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

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Abstract

To date, research on the long-term effects of childhood participation in voucher-assisted and public housing has been limited by the lack of appropriate data and suitable identification strategies. We create a new, national-level longitudinal data set on housing assistance and labor market earnings to explore how children's housing affects their later earnings. While naïve estimates suggest there are substantial negative long-term consequences to childhood participation in voucher-assisted and public housing, these relationships appear to be driven largely by negative selection into housing assistance programs. To mitigate this source of bias, we employ household fixed-effects specifications that use only within-household (across-sibling) variation for identification. Compared to naïve specifications, household fixed-effects estimates are more positive for all demographic groups and, for some groups, positive and statistically significant. Black non-Hispanic females, in particular, benefit from time spent in both voucher-assisted and public housing. Exploiting the between sibling variation accounts for unobserved time-invariant family attributes that may influence outcomes but does not address time varying within household factors that may be at work. We use a number of strategies to address these issues and find our results are results are largely robust to these concerns. (*JEL* H43; I31; I38; J38; J62).

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1. Introduction

In the year 2000, nearly 3 million children under the age of eighteen lived in voucher-supported or public housing, the two most popular subsidized housing programs run by the U.S. Department of Housing and Urban Development (HUD).[1] Although large-scale assisted housing programs have been in place for some time, research on the long-term effects for resident children is scarce and hampered by data and methodological limitations.

This paper estimates the effect of participation in voucher-supported and public housing as a teenager on employment and earnings in early adulthood. To do so, we develop a novel dataset that combines 2000 Census information with comprehensive longitudinal administrative data on housing assistance and earnings. The integrated data permit us to track the universe of teenagers in 2000 and to observe their demographic information, household structure, housing assistance, neighborhood characteristics and parents' earnings over their entire teenage years. In turn, the longitudinal nature of our administrative data permits us to follow these teenagers into the early adult years and measure their labor market employment and earnings outcomes. By linking these different data sources together at the person level, we are able to track millions of individuals as they progress through voucher-supported, public, and unassisted housing as children, and into the labor market as adults. The rich and comprehensive nature of our data implies that we quantify the impact of subsidized housing separately for demographic groups defined by gender as well as race and ethnicity.

A core challenge facing all research on subsidized housing is how to overcome the selection problem associated with a household's decision to participate. That is, households that elect to live in public or voucher-assisted housing are systematically different from those that do not. While measurable characteristics may help to describe some of this difference, they are unlikely to completely explain the participation choices made by households. Research that fails to suitably adjust for the unobservable determinants of subsidized housing demand will produce misleading results. A small literature using instrumental variables procedures confirms that there is substantial negative selection into public housing (Currie and Yelowitz 2000; Newman and Harkness 2000, 2002).

We employ a household fixed-effects specification that exploits variation in children's exposure to voucher-supported and public housing participation within households. This allows us to isolate the effect of

[1] Authors' calculations using 2000 HUD administrative data. The statistic reflects all individuals under the age of 18 residing in public housing (including a small number in moderate rehabilitation housing) and Section 8 tenant-based voucher or certificate-assisted housing. In this paper we focus on households receiving tenant-based vouchers and certificates (now Housing Choice Vouchers) and on public housing residents, thus excluding those in Section 8 place-based housing and other HUD-subsidized housing. We do not consider the Low-Income Housing Tax Credit program, which is administered by the U.S. Treasury (see Table 1a).

each type of subsidized housing on labor market outcomes from observed and unobserved household-level heterogeneity that may impact both labor market outcomes and the program participation decision. Our results confirm that selection into subsidized housing matters. Whereas 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 either close to zero or, for some demographic groups, significant and positive. For example, for females we find that each additional year spent in public housing as a teenager generates a $1,035 (in 2000 dollars) annual premium for young adult earnings evaluated at the mean for young adults who spent some time in public housing when a teenager. The corresponding estimate for voucher-assisted housing is $694 per year of participation. For males, the corresponding estimates are either close to or not significantly different from zero. The positive effects are most pronounced for Black non-Hispanic households in which children of both genders enjoy positive returns from assisted housing.

Our household fixed-effects approach, while addressing important sources of bias inherent in the OLS estimates, still may be subject to time-varying unobserved events or characteristics related to both adult earnings and household subsidized housing participation. To address this possibility, we pursue several strategies. We think the most obvious concern is that differences between siblings in subsidized housing participation reflect changes in parents' economic circumstances. Our longitudinal data permits tracking of parents' labor market earnings and we find our results are robust to adding such longitudinally based controls. Another possible problematic source of between siblings variation is due to changes in household structure (e.g., dissolution of the family). We use an IV strategy that abstracts from such changes and again we find our main results are robust. We also show that, consistent with Jacob et al. (2015), there are considerable wait time differences across locations for receiving housing assistance. Wait time differences are plausibly exogenous sources of between-sibling variation in exposure to housing assistance. We show that our findings are robust to restricting our sample to those locations with the longest wait times, where the between sibling variation is more likely to be driven by wait times.

We also use an alternative IV approach to exploit variation across siblings in time spent in public housing resulting from local public housing supply shocks. This method is motivated by the approach of Newman and Harkness (2000; 2002). Combining the household fixed effects strategy with this IV method, we find similar point estimates to our main results although an increase in standard errors makes these estimates imprecise.

A unique feature of our study is that we estimate the effects of childhood participation in both voucher-assisted *and* public housing on long-term labor market earnings in an integrated analysis. This enables us to directly compare how time spent in private market housing compares to time spent in each subsidized housing

program, as well as to compare how time spent in public housing compares to time spent in voucher-assisted housing.

There are a number of studies that estimate the impact of childhood participation in subsidized housing on a variety of outcomes. To put our results into perspective we think it is helpful to distinguish between these studies on two different dimensions. First, many studies focus on short term outcomes like school achievement (e.g., Currie and Yelowitz 2000; Jacob 2004) rather than longer term outcomes like labor market earnings (Newman and Harkness 2002 or health, crime and school achievement (Jacob et al. 2015). Our focus is on longer term outcomes so it is the latter studies that are especially relevant for purposes of comparison. Second, there is a burgeoning literature estimating neighborhood effects through changes in access to housing vouchers or increases in housing voucher generosity, particularly from evaluations of HUD's Moving to Opportunity (MTO) program (Ludwig et al. 2012; Ludwig et al. 2013; Chetty et al. 2015; Collinson and Ganong 2015). With the exception of Collinson and Ganong (2015), these papers compare outcomes for children from families in low-income public housing projects—some of whom were provided housing vouchers and incentivized or counseled to move to lower-poverty neighborhoods—to outcomes for children from families that initially remain in the original projects. The Chetty et al. (2015) study is especially relevant since it provides estimates of the long term impact on earnings for children in the MTO program. This and the other MTO studies are about the impact of the type of housing subsidy received rather than about the impact of receiving any housing subsidy. Our study provides estimates of the impact of receiving either of the two main types of housing assistance. We discuss how our results fit into these alternative strands of the literature below.

The remainder of the paper proceeds as follows. Section 2 describes the subsidized housing programs we study and discusses why they could lead to variation in labor market earnings. Section 3 presents our research design. Section 4 discusses the data infrastructure. Section 5 describes the study sample and Section 6 presents the benchmark household fixed effect empirical results. Section 7 presents extensions and results from refined identification strategies intended to address endogenous changes within households that may be confounding our household fixed effect results. Section 8 concludes.

2. Background on subsidized housing and evaluations

2.1 Subsidized Housing in the United States

There are a number of different subsidized housing programs in the United States. Table A1 presents the major programs and the number of households and units subsidized. In 2000, there were nearly 5 million subsidized households, with 1.3 million in public housing and 1.8 million in voucher housing. (See Appendix A1 for a discussion of the other programs.)

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

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

While Section 8 subsidized housing began as a project-based housing subsidy in 1974, now much of the housing historically referred to as Section 8 housing is found in the demand-side tenant-based HV program. Created after HUD's ambitious Experimental Housing Allowance Program of the 1970s (see Friedman and Weinberg 1982, 1983), the program was novel in that it separated the idea of housing subsidies from the new production of housing units. Rather than choosing among specific subsidized public housing locations, voucher recipients could choose to live in any structurally adequate rental housing in a specified rent and size range, with the Federal subsidy making the unit affordable. Public Housing Authorities were also allowed to allocate up to 20 percent of their HV funds for project-based vouchers that are tied to specific private housing developments, rather than to the tenant. The HV program provides anonymity and a choice of locations, although landlord willingness to participate limits its extent. There were about 1.1 million voucher households in 1990, with this figure growing dramatically to 1.8 million in 2000 and 2.2 million in 2008, accounting for over 30 percent of U.S. subsidized housing.

2.2 Potential pathways from child housing subsidies to later adult outcomes

There are a number of channels through which childhood participation in subsidized housing might affect their later adult outcomes. Both voucher and public housing provide a positive income effect for households. By expanding the budget 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 should generate higher labor market earnings, and suggests that assisted housing residence in childhood would improve adult labor market outcomes.

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. Oreopoulos (2003) proposed that subsidized housing participation might impact outcomes through peer or neighborhood effects. If, as argued by Oreopoulos (2003), available subsidized housing units are located in worse neighborhoods—i.e., neighborhoods with higher crime rates and lower quality schools—than participants' counterfactual housing options, then public and voucher-assisted housing could have negative neighborhood and peer effects and therefore decrease adult earnings. *Ex ante*, the sign of any neighborhood or peer effect, as well as the overall impact of subsidized housing participation, is unclear.

The impacts of housing vouchers and of public housing participation during childhood may not be the same. Indeed, the perception that public housing might have deleterious effects motivated the shift in subsidized housing policy in the U.S. to providing housing choice through vouchers.[2] 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., 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 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 HV households. Public housing participants, with Public Housing Authorities as their landlords, do not face this type of discrimination.

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.

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[2] The purpose of a tenant-based voucher is to allow recipients to search for housing in the private rental market.

3. Research Design

Our goal is to identify the causal effect of living in subsidized rental housing as a teenager on eventual labor market success. To do so, we begin by specifying a linear, constant effects regression model for the inverse hyperbolic sine of total earnings from 2008 to 2010, *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.[3] The outcome thus measures the child's earnings as an adult. The variables of interest, H_i, are separate measures of teenage participation in subsidized housing (public housing or housing voucher) between the ages of 13 and 18. We include only one variable here for expositional convenience. The vector X_{if} includes observable child and household control variables, such as demographic characteristics. 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.

Further, 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 economic circumstances of the household when that child was a teenager.

Consider first estimating equation (1) using Ordinary Least Squares (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 earnings (γ) and the covariance between participation in the type of subsidized housing and the omitted characteristics. For example, if households that possess unobserved characteristics which adversely impact children's subsequent labor market 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 child 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.

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3 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 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 2000 purchasing power equivalent using the U.S. city average annual purchasing power for all urban consumers.

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 labor market outcomes, conditioning on household fixed effects would eliminate the bias. To that end, we specify a household fixed-effects regression that explores within-household variation in program participation across siblings to identify the impact of 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 (1) Royer (2009) who used over 3,000 twin pairs and twin fixed effects to estimate the effect of birth weight on long-term outcomes; (2) Currie and Walker (2011), who used mother fixed effects to estimate the impact of the introduction of EZ-Pass in New Jersey and Pennsylvania on infant health outcomes; and (3) Currie et al. (2010), who employed sibling fixed effects to identify the relationship between early childhood health problems and outcomes in early adulthood. An especially relevant siblings study is Aaronson (1998), who estimated the effect of neighborhood 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; Dahl and Deleire 2008; Levine and Mazumder 2007; Mazumder 2014; Page and Solon 2003; Vartanian and Buck 2005). These studies have the same motivation as in the current study to use between-sibling variation to abstract from unobserved time invariant family attributes. As emphasized in 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.

In our study, the household fixed-effects estimates control for time-constant, unobserved household-level heterogeneity (Z_f). The household fixed-effects (HFE) regression estimates the effect of subsidized housing participation on labor market outcomes using only variation in housing participation and outcomes across teenagers *within the same household*. In practice, we subtract out the household mean of the dependent and independent variables from each observation within a household.[4] Therefore, HFE only uses observations from household f to help identify $\hat{\beta}_{HFE}$ if there are at least two 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 who 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 $H_j = 1$ and therefore this household would contribute to the identification of $\hat{\beta}_{HFE}$. 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.

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

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, H_i is a vector containing measures of participation in both public housing and housing voucher programs while aged 13-18, X_i contains an indicator for whether the child is male, a set of age dummies, and an interaction between whether the child is male and the set of age dummies. 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. We estimate both a "dummy" version where the "treatment" *H* is a set of binary indicator variables for whether an individual resided in each type of subsidized housing between the ages 13 and 18 and a "dose" version where treatment is the number of years an individual resided in each type of subsidized housing between ages 13 and 18. That is, we consider the treatment "ever resided" and the treatment "years resided."

The HFE estimation provides an unbiased estimate of the effect of youth subsidized housing residence on labor market outcomes under less stringent conditions than a typical conditional-on-observables approach (including propensity-score matching approaches, in which identification also hinges on controlling for all relevant observables that determine selection and impact outcomes). There are, however, characteristics contained in the child-specific factor, Z_i, that could lead to bias in $\hat{\beta}_{HFE}$. Any household-specific and *time-varying* characteristic that is correlated with both subsidized housing residence and labor market outcomes will lead to bias. For example, if families enter subsidized housing in response to negative economic shocks and under the assumption that these are also harmful to the subsequent labor market outcomes of the child, $\hat{\beta}_{HFE}$ would be a downward-biased estimate of the true effect.[5] In fact, HUD requires that program households be below a certain income threshold. This suggests that if any bias from unobserved, time-changing heterogeneity is present, this bias is likely to be negative. To address this possibility, we also consider HFE specifications where we control for the parents' earnings while the child is between 13 and 18. This variable will capture differences in the household earnings across siblings who have different subsidized housing experiences.[6]

[5] 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.

[6] Aaronson (1998) 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 change in family circumstances that may be driving between-sibling variation is changes in household structure (e.g., divorce and dissolution of the family). In one of the robustness checks that follows, we use an IV procedure that abstracts from changes in household structure. This IV procedure also mitigates any spurious between-variation driven by measurement error in the HUD administrative data in terms of tracking the members of the household on a year-by-year basis.

Another potentially confounding unobserved characteristic is any within-household, child-level heterogeneity that is correlated with both labor market outcomes and subsidized housing participation. In this case, the direction of the potential bias is less clear. In particular, between-sibling differences in pre-teen exposure to subsidized housing represents an example of between-sibling heterogeneity that may be correlated both with subsequent labor market outcomes and with the between-sibling differences in the exposure to subsidized housing as teenagers. 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. [7]

There are of course plausibly exogenous factors that are driving between-sibling variation in exposure to housing subsidy exposure, and it is this type of variation that we want to exploit in our identification. 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 plausibly exogenous source of between-sibling variation is the waiting periods typical for receipt of a housing subsidy. In what follows, we use information on waiting times to estimate our results for locations where waiting times are above average to highlight such variation. Another plausibly exogenous source of between-sibling variation is local subsidized housing supply shocks. Such supply shocks are inherently more plausible for public housing especially given the demolitions of public housing that occurred over our sample period on an uneven basis across the country. We use an IV procedure with local public housing supply instruments as part of our strategy for identification.

4. Data

4.1 Siblings sample frame

The core data set brings together person- and household-level records from the 2000 Census and different administrative files. To begin, we use the responses from the 2000 Census to construct a frame of over

[7] Concerns of such omitted variable bias is also mitigated by 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.

1.8 million children aged 13-18 and their households.[8] Because our focus is on employment outcomes from 2008 to 2010, we require that children be at least age 13 in 2000, meaning they will be at least 21 by 2008 and may be entering the labor force even if they attained some higher education.[9] We cap the sample at age 18 and require that in 2000 the child be in a household with their parent(s) (or grandparents or other adults, as appropriate). Including older individuals would undermine the focus of the paper, and our identification approach relies on the assumption of parents making housing decisions for children.

Because our aim is to estimate the effect of childhood environmental factors on later life outcomes, we derive most of our demographic characteristics from the base year 2000 Census short form responses, when subjects are still children.[10] We retain responses for one or two parents or other adults as well as all children between the ages of 13 and 18 and classify all respondents from the same address as a household.[11] To help define the population of interest, subpopulations and key variables we use time-invariant explanatory variables relating to the child such as date of birth, gender, race, and ethnicity, and characteristics of the household in the base year such as housing tenure, number of people, and number of children.[12] We also construct a household-level race/ethnicity variable to allocate households to race/ethnicity subsamples. Specifically, we define a household as Hispanic if any member reports being Hispanic, Black non-Hispanic (Black) if no member reports being Hispanic and at least one member reports being Black 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.

Youth in the Census 2000 are then matched to administrative records on housing subsidies from the Department of Housing and Urban Development's HUD-PIC[13] file, annual place of residence from the Composite Person Record (CPR), and subsequent earnings from the Longitudinal Employer-Household Dynamics (LEHD) Infrastructure Files using a unique person identifier.[14] Person-level record matching is done

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

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

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

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

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

[13] 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.

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

by way of a Protected Identification Key (PIK), which is assigned to survey and administrative records based on personally identifying information. The 2000 Census has PIKs for over 89 percent of the person-records, while almost 98 percent of HUD records have a PIK, and all LEHD records have a PIK. We only retain households with a parent who has a PIK and at least two children aged 13 to 18 that have a PIK and non-missing basic characteristics.[15] To obtain a representative sample from the Census 2000, we reweight the sample as follows: 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.[16] We then reweight the records using the inverse of the probability of having a PIK, based on the model.

4.2 Housing subsidy

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

The person-level file used at the Census Bureau includes demographic and housing unit information, but this study primarily makes use of occupancy as an indicator of housing treatment.[17] We match PIKs from the Census 2000 decennial file to the HUD-PIC file and identify whether a child resided in public or voucher housing in each year from 1997 to 2005. We consider a child to be a HUD-subsidized resident in a particular year if their PIK appears in the HUD administrative data *and* if that individual is still under the age of 18.[18] Thus, the maximum number of years a child could reside in HUD housing is 6 years before turning 18, which

[15] 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.
[16] Characteristics highly associated with not having a PIK include race, ethnicity, age, and sex.
[17] Table B-1 in Appendix B presents the percentage of records with non-missing data in the PIC administrative file. Other tables there present some characteristics of the PIC sample.
[18] 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.

could occur for a 13-year-old first residing in subsidized housing in or before 2000. An 18-year-old in 2000 could only reside in HUD-subsidized housing for at most 4 years (beginning in 1997).

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

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.

4.3 Labor market outcomes

LEHD, a partnership between the Census Bureau and all 50 states and the District of Columbia, produces public use data tabulations that are widely used by state and local governments. 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 (QCEW). The coverage is roughly 96 percent of private non-farm wage and salary employment (Stevens 2007).[19]

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

4.4 *Other factors varying within households*

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

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 identify a residence census block for each child from 1999-2005 where available (approximately 10 percent of children are missing a CPR residence in each year). Where possible, we match the child residence to block group-level tabulations from Census 2000, giving neighborhood characteristics such as the poverty rate.

In addition to using LEHD earnings to construct outcome measures for the 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.[20] 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.[21] 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, or the sum of all adults in the household) while the child was of ages 13-18. Additionally, we use each household's residence county in 2000 and household size in 2000 matched to their average parents' LEHD earnings to create a ratio of parents' earnings to AMI in order to account for the differences in average earnings across regions, which can vary considerably for metropolitan areas within the U.S. Since local housing authorities often require that a household earn less than 50 percent of AMI to be eligible for assistance, we retain only children in households with 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. As with the labor market outcomes, some households may appear to have lower incomes because they do not work in UI-covered employment. In future work, we will assess the significance of such omissions for our sample composition.

We employ both the composite of neighborhood (at the Census block group-level) poverty and average annual parents' earnings between the ages of 13-18 as control variables in some specifications. As we discussed above, changes in household income may be directly associated with moves into and out of subsidized housing. Controlling for the household income during the period each sibling is between 13-18 acts to control for such concerns. Controlling for changes in the poverty rate when each sibling is between 13-18 is designed to capture one of the mechanisms for the impact of subsidized housing. As such, we interpret adding each of these two

[20] 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. In future drafts, we plan to include a robustness check to restrict the sample to teenagers with coverage in LEHD back to 1997.

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

longitudinal controls somewhat differently. We interpret specifications with controls for parents' earnings as one of our strategies for addressing possible unobserved, time-varying characteristics, and those with controls for block group percent poverty as a test of one potential causal mechanism.

Also, as discussed above, we also use methods to abstract from changes in household structure, methods designed to highlight waiting time variation and local supply instruments. We discuss the details of these methods below.

5. The Sample: Basic Facts

In sum, to be included in the estimation sample, we require that children have been between 13 and 18 years of age in the year 2000, have non-missing values for age, gender, ethnicity, treatment status, and residential location, have successfully been assigned a unique PIK based on the 2000 Census, and be from the same 2000 renter household as at least one other child aged 13-18 in 2000.

Because not all households are eligible for subsidized housing, we additionally limit our sample to youth from households more likely to qualify for housing assistance, with parents or other adult caregivers who earn less than 50 percent of HUD-specified Area Median Income (AMI) on average while the youth is a teenager.[22] Finally, we exclude households who lived in the 119 counties participating in HUD's Moving to Work (MTW) demonstration (see Abravanel et al. 2004). Local housing authorities participating in the demonstration were permitted to stop reporting administrative data to HUD on participants.

Of the 2.8 million children aged 13-18 in the U.S. in 2000, we end up with a final sample size of 1.12 million children in sibling households, 30 percent of whom were in households that resided in subsidized housing at some point between 1997 and 2005. Table 2 presents summary statistics for this sample.[23] The first column presents summary statistics for the sample used in estimation – youth aged 13-18 living with another sibling aged 13-18. This sample is subdivided further, into those who lived in households not in subsidized housing anytime during the 1997-2005 study period (column 2), and those who lived in households receiving a subsidy (column 3); the latter are then subdivided further, into those who never lived in subsidized housing while of age 13-18 (column 4), and those who did (column 5). The comparison between columns 4 and 5 provides the raw differences analog to our main empirical results for the dummy treatment effect.

There are a few minor differences between the estimation sample (column 1) and the full sample of youth aged 13-18; that is, the sample including cases in which there is only one relevant child in the household

[22] We use average annual total labor income from years where the child is between 13 and 18 years of age. To avoid dropping observations that do not match to the Composite Person Record (CPR) we use the 2000 census residence county to define AMI. After 2005, HUD defines AMI using American Community Survey data; specified proportions of AMI are used as eligibility and priority criteria.

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

(not shown). Of course, since we require that the estimation sample have at least two teenagers aged 13-18, our average household size is larger. In the estimation sample, the proportion that is non-Hispanic Black is slightly higher, the proportion in single-parent households is slightly lower, and the proportion receiving a housing subsidy is slightly higher. These differences relate to the generalizability of the study, but have no bearing on the internal validity.

A comparison of columns 2 and 3 shows that there are substantial differences in the outcome variables – those in subsidized housing earned less during the 2008-2010 period ($25,000 versus $32,600 on average), they worked fewer quarters (6.375 versus 7.041 on average), and a lower percentage had any labor market earnings during the 2008-2010 period (79.0 percent versus 81.9 percent). In addition, blacks 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 61 percent). In contrast, the comparison between columns 4 and 5 uncovers only small differences.[24] This 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 introduce the within-household variation in subsidized housing participation, Figure 1 displays the distribution of within-household differences—each youth's own subsidized housing participation net the household mean for all relevant youth—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.[25] This subsample included 41.7 percent of housing voucher participants and 69.3 percent of public housing participants. The distribution is unimodal and symmetric around zero, with an overwhelming majority of teenagers within 2 years of the household mean participation.

6. Empirical Results

6.1 Samples and specifications

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

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

The key question we address is whether living in voucher-supported or public housing affects a youth's labor market experiences as an adult. We compare the effects on earnings over the 2008-10 period of each of these two HUD housing types with nonsubsidized housing.[26]

Table 3 presents results for teenagers from all households while Tables 4, 5, and 6 present results for teenagers from non-Hispanic White households, non-Hispanic Black households, and Hispanic households, respectively. Each table presents results for a "dummy treatment," which consists of a binary measure of whether an individual ever participated in each type of subsidized housing while aged 13 to 18, and a "dose treatment," which is defined as the number of years an individual participated in each type of subsidized housing while aged 13 to 18. As described above, the dependent variable is the inverse hyperbolic sine of total earnings over the 2008-10 period. In addition to the treatment variables interacted with gender, unlisted controls include age, gender, and age by gender.[27] Table 7 presents the effect of each type of housing subsidy, separately for each sex and household race-ethnicity type, and it compares the estimated effect across gender and across the two subsidized housing types within each possible sex/household race-ethnicity combination.

In Tables 3 through 6, the first column presents OLS estimates of the specification described in equation (1). The coefficients capture the correlation between earnings and the two different types of subsidized housing participation after controlling for observed covariates, but as discussed before, are susceptible to bias as a result of selection based on unobservable factors. The second column in each table presents estimates from the household fixed effects (HFE) specification, described in equation (2). By using only within-household variation, these estimates purge the treatment effects of all bias resulting from time-invariant, household-level unobserved characteristics. 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 a HFE specification that, in addition to the controls in column (2), also include, in column (3), a control for the average parents' earnings that each individual experienced between 13 and 18 and its interaction with a male dummy, in column (4) a control for average block group percent poverty that each child experienced between 13 and 18 years of age, and in column (5) controls for both parents' earnings and block group poverty. We interpret the estimates in Column 3 as a test for whether our household fixed effects are effectively ridding the treatment effects of bias from unobserved, time-varying heterogeneity. Specifically, if our treatment effects do not change after the

[26] In unreported results, we have also used the total number of quarters worked over the 2008 to 2010 period, an indicator for whether the individual ever worked during the 2008 to 2010 period, and earnings conditional on having some positive earnings as dependent variables. The last specification restricts the sample. The results are qualitatively consistent regardless of which measure of labor market performance is used. But we do find that it is the extensive margin that is the most important in accounting for our results. That is, the magnitudes are substantially diminished when conditioning on positive earnings and we find that there are statistically significant differences in the likelihood of working at all consistent with our main findings.

[27] The complete regression results as well as the results for the other measures of labor market performance are available from the authors.

inclusion of parents' earnings, then either the within-household differences in subsidized housing participation or the within-household differences in adult earnings (or both) are unrelated to within-household differences in parents' earnings. Similarly, the estimates in column 4 are an indicator of whether neighborhood quality, as proxied by block-group percent poverty, is a potential mechanism for the estimated treatment effects. Column 5 accounts for both factors.

6.2 Results for all households

We now turn to the coefficients of interest beginning with the estimates that pool across household race/ethnicity in Table 3. For the OLS results in column (1), for both the dummy and dose treatments, the OLS results show that there are significant negative effects on subsequent total earnings with larger negative effects for males. Significant negative relationships between the two types of subsidized housing participation and adult earnings also occur in each of the race/ethnicity groups (Tables 4-6) although magnitudes vary.[28]

However, the HFE results, which control for all household level time-invariant heterogeneity, paint an entirely different picture. The HFE results for females and males are summarized in Table 7. The negative effects from OLS are attenuated or reversed. Housing voucher participation is not negatively related to adult earnings for female teenagers in the HFE specification. Both living in public housing and living in a housing voucher-subsidized unit lead to positive and significant effects on later earnings for female teenagers. The effect of voucher participation remains negative and statistically significant for male teenagers with the dummy treatment, and is not statistically different from zero for public housing. The effects estimated for the dose treatment (years) reinforce the findings of the dummy treatment. The effects for males are significantly more negative than the effects for females with both the dummy and the dose treatment. For the dummy treatment, public housing is more beneficial than housing vouchers for both females and males (no difference was found for the dose treatment).

To gain perspective on the quantitative implications, we focus on the dose results that are easier to interpret. In addition, since we are including zero earnings in the outcomes via using the inverse hyperbolic sine transformation, evaluations of the predictions using the inverse hyperbolic sine depend on where in the distribution the estimated effects are evaluated. In what follows, we use the mean of the total earnings in 2008-10 for those young adults who when teenagers lived in households that had some housing subsidy. That is, we evaluate predictions using the mean $25,012 (in 2000 dollars). Since that is the cumulative earnings over three years, we convert the predictions to annual changes.

The dose results indicate that each additional year of voucher participation increases adult earnings for females by about $694 and reduces adult earnings for males by -$145. For public housing, the dose results

[28] OLS results are also negative when we use the long form of the 2000 Census to include additional explanatory variables.

suggest that each additional year of public housing participation increases early adult earnings for females by $1,035. For males the prediction is not significantly different from zero.

The results in columns 3 and 4, which add controls for average parents' earnings and average block group percent poverty, are essentially unchanged from the results in column 2. In the following subsection, we find that columns 2 and 3 are similar even when allowing the results to differ for different race/ethnicity samples. Consequently, we discuss just the simple HFE results in the text (and Table 7).

6.2 Race/ethnicity samples

To help understand the results in Table 3, we investigate whether the results differ by household race-ethnicity. Tables 4 through 6 thus explore whether there is treatment effect heterogeneity by household race-ethnicity. We do this by estimating coefficients separately for non-Hispanic White households, non-Hispanic Black households, and Hispanic households, respectively.[29] Comparing results across these three subgroups (see Table 7 for a summary), we find important differences. For example, comparing the HFE results shows substantial heterogeneity across race/ethnicity groups, affirming the importance of considering these groups separately.

The positive effects for non-Hispanic Black females suggest they receive an earnings premium of about $640 and $785 per year in voucher and public housing, respectively. Non-Hispanic Black males also see their adult earnings increase as a result of assisted housing participation, by about $281 and $689 per year of residence in voucher and public housing, respectively. Non-Hispanic White males enjoy earnings premia of similar magnitudes to those of non-Hispanic Black males, whereas the estimated earnings premia are not significantly different from zero for non-Hispanic White females or Hispanics of both genders for either subsidy type.

Table 7, in addition to displaying the average partial effects of each type of subsidized housing separately by gender, also displays tests of whether the effects of each type of subsidized housing are equal. For example, we test whether the effect of voucher housing for females is the same as the effect of public housing for females. We conduct this test for each possible household race/sex combination, and for both the dummy and dose treatments. For the combined sample, we find that vouchers lead to lower outcomes than public housing for the dummy and the dose treatments. The results for the subsamples suggest that the overall difference between voucher and public housing is largely driven by the result for non-Hispanic Black and Hispanic males. Though there are significant differences between males and females in the subsamples, the differences are more muted as compared to the combined sample.

[29] The sample size was too small to allow for separate estimates for Other race non-Hispanics.

7. Extensions and Refinements to Our Identification Strategies

We undertook multiple exercises to understand these results in more detail as well as to abstract from endogenous changes within families that might be driving our between sibling variation.

7.1 Characteristics of public housing

As much of the discussion of public housing in the popular media concerns high-rise projects primarily found in urban areas, we check whether the effect of living in a large public housing project is different from the overall results. That is, we allow for the effect of public housing participation to differ according to project size (population). To do so, we define person-weighted project size quartiles by considering all public housing projects over the period 1997-2005. On the basis of these quartiles, it was determined whether each individual in our sample who ever participated in public housing was also a resident of large public housing project (the top quartile). We then included either an indicator for whether each teenager in our sample ever lived in a large public housing project or a count of the number of years each teenager lived in a large public housing project in addition to the measures of housing voucher participation and general public housing participation included in previous specifications. The coefficient estimates from household fixed effects specifications for these large public housing measures capture any differential effect that large public housing residence as a teenager has on adult earnings. Table 8 presents these results. We present both dummy and dose results for completeness but focus our discussion on the dose results in the text. First, note that the estimated coefficients on the housing voucher and general public housing measures are very similar to those from the more basic household fixed effects specification. We do not find that the size of public housing projects, as an additional characteristic, has the negative effect that is often described in discourse on the topic. We therefore find no evidence to support the idea that living in a large public housing project is particularly harmful for children's later earnings.[30]

Similarly, it might be the case that being assigned to a public housing project where households earn relatively low annual incomes has a differential impact on adult outcomes. Such a differential effect could exist as a result of role model effects (e.g. observing adults who supply more labor while a teenager increases labor supply as an adult) or if project level social networks enable individuals to find a job or a higher paying job more easily. To test for heterogeneity by project-level household income, we compute the person-weighted median household adjusted income for each project year.[31] 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

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

[31] 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.

associated household income quartile for each year they participated in public housing. We define the lowest-income public projects as those that fall into the bottom quartile with respect to median household annual adjusted income. This match is used to create an indicator for whether each teenager ever resided in a lowest-income public housing project and a count of the number of years they resided there. These measures are then included, in addition to the housing voucher and general public housing measures, as discussed in the previous paragraph. Table 9 presents the household fixed effects estimates from these specifications. Again, estimates for voucher and general public housing are similar to the main results. We find positive and significant interactions for low-income projects for girls and negative, but not significant, interactions for boys. Estimates for blacks have a similar pattern, but display stronger negative effects for boys. The main results for blacks (Table 5) and the estimates for general public housing presented here both found a positive effect for girls and boys. The interaction here suggests that if the public housing project is low-income, that gain may be lower. Still, it appears that living in one of the lowest-income public housing projects does not have a negative overall effect. Taken together, Tables 8 and 9 indicate that the most often described negative characteristics of public housing are not associated with worse adult outcomes.

7.2 Predicted housing treatment based on household participation

One concern is the possibility that measurement error or endogenous changes in the structure of households might bias the household fixed effects estimates. While we anticipate that the administrative nature of our data reduces the likelihood that measurement error is a problem, it remains true that housing authorities might not perfectly enumerate all children or households in all years. While administrative data do not in any way preclude the possibility of endogenous changes in household structure, if a correlation between changes in household structure and changes in subsidized housing participation exists, we anticipate that it would bias our main estimates downwards. This is because eligibility constraints are relaxed by most changes in household structure that might also affect adult earnings (e.g. splitting up of a multi-generational family, death of the head of household, early separation of a teenager from the household).

To address these potential biases Table 10 generates a measure of predicted participation in public and voucher housing. To construct the predicted value, we use the age of the children in the household and the observed information about whether the head of household is in subsidized housing. That is, for any given year, if a parent is in subsidized housing and the child is in the 13-18 year-old age range, then the "predicted" participation measure indicates that the child is in subsidized housing in that year. Differences between actual and predicted measures of participation could arise for two reasons, both of which we would like to avoid. The first is measurement error in the recording of child information that could lead to either the omission or erroneous reporting of that child's participation. The second is that the child left the household while still aged

13-18. Such departures might reflect events (e.g. a child leaving to live with a member of the extended family such as a grandparent) that also have an impact later outcomes but are unrelated to the treatment effects we are seeking to identify.

Using this predicted treatment measure, Table 10 reports household fixed-effects results using the actual treatment (the same as Table 3), using the predicted treatment instead of the actual treatment, and instrumenting for the actual treatment with the predicted treatment. The results in Table 10 are quite similar when using any of the participation definitions, albeit with lower earnings outcomes in the dose specifications.

For females, we continue to find significant positive effects for both public housing and voucher-assisted housing, while males receive significantly lower benefits than their female counterparts from both programs. The consistent pattern across columns, and in particular the lack of coefficient attenuation in Columns 1 and 4 relative to other columns, suggests that measurement error is not importantly affecting the results.[32] Had measurement error been driving our estimates, we would expect the IV specifications in Columns 3 and 6 to be substantially larger in absolute value than our main results – they are not.

However, when using the dose treatment, we do estimate slightly more negative effects for both programs when using either the predicted treatment or when instrumenting for the observed treatment with the predicted treatment. The dummy treatment estimates do not display the same variation across columns. That said, the difference between Columns 4 and 6 may be attributable to changes in household structure. The lower Column 6 estimates could possibly be interpreted in terms of the importance of disruption effects associated with household dissolution. However, our main conclusions remain qualitatively unchanged; we still estimate significant positive effects of teenage subsidized housing participation and negative male interactions for both programs.

7.3 Wait times

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 1 year, and then remains in that project. In the absence of the wait time, both children would experience the same amount of public housing participation while of age 13-18: 6

[32] The household-predicted housing subsidy measure could 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 10 suggest that measurement error is not importantly affecting our results.

years each. 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.

There do, in fact, sometimes exist substantial wait times for both public and voucher-assisted housing in our sample. To illustrate these wait times, we use data on all subsidized housing participants from the year 2000. For most households, the data contain information on the date they entered a waitlist as well as the date they were granted admission to the program. In some cases the two dates are the same, indicating there was no wait for the program, but for most households there was a non-trivial wait between when they were placed on a waitlist and when they were admitted. Figure 2 displays the distribution of wait times for individuals in voucher and public housing who entered subsidized housing no earlier than 1995 and who were found in subsidized housing in 2000. We restrict the entrance date to be after 1995 because data quality 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 11, we present estimates for two samples broken out by whether, in 2000, the household resided in a county with average subsidized housing wait times of less than or greater than 9 months (approximately the median wait time by county). The HFE estimates are similar to the main result in Table 3 in both cases. The estimates for the high wait time counties are almost always of equal or of modestly greater magnitude than those for the low wait time counties. Had we found that our main results were driven by estimates for the low wait time counties, we might have been concerned that opportunistic sorting was biasing the HFE results. That we find similar effects in both cases reinforces the case for our main estimates to be interpreted as causal effects.

7.4 Exogenous supply of public housing capacity

While we think our longitudinal controls for parents income and changes in household structure mitigate the primary concerns regarding endogenous between sibling differences in housing subsidy exposure, there still may be unobserved factors that could induce movement into or out of subsidized housing and also impact adult earnings. To account for this possibility, we follow Newman and Harkness (2000, 2002) and generate an instrument for subsidized housing participation based on variation in public housing supply across counties. We focus on public housing since we think it inherently more plausible that there are exogenous changes in local public housing supply since, for example, demolitions of public housing were substantial over our sample

period and varied widely across local areas (see, e.g., Jacob 2004). This focus on public housing changes the nature of our sample and specification. For the exercises in this section, the treatment group is defined by those with exposure to public housing and the controls are eligible households that are not in any form of public housing. As in our benchmark results, we focus on household fixed effects specifications so that we are exploiting between sibling differences in public housing exposure. [33]

Our first task is to generate the county-level observed participation in public housing for each year in our sample. We calculate housing supply by allocating the HUD-PIC population in each year to counties of residence in 2000, using the participation of the teen sample in projects as weights. We find a wide range of expansions and contractions in public housing supply during the study period. These participant counts are then combined with annual county-level data on the number of individuals below 80% of area median income, the natural log of population, the fraction of households living below the poverty line, the fraction of individuals that are white, the fraction of individuals that are over age 65, the fraction of adults over 25 that have some type of college degree, the ratio of median gross rent to median household income, the fraction of households that have single, female household heads, the total employment, and the average annual labor market earnings.[34]

With this county-level information, we estimate a county-level ordinary least squares regression with the ratio of public housing participants to the number of eligible individuals—though there is heterogeneity across counties, household income below 80% area median income is frequently sufficient to qualify for public housing—as the dependent variable and the remaining county-level information as explanatory variables for each year between 1997 and 2005 (see Appendix Table C1 for an example that pools all years). The explained variation in these specifications can be thought of as demand-driven; all explanatory variables are characteristics that should indicate either increased or decreased household demand for housing subsidies. We treat the remaining variation, i.e. the residual from each year-specific regression, as a measure of residual public housing supply. It is variation in the county-level likelihood of participating in public housing that is not driven by demographic characteristics associated with the demand for public housing. We match the residuals from these regressions to the teenagers in our sample using their 2000 county of residence and age. We average these public housing supply residuals for each teenager across the years when they are between the ages of 13 and 18.

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[33] It is important to emphasize that we drop voucher-assisted households from the sample to avoid treating them as "control" households. While we believe the public housing supply residuals provide a valid instrument, at least within household, for public housing participation, we are skeptical the analogous measure would satisfy the exclusion restriction for voucher-assisted housing. This is because while public housing participation is limited by the available stock of public housing projects, voucher-assisted households are able to reside—at least in theory—in any rental unit. Changes over time in the ratio of participants to eligible household are therefore more likely to represent changes in the demand for housing vouchers.

[34] The number of individuals below 80% of area median income, the fraction of households living below the poverty line, the fraction of individuals that are white, the fraction over 65, the fraction over 25 with some college degree, the ratio of median gross rent to median household income, and the fraction of households that have single female household heads are taken from the 1990 census, the 2000 census, and the 2005-2009 ACS 5-year sample. For all non-census or ACS years (1997-1999, 2001-2005), we linearly interpolate between the two closest available years of data. The natural log of population in each year comes from the intercensal estimates, and county employment and average annual earnings are from the Bureau of Labor Statistics.

This average public housing supply residual—which varies both across households and teenagers of different ages within-households—is then used as an instrument for public housing participation.

To be a valid instrumental variable, it must be true that these public housing supply residuals are correlated with public housing participation—which is empirically testable—and that they are uncorrelated with the error term in the adult earnings equation. We show in Table 12 that these residuals are very strongly related to public housing participation; for both endogenous variables, years spent in public housing and its interaction with a male indicator, first stage F-statistics far exceed 10 (the suggested threshold for weak instruments). This is true in both the case of ordinary instrumental variables and the household fixed-effects instrumental variables specifications.

Unfortunately, without satisfying the exclusion restriction, a strong first-stage relationship may simply yield more precisely estimated, but still biased, IV estimates. If, for example, counties with more generous public housing supply also provide more generous access to safety net programs or offer higher quality publicly-provided amenities like schools, an IV strategy based on the public housing supply residuals may conflate the impact of public housing participation with unobserved participation in other programs or increased access to high quality public amenities. Under the assumption that access to these unobserved public services has a positive impact on later earnings, the simple IV estimates will be upwards biased.

However, in our household fixed effects specification, we require a substantially weaker assumption. We need only that the public housing supply residuals be unrelated to the error term in our household fixed-effects earnings equation, after having conditioned both public housing participation and adult earnings on household fixed-effects. Therefore, the scenario where some counties always provide more generous public amenities and always provide more access to public housing is no longer necessarily problematic. Instead, it would have to be true that changes over a short time period in the quality of public schools or the generosity or effectiveness of social programs move in tandem with unexplained changes in public housing capacity. Given the closeness in age between the siblings in our sample (at most six years difference) it seems likely that, conditional on our household-fixed effects, the exclusion restriction will be met.

Table 12 presents the results when using the public housing supply residuals as instruments for the number of years of teenage participation in public housing (i.e. the dose results from Table 3). Column 1 displays the OLS estimates, Column 2 the instrumental variables estimates, Column 3 the household fixed-effects estimates, and Column 4 the household fixed-effects instrumental variables estimates. In all columns, households that had any teenager who participated in voucher-assisted housing while between the ages of 13 and 18 are dropped from the sample.

Consistent with our OLS estimates for the overall sample, Column 1 suggests public housing is negatively correlated with public housing participation for both males and females; both estimates are

significant at the 99-percent level. However, Columns 2-4 suggest this negative association is entirely due to selection into public housing. The basic IV estimates are large, significant, and positive.

As discussed above, these IV estimates may be positively biased. Therefore, Columns 3 and 4 present both the HFE and the HFE IV estimates. The point estimates in Column 3 are reassuringly close to those presented in Table 3 despite the change in sample. Further, the estimates in Column 4 are similar in size and direction to the Column 3 estimates but substantially more positive than the OLS estimates in Column 1. This suggests that OLS importantly underestimates the impact of public housing. However, columns 3 and 4, in addition to being qualitatively similar, are never statistically different from one another. In both specifications, females receive a large adult earnings premium from public housing participation in childhood, while males receive a much smaller positive benefit. Together, the lack of differences across columns 3 and 4 lends further support to the validity of our main results.

7.5 Entry into housing as a teenager

While some of the MTO studies focus on effects for children who experienced subsidized housing at a young age, this study, due to data limitation, examines only treatment during the teenage years. Our estimates of the impact of teenage exposure to subsidized housing may be contaminated by the omission of pre-teenage exposure to subsidized housing. While data limitations prevent us from directly controlling 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 Table 13, we present a dose specification that adds an interaction effect for whether the child's household participated in voucher or public housing in 1997, at the beginning of our study. The goal of this specification (column two) is to examine whether our main result is present for children who entered housing as teens and whether there is any marginal effect for additional years for those who had early experience with housing (those in housing in 1997 may have experienced subsidized housing for their entire youth). We find that the effects for children who entered housing as teens are of similar or larger magnitude than our main dose results. The interactions for having received a housing subsidy in 1997, suggest lower magnitude effects for additional years for those children who likely had greater, total, experience with subsidized housing.

7.6 Left-Censoring of Subsidized Housing Treatment

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. We test for the importance of censoring by limiting the sample to only teenagers aged 13-16 in 2000; that is, those teenagers with uncensored treatment.[35] In unreported results, we find no differences between our main estimates and the results from estimating the age-limited sample.

8. Concluding Comments

In spite of the policy importance of a sound understanding of the effects of subsidized rental housing on long-term outcomes, the existing literature lacks a well-identified comparison of public housing, voucher-assisted housing, and private market housing. In this paper, we report results from a project that estimates the long-term effects of public housing and voucher-assisted housing participation as a teenager on adult earnings, enabling the direct comparison of both programs to each other and to private market housing.

We create a national data set on housing assistance, household structure, and earnings by merging administrative records, censuses, and surveys at the U.S. Census Bureau. 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 investigate their employment and earnings up to 10 years later.

We address potential unobserved heterogeneity and selection bias by estimating household fixed effects models that identify the impact of assisted housing exploiting only variation within households. We find that the substantial negative effects of subsidized housing often found in the literature may be largely attributable to the selection of households entering assisted housing. After accounting for this negative household-level selection, we find that subsidized housing participation as a teenager yields large positive effects on young adult earnings for females. The effects for males are substantially smaller and generally not significantly different from zero. The point estimates suggest that females earn $694 more annually for each additional year of voucher-supported housing participation and $1035 more annually for each additional year of public housing participation. The corresponding estimates for males suggest they experience a small decrease in adult earnings from time spent in voucher-assisted housing and a small increase from time spent in public housing. Disaggregating our sample by the race/ethnicity of households suggests that non-Hispanic Blacks, and especially non-Hispanic Black females, benefit more than Hispanics or non-Hispanic Whites from both programs. However, both non-Hispanic Black males and females earn more as young adults as a result of time spent in public housing as a teenager.

We also investigate heterogeneous treatment effects by type of public housing project (project size and project median income). Contrary to conventional wisdom, we find no evidence that the effects of public housing on labor market outcomes are more negative for those individuals living in especially large or poor

[35] Results available upon request.

public housing projects. In fact, if anything, the point estimates suggest that low-income public housing projects are *more* beneficial for females, perhaps because they provide a larger—in relative terms—income effect for resident households.

To account for the possibility that time-variant unobserved heterogeneity may be biasing our household fixed-effects estimates, we also consider specifications that include time-varying household measures that vary across children, including parent's income and average neighborhood poverty. These controls do not affect our estimates. Further, leveraging the fact that most households spend a substantial amount of time on a wait list prior to receiving admission to both public and voucher-assisted housing, we estimate the effect of both programs separately by county-level average wait time. The results are similar in counties with shorter- and longer-wait times, suggesting our main estimates are not driven by households that are able to quickly respond to economic shocks by adjusting their participation in subsidized housing. Finally, we estimate two fixed-effects instrumental variables specifications intended to address the possibility that other time-varying factors correlated with housing participation and adult earnings are driving our estimates. Our findings are largely robust across both IV specifications.

There remain a number of limitations of our analysis. First, our results apply to just two of the many subsidized housing programs, albeit the largest – public housing and housing vouchers. Second, while we demonstrate that this pattern is consistent across a range of different specifications, a more thorough understanding of why subsidized housing participation in childhood might be more beneficial for females than for males would provide useful information for policymakers. Finally, our results might not be representative of all subsidized households (that is, households with younger children, and those with just one teenager) since our results pertain only to teenagers between the ages 13 and 18. While this is a formative period, other research on siblings has shown substantial effects for within household differences occurring early in childhood or even before birth. Future work should investigate whether exposure to subsidized housing during earlier periods of life has long-term implications as well.

Our results imply more positive longer term benefits to children from subsidized housing than the existing literature has found, at least for females and for black, non-Hispanic households. Strikingly, we find these positive benefits for both public housing and voucher based housing and the quantitative effects are similar across these programs. Most studies of longer term effects have not focused on labor market earnings effects. Newman and Harkness (2002) in a relatively small sample from the PSID did find positive long term earnings effects using an IV strategy that we build upon that is broadly consistent with our findings. In addition, the recent MTO study by Chetty et al. (2015) finds substantial long term earnings gains for children from those in the experimental group that were provided with incentives or encouragement to move to lower-poverty neighborhoods. But they find these longer term effects are positive only for children who received

treatment while relatively young. In addition, this interesting finding is more about the differential impact of the type of housing subsidy rather than the impact of receiving any housing subsidy.

We recognize that a limitation of our analysis relative to some of these studies is that we are not exploiting randomized variation as in the MTO studies and in the recent Jacob et al. (2015) study using voucher housing lottery variation in Chicago. But we think our household fixed effect estimation strategy with additional longitudinal controls and our alternative IV strategies address the most compelling selection issues that plague estimates that control for selection only on observables. In addition, the national scope of our study is a unique feature of our analysis. Reconciling our more positive estimated long term effects for public and voucher-assisted housing with other research that shows little or no effects should be an important area for future research.

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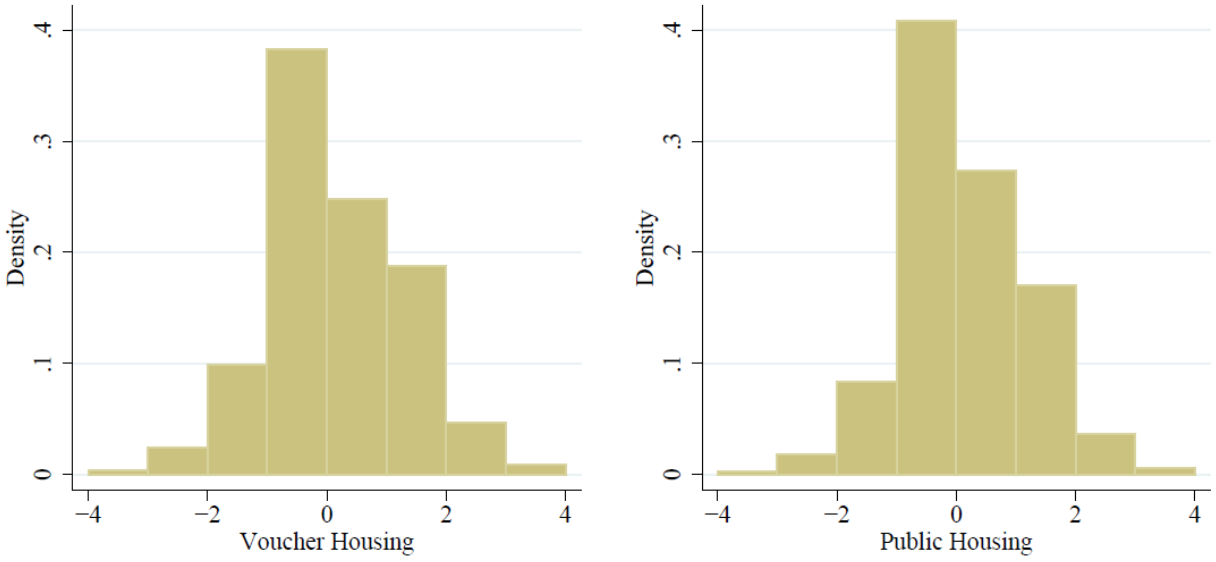
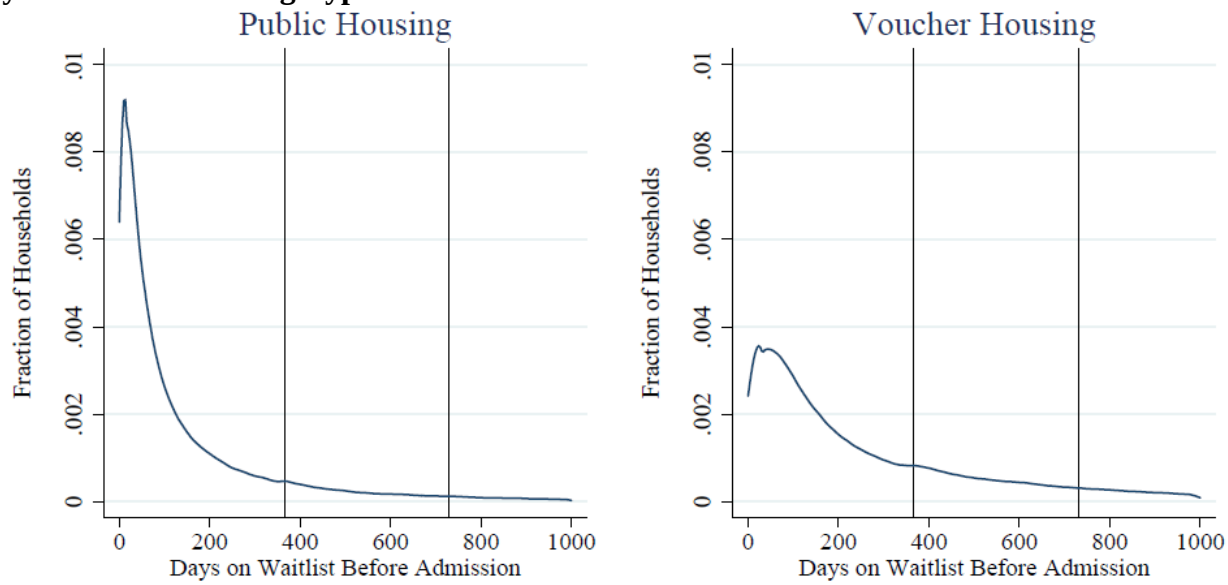
Figure 1: Distribution of Within−Household Differences among Households with Some Subsidized Housing Participation



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

Figure 2: Days Spent on Waitlist Before Program Admission
By Subsidized Housing Type in 2000



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

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

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

SOURCE: HUDUSER, HUD Public Use Data.

Table 2: Summary Statistics for Teenagers included in Sample

	Individuals Aged 13-18 in 2000 With at least One Other Sibling Aged 13-18 in 2000				
			In Households who received some housing subsidy 1997-2005		
Variable	(1) Total	(2) In households not receiving any housing subsidy 1997-2005	(3) Total	(4) Teenagers who never lived in subsidized housing while aged 13-18	(5) Teenagers who lived in subsidized housing while aged 13-18
Household size in 2000	5.371	5.351	5.418	5.673	5.372
Age in 2000	15.4	15.5	15.3	15.6	15.3
Proportion male	0.499	0.504	0.487	0.506	0.483
Proportion White non-Hispanic household	0.339	0.396	0.203	0.228	0.199
Proportion Black non-Hispanic household	0.292	0.218	0.471	0.450	0.475
Proportion Hispanic household	0.286	0.299	0.256	0.256	0.256
Proportion other non-Hispanic household	0.082	0.088	0.070	0.066	0.071
Average block group percent poverty	0.112	0.109	0.121	0.120	0.121
Average inverse hyperbolic sine of parents' earnings	8.041	8.235	7.579	7.694	7.559
Total parents' earnings between ages 16-18	34,620	37,887	26,808	28,397	26,525
Proportion single-headed household	0.661	0.614	0.773	0.751	0.777
Proportion public housing resident between ages 13-18	0.086	0	0.292	0	0.344
Proportion housing voucher recipient ages 13-18	0.173	0	0.588	0	0.693
Years in public housing ages 13-18	0.278	0	0.943	0	1.111
Years in voucher housing ages 13-18	0.595	0	2.018	0	2.377
Total labor market earnings 2008-2010	30,342	32,570	25,012	24,740	25,061
Total number of quarters worked 2008-10	6.845	7.041	6.375	6.248	6.398
Proportion with any labor market earnings 2008-10	0.810	0.819	0.790	0.781	0.792
Number of observations (rounded)	1,120,000	793,000	327,000	50,000	277,000

Source: Authors' tabulations of matched 2000 Census, HUD-PIC, and LEHD files (see text).

Note: Excludes teenagers in owner occupied housing, teenagers from household earnings above 50% area median income in the year 2000, and teenagers who lived in counties with at least one Housing Authority participating in HUD's Moving to Work Program.

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

	Dummy Treatment (Ever in Program)					Dose Treatment (Years Spent in Program)				
	OLS	HFE	HFE EC	HFE BGC	HFE LC	OLS	HFE	HFE EC	HFE BGC	HFE LC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Voucher Housing	-0.289***	0.191***	0.191***	0.191***	0.191***	-0.062***	0.080***	0.080***	0.080***	0.080***
	0.015	0.033	0.033	0.033	0.033	0.004	0.009	0.009	0.009	0.009
Voucher Housing*Male	-0.375***	-0.402***	-0.401***	-0.402***	-0.401***	-0.091***	-0.098***	-0.097***	-0.098***	-0.097***
	0.022	0.028	0.028	0.028	0.028	0.006	0.007	0.007	0.007	0.007
Public Housing	-0.275***	0.360***	0.359***	0.363***	0.362***	-0.060***	0.117***	0.117***	0.118***	0.117***
	0.020	0.043	0.043	0.043	0.043	0.005	0.013	0.013	0.013	0.013
Public Housing*Male	-0.398***	-0.418***	-0.417***	-0.424***	-0.423***	-0.099***	-0.108***	-0.107***	-0.109***	-0.109***
	0.031	0.038	0.038	0.038	0.039	0.008	0.011	0.011	0.011	0.011
IHS Average Parents Earnings			0.005		0.005			0.004		0.004
			0.011		0.011			0.011		0.011
IHS Average Parents Earnings*Male			0.002		0.002			0.002		0.002
			0.003		0.003			0.003		0.003
Average Block Group % Poverty				-0.428	-0.434				-0.426	-0.433
				0.308	0.309				0.308	0.309
Average Block Group % Poverty*Male				0.217	0.230				0.201	0.215
				0.174	0.175				0.174	0.175
Age by Male Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Household Fixed Effects	no	yes	yes	yes	yes	no	yes	yes	yes	yes

Note: Number of observations = 1,120,000 rounded to the nearest thousand. See text for the description of the sample. The dependent variable in each column is the inverse hyperbolic sine of total earnings between 2008 and 2010. All columns include controls for age, sex, and age by sex . Columns 1 and 6 present ordinary least squares (OLS) estimates. Columns 2-5 and 7-10 present household fixed effects (HFE) estimates. Columns 3 and 8 display HFE estimates with a control for the inverse hyperbolic sine (IHS) of average parents' annual earnings while the teenager was 13-18 years of age (HFE EC). Columns 4 and 9 display HFE estimates with a control for the average block group percent poverty in the teenagers block group of residence while the teenager was 13-18 years of age (HFE BGC). Columns 5 and 10 display HFE estimates with controls for both parents' earnings and block group percent poverty (HFE LC). 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. Standard errors, clustered at the household level, are displayed under each estimate. Based on the authors' tabulations from matched Census 2000-LEHD-PIC file. *** p<=0.001, ** p<=0.01, *p<=0.05.

Table 4: The Effect of Teenage Residence in HUD-Subsidized Housing on Adult Earnings
White non-Hispanic Households Only

	Dummy Treatment (Ever in Program)					Dose Treatment (Years Spent in Program)				
	OLS	HFE	HFE EC	HFE BGC	HFE LC	OLS	HFE	HFE EC	HFE BGC	HFE LC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Voucher Housing	-0.504***	-0.062	-0.065	-0.054	-0.057	-0.129***	0.028	0.027	0.030	0.029
	0.029	0.062	0.062	0.062	0.062	0.008	0.018	0.018	0.018	0.018
Voucher Housing*Male	0.083	0.047	0.051	0.031	0.036	0.017	0.006	0.008	0.003	0.004
	0.042	0.053	0.053	0.053	0.053	0.012	0.014	0.014	0.014	0.014
Public Housing	-0.477***	-0.097	-0.098	-0.077	-0.078	-0.113***	0.008	0.008	0.013	0.013
	0.049	0.094	0.094	0.094	0.094	0.015	0.034	0.034	0.034	0.034
Public Housing*Male	0.332***	0.266**	0.269**	0.229*	0.232*	0.075***	0.054*	0.055*	0.046	0.047
	0.072	0.091	0.091	0.091	0.091	0.022	0.027	0.027	0.027	0.027
IHS Av Parents Earnings			0.013		0.012			0.013		0.012
			0.017		0.017			0.017		0.017
IHS Av Parents Earnings*Male			0.007		0.008			0.006		0.008
			0.005		0.005			0.005		0.005
Av Block Grp % Poverty				-2.335***	-2.344***				-2.349***	-2.358***
				0.601	0.601				0.601	0.601
Av Block Grp % Poverty*Male				2.435***	2.457***				2.471***	2.492***
				0.371	0.372				0.371	0.372
Age by Male Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Household Fixed Effects		yes	yes	yes	yes		yes	yes	yes	yes

Note: Number of observations = 1,120,000 rounded to the nearest thousand. See text for the description of the sample. The dependent variable in each column is the inverse hyperbolic sine of total earnings between 2008 and 2010. All columns include controls for age, sex, and age by sex. Columns 1 and 6 present ordinary least squares (OLS) estimates. Columns 2-5 and 7-10 present household fixed effects (HFE) estimates. Columns 3 and 8 display HFE estimates with a control for the inverse hyperbolic sine (IHS) of average parents' annual earnings while the teenager was 13-18 years of age (HFE EC). Columns 4 and 9 display HFE estimates with a control for the average block group percent poverty in the teenagers block group of residence while the teenager was 13-18 years of age (HFE BGC). Columns 5 and 10 display HFE estimates with controls for both parents' earnings and block group percent poverty (HFE LC). 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. Standard errors, clustered at the household level, are displayed under each estimate. Based on the authors' tabulations from matched Census 2000-LEHD-PIC file. *** p<=0.001, ** p<=0.01, *p<=0.05.

Table 5: The Effect of Teenage Residence in HUD-Subsidized Housing on Adult Earnings
Black non-Hispanic Households Only

	Dummy Treatment (Ever in Program)					Dose Treatment (Years Spent in Program)				
	OLS	HFE	HFE EC	HFE BGC	HFE LC	OLS	HFE	HFE EC	HFE BGC	HFE LC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Voucher Housing	-0.120***	0.178***	0.180***	0.169***	0.173***	-0.014**	0.074***	0.075***	0.072***	0.073***
	0.022	0.049	0.049	0.049	0.049	0.005	0.013	0.013	0.013	0.013
Voucher Housing*Male	-0.210***	-0.247***	-0.251***	-0.230***	-0.236***	-0.036***	-0.041***	-0.042***	-0.037***	-0.039***
	0.035	0.044	0.044	0.044	0.044	0.009	0.011	0.011	0.011	0.011
Public Housing	-0.175***	0.259***	0.263***	0.267***	0.269***	-0.031***	0.090***	0.091***	0.093***	0.094***
	0.026	0.057	0.057	0.057	0.057	0.007	0.018	0.018	0.018	0.018
Public Housing*Male	-0.099*	-0.112*	-0.121*	-0.131*	-0.137*	-0.009	-0.011	-0.014	-0.017	-0.019
	0.042	0.054	0.054	0.054	0.054	0.011	0.015	0.015	0.015	0.015
IHS Av Parents Earnings			0.011		0.010			0.011		0.009
			0.022		0.022			0.022		0.022
IHS Av Parents Earnings*Male			-0.027***		-0.022***			-0.026***		-0.022***
			0.005		0.006			0.005		0.006
Av Block Grp % Poverty				-0.829	-0.717				-0.849	-0.739
				0.510	0.510				0.510	0.510
Av Block Grp % Poverty*Male				1.512***	1.281***				1.536***	1.310***
				0.304	0.308				0.304	0.308
Age by Male Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Household Fixed Effects		yes	yes	yes	yes		yes	yes	yes	yes

Note: Number of observations = 1,120,000 rounded to the nearest thousand. See text for the description of the sample. The dependent variable in each column is the inverse hyperbolic sine of total earnings between 2008 and 2010. All columns include controls for age, sex, and age by sex. Columns 1 and 6 present ordinary least squares (OLS) estimates. Columns 2-5 and 7-10 present household fixed effects (HFE) estimates. Columns 3 and 8 display HFE estimates with a control for the inverse hyperbolic sine (IHS) of average parents' annual earnings while the teenager was 13-18 years of age (HFE EC). Columns 4 and 9 display HFE estimates with a control for the average block group percent poverty in the teenagers block group of residence while the teenager was 13-18 years of age (HFE BGC). Columns 5 and 10 display HFE estimates with controls for both parents' earnings and block group percent poverty (HFE LC). 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. Standard errors, clustered at the household level, are displayed under each estimate. Based on the authors' tabulations from matched Census 2000-LEHD-PIC file. *** p<=0.001, ** p<=0.01, *p<=0.05.

Table 6: The Effect of Teenage Residence in HUD-Subsidized Housing on Adult Earnings
Hispanic Households Only

	Dummy Treatment (Ever in Program)					Dose Treatment (Years Spent in Program)				
	OLS	HFE	HFE EC	HFE BGC	HFE LC	OLS	HFE	HFE EC	HFE BGC	HFE LC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Voucher Housing	-0.678***	-0.049	-0.059	-0.049	-0.059	-0.155***	0.032	0.030	0.032	0.030
	0.034	0.070	0.070	0.070	0.070	0.009	0.019	0.019	0.019	0.019
Voucher Housing*Male	-0.041	-0.032	-0.012	-0.033	-0.013	-0.015	-0.015	-0.011	-0.015	-0.011
	0.049	0.058	0.058	0.058	0.058	0.013	0.015	0.015	0.015	0.015
Public Housing	-0.638***	0.076	0.068	0.076	0.070	-0.152***	0.043	0.041	0.043	0.041
	0.044	0.093	0.093	0.093	0.093	0.012	0.027	0.027	0.027	0.027
Public Housing*Male	0.055	0.047	0.066	0.045	0.061	0.011	-0.001	0.003	-0.002	0.002
	0.064	0.077	0.077	0.078	0.078	0.017	0.021	0.021	0.021	0.021
IHS Av Parents Earnings			-0.002		-0.002			-0.001		-0.001
			0.021		0.021			0.021		0.021
IHS Av Parents Earnings*Male			0.018**		0.018**			0.017**		0.018**
			0.006		0.006			0.006		0.006
Av Block Grp % Poverty				0.387	0.348				0.359	0.321
				0.550	0.550				0.550	0.550
Av Block Grp % Poverty*Male				0.076	0.155				0.103	0.180
				0.294	0.295				0.294	0.295
Age by Male Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Household Fixed Effects		yes	yes	yes	yes		yes	yes	yes	yes

Note: Number of observations = 1,120,000 rounded to the nearest thousand. See text for the description of the sample. The dependent variable in each column is the inverse hyperbolic sine of total earnings between 2008 and 2010. All columns include controls for age, sex, and age by sex. Columns 1 and 6 present ordinary least squares (OLS) estimates. Columns 2-5 and 7-10 present household fixed effects (HFE) estimates. Columns 3 and 8 display HFE estimates with a control for the inverse hyperbolic sine (IHS) of average parents' annual earnings while the teenager was 13-18 years of age (HFE EC). Columns 4 and 9 display HFE estimates with a control for the average block group percent poverty in the teenagers block group of residence while the teenager was 13-18 years of age (HFE BGC). Columns 5 and 10 display HFE estimates with controls for both parents' earnings and block group percent poverty (HFE LC). 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. Standard errors, clustered at the household level, are displayed under each estimate. Based on the authors' tabulations from matched Census 2000-LEHD-PIC file. *** p<=0.001, ** p<=0.01, *p<=0.05.

Table 7: Summary of Household Fixed Effects Estimates
By Gender, Subsidy Type, and Race/Ethnicity

	Dummy Treatment			Dose Treatment		
	Housing Voucher (VO) Treatment Effect	*Public Housing (PH) Treatment Effect*	*Are Subsidy Effects Different? (VO vs. PH)*	*Housing Voucher (VO) Treatment Effect*	*Public Housing (PH) Treatment Effect*	*Are Subsidy Effects Different? (VO vs. PH)*
All Households						
Females (F)	0.191***	0.360***	Yes***	0.080***	0.117***	Yes**
	0.033	0.043		0.009	0.013	
Males (M)	-0.211***	-0.0588	Yes***	-0.0176*	0.00904	Yes*
	0.0337	0.0460		0.00904	0.0138	
Are Effects Different by Sex (F vs. M)	Yes***	Yes***		Yes***	Yes***	
Non-Hispanic White Households						
Females (F)	-0.062	-0.097	No	0.028	0.008	No
	0.062	0.094		0.018	0.034	
Males (M)	-0.0155	0.169*	No	0.0346*	0.0626*	No
	0.0633	0.101		0.0183	0.0355	
Are Effects Different by Sex (F vs. M)	No	Yes***		No	Yes**	
Non-Hispanic Black Households						
Females (F)	0.178***	0.259***	No	0.074***	0.090***	No
	0.049	0.057		0.013	0.018	
Males (M)	-0.0688	0.148***	Yes***	0.0332**	0.0794***	Yes**
	0.0515	0.0632		0.0136	0.0186	
Are Effects Different by Sex (F vs. M)	Yes***	Yes**		Yes***	No	
Hispanic Households						
Females (F)	-0.049	0.076	No	0.032*	0.043	No
	0.070	0.093		0.019	0.027	
Males (M)	-0.0813	0.123	Yes*	0.0171	0.0416	No
	0.0710	0.0969		0.0187	0.0275	
Are Effects Different by Sex (F vs. M)	No	No		No	No	

Note: All columns present household fixed effects estimates of the effect of subsidized housing participation as a teenager on the inverse hyperbolic sine of total earnings 2008-2010. Estimates do not control for parents' earnings as a teenager or average block group percent poverty as a teenager but do include a male indicator and a full set of age in years by male fixed effects. See Tables 3-6 for observations rounded to the nearest thousand. Standard errors, clustered at the household level, presented below the point estimates. *** p<=0.01, ** p<=0.05, * p<=0.10.

Table 8: Subsidized Housing Residence and Adult Earnings
Differentiating Large Public Housing Projects

	All Households		White Households		Black Households		Hispanic Households	
	Dummy	Dose	Dummy	Dose	Dummy	Dose	Dummy	Dose
Voucher Housing	0.192***	0.080***	-0.062	0.028	0.179***	0.074***	-0.048	0.032
	0.033	0.009	0.062	0.018	0.049	0.013	0.070	0.019
Voucher Housing*Male	-0.403***	-0.098***	0.047	0.006	-0.247***	-0.041***	-0.035	-0.015
	0.028	0.007	0.053	0.014	0.044	0.011	0.058	0.015
Public Housing	0.258***	0.101***	-0.100	0.008	0.169**	0.070***	-0.025	0.034
	0.047	0.015	0.097	0.036	0.064	0.021	0.107	0.033
Public Housing*Male	-0.302***	-0.093***	0.290**	0.069*	-0.048	-0.007	0.190*	0.026
	0.045	0.012	0.095	0.028	0.064	0.017	0.095	0.026
Public Housing *Large Public Housing	0.353***	0.050	0.034	0.015	0.299**	0.062	0.268	0.020
	0.088	0.029	0.331	0.122	0.110	0.038	0.184	0.055
Public Housing *Large Public Housing*Male	-0.359***	-0.046	-0.255	-0.204	-0.186	-0.011	-0.349*	-0.064
	0.080	0.024	0.336	0.114	0.104	0.032	0.152	0.044
Observations	1,120,000	1,120,000	437,000	437,000	325,000	325,000	269,000	269,000

Note: Each column displays a household fixed effects estimate of the effect of teenage participation in subsidized housing on the inverse hyperbolic sine of total earnings between 2008 and 2010. See 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. Standard errors, clustered at the household level, are displayed below each point estimate. *** p<=0.001, ** p<=0.01, * p<=0.05.

Table 9: Subsidized Housing Residence and Adult Earnings
Differentiating Low-Income Public Housing Projects

	All Households		White Households		Black Households		Hispanic Households	
	Dummy	Dose	Dummy	Dose	Dummy	Dose	Dummy	Dose
Voucher Housing	0.190***	0.080***	-0.062	0.028	0.177***	0.074***	-0.049	0.032
	0.033	0.009	0.062	0.018	0.049	0.013	0.070	0.019
Voucher Housing*Male	-0.399***	-0.097***	0.047	0.006	-0.245***	-0.041***	-0.034	-0.015
	0.028	0.007	0.053	0.014	0.044	0.011	0.058	0.015
Public Housing	0.286***	0.096***	-0.132	-0.004	0.221**	0.076***	0.137	0.052
	0.049	0.015	0.108	0.038	0.069	0.021	0.101	0.029
Public Housing*Male	-0.310***	-0.082***	0.226*	0.045	-0.031	0.014	-0.053	-0.026
	0.045	0.012	0.105	0.030	0.067	0.017	0.083	0.022
Public Housing *Low Income Public Housing	0.223**	0.098**	0.117	0.058	0.088	0.047	-0.347	-0.107
	0.075	0.032	0.183	0.096	0.092	0.038	0.193	0.085
Public Housing *Low Income Public Housing*Male	-0.358***	-0.129***	0.170	0.067	-0.202*	-0.089*	0.608**	0.242**
	0.080	0.030	0.208	0.096	0.100	0.035	0.202	0.078
Observations	1,120,000	1,120,000	437,000	437,000	325,000	325,000	269,000	269,000

Note: Each column displays a household fixed effects estimate of the effect of teenage participation in subsidized housing on the inverse hyperbolic sine of total earnings between 2008 and 2010. See 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. Standard errors, clustered at the household level, are displayed below each point estimate. *** p<=0.001, ** p<=0.01, * p<=0.05.

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

	Dummy Treatment (Ever in Program)			Dose Treatment (Years in Program)		
	HFE	HFE PRED	HFE IV	HFE	HFE PRED	HFE IV
	(1)	(2)	(3)	(4)	(5)	(6)
Voucher Housing	0.191***	0.178***	0.205**	0.080***	0.042***	0.050***
	0.033	0.048	0.069	0.009	0.012	0.015
Voucher Housing*Male	-0.402***	-0.411***	-0.482***	-0.098***	-0.097***	-0.115***
	0.028	0.028	0.032	0.007	0.007	0.008
Public Housing	0.360***	0.269***	0.333**	0.117***	0.048**	0.057*
	0.043	0.067	0.103	0.013	0.019	0.024
Public Housing*Male	-0.418***	-0.496***	-0.590***	-0.108***	-0.118***	-0.140***
	0.038	0.039	0.046	0.011	0.010	0.012
First Stage Estimates						
Predicted Voucher Housing			0.696***			0.833***
			(0.004)			(0.003)
Predicted Voucher Housing*Male			-0.001			-0.001
			(0.002)			(0.002)
Predicted Public Housing			0.646***			0.803***
			(0.006)			(0.005)
Predicted Public Housing*Male			-0.005*			-0.014***
			(0.003)			(0.003)
Age by Male Fixed Effects	yes	yes	yes	yes	yes	yes
Household Fixed Effects	yes	yes	yes	yes	yes	yes

Note: Number of observations = 1,112,000 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 inverse hyperbolic sine of total earnings between 2008 and 2010 is the dependent variable. Columns 1-3 present estimates with an indicator for whether an individual ever participated in the specified subsidized housing program as a teenager. Columns 4-6 present estimates with a county of the number of years an individual participated in the specified subsidized housing program as a teenager. Columns 1 and 4 replicate the household fixed effects specifications from Table 3. Columns 2 and 4 use a predicted participation measure in place of the individuals observed participation (HFE PRED). Predicted participation is defined using the observed subsidized housing participation of the head of household from the 2000 Census and the ages of household members. Columns 3 and 6 show instrumental variables estimates of the participation in subsidized housing using predicted participation and its male interaction as instruments for observed participation and its male interaction (HFE IV). Standard errors, clustered at the household level, are displayed below each estimate. *** p<=0.001, ** p<=0.01, *p<=0.05.

Table 11: The Effect of Teenage Residence in HUD-Subsidized Housing on Adult Earnings By Average Time Spent on a Waitlist Prior to Program Admission in 2000 County of Residence

	Dummy Treatment (Ever in Program)		Dose Treatment (Years in Program)	
	<=9 Months Wait	>9 Months Wait	<=9 Months Wait	>9 Months Wait
	(1)	(2)	(3)	(4)
Voucher Housing	0.135**	0.257***	0.078***	0.082***
	0.044	0.048	0.012	0.013
Voucher Housing*Male	-0.358***	-0.450***	-0.093***	-0.102***
	0.039	0.040	0.010	0.010
Public Housing	0.361***	0.378***	0.120***	0.114***
	0.053	0.072	0.017	0.021
Public Housing*male	-0.420***	-0.455***	-0.109***	-0.114***
	0.049	0.062	0.014	0.017
Male	-0.363***	-0.363***	-0.373***	-0.380***
	0.035	0.036	0.035	0.036
Constant	8.451***	8.619***	8.421***	8.608***
	0.024	0.025	0.024	0.025
Number of observations (rounded)	581,000	539,000	581,000	539,000
P-Values: Treatment Effects are Equal Below and Above 9 Months Wait				
Females in Voucher Housing	0.0620		0.809	
Females in Public Housing	0.850		0.820	
Males in Voucher Housing	0.667		0.801	
Males in Public Housing	0.846		0.682	

Note: Each column presents a household fixed effects estimate of HUD-subsidized housing participation while a teenager on the inverse hyperbolic sine of total earnings between 208 and 2010. See text for a more detailed description of the sample. 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 for each program. The weights used for each average are the number of teenagers observed in a given 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 county-level wait time is greater than 273 days and below nine months if it is less than or equal to 273 days. Bottom panel displays p-values from tests of whether the effect of each program is the same for teenagers from counties with long (>9 months) wait times and short (<= 9 months) wait times. Standard errors, clustered at the household level, are displayed under each estimate. *** p<=0.001, ** p<=0.01, * p<=0.05.

Table 12: Instrumental Variables Estimates of Teenage Public Housing Participation

	OLS	IV	HFE	HFE IV
Panel A: Main Estimates	(1)	(2)	(3)	(4)
Public Housing	-0.020***	0.598***	0.116***	0.215
	0.006	0.037	0.014	0.280
Public Housing*Male	-0.102***	-0.168***	-0.110***	-0.176**
	0.009	0.049	0.011	0.062
Age by Male Fixed Effects	yes	yes	yes	yes
Age 15 to 18 Average County Characteristics	yes	yes	yes	yes
Household Fixed Effects			yes	yes
Public Housing Supply Instruments		yes		yes

Panel B: First Stage Estimates	No Household Fixed Effects		Household Fixed Effects	
	Public Housing	Public Housing *Male	Public Housing	Public Housing *Male
Residual Public Housing Supply (Average Ages 15 to 18)	3.443***	-0.035**	2.442***	-0.473***
	0.044	0.013	0.124	0.140
Residual Public Housing Supply*Male (Average Ages 15 to 18)	-0.127*	3.333***	-0.091**	3.359***
	0.059	0.042	0.035	0.057

Note: Number of observations = 888,000 rounded to the nearest thousand. In each column of Panel A, the dependent variable is the inverse hyperbolic sine of total earnings between 2008 and 2010. The sample is limited to households that contained no teenagers who participated in voucher assisted housing while still a teenager. All columns include controls for the average (between ages 15 and 18) natural log of county population, median rent to median household income, fraction white, fraction over age 65, fraction of households in poverty, fraction of households that are female-headed, fraction of adults over 25 with a college degree, total employment, and average annual earnings experienced by each teenager based on their 2000 county of residence as well as a full set of age by male fixed effects. Column 1 presents the ordinary least squares (OLS) estimates. Column 2 presents instrumental variables (IV) estimates using the average, between age 15 and 18, residual and its male interaction from by-year county-level public housing supply specifications as an instrument for participation in Public Housing. Column 3 presents the household fixed effects (HFE) estimates. Column 4 presents household fixed effects instrumental variables (HFE IV) estimates using the same instrument as in Column 2. Panel B presents the first stage coefficients on the excluded instruments and their standard errors. In all columns, standard errors are clustered at the household level. *** p<=0.001, ** p<=0.01, *p<=0.05.

Table 13: Treatment Effects by Subsidized Housing Participation in 1997

	(1)	(2)
Years in Voucher Housing	0.080***	0.096***
	0.009	0.013
Years in Voucher Housing*Male	-0.097***	-0.115***
	0.007	0.012
Years in Voucher Housing*Household in Subsidized Housing in 1997		-0.063***
		0.018
Years in Voucher Housing*Male*Household in Subsidized Housing		0.099***
		0.018
Years in Public Housing	0.117***	0.124***
	0.013	0.020
Years in Public Housing*Male	-0.112***	-0.104***
	0.011	0.018
Years in Public Housing*Household in Subsidized Housing in 1997		-0.052
		0.027
Years in Public Housing*Male*Household in Subsidized Housing		0.060*
		0.024
Male*Household In Subsidized Housing in 1997		-0.359***
		0.053
Number of observations (rounded)	1,119,000	1,119,000
Age by Male Fixed Effects	yes	yes
Household Fixed Effects	yes	yes
Household in Subsidized Housing in 1997 Interactions		yes

Note: Table presents household fixed effects estimates of teenage participation in HUD-subsidized housing on the inverse hyperbolic sine of total earnings between 2008 and 2010. Column 1 replicates the main household fixed effects estimates from Column 8 of Table 3. 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 (public and voucher assisted) and whether the teenager's household participated in subsidized housing in the first available year of administrative data (1997). An indicator for whether the teenager's household participated in subsidized housing in 1997 and its interaction with the male indicator are also included. Standard errors, clustered at the household level, are shown below the point estimates. *** p<=0.001, ** p<=0.01, * p<=0.05.

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

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

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

While Section 8 subsidized housing began as project-based housing subsidy in 1974 and at that time was based on new construction, now much of the housing historically referred to as Section 8 housing is found in the tenant-based HCVP program. HCVP has developed more recently and is solely a demand-side, tenant-based subsidy program. Stemming from the ambitious Experimental Housing Allowance Program of the 1970s (see Friedman and Weinberg 1982, 1983) this program brings a different perspective to housing policy by separating itself from new production. Rather than choosing among specific subsidized housing locations, voucher recipients may live in any structurally adequate rental housing in a specified rent and size range, with the Federal subsidy making the unit affordable. Public Housing Authorities may to allocate up to 20 percent of their HCVP funds for project-based vouchers that are tied to specific private housing developments, rather than to the tenant. Tenant vouchers can be used by those wishing to live in Low Income Housing Tax Credit housing (described below) and thus there is the potential for multiple types of subsidies for a given unit. This program provides anonymity and a choice of locations, although landlord willingness to participate limits its extent. There were about 1.1 million voucher households in 1990, growing dramatically to 1.8 million in 2000, and continuing to grow. Currently, over 30 percent of U.S. subsidized housing is provided by vouchers.

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

Table A1. Total Subsidized Rental Dwelling Units, 1990, 2000, and 2008

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

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

* Data not readily available.

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

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

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

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

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

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

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

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

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

Table C1: Pre-First Stage Estimates

	Public Housing	
	OLS	OLS with BLS Employment and Earnings Controls
	(1)	(2)
Log Population	0.003	0.004*
	0.002	0.002
Fraction Below Poverty	0.185**	0.192**
	0.062	0.063
Fraction White	-0.005	-0.008
	0.023	0.023
Fraction Over 65	0.278***	0.287***
	0.055	0.054
Fraction adults with college degree	0.042	0.039
	0.026	0.029
Median rent to median income	-2.473	-2.514
	1.315	1.344
Fraction female-headed	0.598***	0.583***
	0.123	0.122
Constant	-0.030	-0.038
	0.038	0.040
Number of observations (rounded)	28,000	28,000
R-squared	0.337	0.338

Note: Each column presents an ordinary least squares estimate of county-level participation in a subsidized housing program. In Columns 1 and 2, the dependent variable is the fraction of county residents below 80% of area median income who participated in public housing in a given year. All columns include state fixed effects (not shown) and include one observation from each year in the 1997-2005 period. In practice, the residuals used in Table 12 are generated using the by-year county-level regression analog to the estimates displayed above. In addition to the displayed controls, Column 2 also controls for county level total employment and the average county annual wage. *** p<=0.001, ** p<=0.01, * p<=0.05.