes:
Statistics computed from HUDUSER public use Picture of Subsidized Housing data in the year 2000.

Table 2: Summary Statistics for Analysis Sample

	13-18 in 2000 with at least one other sibling 13-18				
			In HH that received a housing subsidy		
	Total (1)	In households not receiving any housing subsidy (2)	Total (3)	Never lived in subsidized housing while 13-18 (4)	Lived in subsidized housing while 13-18 (5)
Household size in 2000	5.355	5.331	5.415	5.669	5.369
Age in 2000	15.415	15.456	15.313	15.525	15.275
Male	0.499	0.504	0.487	0.507	0.483
White non-Hispanic household	0.345	0.402	0.203	0.230	0.198
Black non-Hispanic household	0.289	0.216	0.470	0.448	0.474
Hispanic household	0.285	0.295	0.258	0.256	0.258
Other non-Hispanic household	0.082	0.087	0.069	0.065	0.070
Block group % poverty while 13-18	0.113	0.109	0.121	0.120	0.122
Inverse hyperbolic sine parents' earnings	7.889	8.069	7.441	7.589	7.415
Total parents' earnings while 16-18	$36,056	$39,625	$27,189	$29,106	$26,848
Single-parent household	0.647	0.599	0.767	0.743	0.771
Public housing resident while 13-18	0.085	0.000	0.296	0.000	0.348
Housing voucher recipient while 13-18	0.168	0.000	0.585	0.000	0.689
Years in public housing ages 13-18	0.295	0.000	1.026	0.000	1.209
Years in voucher housing ages 13-18	0.593	0.000	2.067	0.000	2.434
Total labor market earnings 2008-2013	$69,571	$74,695	$56,840	$55,801	$57,024
Total labor market earnings age 26	$11,818	$12,681	$9,673	$9,428	$9,716
Total number of years worked 2008-2013	4.240	4.310	4.068	3.998	4.080
Observed in 2010 Census	0.764	0.778	0.730	0.721	0.731
Incarcerated in 2010	0.031	0.025	0.046	0.054	0.045
Observations	1,172,000	840,000	333,000	50,000	282,000

Notes:
Excludes teenagers in owner-occupied housing, those from households earning above 50% area median income in the year and teenagers who lived in counties that participated in HUD's Moving to Work program prior to 2005. Based on authors' tabulations of matched 2000 and 2010 Census, HUD-PIC, and LEHD files. See text for more details. Number of observations rounded to the nearest thousand.

Table 3: The Effect of Teenage Residence in HUD-Subsidized Housing on Age 26 Earnings
All Household Race/Ethnicities

	Dose Treatment (Years Spent in Program)				
	OLS	HFE	HFE EC	HFE BGC	HFE LC
	(1)	(2)	(3)	(4)	(5)
Voucher Housing	-0.062***	0.047***	0.047***	0.047***	0.046***
	(0.004)	(0.010)	(0.010)	(0.010)	(0.010)
Voucher Housing*Male	-0.015**	-0.021***	-0.020**	-0.020**	-0.018**
	(0.006)	(0.008)	(0.008)	(0.008)	(0.008)
Public Housing	-0.081***	0.049***	0.049***	0.054***	0.054***
	(0.006)	(0.013)	(0.013)	(0.013)	(0.013)
Public Housing*Male	0.015*	0.002	0.003	-0.008	-0.006
	(0.008)	(0.011)	(0.011)	(0.011)	(0.011)
IHS Average Parents' Earnings			0.025*		0.023*
			(0.013)		(0.013)
IHS Average Parents' Earnings*Male			0.006**		0.009***
			(0.003)		(0.003)
Average Block Group % Poverty				-1.729***	-1.751***
				(0.323)	(0.324)
Average Block Group % Poverty*Male				1.588***	1.639***
				(0.187)	(0.188)
Demographic Controls	yes	yes	yes	yes	yes
Household Fixed Effects	no	yes	yes	yes	yes

Notes:
Number of observations 1172000 rounded to the nearest thousand. See text for a detailed sample description. The dependent variable in each column is the inverse hyperbolic sine of total earnings between 2011 and 2013. Column 1 presents ordinary least squares (OLS) estimates. All remaining columns present household fixed effects (HFE) estimates. All columns include controls for male by age and male by household race. Column 3 (HFE EC) also includes a control for the inverse hyperbolic sine (IHS) of parents' average annual earnings while a teenager and its interaction with whether the child was male. Column 4 (HFE BGC) includes a control for the average block group percent poverty in the block group of residence between the ages of 13 and 18 and its interaction with a male indicator. Column 5 (HFE LC) includes both the parents' earnings and block group percent poverty controls, along with interactions with the male indicator. In cases where the teenager's block group of residence is unknown, the average block group percent poverty in their county of residence is used. Race and ethnicity is assigned at the household level using information from the 2000 Census. Subsidized housing participation is defined using a count of the number of years each individual ever lived in each type of subsidized housing while a teenager. Robust standard errors clustered at the household are displayed below each point estimate. *** p<0.01, ** p<0.05, * p<0.1. Based on the authors' tabulations from matched Census 2000-LEHD-PIC file.

Table 4: Household Fixed Effects Estimates
By Gender, Subsidy Type, and Race/Ethnicity

	Dose Treatment (Years Spent in Program)		
	Housing Voucher (HV) Treatment Effect	Public Housing (PH) Treatment Effect	Are Subsidy Effects Different? (HV vs. PH)
	(1)	(2)	(3)
All Households			
Females (F)	0.047***	0.049***	No
	(0.010)	(0.013)	
Males (M)	0.026***	0.051***	No
	(0.010)	(0.014)	
Are Subsidy Effects Different? (F vs. M)	Yes***	No	
Non-Hispanic White Households			
Females (F)	0.006	-0.000	No
	(0.020)	(0.035)	
Males (M)	0.034*	0.065*	No
	(0.020)	(0.035)	
Are Subsidy Effects Different? (F vs. M)	Yes*	Yes**	
Non-Hispanic Black Households			
Females (F)	0.070***	0.055***	No
	(0.014)	(0.017)	
Males (M)	0.030**	0.051***	No
	(0.014)	(0.018)	
Are Subsidy Effects Different? (F vs. M)	Yes***	No	
Hispanic Households			
Females (F)	0.045**	0.071***	No
	(0.021)	(0.027)	
Males (M)	0.030	0.051*	No
	(0.021)	(0.028)	
Are Subsidy Effects Different? (F vs. M)	No	No	

Notes:
All columns present household fixed effects estimates of the impact of subsidized housing participation as a teenager on the inverse hyperbolic sine of total age 26 earnings. Teenage participation in each subsidized housing program is defined as the count of the number of years as a teenager spent in the program. Estimates do not control for parents' earnings as a teenager or average block group percent poverty as a teenager but include a male indicator, a full set of age in years by male fixed effects, and, in the All Household Rows, male by household race interactions with non-Hispanic White by Male as the omitted category. See Tables 3 and C1-C3 for observations rounded to the nearest thousand. Robust standard errors clustered at the household are displayed below each point estimate. *** p<0.01, ** p<0.05, * p<0.1. Based on the authors' tabulations from matched Census 2000-LEHD-PIC file.

Table 5: Subsidized Housing and Adult Incarceration

	2010 Incarceration			
	All Households	White Households	Black Households	Hispanic Households
	(1)	(2)	(3)	(4)
Voucher Housing	-0.004***	-0.002**	-0.007***	-0.003***’
	(0.001)	(0.001)	(0.001)	(0.001)
Voucher Housing*Male	0.003***	0.002***	0.004***	0.003***
	(0.000)	(0.001)	(0.001)	(0.001)
Public Housing	-0.005***	-0.005***	-0.006***	-0.003**
	(0.001)	(0.002)	(0.001)	(0.001)
Public*Male	0.002***	0.002*	0.003**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	673,000	291,000	160,000	168,000
Demographic Controls	yes	yes	yes	yes
Household fixed effects	yes	yes	yes	yes

Notes:
Table displays household fixed effects estimates of the effect of teenage subsidized housing participation on 2010 incarceration in an adult correctional facility as observed in the 2010 census. Participation in subsidized housing is captured by a count of the number of years each individual ever resided in public housing or voucher-supported housing while between the ages of 13 and 18. Treatment is observed between 1997 and 2005 and imputed for 1995 and 1996 when possible. To be included in the sample individuals must be in households that did not have any attrition between the 2000 and 2010 census. To adjust for this, we re-weight the observations by the inverse probability that a household would not lose any observations between 2000 and 2010, using household race indicators, the number of people in the household the number of teenagers in the household, whether the household rented in 2000, whether the household lived rent free (but did not own) their household in 2000, and state fixed effects as predictors. Heteroskedasticity-robust standard errors clustered at the household level are shown in parentheses below each point estimate.
* p<.1, ** p<.05, *** p<.01

Table 6: Subsidized Housing, Incarceration, and Adult Earnings

	Subsidized Housing, Incarceration, and Earnings			
	All Households	White Households	Black Households	Hispanic Households
	(1)	(2)	(3)	(4)
(A) Effect of Subsidized Housing on Age 26 Earnings				
Voucher Housing:				
Females	0.041	0.027	0.042	0.047
Males	0.016	0.063	-0.003	0.030
Public Housing:				
Females	0.061	-0.007	0.072	0.083
Males	0.051	0.021	0.064	0.047
(B) Effect of Subsidized Housing on 2010 Incarceration				
Voucher Housing:				
Females	-0.004	-0.002	-0.007	-0.003
Males	-0.001	0.000	-0.003	0.000
Public Housing:				
Females	-0.005	-0.005	-0.006	-0.003
Males	-0.003	-0.003	-0.003	-0.002
(C) Association Between Age 26 Earnings and 2010 Incarceration				
Females	-3.012	-2.499	-3.575	-2.547
Males	-3.757	-3.487	-3.819	-3.827
(D) % Of Earnings Effect Explainable by 2010 Incarceration [(B)(C)]/(A)*				
Voucher Housing:				
Females	29	(0)	60	17
Males	(0)	0	(0)	(0)
Public Housing:				
Females	25	(0)	29	10
Males	22	(0)	17	(0)
Observations	673,000	291,000	160,000	168,000
Demographic Controls	yes	yes	yes	yes
Household fixed effects	yes	yes	yes	yes

Notes:
Table displays the percent of the earnings effects that can potentially be explained by different 2010 incarceration rates. (A) displays the household fixed effects estimates of subsidized housing participation while a teenager on the inverse hyperbolic sine of age 26 earnings, using the sample of individuals, households, and weights from Table 5. (B) replicates the household fixed effects estimates of subsidized housing participation while a teenager on 2010 incarceration from Table 5. (C) shows the difference in the inverse hyperbolic sine of age 26 earnings between individuals who were incarcerated in 2010 and those not incarcerated in 2010, based on household fixed effects specifications that control for age and household race and which are estimated separately by sex. Finally (D) shows the percent of the observed earnings difference that can potentially be attributed to the observed difference in incarceration, calculated as [(B)*(C)]/(A). A (0) indicates that the estimated effect of public or voucher housing on age 26 earnings is insignificantly different from zero. A (-) indicates that the predicted effect of the subsidized housing program on age 26 earnings and the predicted effect of the subsidized housing program on 2010 incarceration are of the same sign, which given the negative relationship between incarceration and age 26 earnings suggests that the incarceration association can not explain any of the observed effect on age 26 earnings.

APPENDIX A. SUMMARY STATISTICS FOR MAJOR US SUBSIDIZED RENTAL HOUSING PROGRAMS

Table A1: Total Subsidized Rental Dwelling Units in 1990, 2000, 2010, and 2013

	1990	**2000**	**2010**	**2013**
Public Housing	1,404,870	1,282,099	1,168,503	1,150,867
Housing Choice Vouchers (previously Voucher-supported housing - Tenant-Based)	1,137,244	1,817,360	2,250,221	2,386,237
Voucher-supported housing - Moderate Rehabilitation	*	111,392	21,579	19,148
Voucher-supported housing - New Construction or Substantial Rehabilitation	822,962	877,830	842,693	840,900
Federal Housing Authority (FHA) Section 236 Projects	530,625	440,329	214,419	126,859
All Other Multifamily Assisted Properties with FHA Insurance or Department of Housing and Urban Development (HUD) Subsidy	*	352,337	597,711	656,456
All HUD-subsidized units	**4,515,000**	**4,881,081**	**5,095,126**	**5,180,467**
Low Income Housing Tax Credit (LIHTC)	139,094	945,347	1,974,163	1,974,163

Notes:
Data from Olsen (2003) for 1990; HUDUSER, U.S. Department of Housing and Urban Development (HUD), for 2000, 2010, and 2013. {*} Data not readily available.

Table A2: Public Housing Participant Characteristics
For All, non-MTW, and MTO Housing Authorities

	All (1)	Non MTW Areas (2)	MTO Areas (3)
Household size	2.260	2.257	2.383
	(0.459)	(0.470)	(0.233)
Tenant monthly contribution	210.118	209.799	270.499
	(54.390)	(55.151)	(25.196)
Income mostly wages	26.911	27.230	30.345
	(9.869)	(10.096)	(6.246)
Income mostly welfare	11.017	10.472	16.264
	(6.593)	(6.565)	(4.056)
Household income (thousands)	10.333	10.404	13.262
	(2.556)	(2.617)	(2.477)
% of area median income	25.046	25.487	27.614
	(6.041)	(5.564)	(6.253)
% single-parent household w/children	31.503	31.584	25.190
	(13.269)	(13.684)	(4.346)
% Black non-Hispanic	49.752	47.456	55.408
	(33.296)	(32.844)	(17.026)
Mean time on waitlist (months)	15.374	14.189	17.587
	(32.560)	(33.708)	(15.939)
% minority in census tract	56.072	54.446	84.877
	(30.163)	(30.693)	(9.401)
% poverty in census tract	28.544	27.966	37.622
	(11.308)	(11.323)	(4.193)
Number of households	1,080,359	977,790	215,789

Notes:
Table displays summary statistics for Public Housing participants in all housing authorities, in non-Moving to Work housing authorities which are retained in the main sample of this paper (non-MTW), and Moving to Opportunity (MTO) housing authorities. For each characteristic, the mean and standard deviation in all housing authorities are shown in Column 1, the mean and standard deviation for non-MTW housing authorities are shown in Column 2, and the mean and standard deviation for MTO housing authorities are shown in column 3. Income mostly wages is the percent of participating households who receive the majority of their household income from wages and Income mostly welfare is the percent of participating households who receive the majority of their income from welfare. Minority includes Black non-Hispanics, Native American non-Hispanics, Asian non-Hispanics, and Hispanics. Summary statistics are computed using housing authority level means weighted by the number of households participating in Public housing through that housing authority. Standard deviations appear in (). Numbers based on Authors' calculations using HUDUSER Picture of Subsidized Households data from the year 2000.

Table A3: Section 8 Voucher-Supported Housing Participant Characteristics
For All, non-MTW, and MTO Housing Authorities

	All (1)	Non MTW Areas (2)	MTO Areas (3)
Household size	2.653	2.636	2.738
	(0.371)	(0.379)	(0.086)
Tenant monthly contribution	227.348	226.804	225.122
	(59.291)	(59.025)	(37.787)
Income mostly wages	34.511	34.509	31.164
	(8.094)	(8.246)	(4.402)
Income mostly welfare	12.172	11.779	21.229
	(7.406)	(7.383)	(6.740)
Household income (thousands)	10.667	10.595	11.239
	(2.022)	(2.016)	(0.970)
% of area median income	23.020	23.196	22.426
	(3.571)	(3.524)	(3.724)
% single-parent household w/Children	44.864	44.858	39.421
	(12.000)	(12.166)	(6.006)
% Black non-Hispanic	41.771	40.440	52.465
	(31.599)	(31.282)	(21.370)
Mean time on waitlist (months)	28.630	27.996	35.430
	(19.380)	(19.223)	(13.574)
% minority in census tract	32.140	29.777	37.014
	(27.415)	(26.018)	(34.509)
% poverty in census tract	13.379	12.806	11.798
	(9.420)	(9.236)	(11.150)
Number of households	1,447,688	1,341,182	170,922

Notes:
Table displays summary statistics for Section 8 Tenant-based voucher housing participants in all housing authorities, in non-Moving to Work housing authorities which are retained in the main sample of this paper (non-MTW), and Moving to Opportunity (MTO) housing authorities. For each characteristic, the mean and standard deviation in all housing authorities are shown in Column 1, the mean and standard deviation for non-MTW housing authorities are shown in Column 2, and the mean and standard deviation for MTO housing authorities are shown in column 3. Income mostly wages is the percent of participating households who receive the majority of their household income from wages and Income mostly welfare is the percent of participating households who receive the majority of their income from welfare. Minority includes Black non-Hispanics, Native American non-Hispanics, Asian non-Hispanics, and Hispanics. Summary statistics are computed using housing authority level means weighted by the number of households participating in the Section 8 tenant-based voucher housing through that housing authority. Standard deviations appear in (). Numbers based on Authors' calculations using HUDUSER Picture of Subsidized Households data from the year 2000.

APPENDIX B. ROBUSTNESS

B.1 Wait times and selection into housing

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 thereafter. In the absence of the wait time, both children would experience the same amount of public housing participation while of age 13-18: 6 years. However, because of the 1-year wait, the 13-year-old will end up spending only 5 years in public housing between the age of 13 and 18 while the 12-year-old will spend 6 years.

Our data confirm that there sometimes exist substantial wait times for both public and voucher-assisted housing. 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 most households face non-trivial waiting periods. As noted in the main text, 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 is lower 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 percent of public housing residents and 29 percent of housing voucher recipients faced wait times of 1 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.

In Table B1 we present estimates for two subsamples that differ by whether the household resided in a county in 2000 with average subsidized housing wait times of less than or greater than 9 months (approximately the median county-level wait time). The HFE estimates are similar to the main results in Table 3 for both low and high wait time areas. In no case can we

reject the hypotheses that the estimated treatment effects are the same in two samples. Had we found that our results were driven by estimates for the low wait time counties, we might be concerned that opportunistic sorting was biasing the HFE results. The finding of similar effects in both areas reinforces the conclusion that time-varying economic shocks are unlikely to generate bias for our results. We believe that together with the lack of movement in the treatment effects when we control for time-varying parents' earnings, the lack of a difference in low and high wait time areas further supports the conclusion that the HFE estimates are likely to represent causal effects.

B.2 Within-household selection into housing

Early departure of teenagers from households could also potentially bias our HFE results in an ambiguous direction. If children depart their home early to attend a post-secondary institution, we would observe them having less participation in subsidized housing while a teenager and, most likely, higher earnings at age 26 and a reduced likelihood of incarceration in 2010. Our estimates of the impact of subsidized housing on earnings would therefore be biased downwards. Conversely, if children depart home early because they are institutionalized in a juvenile (or adult) facility, our HFE estimates would be positively biased.

To address these concerns, we implement a household fixed-effects instrumental variables specification (HFE-IV) that uses the observed participation in public and voucher assisted housing of the head of household from the 2000 Census, along with the birth dates of the teenagers in our sample, to define a predicted measure of teenaged participation in both public and voucher assisted housing. We then use these predicted participation measures as instruments for the observed participation of the teenager. For example, consider the case where some children in public housing depart their homes at age 17 to attend college. Compared to their siblings who don't attend any post-secondary school, they are likely to earn more at age 26. The HFE specifications will wrongly attribute this earnings difference to the reduced time in public housing. However, as long as the heads of household remain in public housing, the predicted public housing measure for these child will not reflect their early departures. Instead, the teenagers will be treated as though they remained in public housing through age eighteen. The same will be true of individuals who are incarcerated while still a teenager. By instrumenting the observed participation using the predicted participation, we are discarding any variation from early child departures from the household.

The predicted participation measures are calculated in the following way. In 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. If either the child is not between the ages of 13 and 18 or the head of household is not observed in subsidized housing, then the predicted participation measure will take on a value of zero for that year. As with our main treatment measures, we sum up the predicted years spent in voucher housing and the predicted years spent in public housing while each individual was between 13 and 18 years of age.[47] We also include interactions between the predicted treatment measures and the male indicator as instruments for the interactions between the observed treatment and the male indicator.

Using this predicted treatment measure, Table B2 reports household fixed-effects results using the actual treatment (also found in column 2 of Table 3), using the predicted treatment instead of the actual treatment, and instrumenting for the actual treatment with the predicted treatment. We transform the earnings variable in all specifications into a distributional measure, giving the earnings percentile of each child in their age 26 year among all children in the sample. We use this transformation so that the outcome is more robust to outliers and is less sensitive to extremely small within-household differences which may be particularly troublesome as the HFE-IV estimates use only a fraction of the total within-household variation in subsidized housing. Given this transformation, the HFE estimates in column two are not directly comparable to the main result in Table 3. Instead, the results provide estimates of the impact on the percentile rank. As the outcome is more robust and less subject to outliers, the results for the actual treatment in column two are of independent interest.

The OLS estimates with the percentile rank dependent variable suggest large, negative, and statistically significant effects of both subsidized housing programs on age 26 earnings for males and females. As with the inverse hyperbolic sine of age 26 earnings however, these associations appear to be driven entirely by the negative selection of households into assisted housing. The HFE estimates in column 2 follow the same pattern as those displayed in Table 3: the effect of public housing and voucher housing on age 26 earnings is positive, with larger

[47] The household-predicted housing subsidy measure could also be thought of as another, noisy measure of child housing subsidy. For an example of how a one noisy measure can be used to instrument for another, see Ashenfelter and Krueger (1994). In that study, IV first-differences estimates turn out to be substantially higher than first-differences estimates with no IV, suggesting that noise was attenuating the baseline result. In any event, the results in Table B2 suggest that measurement error is not importantly affecting our results.

effects of voucher housing for females than males and slightly larger effects of public housing for males than female. The point estimates suggest that each additional year of voucher housing increases age 26 earnings by over a quarter of a percentile for females and roughly a tenth of a percentile for males. The corresponding effects for public housing are .237 of a percentile for females and nearly three tenths of a percentile for males.

Turning to the estimates that use the predicted treatment measures (Column 3), there is little movement in the housing voucher estimates relative to the HFE estimates in column 2. The effect for females remains large, positive, and statistically significant at the 1% level and the male interaction is negative and statistically significant. The female public housing estimate is the only one of the four coefficients that is qualitatively different in columns 2 and 3, though the confidence intervals are sufficiently large that we can't reject that the effect of public housing is equal when using the observed participation measure and the predicted participation measure.

Before discussing the HFE-IV estimates in column 4, we note that the first stage estimates—shown in the lower panel of the table—indicate that the predicted participation measures are highly predictive of observed participation. The first stage coefficients are all significant at the 1% level and of the expected signs. An additional year of predicted participation in subsidized housing is associated with between .73 and .90 additional years of observed participation, depending on the program and gender of the child. Further, the Kleinbergen-Paap Wald statistic (4568.090) is well above the critical values suggested by Stock and Yogo (2005), suggesting that we are not likely to run into any weak instrument-related issues.

The HFE-IV estimates in Column 4 are never significantly different from either the HFE estimates which use observed participation (column 2) or the HFE estimates that use predicted participation (column 3). Qualitatively, the only estimate that differs somewhat from column 2 is the treatment effect for females in public housing which is just over one third of the size, though it remains positive and economically meaningful in size. The voucher housing coefficients suggest that each additional year of public housing increases age 26 earnings by .08 of a percentile for females and .17 of a percentile for males. The HFE-IV estimates therefore confirm that the early departure of children from subsidized households is not driving our main results.

B.3 Heterogeneity by Subsidized Housing Participation in 1997

While some of the MTO studies focus on effects for children who experienced subsidized housing at a young age, due to data limitations, this study examines only treatment during the teenage years. Our estimates of the impact of teenage exposure to subsidized housing could potentially be contaminated by the omission of pre-teenage exposure to subsidized housing. While we are unable to directly control for the amount of pre-teenage exposure, we can at least partially test the robustness of our results by controlling for whether the household was in subsidized housing at the start of the sample period. Households that begin the study period living in unsubsidized housing should be less likely to have pre-1997 differences in subsidized housing participation; instead of requiring one pre-1997 move, households would have to move twice before 1997: one move into subsidized housing and another back out of subsidized housing. In Table B3, we present results that add an interaction between each of the subsidized housing measures and whether the child's household participated in voucher or public housing in 1997, the first year of available data. We find that the effects for children who entered housing as teenagers are similar to our main results. In addition, the interactions for having received a housing subsidy in 1997 are very small and statistically insignificant. It therefore appears unlikely that pre-1997 differences in housing participation are biasing our main results.

B.4 Heterogeneity by Public Housing Characteristics

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 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 HFE specifications for these large public housing measures capture any differential effect that large public housing residence as a teenager has on adult earnings.

Table B4 presents these results. We note that the estimated coefficients on the housing voucher and measures are very similar to those from the more basic HFE specification, while the public housing coefficients imply slightly smaller effects. The results provide little evidence that

large projects are worse overall for individuals who reside in them as teenagers. In the pooled sample, large public housing projects do not have a differential effect on age 26 earnings relative to smaller housing projects for males or for females. Moving to the race/ethnicity specific results, there is weak evidence that especially large public housing projects are less beneficial for Hispanic males.

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. teenagers observing adults who supply more labor could increase 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 income for each project year.[48] 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 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 B5 presents the household fixed effects estimates from these specifications. Again, estimates for voucher housing are similar to the main results, although in this case the resulting effect for general public housing is somewhat larger. Overall, in the first column, we find insignificant interactions for low-income projects for both girls and boys. The by race/ethnicity results are similarly uninformative. The benefit of public housing for Hispanic females disappears for those in low-income public housing projects, but White non-Hispanic males seem to benefit more from low-income public housing. Together, Tables B4 and B5 indicate that the most often described negative characteristics of public housing are not, on average, associated with worse adult outcomes.

[48] 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.

Table B1: Subsidized Housing Residence and Adult Earnings
By Average Time Spent on a Waitlist

	All households	
	Dose Treatment (Years Spent in Program)	
	<=9 Months Wait	>9 Months Wait
	(1)	(2)
Voucher Housing	0.040***	0.053***
	(0.014)	(0.014)
Voucher Housing*Male	-0.012	-0.027**
	(0.011)	(0.011)
Public Housing	0.051***	0.050**
	(0.017)	(0.022)
Public*Male	-0.003	0.005
	(0.014)	(0.017)
Male	0.515***	0.352***
	(0.041)	(0.046)
Observations	611,000	562,000
	P-value: Treatment effects are equal below and above 9 months wait	
Females in Voucher Housing	0.494	
Females in Public Housing	0.961	
Males in Voucher Housing	0.917	
Males in Public Housing	0.803	

Notes:
Each column presents a household fixed effects estimate of HUD subsidized housing participation while a teenager on the inverse hyperbolic sine of total age 26 earnings. Average wait time for public housing and voucher assisted housing in a county is calculated as the weighted housing authority average of the mean days spent on a waitlist prior to admission each program. The weights used for each average are the number of teenagers observed in each housing authority-program type-county cell in the year 2000. The overall average county-level wait time is then the arithmetic mean of the public housing and voucher housing county-level average wait time. Counties are classified as having a wait of above nine months if this average is greater than 273 days and below nine months if it is less than or equal to 273 days. The bottom panel displays p-values from tests of whether the effect is the same in counties with long (>9 months) and short (<=9 months) wait times. Robust standard errors, clustered at the household level, are displayed under each estimate. *** p<0.01, ** p<0.05, * p<0.1.

Table B2: Subsidized Housing Residence and Adult Earnings
Predicting Observed Subsidized Housing Participation using the Head of Household in 2000

	Dose Treatment (Years in Program)			
	OLS	HFE	HFE PRED	HFE IV
	(1)	(2)	(3)	(4)
Voucher Housing	-0.667***	0.271***	0.258***	0.325***
	(0.028)	(0.062)	(0.088)	(0.115)
Voucher Housing*Male	-0.045	-0.173***	-0.195***	-0.224***
	(0.040)	(0.051)	(0.050)	(0.058)
Public Housing	-0.878***	0.237***	0.047	0.081
	(0.038)	(0.085)	(0.131)	(0.179)
Public Housing*Male	0.228***	0.062	0.087	0.094
	(0.055)	(0.069)	(0.072)	(0.080)
First Stage Estimates				
	Public Housing	Male*Public Housing	Voucher Housing	Male*Voucher Housing
Predicted Voucher Housing	-0.011***	-0.002***	0.762***	-0.051***
	(0.001)	(0.001)	(0.003)	(0.002)
Predicted Voucher Housing*Male	0.000	-0.008***	0.001	0.869***
	(0.000)	(0.000)	(0.002)	(0.002)
Predicted Public Housing	0.729***	-0.085***	-0.012***	0.001
	(0.006)	(0.004)	(0.002)	(0.001)
Predicted Public Housing*Male	-0.016***	0.897***	0.002***	-0.014***
	(0.003)	(0.003)	(0.001)	(0.001)
Kleinbergen-Paap rk Wald	4568.090			

Notes:
Number of observations = 1172000 rounded to the nearest thousand. Table presents only the coefficients on the housing subsidy measures and their interactions with a male indicator. In each column the percentile in the earnings distribution when age 26 is the dependent variable. Treatment is defined using a count of the number of years the individual participated in each program between the ages of 13 and 18. The sample is limited to teenagers from All households. See the main text for a more detailed description of the sample. Columns 1 and 2 of the top panel present OLS and HFE estimates. Column 3 defines participation using the observed subsidized housing participation of the head of household and the ages of household members rather than using the observed participation of each individual. Column 4 presents household fixed effects instrumental variables estimates using the predicted treatment based on the head of household participation and the individual's age in 2000 as instruments for observed participation. A full set of male by age fixed effects and male by household race fixed effects are included as controls. The bottom panel presents the first stage estimates corresponding to the four endogenous variables. Kleinbergen-Paap Wald statistic is also shown at the bottom of the table. Robust standard errors clustered at the household are displayed below each point estimate. *** p<0.01, ** p<0.05, * p<0.1.

Table B3: Treatment Effect Heterogeneity by Subsidized Housing Participation in 1997

	All households	
	No Interactions	HH in Subsidized Housing in 1997
	(1)	(2)
Voucher Housing	0.047***	0.045***
	(0.010)	(0.015)
Voucher Housing*Male	-0.021***	-0.017
	(0.008)	(0.013)
Public Housing	0.049***	0.053**
	(0.013)	(0.021)
Public*Male	0.002	0.015
	(0.011)	(0.018)
Voucher Housing*HH in Subsidized Housing in 1997		-0.001
		(0.020)
Voucher Housing*HH in Subsidized Housing in 1997*Male		0.003
		(0.019)
Public Housing*HH in Subsidized Housing in 1997		-0.011
		(0.028)
Public Housing*HH in Subsidized Housing in 1997*Male		-0.011
		(0.025)
Observations	1,172,000	1,172,000

Notes:
Table presents household fixed effects estimates of years of teenage participation in subsidized housing on the inverse hyperbolic sine of total age 26 earnings. Column 1 replicates the dose specification from the main results. See main text for a more detailed description of the sample. Column 2 additionally includes interactions between the number of teenage years spent in each housing program type and whether the teenager's household participated in subsidized housing in the first available year of administrative data (1997). Robust standard errors, clustered at the household level, are displayed under each estimate. *** p<0.01, ** p<0.05, * p<0.1.

Table B4: Subsidized Housing Residence and Adult Earnings
Differentiating Large Public Housing Projects

	Dose Treatment (Years spent in program)			
	All Households	White Households	Black Households	Hispanic Households
	(1)	(2)	(3)	(4)
Voucher Housing	0.047***	0.006	0.070***	0.045**
	(0.010)	(0.020)	(0.014)	(0.021)
Voucher Housing*Male	-0.021***	0.029*	-0.039***	-0.015
	(0.008)	(0.015)	(0.012)	(0.016)
Public Housing	0.041***	0.003	0.050**	0.042
	(0.015)	(0.036)	(0.020)	(0.034)
Public Housing*Male	0.016	0.069**	-0.006	0.030
	(0.013)	(0.029)	(0.017)	(0.027)
Public Housing*Large Public Housing	0.030	-0.033	0.020	0.071
	(0.030)	(0.129)	(0.040)	(0.056)
Public Housing*Large Public Housing*Male	-0.049*	-0.059	0.004	-0.125***
	(0.025)	(0.121)	(0.034)	(0.046)
Observations	1,172,000	464,000	336,000	279,000
Mean of dependent variable	6.981	7.101	6.444	7.352

Notes:
Each column displays a household fixed effects estimate of the impact of teenage participation in subsidized housing on the inverse hyperbolic sine of total age 26 earnings. Each type of subsidized housing participation is defined using a count of the number of years the individual participated in that program while between the ages of 13 and 18. See the main text for a more detailed description of the sample. Large public housing projects are defined as projects in the top quartile of total population over the 1997 to 2005 period. A full set of male by age fixed effects and male by household race fixed effects are included as controls. Robust standard errors clustered at the household are displayed below each point estimate. *** p<0.01, ** p<0.05, * p<0.1.

Table B5: Subsidized Housing Residence and Adult Earnings
Differentiating Low-Income Public Housing Projects

	Dose Treatment (Years spent in program)			
	All Households	White Households	Black Households	Hispanic Households
	(1)	(2)	(3)	(4)
Voucher Housing	0.047***	0.006	0.070***	0.045**
	(0.010)	(0.020)	(0.014)	(0.021)
Voucher Housing*Male	-0.021***	0.029*	-0.039***	-0.015
	(0.008)	(0.015)	(0.012)	(0.016)
Public Housing	0.051***	-0.007	0.045**	0.085***
	(0.015)	(0.038)	(0.020)	(0.029)
Public Housing*Male	-0.005	0.042	0.012	-0.055**
	(0.012)	(0.031)	(0.017)	(0.022)
Public Housing*Low Income Public Housing	-0.010	0.031	0.041	-0.180**
	(0.034)	(0.102)	(0.041)	(0.087)
Public Housing*Low Income Public Housing*Male	0.038	0.180*	-0.066*	0.391***
	(0.031)	(0.103)	(0.037)	(0.079)
Observations	1,172,000	464,000	336,000	279,000
Mean of dependent variable	6.981	7.101	6.444	7.352

Notes:
Each column displays a household fixed effects estimate of the impact of teenage participation in subsidized housing on the inverse hyperbolic sine of total age 26 earnings. Each type of subsidized housing participation is defined using a count of the number of years the individual participated in that program while between the ages of 13 and 18. See the main text for a more detailed description of the sample. Low income public housing projects are defined as projects in the bottom quartile of person-weighted median household income over the 1997 to 2005 period. A full set of male by age fixed effects and male by household race fixed effects are included as controls. Robust standard errors clustered at the household are displayed below each point estimate. *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX C. ADDITIONAL TABLES

Table C1: The Effect of Teenage Residence in HUD-Subsidized Housing on Age 26 Earnings
White non-Hispanic Households Only

	Dose Treatment (Years Spent in Program)				
	OLS	HFE	HFE EC	HFE BGC	HFE LC
	(1)	(2)	(3)	(4)	(5)
Voucher Housing	-0.149***	0.006	0.004	0.008	0.006
	(0.008)	(0.020)	(0.020)	(0.020)	(0.020)
Voucher Housing*Male	0.034***	0.029*	0.032**	0.024	0.028*
	(0.012)	(0.015)	(0.015)	(0.015)	(0.015)
Public Housing	-0.161***	-0.000	-0.001	0.006	0.005
	(0.016)	(0.035)	(0.035)	(0.035)	(0.035)
Public Housing*Male	0.063***	0.065**	0.068**	0.054*	0.057**
	(0.022)	(0.028)	(0.028)	(0.028)	(0.028)
IHS Average Parents' Earnings			0.027		0.026
			(0.020)		(0.020)
IHS Average Parents' Earnings*Male			0.016***		0.018***
			(0.005)		(0.005)
Average Block Group % Poverty				-3.135***	-3.160***
				(0.621)	(0.622)
Average Block Group % Poverty*Male				3.235***	3.302***
				(0.386)	(0.387)
Demographic Controls	yes	yes	yes	yes	yes
Household Fixed Effects	no	yes	yes	yes	yes

Notes:
Number of observations 464000 rounded to the nearest thousand. See text for a detailed sample description. The dependent variable in each column is the inverse hyperbolic sine of total age 26 earnings. Column 1 presents ordinary least squares (OLS) estimates. All remaining columns present household fixed effects (HFE) estimates. All columns include controls for male by age and male by household race. Column 3 (HFE EC) also includes a control for the inverse hyperbolic sine (IHS) of parents' average annual earnings while a teenager and its interaction with whether the child was male. Column 4 (HFE BGC) includes a control for the average block group percent poverty in the block group of residence between the ages of 13 and 18 and its interaction with a male indicator. Column 5 (HFE LC) includes both the parents' earnings and block group percent poverty controls, along with interactions with the male indicator. In cases where the teenager's block group of residence is unknown, the average block group percent poverty in their county of residence is used. Race and ethnicity is assigned at the household level using information from the 2000 Census. Subsidized housing participation is defined using a count of the number of years each individual ever lived in each type of subsidized housing while a teenager. Robust standard errors clustered at the household are displayed below each point estimate. *** p<0.01, ** p<0.05, * p<0.1. Based on the authors' tabulations from matched Census 2000-LEHD-PIC file.

Table C2: The Effect of Teenage Residence in HUD-Subsidized Housing on Age 26 Earnings
Black non-Hispanic Households Only

	Dose Treatment (Years Spent in Program)				
	OLS	HFE	HFE EC	HFE BGC	HFE LC
	(1)	(2)	(3)	(4)	(5)
Voucher Housing	-0.041***	0.070***	0.070***	0.068***	0.068***
	(0.006)	(0.014)	(0.014)	(0.014)	(0.014)
Voucher Housing*Male	-0.032***	-0.039***	-0.041***	-0.036***	-0.037***
	(0.009)	(0.012)	(0.012)	(0.012)	(0.012)
Public Housing	-0.067***	0.055***	0.057***	0.059***	0.059***
	(0.007)	(0.017)	(0.017)	(0.017)	(0.017)
Public Housing*Male	0.005	-0.005	-0.007	-0.011	-0.012
	(0.011)	(0.014)	(0.014)	(0.015)	(0.015)
IHS Average Parents' Earnings			0.064**		0.062**
			(0.026)		(0.026)
IHS Average Parents' Earnings*Male			-0.021***		-0.017***
			(0.006)		(0.006)
Average Block Group % Poverty				-1.627***	-1.550***
				(0.542)	(0.543)
Average Block Group % Poverty*Male				1.496***	1.340***
				(0.320)	(0.325)
Demographic Controls	yes	yes	yes	yes	yes
Household Fixed Effects	no	yes	yes	yes	yes

Notes:
Number of observations 336000 rounded to the nearest thousand. See text for a detailed sample description. The dependent variable in each column is the inverse hyperbolic sine of total age 26 earnings. Column 1 presents ordinary least squares (OLS) estimates. All remaining columns present household fixed effects (HFE) estimates. All columns include controls for male by age and male by household race. Column 3 (HFE EC) also includes a control for the inverse hyperbolic sine (IHS) of parents' average annual earnings while a teenager and its interaction with whether the child was male. Column 4 (HFE BGC) includes a control for the average block group percent poverty in the block group of residence between the ages of 13 and 18 and its interaction with a male indicator. Column 5 (HFE LC) includes both the parents' earnings and block group percent poverty controls, along with interactions with the male indicator. In cases where the teenager's block group of residence is unknown, the average block group percent poverty in their county of residence is used. Race and ethnicity is assigned at the household level using information from the 2000 Census. Subsidized housing participation is defined using a count of the number of years each individual ever lived in each type of subsidized housing while a teenager. Robust standard errors clustered at the household are displayed below each point estimate. *** p<0.01, ** p<0.05, * p<0.1. Based on the authors' tabulations from matched Census 2000-LEHD-PIC file.

Table C3: The Effect of Teenage Residence in HUD-Subsidized Housing on Age 26 Earnings
Hispanic Households Only

	Dose Treatment (Years Spent in Program)				
	OLS	HFE	HFE EC	HFE BGC	HFE LC
	(1)	(2)	(3)	(4)	(5)
Voucher Housing	-0.068***	0.045**	0.042**	0.045**	0.043**
	(0.009)	(0.021)	(0.021)	(0.021)	(0.021)
Voucher Housing*Male	-0.014	-0.015	-0.011	-0.017	-0.012
	(0.012)	(0.016)	(0.016)	(0.016)	(0.016)
Public Housing	-0.085***	0.071***	0.068**	0.076***	0.074***
	(0.011)	(0.027)	(0.027)	(0.028)	(0.028)
Public Housing*Male	0.003	-0.020	-0.015	-0.030	-0.025
	(0.016)	(0.021)	(0.021)	(0.021)	(0.021)
IHS Average Parents' Earnings			0.010		0.010
			(0.025)		(0.025)
IHS Average Parents' Earnings*Male			0.020***		0.021***
			(0.006)		(0.006)
Average Block Group % Poverty				-0.854	-0.884
				(0.575)	(0.575)
Average Block Group % Poverty*Male				0.924***	0.995***
				(0.315)	(0.315)
Demographic Controls	yes	yes	yes	yes	yes
Household Fixed Effects	no	yes	yes	yes	yes

Notes:
Number of observations 279000 rounded to the nearest thousand. See text for a detailed sample description. The dependent variable in each column is the inverse hyperbolic sine of total age 26 earnings. Column 1 presents ordinary least squares (OLS) estimates. All remaining columns present household fixed effects (HFE) estimates. All columns include controls for male by age and male by household race. Column 3 (HFE EC) also includes a control for the inverse hyperbolic sine (IHS) of parents' average annual earnings while a teenager and its interaction with whether the child was male. Column 4 (HFE BGC) includes a control for the average block group percent poverty in the block group of residence between the ages of 13 and 18 and its interaction with a male indicator. Column 5 (HFE LC) includes both the parents' earnings and block group percent poverty controls, along with interactions with the male indicator. In cases where the teenager's block group of residence is unknown, the average block group percent poverty in their county of residence is used. Race and ethnicity is assigned at the household level using information from the 2000 Census. Subsidized housing participation is defined using a count of the number of years each individual ever lived in each type of subsidized housing while a teenager. Robust standard errors clustered at the household are displayed below each point estimate. *** p<0.01, ** p<0.05, * p<0.1. Based on the authors' tabulations from matched Census 2000-LEHD-PIC file.