From Marcy to Madison Square? The Effects of Growing Up in Public Housing on Early Adulthood Outcomes

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Abstract

This paper studies the effects of growing up in public housing in New York City on children's long-run outcomes. Using linked administrative data, we exploit variation in the age children move into public housing to estimate the effects of spending an additional year of childhood in public housing on a range of economic and social outcomes in early adulthood. We find that childhood exposure to public housing improves labor market outcomes and reduces participation in federal safety net programs, particularly for children from the most disadvantaged families. Additionally, we find there is some heterogeneity in impacts across public housing developments. Developments located in neighborhoods with relatively fewer renters and higher household incomes are better for children overall. Our estimate of the marginal value of public funds suggests that for every $1 the government spends per child on public housing, children receive $1.40 in benefits, including $2.30 for children from the most disadvantaged families.

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1 Introduction

Social scientists have long understood the potential negative consequences of growing up in low-income neighborhoods on children's outcomes (Wilson, 1987; Mayer and Jencks, 1990). At the same time, there has been growing recognition of the role federal housing policy has played in concentrating low-income individuals in disadvantaged areas (Massey and Denton, 1993; Rothstein, 2017). Partly in response to these concerns, federal low-income housing policy has evolved in an effort to deconcentrate poverty.

One element of the federal government's policy has been to promote the demolition of public housing, which has long had a negative perception among politicians and the public. As a result, the public housing stock has shrunk by roughly a quarter since its peak in the 1990s. The government has instead shifted to providing affordable housing in two key ways. First, the government has increasingly turned to providing housing vouchers to low-income families to help secure rentals on the private market. Overall, the number of housing vouchers provided by the government has doubled since the widespread demolition of public housing began in the 1990s. Second, the government has substantially increased expenditures on the Low-Income Housing Tax Credit (LIHTC), offered to developers of multifamily buildings on the condition that they reserve units for low-income families. Today, more than twice as many people live in LIHTC buildings as in public housing, making it the single largest source of affordable units in the country.

Despite this sea change in how the government supports families in securing affordable housing, there is relatively little research on the long-term consequences of living in government-subsidized developments. To address this gap, we study the effects of growing up in public housing on children's outcomes in early adulthood. Using administrative data from the Department of Housing and Urban Development (HUD), we identify individuals who lived in public housing in New York City during childhood. We link these children to tax records and to administrative data from state and local agencies to track their labor market outcomes, safety net participation, involvement with the criminal justice system, family formation, and mortality.

For our baseline results, we exploit variation in the age children move into public housing to estimate the effect of spending one additional year of childhood in public housing. To attenuate concerns about selection bias, we restrict attention to individuals who lived in New York City public housing at some point during childhood.

We find that growing up in public housing leads to modest improvements in children's outcomes in early adulthood. We find that spending one additional year of childhood in public housing increases an individual's earnings by .45 percentiles within their birth cohort earnings distribution. We estimate that this amounts to an increase of about \$247, or 1.3% of the overall mean. We also find that employment increases by .12 percentage points (.2% of the mean) and participation in safety net programs falls by .4 (.7% of the mean) for each year a child spends in public housing.

Impacts on other outcomes are more mixed. While we find no evidence of effects on incarceration or mortality on average, there are heterogeneous effects by age of entry: Among children who enter public housing when they are young, increased time in public housing leads to lower incarceration and mortality rates, while the opposite is true for children who enter at older ages, though impacts on mortality are not precisely estimated.

We leverage unique elements of New York City's public housing program to investigate heterogeneity across families with different needs. Public Housing Authorities (PHAs), the local agencies that typically own and operate public housing locally, have some leeway in developing their own processes for admissions. The New York City Housing Authority (NYCHA) groups applicants into two groups depending on family circumstances. NYCHA has different assignment rules for each group and, furthermore, tends to steer groups to different types of developments. For this reason, it may be important to conduct analysis separately for the two groups. Since we do not observe how NYCHA classifies families, we generate a proxy for group assignment. Specifically, we rank children on family income within an admission year and group children based on whether their family income is above or below the median in their admission year. We believe this procedure generates groupings that reasonably approximate NYCHA's.

We find that children from the most disadvantaged households – those from families with incomes in the bottom half of their admission year distribution – see even larger gains. Children from these families who spend one additional year in public housing experience a .64 percentile increase in earnings (about \$354, or 1.9% of the mean), a .3 percentage point increase in employment (.4% of the mean), a .5 percentage point decline in safety net participation (.8% of the mean), and a .3 percentage point reduction in the likelihood of being a teen parent (1.5% of the mean). Point estimates of impacts on incarceration and mortality are also negative, though are not statistically significant.

We also explore heterogeneity by race/ethnicity and by gender. While point estimates of effects are consistently larger in magnitude for Black individuals than for Hispanic individuals, differences are not statistically significant. When we examine differences by gender, we do find that women benefit much more from a year in public housing in childhood than men. Impacts on earnings, for example, are about twice as large for women as for men.

Our empirical strategy rests on the assumption that the age a child first moves into public housing is uncorrelated with potential outcomes, conditional on observables. We justify this assumption partly by appealing to features of New York City's public housing program. Given the high demand – there are currently about twice as many families on the waitlist as there are families living in public housing – and low turnover, the program suffers from long waitlists. Moreover, the waitlist sometimes becomes so long that it is temporarily closed. For these reasons, families cannot precisely time when they will move into public housing.

Nevertheless, we may be concerned that the age a child enters public housing is correlated with family characteristics in ways that affect long-term outcomes. We conduct a series of tests and show that children who enter public housing at earlier ages are in fact more likely to have characteristics that predict slightly worse outcomes in adulthood, which would bias *against* our findings that children with more exposure (i.e., those who enter at earlier ages) see long-term gains.

We test the sensitivity of our main results using a siblings design, where we compare the outcomes of siblings who spend varying amounts of their childhood in public housing. When we estimate exposure effects using a model with household fixed effects and only individual demographic characteristics that may vary within a household (e.g., gender), we find effects that are generally much larger than those found in our main specification. For example, we find that individuals who spend one more year in public housing see a .73 percentile increase in their earnings rank relative to their siblings. One concern with this design is that results may be driven by economic conditions in the year outcomes are measured, rather than by differences in childhood exposure to public housing. For this reason, our preferred specification for the siblings design includes birth year fixed effects. Including both household and birth year fixed effects generates large standard errors, leading to estimates that are not precisely estimated. Nevertheless, point estimates are consistent with our main findings.

There has long been an emphasis in the social sciences on the importance of neighborhood environment on individuals' outcomes. Given the heterogeneity in the spatial distribution of developments in New York City, we examine the extent to which development assignment drives changes in children's long-run outcomes. We first estimate development effects by adapting our model for estimating childhood exposure effects to allow exposure effects to vary by development. We find evidence of some heterogeneity across developments. For the full sample, the effect of spending an additional year in public housing on an individual's earnings rank is .13 percentiles higher for children assigned to developments that are one standard deviation higher in quality, 29% of the mean effect. Spending one more year in developments that are one standard deviation better increases earnings by .26 percentiles for children from the most disadvantaged families (42% of the mean) and by .29 percentiles for children in less disadvantaged families (88% of the mean).

We next investigate which characteristics of a development are most strongly correlated with development effects. Overall, we find that places with relatively more homeowners and with greater household incomes have higher quality developments. Developments located in neighborhoods with lower poverty rates and immigrant shares also tend to have more positive effects on earnings, though estimates are not statistically significant.

Lastly, we use the framework proposed by Hendren and Sprung-Keyser (2020) to estimate the marginal value of public funds (MVPF), defined as the ratio of benefits to net government costs. Based only on the changes to children's earnings and program participation, we estimate a MVPF of 1.4 for the full sample, 2.3 for children from the most disadvantaged families, and 1.1 for children from less disadvantaged families.

This paper contributes to several strands of literature. Most directly, it speaks to work on the effects of subsidized housing on children. Most of the existing work on the causal effects of housing assistance on children focuses on the impacts of housing vouchers. One prominent set of studies analyzes the effects of the Moving to Opportunity (MTO) experiment, where families living in public housing were randomly assigned to receive housing vouchers. In general, studies of MTO find that vouchers had no effect on children's educational achievement and physical health, and find mixed results on mental health and behavioral outcomes, with generally positive effects for girls and negative effects for boys (Sanbonmatsu et al., 2006; Kling et al., 2005; Kling et al., 2007; Gennetian et al., 2012; Ludwig et al., 2013). Chetty et al. (2016) find that children whose families receive vouchers and move to lower poverty neighborhoods when the children are young see gains in educational attainment and labor market outcomes in early adulthood. One limitation of MTO studies is that they compare children who receive housing vouchers to those who live in public housing. Jacob et al. (2015) instead study the effects of a lottery for housing vouchers to estimate the effects of housing vouchers relative to no rental assistance. They similarly find no effect of vouchers on education, health, or crime outcomes.

Much of the literature on the impacts of public housing comes from work analyzing the demolitions of distressed public housing. Jacob (2004) studies public housing demolitions in Chicago and finds no effects on student achievement for children displaced by the demolitions. In a related study, Chyn (2018) examines the long-run effects of Chicago's public housing demolitions and finds that displaced children have improved labor market outcomes and educational attainment, and are less likely to be arrested in adulthood. Haltiwanger et al. (2020) build on this work to examine the effects of demolitions nationally and similarly find that displaced children have higher earnings in adulthood.

One challenge in interpreting these studies – mirroring a similar challenge with MTO studies – is that many families who were displaced were typically offered housing vouchers or places in other, generally less distressed, public housing developments. We instead build on research that instead attempts to identify the impacts of living in public housing against a more general counterfactual, where individuals may not receive rental assistance at all. Currie and Yelowitz (2000) develop an instrumental variables design that exploits family structure and find that children who live in public housing are less likely to suffer from overcrowding or to be held back in school. There is also some research that studies the effects of growing up in public housing and test scores. Evidence from England (Weinhardt, 2014) and Wisconsin (Carlson et al., 2019) find no effects on student achievement. Han and Schwartz (2024) exploit variation in the age children move into public housing and find that children who move into public housing in New York City do see improvements in test scores, particularly among those that move into higher income neighborhoods.

To the best of our knowledge, Pollakowski et al. (2022) is the only paper other than this one that produces causal estimates of the long-run impacts of growing up in public housing without comparisons to voucher holders. They exploit within-household variation in teenage exposure to rental assistance to estimate the effects of public housing and housing vouchers separately. They find that greater exposure to both programs increases earnings and reduces incarceration rates in early adulthood.

We build on their work in several ways. First, we look at a more general set of outcomes beyond solely labor market outcomes and incarceration. Second, we expand our analysis to examine impacts on children of all ages, not just teenagers. Third, by focusing narrowly on one city, we are able to leverage certain features of the program to learn about heterogeneity in impacts for children from families with different levels of resources. Finally, by relying on a different source of identification, we are able to investigate heterogeneity in outcomes across public housing developments.

We also contribute to a growing body of research documenting the long-term consequences of investments in childhood. There is a large body of work documenting the importance of childhood environment and circumstances to well-being in adulthood (for a recent review, see Almond et al., 2018). Much of this work has focused on understanding the consequences of poverty and, conversely, the benefits of income supports during childhood. Consequently, there is considerable evidence on the short-term effects of receiving cash assistance and in-kind transfers during childhood (for recent reviews, see The National Academies of Sciences, Engineering, and Medicine, 2019; Aizer et al., 2022). The literature documents substantial improvements in health, educational achievement, and behavioral outcomes in childhood in response to a range of childhood investments.

A smaller but growing literature documents the longer-run effects of government investments during childhood. Research finds that access to cash welfare in childhood increases educational attainment, income, and longevity (Aizer et al., 2016). Bastian and Michelmore

(2018) find that children whose families are potentially eligible for higher benefits from the Earned Income Tax Credit (EITC) see increases in educational attainment and labor market outcomes in early adulthood. Recent work also documents the benefits of in-kind transfers, beyond the effects of rental assistance discussed above. Studies find that greater access to SNAP in childhood leads to significant long-term improvements in health, educational attainment, labor market outcomes, and mortality (Hoynes et al., 2016; Bailey et al., 2023). Evidence from Medicaid finds that investments in public health insurance programs have long-term (Goodman-Bacon, 2021) and intergenerational (Miller and Wherry, 2019; East et al., 2023) benefits on health outcomes. Studies also find that Medicaid leads to long-run improvements in educational attainment and labor market outcomes (Cohodes et al., 2016; Brown et al., 2019; Goodman-Bacon, 2021).

Lastly, we build on the growing body of work that seeks to understand the consequences of place for children from families who benefit from subsidized housing. There is a long literature in the social sciences documenting the consequences of growing up in disadvantaged neighborhoods on outcomes in adulthood (e.g., Wilson, 1987; Mayer and Jencks, 1990). More recent work estimates credible, causal estimates of place, reinforcing the importance of place to life outcomes (Chetty and Hendren, 2018; Chetty and Hendren, 2018). While there is a burgeoning literature estimating place effects and investigating mechanisms (see Chyn and Katz, 2021, for an overview), we see this paper in dialogue with a subset of the literature that uses the structure of rental assistance programs to understand the consequences of place for disadvantaged families. Research finds that, among children whose families receive housing vouchers, benefits accrue to families that move to neighborhoods that have lower poverty rates or are less segregated (Chetty et al., 2016; Chyn et al., 2023). Han et al. (2020) exploit the random assignment of individuals to units within developments in New York City to estimate how proximity to fast food establishments affects obesity among children who live in public housing. Billings et al. (2022) similarly leverage random assignment to public housing units in Denmark to examine the relationship between neighborhood characteristics and children's outcomes. We build on this work by instead generating credible causal estimates of developments and examining the relationship between developments' effects and their neighborhood characteristics.

The rest of this paper is structured as follows. Section 2 provides some background on public housing in the United States generally and in New York City in particular. We describe the data and sample in Section 3. We introduce the empirical strategy used to estimate the effects of spending an additional year in childhood in public housing in Section 4. In Section 5, we present the main results. In Section 6, we investigate heterogeneity in impacts across public housing developments. We estimate the marginal value of public funds in Section 7.

Section 8 concludes.

2 Background

Overview of the Public Housing Program

Established in the Housing Act of 1937 as a part of the New Deal, the public housing program is the nation's oldest major federal rental assistance program. The program provides housing at subsidized rents to low-income families, the elderly, and to individuals with disabilities. Nationally, the public housing program currently serves nearly two million individuals living in roughly one million households, making it an important support for low-income renters (Center on Budget and Policy Priorities, 2021).

Like many New Deal-era programs, public housing has a unique structure that emphasizes cooperation and a division of labor between different levels of government. At the federal level, the program is overseen by the Department of Housing and Urban Development (HUD). The federal government provides localities with subsidies for the construction, operation, and maintenance of public housing developments. The government also establishes federal regulations and guidelines for the program. Most visibly, HUD establishes upper income limits for eligibility, which it sets to 80% of local area median income (AMI), which HUD calculates separately for different geographic areas each year.

To administer the program, the federal government contracts with over 3,000 public housing authorities (PHAs) across the country. PHAs are local housing agencies that typically own and operate the public housing developments they oversee. While subject to federal rules and regulations, PHAs do have considerable scope to shape the program within their jurisdiction. Importantly, they have flexibility in developing their admissions policies. As a result, some PHAs have historically targeted assistance to the most vulnerable families, while others have attempted to recruit residents from a somewhat wider range of socioeconomic backgrounds. PHAs can influence the development of their programs in other ways, both obvious and subtle. For instance, PHAs might influence the character of public housing in their city through their decisions on what type of housing to build and where to build it. They also play a role in determining residents' experiences through investments in management or security. Given PHAs' wide latitude in developing their programs, there is substantial heterogeneity in the experiences of public housing residents across the country.

The Public Housing Program in New York City

In this paper, we study the effects of growing up in New York City public housing. The New York City Housing Authority (NYCHA) is the largest housing authority in the country. Its public housing program alone serves over 300,000 individuals in roughly 150,000 families, making it home to about one in six public housing residents nationwide. These families are spread across about 2,100 buildings in 134 developments across New York City's five boroughs (New York City Housing Authority, 2023).

Given the large number of public housing developments and their spatial distribution across the country's most populous city, families that live in New York City's public housing are exposed to a range of living conditions and neighborhood environments (Dastrup and Ellen, 2016). For example, using the 2018-2022 five-year American Community Survey, we estimate New York City zip codes with public housing units had an average poverty rate of roughly 19%. However, a quarter of these zip codes had poverty rates of 11%, lower than about half of zip codes nationwide. On the other hand, another quarter of zip codes with public housing developments had poverty rates above 25%, placing them among the poorest 10% of zip codes in the US.

Besides its sheer size, NYCHA's public housing program has traditionally differed from those in other large cities in a crucial way: historically, NYCHA's program has served a population from a broader range of socioeconomic backgrounds. As mentioned above, PHAs have leeway in developing their own admissions policies.[1] Since the onset of the program, many large cities admitted only the most vulnerable residents. New York City, however, developed policies to encourage a somewhat broader mix of tenants, sometimes placing the agency at odds with federal administrators. Historically, NYCHA administrators defended their policies by arguing that having a mix of residents would help the agency raise enough revenue from rents to support maintenance costs. They also believed that having working families in developments would be beneficial to the poorest tenants (Bloom, 2008, p.81).

As cities fell into decline in the latter half of the twentieth century, NYCHA's population became more disadvantaged over time. This was due partly to NYCHA loosening admissions standards in response to political pressure (from above and below), budgetary strains, and judicial challenges (Bloom, 2008, Ch.11). Consequently, by the mid-1990s, roughly one-third of families in public housing were on welfare and just under one-third were identified as working families (Bloom, 2008, p.247). Throughout the 1990s, NYCHA began revising its policies to give preference to working families and re-balance its tenant mix. By the mid-2000s, roughly half of all new tenants were categorized as working families.

[1] The information in this section is based on Bloom (2008), which provides a history of NYCHA's public housing program from its inception.

NYCHA's Tenant Selection Process

Across the country, public housing programs are typically greatly oversubscribed. That is, there are considerably more families that are eligible and that desire public housing than there are available units. As a result, PHAs generally have long waitlists. In New York City, for instance, families who are eventually offered admission are on the waitlist for upwards of three years before moving into public housing (Han and Schwartz, 2024).[2] Occasionally, waitlists becomes so long that PHAs close applications to the program altogether.

To manage this challenge, PHAs develop their own admissions policies. Below, we share details of NYCHA's tenant selection and assignment plan.[3] Understanding this process informs our identification strategy and aids in the interpretation of results.

Shortly after applying for public housing in New York City, NYCHA classifies each family to one of two "priority groups" based on information families share on the application. Families are categorized to the "working family" priority group if they have income between 30% and 80% AMI, or if they have incomes below 30% AMI and work 20 hours per week, receive unemployment benefits, or receive some form of disability payments. Families are assigned to the "need based" priority group if they are referred to NYCHA from certain city agencies, or meet certain other conditions. Families may be assigned to both priority groups, and their priority group may change over time if families' situations change.

After an eligibility interview, NYCHA presents families with a list of developments that the housing authority anticipates will have openings in the near term, and which are located in one of up to two boroughs that applicants' specify. In practice, NYCHA only offers families a choice between a small number of developments. If families reject all developments on the list, their application is closed and they must restart the process.

Once families select a development, they are placed on a development-specific waitlist based on application date and other preferences, such as income. When there is an opening for a unit, NYCHA first prioritizes current residents in extenuating circumstances (e.g., families in apartments deemed uninhabitable). Thereafter, NYCHA rotates offers between families in five groups: current residents in underoccupied units, current residents in overcrowded units, current residents that meet certain other conditions, new applicants in the working family priority group, and new applicants in the need based priority group. NYCHA may offer families choices of up to two units within a development, but if families reject both

[2] This estimate is conservative because it does not include the time between the initial application and when families are offered a place on a development- or borough-wide waitlist (discussed further below). This statistic also does not capture individuals who apply and are ultimately not placed on a waitlist or who do not move into public housing, who together make up the majority of applicants.

[3] The information in this section comes from New York City Housing Authority (2020) and is supplemented with discussions with NYCHA staff.

offers, their application is closed and they must restart the entire process.

The assignment process differs some for families in particularly difficult circumstances. NYCHA classifies some families – usually those that are referred by the city's Department of Homeless Services – as "emergency applicants." These families are given the highest preference for admission within their priority group, but are not given a choice of development and are instead placed on a borough-wide waitlist.

Though families have had limited choice in choosing their development in recent years, developments may nevertheless differ in the characteristics of their residents, including new admissions. In the last three decades, NYCHA has attempted to steer more working families to the most disadvantaged developments through their offer process (Bloom, 2008, p.248). Additionally, once NYCHA offers families a choice between developments, families can reject all offers. If certain types of families disproportionately reject offers to certain developments, then this may generate differences between populations of different developments.

3 Data

To estimate the effects of public housing on children's long-run outcomes, we rely on restricted-use administrative data from a number of sources, hosted by the US Census Bureau. We use data from the Department of Housing and Urban Development (HUD) to identify individuals who lived in New York City's public housing as children. We then link these individuals to a range of administrative data to construct measures of outcomes in early adulthood. We summarize this process below. Additional details can be found in Appendix B.

3.1 Sample Construction

For each dataset we use, Census assigns each individual a Protected Identification Key (PIK) – an anonymous person identifier intended to be as unique as a Social Security Number – using personally identifiable information (PII). These PIKs enable us to link individuals across datasets hosted by Census without accessing PII ourselves.[4]

We construct our sample using longitudinal administrative data from HUD's Public and Indian Housing Information Center (PIC), which is intended to cover the universe of individuals who lived in public housing each year from 1995 onwards. We drop the small share of individuals who are not successfully assigned a PIK since we cannot reliably link these individuals within the HUD data longitudinally, or to other data sources at all.[5] We restrict

[4]See Wagner and Layne (2014) for details on the PIK assignment process.

[5]Over 96% of individuals in the HUD data are successfully assigned a PIK between 1995-2014, the range of years of data we use in this analysis. The PIK rate ranges from 92% in 1995 to over 99% in 2014. Bond

our focus to individuals who ever lived in New York City public housing, and further exclude a small share of individuals who either lived in public housing elsewhere or who received federal rental assistance through another HUD program (e.g., housing vouchers) before moving into public housing in New York City.[6]

Next, we identify the calendar year individuals first moved into public housing. Although we do know the year individuals first appear in the data, and though the data do generally include a household's admission date, there are data quality issues – particularly in the earliest years data are available – that make it challenging to determine precisely when an individual was actually admitted to public housing. Nevertheless, we are able to develop a method to assign individuals an admission year. Our sample includes only those individuals for whom we are most confident in our imputation method. The majority of those we exclude from our sample due to lack of confidence in the imputed admission date are those who most likely entered the program before 1995, the first year HUD data are available. We discuss data quality challenges and explain our routine for imputing admission years at length in Appendix B.

We then link individuals to the Social Security Administration's Numident file to get each individual's date and country of birth. Using the date of birth and the imputed admission date, we can calculate each individual's age on December 31 of the year they were admitted and restrict our sample to those who were 17 or younger at admission, and who were not the head of a household, or spouse of a household head, when they move into New York City public housing.

To demonstrate the robustness of our results, we additionally estimate impacts using a siblings design (discussed in further detail in Section 4). We construct the sample used in these analyses by subsetting our main sample to include just individuals from households that had at least two individuals who lived in New York City public housing during childhood.

Finally, throughout this paper, we separately present results for all analyses for two subsamples. NYCHA categorizes applicant families to either the "working family" or "need based" priority groups under certain conditions. As detailed in Section 2, the priority group classification is used in the admissions process in two key ways. First, when there is an apartment available for new applicants, NYCHA alternately selects applicants from these two categories of families. Second, NYCHA may offer individuals in different priority groups

et al. (2014) analyze how PIK rates vary based on household and individual characteristics. Of particular relevance to our study, they find that younger children, immigrants, and low-income individuals tend to have lower PIK rates overall.

[6] Among all children who ever lived in New York City public housing between 1995-2014 and turned 26 between 2005-2020 (and therefore may be eligible to be in our sample), 4.2% lived in public housing elsewhere or received rental assistance through another program before moving into New York City public housing.

choices among different sets of developments. For these reasons, it is important to analyze impacts separately for these two groups. However, we do not observe how NYCHA classifies families. Instead, we proxy for these categories based on whether individuals were in families that had above or below median household income in their admission year.[7] This procedure is motivated by the roughly equal distribution of new admissions between the two groups. Those in the lower half of the income distribution tend to have lower earnings,[8] making them less likely to be classified as working families.

3.2 Outcomes

Below we define the key outcomes we analyze in this paper and briefly describe their construction. Note we measure outcomes at, or by, age 26 to avoid measuring outcomes when many individuals may be in school while trying to maximize sample size.

1. *Earnings and Employment:* We have data extracts based on W-2 records for 2005-2020 and from 1040s for 1994, 1995, and 1998-2020. We calculate individual earnings in each tax year by taking the sum of wage earnings across all W-2s. In years where we do not find W-2 records for an individual, we impute $0 of earnings. For our primary analysis, we transform earnings to in-sample, within-birth cohort percentile ranks. We further create an indicator for individual employment in a tax year based on whether annual W-2 earnings are greater than $0. We construct measures of family-level earnings and employment by applying analogous procedures to 1040 data. We construct outcome measures based on labor market outcomes in the calendar year an individual turned age 26.

2. *Safety Net Participation:* We have access to SNAP and TANF administrative data in three states, in years spanning 2004-2020. We construct indicators for whether individuals were ever enrolled in SNAP or TANF in the calendar year they turned age 26.

[7] For some individuals, we assign an admission year that is earlier than the year they appear in the HUD data (we might do this if, for example, an individual appears in public housing in the tax data before they appear in HUD data). Since we calculate an individual's baseline income as the household income their family reports to HUD the first year they appear in HUD data, there may be a gap between the admission year and the year baseline household income is measured for these individuals. As a result, there may be differences between true household income in the admission year and the amount we impute for a number of reasons, not least because of macroeconomic conditions. Therefore, we rank individuals on a measure of household income that accounts for wage growth (for details, see Appendix B). A very small share of individuals move from being above or below median (or vice versa) if we instead switch from the adjusted to the observed measure of household income.

[8] This is not exactly mechanical, since families may have income from sources other than earnings.

3. *Criminal Justice System Involvement:* The Criminal Justice Administrative Records System (CJARS) collects criminal justice records from jurisdictions across the country. It includes data across a variety of domains (arrests, court records, incarceration, probation, and parole), though data availability vary by jurisdiction and year. For some states, data extend from before any individual in our sample is born. In New York state, CJARS includes data on incarceration from 2016-2023. However, we use data from agencies nationwide where available. We construct an indicator for whether an individual was ever incarcerated by the end of the calendar year they turned 26.

4. *Marriage and Fertility:* Using data from 1040s, we can identify whether individuals are married and whether they have any dependents. We have access to 1040s for 1994, 1995, and 1998-2020. We construct indicators for whether an individual ever filed *and* was ever married or ever had a dependent by the end of the calendar year when they turned 26. Additionally, we leverage parent-child linkages from the Census Household Composition Key (CHCK) to identify individuals in our sample who had a child before age 20.

5. *Mortality:* From the Social Security Administration's Numident file, we have information on date of death. Using this data, we construct an indicator on whether an individual passed away by the end of the calendar year they turned 26.

3.3 Summary Statistics

In Table 1, we report summary statistics for the main sample. The table contains information on demographic and family characteristics, measured in the year the family moves into public housing. On average, individuals in our sample move into public housing between ages 10 and 11. The mean birth year among individuals in our sample is 1989 and the mean admission year is 2000.[9] Once they enter public housing, they typically remain there until adulthood. As expected, individuals tend to come from families with very low incomes (note all dollar values in this paper are reported in 2020 dollars). Additionally, nearly 95% of individuals in our sample identify as non-Hispanic Black or Hispanic, compared to about one-third of 26 year olds nationally.[10]

Table A.3 presents the same statistics for the subsample of individuals from households where at least two individuals lived in NYC public housing as children. Overall, the two

[9] There is a wide distribution of admission and birth years among children in our sample. In Table A.1 and Table A.2, we present the distribution of admission and birth years, respectively.

[10] National-level statistics come from our calculations of the 2015 American Community Survey public-use microdata. We use 2015 data since that is the median year when our sample turns 26.

samples are very similar across observable characteristics.

4 Empirical Strategy

4.1 Estimation

In the first part of the paper, we estimate the effects of public housing on children's economic and social outcomes in early adulthood. Specifically, we estimate the effect of spending one additional year of childhood in public housing among those eventually admitted into the program (i.e., the intensive margin effect).[11]

One way to estimate exposure effects would be to estimate the following specification

$$y_i = \alpha^{RF} + \beta^{RF} M_i + X_i' \gamma^{RF} + \delta_{c(i)}^{RF} + \epsilon_i^{RF} \tag{1}$$

where y_i is some early adulthood outcome (e.g., earnings measured at age 26) for individual i. We define $M_i \equiv 18 - A_i$, where A_i is the age a child first moves into New York City public housing. M_i can be thought of as the maximum number of years an individual can spend in public housing during their childhood given the age they enter. X_i' is a vector of child demographics and baseline family characteristics. In our preferred specification, we include controls for family income, family size, family marital status, the borough of the development the individual is initially assigned to, race/ethnicity, gender, nativity (whether an individual was born in the US), and parent nativity. Family income, family size, family marital status, and borough are measured in the year an individual moves into public housing. Family income, size, and marital status are adjusted to account for parent lifecycle effects and family income is adjusted for macroeconomic trends.[12] Finally, $\delta_{c(i)}$ are birth year fixed effects. Note that since all outcomes are defined at a particular age, the birth

[11] We restrict our sample in this way in part to address concerns about selection bias, since families who eventually live in public housing may differ from those who never live in public housing, even if they appear similar on observable characteristics, such as family income. To interpret results, it is important to consider where individuals in our sample may live if not in New York City public housing. Many families face considerable residential instability before moving into New York City public housing, and may access shelters, live with other relatives, or benefit from other HUD rental assistance programs. We discuss families' other options in greater detail in Appendix C.

[12] Values of baseline characteristics may differ among children in part because baseline household characteristics may be measured in different calendar years (and therefore under different economic conditions) or at different points in a parent's lifecycle. To account for these possibilities, we residualize baseline family income on the age of the household head at baseline and a measure of wage growth. For similar reasons, we residualize baseline family size on the age of the household head at baseline. For details on the residualization procedure, see Appendix B.

year fixed effects $\delta_{c(i)}$ double as fixed effects for the calendar year in which the outcome is measured.

Since M_i is a measure of potential years of childhood exposure, β^{RF} can be interpreted as the impact on individuals "assigned" to live in public housing for an additional year in childhood.

It may be of interest to instead estimate the effect of realized, rather than potential, years of childhood exposure to public housing. To estimate the impacts of realized exposure, we first need to calculate the number of years individuals spent in New York City public housing in childhood. One obstacle to calculating realized years of exposure is that, due to data quality issues (particularly in the early years of the data), individuals are sometimes missing in the data when they were likely living in public housing. To overcome this challenge, we calculate childhood years of exposure by subtracting each child's admission year from the last year we observe them living in New York City public housing as a child.[13] One limitation of this approach is that we may overestimate years of exposure for a child that repeatedly enters and exits public housing. In practice, however, we observe only a small share of individuals in our sample (3%) ever leaving New York City public housing as children and, of those who do, few (11%) re-enter during childhood. Given the aforementioned challenges with the administrative data, this is likely an upper bound on the share who leave and on the share who re-enter.

We cannot, however, simply re-estimate Equation 1 by replacing M_i with the actual number of years individuals spent in public housing in childhood, E_i, since the amount of time a family spends in public housing is endogenous. Instead, we instrument for realized exposure E_i using potential exposure M_i. We estimate exposure effects using a two-stage least squares procedure, where the second stage is given by

$$y_i = \alpha^{2SLS} + \beta^{2SLS}\widehat{E}_i + X_i'\gamma^{2SLS} + \delta_{c(i)}^{2SLS} + \epsilon_i^{2SLS} \tag{2}$$

Here, $\widehat{E}_i$ represents the instrumented years of childhood exposure. β^{2SLS} can be interpreted as a rescaling of β^{RF} by a factor equal to the expected share of an additional year a child will live in public housing if they were admitted one year earlier. We cluster standard errors at the household level.

[13] By defining exposure in this way, we do not count years where a child lives in public housing outside of New York City or participates in another federal rental assistance program, such as the Housing Choice Voucher program.

4.2 Identification

The key identifying assumption is that the age a child moves into public housing is uncorrelated with potential outcomes, conditional on observables. This assumption can be justified in part by features of New York City's public housing program. As mentioned in Section 2, the public housing program is significantly oversubscribed and, consequently, the program has long waitlists. Among those eventually admitted, families are typically on the waitlist for over three years, excluding the time between the initial application and their placement on a development- or borough- waitlist, which may be substantial. Additionally, the waitlist frequently closes and so families may not be able to apply for the program even when they'd like to. Together, these facts suggest families cannot precisely time entry into public housing.

Nevertheless, we may be concerned that the age a child enters public housing is correlated with family characteristics or circumstances that are, in turn, correlated with outcomes. We test for this possibility by estimating regressions of child demographics or baseline family characteristics on potential years of exposure, birth year fixed effects, and an indicator for whether an individual was born in the US.[14] For ease of interpretation, we standardize these characteristics by subtracting the sample mean and dividing by the sample standard deviation. We plot the coefficient on potential childhood exposure from each regression in Figure 1. For some variables, such as family income, there is no statistical difference among children with more or fewer potential years of exposure (i.e., between children who enter at different ages). For most characteristics, there are statistically significant differences, but they tend to be small in magnitude. For example, children with more potential years of exposure have families that tend to be larger, but children with one additional year of potential exposure (those who enter one year earlier) belong to families that are only .01 standard deviations larger on average.

To assess how meaningful this imbalance is more systematically, we generate predictions for our outcomes by regressing each outcome of interest (e.g., earnings at age 26) on our full set of controls and on birth year fixed effects. We then repeat the above exercise using the predicted outcomes in lieu of child or family characteristics. This exercise illustrates how the age at which a child enters public housing is related to the component of an outcome

[14] We control for nativity because immigration to New York City during this period rapidly changed the pool of families eligible for public housing. To illustrate this concern, in Figure A.1 we repeat the balance tests but do not control for whether an individual was born in the US. Overall, results are similar but there is much more imbalance on covariates related to race and ethnicity. If we do not control for nativity, we find that children who enter one year earlier are .02 standard deviations more likely to be Hispanic. If we do control for whether an individual was born in the US, the imbalance is cut in half. The results suggest that Hispanic children do tend to move into public housing at older ages, but this is partly due to the fact that New York City is becoming more diverse due to immigration over time. The results in this paper are similar whether we exclude the foreign born altogether or if we include them, as long as we control for nativity.

predicted by baseline family characteristics and child demographics.

We present the results of this exercise in Figure 2. Each coefficient represents the effect of one additional year of potential childhood exposure on some predicted outcome. We see that one additional year of potential exposure is associated with a .02 standard deviation decrease in the portion of rank earnings that can be predicted from baseline characteristics and child demographics alone. The same pattern holds across the other predicted outcomes. We also repeat this exercise separately for our two income groups. The results, displayed in Figure A.3, suggest that the relationship between age of entry and predicted outcomes holds for the two subsamples, though the patterns are attenuated for children from the most disadvantaged families. Overall, this exercise suggests that, for most outcomes, children who enter at younger ages are more likely to have characteristics that predict slightly *worse* economic and social outcomes in early adulthood.

To account for these differences among children who enter at different ages, our main specification includes controls for observable characteristics. However, if children who enter at younger ages are still more disadvantaged than those who enter at older ages – even after controlling for observables – then our results may be biased downwards.

4.3 Household Fixed Effects

We may still be concerned that there exist unobserved household characteristics that are correlated with the age a child enters public housing and their outcomes that are not captured with our set of controls. For example, if individuals from families with weaker support networks enter at earlier ages, and if weaker support networks are associated with worse outcomes, then our main results may be downward biased, even after controlling for observable family characteristics.

To address this concern, we present additional results using a siblings design in Appendix A.[15] For our siblings design, we estimate the effect of spending an additional year in public housing on early adulthood outcomes among children living in the same household. In practice, we adapt Equation 2 by adding household fixed effects and a control for birth order, and we exclude all controls common to individuals within a household (e.g., family income). We then estimate this model on the subset of children who live in families where at least two individuals lived in New York City public housing during childhood.

[15] Though we label this a "siblings" design, children are not necessarily biological siblings. For example, the children we compare might be relatives that both live in the same HUD-assisted household. For this reason, we sometimes alternatively refer to this as our "household fixed effects" design.

5 Childhood Exposure Effects

5.1 First Stage

Table 2 presents the first stage results on the effects of potential years of exposure on realized exposure. The first column reflects estimates for the full sample. The second focuses just on individuals from families with incomes below the median in the year they were admitted, and the third focuses on individuals from families with above median incomes. Throughout the remainder of the paper, we disaggregate all results for these two income groups, as these groups approximate groups NYCHA treats differently throughout the admissions process (see Section 3 for details). In Table A.4, we reproduce this table using our siblings design.

Overall, a one year increase in an individual's potential years of exposure is associated with a .92 increase in the actual number of years spent in public housing in childhood. That is, there is nearly a one-to-one change between the age a child enters public housing and how long they spend there. This suggests that, once children move into public housing, they generally stay there until adulthood. This relationship is stable across our two specifications and across the main sample and the two subsamples.

5.2 Labor Market Outcomes

In Table 3, we present 2SLS estimates of the impacts of spending one additional year in public housing on labor market outcomes measured at age 26.

We find that spending an additional year of childhood in public housing leads to a .45 percentile increase in one's within-cohort earnings rank. To interpret these findings, we regress earnings on within-cohort percentile ranks and birth year fixed effects. We find that a one percentile increase in an individual's earnings rank is associated with a \$550.90 increase in earnings. This implies that spending an additional year of childhood in public housing results in a $.4482 \times \$550.90 = \$246.91$ increase in earnings, or 1.3% of the overall mean. The effects are stronger for children from families with incomes in the bottom half of the income distribution in their admission year. For this group, spending an additional year in public housing leads to a .64 increase in within-cohort percentile rank, which is associated with an increase in earnings of \$354, or 1.9% of the mean. For children from families in the top half of the income distribution, the effect is about half: spending an additional year in public housing leads to a .35 percentile increase in rank earnings, or \$190 (1% of the mean).

The effects are more modest for employment. We find that one additional year of childhood exposure leads to a .12 percentage point increase in employment (.2% of the mean). For children from the most disadvantaged families, the effect is a .3 percentage point increase, or .4% of the mean. The effect on children from less disadvantaged families is just .02 percentage points and is not statistically significant.

Results for family-level labor market measures are similar to those of their individual-level analogues. For example, family-level wages increase by one-third of a percentile for the full sample. To interpret these results, we regress family-level wages on within-cohort percentile wage ranks and birth year fixed effects and find that a percentile increase in family wages is associated with a $556.40 increase in wages. This implies that spending an additional year in public housing in childhood is associated with a $185 increase in family wages, or about 1% of the mean. As with individual earnings, effects are stronger for children from the most disadvantaged families and weaker (but still positive) for the less disadvantaged sample.

Next, we investigate whether there is variation in effects based on the age when individuals move into public housing. We first adapt the reduced form equation presented in Equation 1, replacing the continuous exposure variable with dummies for each potential year of exposure (defined as 18 minus the age an individual moves into public housing). In Figure 3, we plot coefficients from this regression using rank earnings as the dependent variable. Visual inspection shows that there are no effects of relatively few years of potential exposure (that is, for individuals who enter in their teenage years), but effects are approximately linear thereafter. In Figure 4, we break out estimates separately for the two income groups and find the same pattern.

We investigate this heterogeneity more systematically by adapting our 2SLS procedure to include an interaction between exposure years and an indicator for whether an individual had at least six potential years of exposure (i.e., they moved into public housing before age 13). The results, presented in Table 4, suggest that there is limited heterogeneity in impacts based on potential years of exposure overall. However, among the most disadvantaged children, impacts are greater for children who enter at earlier ages. For those who enter as teenagers, a year of public housing leads to a statistically insignificant increase in earnings of .20 percentiles. For children who enter at younger ages, a year in public housing leads to a .62 percentile increase in earnings. The pattern is similar for employment: among the most disadvantaged children, a year in public housing leads to a small, statistically insignificant decline in earnings for children who enter when they are older, but to a large, statistically significant increase for children who enter before age 13.

In Table A.5, we provide tests for the robustness of the main results using our household fixed effects strategy. The point estimates on the impacts on individual earnings ranks are similar for the full sample and the children from the most disadvantaged families. For employment, impacts are about the same for the most disadvantaged children, but negative for the full sample and for less disadvantaged children (though not statistically significant).

For family wages, point estimates are larger than for the main specification. For the full sample and for the most disadvantaged children, the point estimates on family employment are a bit larger. However, standard errors using the household fixed effects design are large and, therefore, impacts are rarely statistically significant.

5.3 Safety Net Participation

In Table 5, we investigate how growing up in public housing affects participation in federal safety net programs. Consistent with the results on labor market outcomes, we find that spending more years in childhood in public housing is associated with lower take-up of safety net programs.

Individuals who spend one additional year in public housing are .36 percentage points less likely to be receiving HUD rental assistance or participating in SNAP or TANF at age 26, a .7% decline relative to the mean.

Declines in SNAP and TANF participation are concentrated entirely among children from families with below median incomes. Individuals from these families who spend one additional year in public housing are .39 percentage points less likely to participate in SNAP (1.2% of the mean) and .15 percentage points less likely to participate in TANF (1.6% of the mean). Estimates for individuals from families with above median incomes are proportionally small and not statistically significant.

We next examine whether there is heterogeneity in impacts by age of entry in Figure 7. Visual inspection shows that there appears to be no impact of living in public housing on safety net participation for individuals with eight or fewer years of potential exposure (those that enter after age 10). Thereafter, impacts appear approximately linear in potential years of exposure. In Figure 8, we break out results separately for the two income groups and find similar patterns.

To test for heterogeneity more systematically, we re-estimate our main results, now with an indicator for whether an individual had at least six potential years of exposure. In Table 6, we find that the impact of spending an additional year in public housing among children with few potential years of exposure is small and statistically insignificant. However, for children with at least six potential years of exposure, spending a year in public housing leads to a .45 percentage point reduction in the likelihood of participating in safety net programs at age 26 (a decline of about .8%). These patterns are qualitatively similar across the two income group subsamples, though impacts are much larger for children from the bottom half of the income distribution.

As before, estimates from our siblings analysis, reported in Table A.7 are broadly consistent with the findings of our main analysis, though standard errors are large. As with the main results, the point estimates suggest that individuals who have more potential years of exposure to public housing are less likely to participate in any federal safety net program, with larger effects for children from the most disadvantaged families, though estimates are not statistically significant. For participation in any program, SNAP, and TANF, the point estimates of the declines are larger than for the main analysis. For HUD, the findings are more mixed across samples and not statistically significant. Unlike in the main analysis, the siblings analysis does find large, statistically significant declines in SNAP and TANF participation among the children from less disadvantaged families.

5.4 Incarceration

In Table 7, we report the effects on ever being incarcerated by age 26. Overall, we find that additional exposure to public housing has no effect on incarceration rates. For children from the most disadvantaged sample, the point estimate corresponds to a .05 percentage point decline (1.6% of the mean). For the less disadvantaged sample, we observe a .04 percentage point increase (1.3% of the mean). However, neither of these estimates are statistically significant.

Visual inspection of Figure 9 suggests there may be heterogeneity based on age of entry. Incarceration rates appear to increase in exposure for children with the least exposure (those who enter when they are older) and decrease for children with the most exposure (those who enter when they are young).

In Table 8, we find evidence of heterogeneity based on potential years of exposure. Among children with fewer than six years of potential exposure (those who move into public housing as teenagers), a year in public housing increases the likelihood of being incarcerated by age 26 by .25 percentage points, or about 8% of the mean. However, for children from families with below median incomes, another year in public housing leads to a decline in the likelihood of being incarcerated of .09 percentage points, or roughly 2.9% of the mean. This pattern holds across the two subsamples.

5.5 Marriage and Fertility

We present estimates of effects on marriage and fertility in Table 9. For the full sample, we find that spending one additional year in public housing leads to a .09 percentage point reduction in marriage by age 26, or about .8% of the mean. This effect is larger for children from less disadvantaged families, who see a .15 percentage point reduction (1.3% of the mean).

Additionally, we find that children who spend an additional year in public housing are .18 percentage points (.4%) less likely to report having a dependent on a tax return by age 26. This effect is larger for children from the most disadvantaged families, who see a .3 percentage point decline in the likelihood of having a child by 26 (.6% of the mean).

The outcomes in this section are constructed by examining whether an individual ever filed a 1040 *and* reported being married or having a dependent on the 1040 at some point by the calendar year when they turned 26. We might be concerned that the results here are driven by endogenous tax filing behavior. This is likely not an issue here for several reasons.

First, from Table 3, we see only modest effects on employment, defined as the share of individuals in families with positive 1040 wages. Second, over 90% of married families with dependents file taxes (Mok, 2017), though this figure may be lower among public housing residents, who tend to have low incomes. Even if there were substantial shifts in tax filing overall, the impact on this group would be attenuated. For both of these reasons, it is unlikely that tax filing response could have a significant effect on these outcomes.

Additionally, we see declines in marriage and fertility despite modest increases in employment. This would suggest that the findings presented in this section are, if anything, underestimates on the decline in marriage and fertility.

We next investigate impacts on teen parenthood among women in our sample in Table 10. Overall, we find no evidence of impacts for the full sample. However, this masks heterogeneity between income groups. For girls from the most disadvantaged families, spending one additional year in public housing leads to a decline in the likelihood of having a child before turning 20 by .3 percentage points, or 1.5% of the mean. For children from less disadvantaged families, spending a year in public housing increases the likelihood of having a child as a teenager by a statistically insignificant .16 percentage points (.8% of the mean).

5.6 Mortality

We next investigate impacts on mortality in Table 12. Overall, we find no evidence that spending an additional year in public housing in childhood has any impact on mortality. Though point estimates vary between subgroups, estimates are statistically insignificant and we can rule out large increases or decreases to mortality.

In Table 13, we repeat our analysis, but allow exposure effects to differ for children with more or fewer years of potential exposure. For children with few years of potential exposure, an additional year of public housing leads to a .03 percentage point increase in mortality for the full sample (3.1% of the mean) and a .08 percentage point increase for children from families in the bottom half of the income distribution (8.2% of the mean), though these estimates are not statistically significant. For children with at least six years of potential exposure, an additional year in public housing leads to a .02 percentage point decline in mortality (2% of the mean), including a .05 percentage point decline for the most disadvantaged children (5.1% of the mean), though, again, these estimates are not significant. Though estimates are not statistically significant, they are broadly consistent with our other findings, which show that benefits tend to accrue to children who enter at younger ages.

5.7 Heterogeneity

Finally, we explore heterogeneity in impacts by race/ethnicity and by gender.

In Table 14, we investigate differences between individuals of different race and ethnic groups. For this analysis, we focus only on individuals who are non-Hispanic Black or Hispanic. Together, these two groups make up nearly 95% of individuals in our sample. To assess heterogeneity in impacts, we adapt our two-stage least squares procedure to include an interaction term between years of exposure and an indicator for whether an individual is Black.

In general, we find no evidence of significant differences by race. For individuals from families with below median incomes, we estimate spending an additional year in public housing leads to a .1 percentile increase in earnings rank and a .1 percentage point increase in employment for Black children relative to Hispanic children, though these estimates are not statistically significant. Additionally, we estimate that Black children who spend an additional year in public housing are also .2 percentage points less likely to participate in safety net programs and are .07 percentage points less likely to be incarcerated relative to their Hispanic counterparts. Again, these estimates are not statistically significant.

Next, we examine heterogeneity by gender in Table 15. Across all samples, we find substantial differences in impacts between men and women in our sample. For earnings, women who spend one additional year in public housing during childhood see a total increase of $.2953 + .3095 = .6048$ percentiles in their within-cohort earnings rank (using the same process for converting estimates to dollar values as above, this translates to \$333, or 1.8% of the mean for the full sample). For the most disadvantaged children, this estimate is $.3992 + .5014 = .8936$ percentiles (\$492, or 2.6% of the mean). These effects are approximately twice as large as impacts on men.

For employment, we do not find statistically significant impacts on men. However, we do find evidence of modest, statistically significant benefits for women. For the full sample, spending an extra year in public housing leads to a .28 percentage point increase in employment (.4% of the mean). Estimates for the two income groups are similar.

Women who spend an additional year in public housing in childhood see an additional .21 percentage point reduction in participation in any safety net program relative to men, though this estimate is not statistically significant at the .05 level. Among children from families with below median incomes, women who spend an additional year in public housing during childhood see a statistically significant .58 percentage point decline relative to men (1.8% of the mean).

The results are more mixed for incarceration. Using the main specification, we see that women who spend an additional year in public housing during childhood see larger reductions in incarceration among the full sample and among children from less disadvantaged families (no effects among the most disadvantaged children). However, the results are sensitive to specification. Using the siblings design (see Table A.17), we see no differences in impact between men and women for the full sample and women from the most disadvantaged families actually see relative increases in incarceration.

5.8 Alternative Specifications

In this section, we have presented results from our main analysis. In Appendix A, we present robustness tests for the full set of results using a siblings design. We show that point estimates from the two approaches are generally similar across outcomes and samples.

In Appendix C, we present results from alternative specifications and discuss their implications for our main results.

6 Heterogeneity Across Public Housing Developments

Given the large number of public housing developments in New York City and their placement across the city, there is substantial heterogeneity in living environments among public housing residents. In this section, we investigate heterogeneity in impacts based on the public housing developments where children are initially assigned to live.

6.1 Descriptive Statistics

In Table 16, we report summary statistics related to public housing developments. In Panel A, we report average characteristics of the Census tracts where developments are located. These neighborhood characteristics are calculated using data from the 2000 decennial census long form. The table shows that tracts with public housing developments tend to be very disadvantaged. For instance, roughly 44% of individuals in tracts with public housing developments lived below the federal poverty line, roughly four times the national poverty rate of

11.3% in 2000.[16] Tracts with public housing developments may be so disadvantaged for two reasons. The first reason is mechanical: Families must have low incomes to qualify for public housing, so tracts with a large share of public housing residents will tend to have low family incomes. Second, it has been well documented that public housing developments were placed in more disadvantaged neighborhoods historically, so even the neighborhoods surrounding public housing developments tend to be relatively disadvantaged (e.g., Rothstein, 2017).

Nevertheless, there is heterogeneity in neighborhood environments across developments. For example, the standard deviation of median neighborhood household incomes is nearly $11,000, or almost 42% of the mean.

In Panel B, we repeat the same exercise but leverage information on neighboring tracts. To do this, we summarize the characteristics of nearby tracts by taking an average of the five closest tracts. Then, we calculate the average of neighbor characteristics by development. Overall, nearby tracts look less disadvantaged than the tract a development is in. For example, median household incomes are about 60% higher and poverty rates are about 28% lower. Yet, even neighboring tracts are relatively disadvantaged and we still see substantial heterogeneity, as before.

Panel C displays select characteristics of developments.[17] The average development contains almost 1,400 units, which house roughly 2,900 individuals. However, developments that are one standard deviation larger are home to 1,000 more people. The average rent paid per unit is roughly $470. This varies by development, but the difference is modest: Developments that collect rents that are one standard deviation higher, receive about $40, or 8% of the mean, more per unit. Since rents are determined based on family income, this implies that family incomes also vary modestly by development. As of 2023, the capital needs of public housing come to nearly $320,000 per unit, though developments with needs that are one standard deviation greater need about 13% more in investment.[18]

[16]See Table 2 at https://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-people.html

[17]Most development characteristics come from NYCHA's "Development Data Book 2023", which can be accessed at `https://www.nyc.gov/assets/nycha/downloads/pdf/pdb2023.pdf`. Inspection scores come from inspections conducted between 2017-2019 and can be downloaded at `https://www.huduser.gov/portal/datasets/pis.html`. Data on capital needs come from NYCHA's "2023 Physical Needs Assessment" and can be found at https://www.nyc.gov/site/nycha/about/reports.page.

[18]This average is calculated among developments where individuals in our sample were assigned. This may differ some from the average among developments in the Development Data Book linked above as not all developments have a match to individuals in our sample.

6.2 Empirical Strategy

Given this variation in living environments induced by the program, we aim to document and understand heterogeneity based on the public housing development where families are first assigned to live.

We first recover the effect of each development by estimating

$$y_i = \sum_{j=1}^{J} \eta_j D_{j(i)} + \sum_{j=1}^{J} \theta_j D_{j(i)} \times M_i + X_i'\mu + \delta_{c(i)} + \nu_i \tag{3}$$

where y_i again represents some early adulthood outcome. Though we can estimate as many sets of development effects as we have outcomes, we focus primarily on the effects of developments on earnings rank. The $D_{j(i)}$ are a set of dummy variables that represent whether individual i was assigned to development j. The set of controls are identical to those we use in estimating childhood exposure effects.

One challenge to identification is that individuals assigned to different developments may differ in their potential outcomes. As discussed in Section 2, families have limited choice in where they will live. However, NYCHA may steer certain types of families to certain developments (for example, in recent years, they have tried to increase the number of working families in the lowest-income developments). Furthermore, families may reject all of NYCHA's offers and decline public housing altogether. If certain types of families tend to reject certain developments, this may give rise to differences in the characteristics of new admissions across developments.

For this reason, the intercepts, denoted by η_j, do not have a causal interpretation. Nevertheless, analyzing the intercepts may be informative for a number of reasons. Under the assumption that the childhood exposure effects are linear for all developments, the variation in intercepts represent a measure of selection bias. If we instead assume there is no selection bias, the spread in intercepts may shed light on how misspecified the model is.

Instead, we focus on understanding the distribution of the slopes, or the coefficients on the interaction between development dummies and potential years of exposure. Under the assumption that, for all developments, the age one enters any development is uncorrelated with potential outcomes (conditional on observables), the θ_j are identified. For the remainder of this paper, when we refer to "development effects," we refer to the coefficients on the slopes θ_j, unless otherwise noted.

We assess the identifying assumptions by adapting our balance tests from Section 4. We take the same predicted outcomes we generated above, constructed by regressing some outcome in early adulthood on child demographics, baseline family characteristics, and birth year fixed effects. We standardize the predicted outcomes by subtracting the mean value and dividing by the standard deviation. Then we estimate Equation 3 but replace the dependent variable with our standardized predicted outcomes.

We present the results from these tests in Table 17. Each panel is associated with tests featuring a single predicted outcome and each set of columns estimates development effects for a different sample.

For the first panel, we estimate development effects using the standardized predicted earnings rank. We report estimates associated with the intercepts in the first column of each set of columns, and estimates associated with the slopes in the second. In the first row of the panel, we report the average of the estimated coefficients. On average, individuals with one additional year of potential exposure have predicted earnings ranks that are .03 standard deviations lower, consistent with our earlier results.

In the second row, we report the observed standard deviation of the distribution of estimated development effects. This is an overestimate of the dispersion of the distribution of true development effects because it in part captures sampling error.[19] In the third row, we present bias-corrected standard deviations that account for correlations in effects across developments. In the fourth row, we report standard errors for the bias-corrected standard deviations, produced using a bootstrap procedure clustered at the development level. Details on how we construct bias-corrected standard deviations and estimate their standard errors can be found in Appendix D.

For the main sample, the bias-corrected standard deviation of the development effects on the predicted earnings rank is .009 (standard error of .004), or about one-third of the mean. The results suggest, in a majority of developments, children with more potential years of exposure (those who enter at an earlier age) tend to have characteristics that predict worse outcomes.

The second and third panels repeat this exercise, but use predicted safety net participation and incarceration as the dependent variable when calculating development effects, respectively. Again, the results are both consistent with the results of our previous analysis on childhood exposure effects and with the results of the first panel. That is, individuals with more potential years of exposure within a development have characteristics that predict higher safety net participation and incarceration rates.

In the second and third set of columns, we estimate development effects for the two income

[19] Any estimated development effect can be represented as the sum of the true development effect and some sampling error. The variance of the estimates, therefore, will be equal to the variance of this sum. Assuming the sampling error is orthogonal to the true effect, the variance of the sum will be greater than the variance of the true effects.

group subsamples, respectively. The results of the tests are very similar across groups.[20]

6.3 Distribution of Development Effects

In Table 18, we summarize the distribution of the development effects, where development effects are calculated using rank earnings as the dependent variable when estimating Equation 3.

Though we are primarily interested in the distribution of the slopes (the θ_j), we first examine the distribution of the coefficients on the intercepts (the η_j). For the full sample, the average of the estimated intercepts is 49.7 percentiles. The bias-corrected standard deviation is 1.2 percentiles (with a standard error of .9), or 2.4% of the mean, suggesting there is little variation in the intercepts across developments. There is somewhat more heterogeneity if we estimate the model separately for the two income groups. For individuals from the most disadvantaged families, the mean of the intercepts is 44.9, with a standard deviation of 2.8 (standard error of .9), or 6.3% of the mean. For children from less disadvantaged families, the mean is 52.5 with a standard deviation of 2.1 (standard error of 1.1), or 4.1% of the mean. Overall, the results suggest there is little variation in the intercepts across developments. Under the assumption that exposure effects are linear for all developments, this may imply that there is little systematic selection into developments in ways that affect earnings, despite concern that NYCHA may steer families to certain developments or that families may choose to reject offers to certain developments.

Turning to the development effects, we find that, across samples, the averages of the slope coefficients are similar to the overall impacts of childhood exposure on children's earnings rank. There is, however, some heterogeneity in effects across developments. For development effects estimated on the full sample, the standard deviation of effects is .13, but the standard error on this estimate is .1. If we estimate development effects separately for the two income groups, we see more heterogeneity in effects across developments. For children in the bottom half of their admission year income distribution, spending an additional year in public housing in the mean development leads to a .61 percentile increase in earnings rank. The standard deviation of this estimate is .26 percentiles (standard error of .13), or 42% of the mean. Repeating the same calculations we performed to convert ranks to dollars in Section 5, we estimate individuals who move into developments with effects that are one standard deviation larger earn about $140 more for each additional year they spend in public housing. For children in the top half of their admission year income distribution, the mean effect is

[20]When we correct the observed variances for bias, the resulting variances may be negative. When that is the case, we denote the bias-corrected standard deviation with a dash ("-"). We interpret this as finding no evidence of variation in development effects up to sampling error.

smaller, but the standard deviation of effects is about the same: Children from families with above median incomes placed in a development with effects that are one standard deviation larger move up .29 percentiles in the earnings distribution (standard error of .12), or 88% of the mean development effect. These findings imply that there are substantial gains to children who are placed in the best developments.

6.4 Predictors of Development Effects

Given the heterogeneity in outcomes and living environments for individuals assigned to different developments, we are interested in whether we can learn about which development and neighborhood characteristics matter for children's long-term well-being.

First, we investigate whether observable development and neighborhood characteristics are predictive of development effects at all. To do this, we estimate equations of the form

$$\widehat{\theta}_j = \kappa_b + Z_j'\lambda + u_j \tag{4}$$

where $\widehat{\theta}_j$ are the development effects estimated in Equation 3 and Z_j' is a vector of neighborhood and development characteristics associated with development j. For ease of interpretation, we transform the development effects and the predictors so they are expressed in units of standard deviation (that is, we demean and divide by the standard deviation).

In Table 19, we report the R^2 on regressions of development effects on different sets of characteristics. In the first column, we report results from regressions using development effects estimated on the full sample. Estimates in the first row come from regressions of development effects on all the characteristics in Table 16. The second row uses just the tract characteristics as predictors. The third row reports results from regressions using just the characteristics of a development's neighboring tracts. The last row focuses just on the development's own characteristics. Overall, the observed characteristics explain about one-third of the variation in development effects. Just own tract characteristics explain 12.5% of the variation, while just the characteristics of neighboring tracts explain 17.0% of the variation. Regressions of development effects on development characteristics have a R^2 of .07.

In the second and third columns, we report the results from regressions on development effects calculated on the two income groups. When regressing development effects on all characteristics, the resulting R^2 are .28 for the most disadvantaged sample and .29 for the less disadvantaged sample. However, the R^2 are much lower when estimating regressions on any of the subsets of variables, particularly for the most disadvantaged sample. For example, the regression of development effects estimated on the children from families in the bottom half of their admission year income distribution on own tract characteristics has a R^2 of .04 and on neighboring tract characteristics has a R^2 of .07.

Overall, our observable development and neighborhood characteristics do predict development effects. We next investigate which characteristics seem to matter most in determining whether a development has a positive effect on children's long-term outcomes.

To do this, we estimate Equation 4 separately for each characteristic in Table 16 and for each of our samples. We plot the coefficients from each of these regressions in Figure 11. In Figure 11a, we examine the impacts of a development's own tract characteristics. Economic characteristics of tracts do tend to predict how positive an effect a development has on adulthood earnings. For a standard deviation increase in the renter share, development effects fall by .37 standard deviations. A one standard deviation increase in median household incomes is associated with a .23 standard deviation increase in development effects, while a one standard deviation decrease in the tract poverty rate is associated with a .16 standard deviation decline, though the latter is not statistically significant.

Factors related to immigration and race also predict how good a development is for children's long-term well-being. Tracts with higher shares of US born and with more Black residents tend to be better for children (though the results are not significant at the .05 level), while tracts with Hispanic shares that are one standard deviation higher have development effects that are .34 standard deviations smaller. Other tract characteristics, such as those related to household structure or the age distribution of tract residents, seem to have little relationship to development effects.

In Figure 12, we repeat the analysis separately for each of the two income groups. In general, the relationships between development effects and predictors we observe for the full sample tend to hold for the income group subsamples, though the relationships tend to be slightly weaker for children from families with above median incomes.

In Figure 11b, we examine the relationship between development effects and the characteristics of neighboring tracts. The patterns we observe are similar to those found in Figure 11a, though estimates are attenuated and standard errors are slightly larger.

Lastly, we find little relationship between development characteristics and development effects, as illustrated in Figure 11c. One limitation of this analysis is that the inspection score and the capital needs of a development are measured in recent years, and so may not reflect the condition of the development when members of our sample were living in those developments.

7 Calculating the Marginal Value of Public Funds

In this section, we calculate the marginal value of public funds (MVPF) associated with the public housing program in New York City (See Hendren and Sprung-Keyser, 2020, for an introduction to this framework). The MVPF is calculated as the ratio of benefits, defined as the willingness of participants to pay for a benefit, to the *net* costs to the government.

Below, we describe how we calculate the individual components of the MVPF. We report the components in Table 20. We provide additional details on our methods in Appendix E.

Change in Lifetime Earnings and Benefits

Ultimately, we need estimates for individual willingness to pay for a year of public housing, as well as the net cost of the program to the government. To do this, we first need to understand how living in public housing affects earnings and program participation over the lifetime.

We start by calculating the present value of lifetime earnings and benefits nationally, which we report in row 1 of Table 20.

For earnings, we use the 2016 Current Population Survey Annual Social and Economic Supplement (CPS ASEC) to calculate average earnings for all individuals 18-65 years old in calendar year 2015 (the median year we measure outcomes for children in our sample). We apply a real wage growth rate of .5 percent to generate a profile over the lifecycle. Applying a discount rate of 3%, we estimate that the present value of earnings at age 10 (the median age children from our sample move into public housing) is about $865,127 nationally.

To estimate expected expenditures on federal safety net programs, we use the 2016 CPS ASEC to calculate the share of individuals who receive rental assistance, SNAP, or TANF by age, generating a profile of lifecycle participation by program. Since benefits are underreported in survey data, we merge in information on program participation from the Urban Institute's Transfer Income Model (TRIM), a microsimulation model that corrects for underreporting in the CPS.[21] We multiply these shares by per beneficiary spending on each program. Applying a 3% discount rate, we estimate that the present value of rental assistance, SNAP, and TANF is $2,427, $4,994, and $285, respectively.

Our sample comprises individuals that are relatively more disadvantaged than the national population that these survey data reflect. For this reason, lifetime earnings tend to be lower and program participation higher than the national averages. We next calculate the present values for earnings and federal benefits for our sample and report the results in row 2.

[21] https://boreas.urban.org/T3Welcome.php

For earnings, we rescale the present value in row 1 by the ratio of mean earnings for our sample (reported in Table 3) to the mean earnings for the national sample at age 26 (calculated using the CPS ASEC), which comes to .57. Thus, the present value of lifetime earnings for our sample is $489,080.

For program participation, we rescale the participation rate we estimate at each age by the ratio of mean participation at age 26 (calculated using administrative data and reported in Table 5) to that of the overall population (measured using the CPS ASEC-TRIM). We then perform the same calculation as before and find the present value of expenditures per person in our sample comes to $31,043, $9,846, and $3,093 for rental assistance, SNAP, and TANF, respectively.

For the remainder of Table 20, we display estimates separately for each sample. In Panel A, we derive components using the estimates for the full sample, while Panels B and C use estimates for the two income groups.

We next calculate how spending a year in public housing changes the present value of the quantities we have estimated above. To do so, we first need to convert the impacts in Table 3 and Table 5 to percent impacts, which we report in the first row of each panel. Then, we multiply the present value for each quantity by the appropriate percent impact, which yields the change in the present value, which we share in row 2 of each panel. Spending one additional year in public housing leads to an increase in the present value of earnings of $6,430 for the full sample. One additional year of exposure leads to a reduction of $221, $54, and $10 in expenditures on rental assistance, SNAP, and TANF, respectively.

Net Cost to the Government

The denominator for the MVPF is the net cost of providing the program to the government. The net cost is the upfront cost, plus any costs or savings as a result of individuals' behavioral responses to the program.

To calculate the upfront costs, we use HUD's Picture of Subsidized Households (PSH) data for 2000 (the median year individuals in our sample moved into public housing).[22] We estimate that the government spent $3,185 per individual in New York City public housing in 2000.

We also account for the potential effects of rental assistance on the labor supply of parents. We first calculate the present value of household income by applying methods similar to those used to calculate children's lifetime earnings to the mean baseline household income in our sample. Previous research estimates that housing vouchers decrease earnings by about 11%

[22] https://www.huduser.gov/portal/datasets/assthsg.html

(Jacob and Ludwig, 2012). Given research from MTO that finds no labor market effects of switching from public housing to vouchers (Ludwig et al., 2013), we use this as an estimate of the effect of public housing on earnings. We multiply the present value of household income by this factor to calculate the present value of lost income. We follow Bailey et al. (2023) and assume households face an average tax rate of 12.9%, implying the government will lose $6,991 in revenue.[23] We divide this by the average family size at baseline and estimate that the government loses $1,697 per individual in revenue due to the negative labor supply effects of public housing.

Next we consider any additional costs or savings that the government incurs due to the program's long-term effects. Focusing on the full sample, we see from row 2 of Panel A that children who spend an additional year in public housing experience an increase of $6,430 in discounted lifetime earnings. Again applying an average tax rate of 12.9%, we estimate the government will receive an additional $830 in revenue. For program participation, the reduction in benefits is exactly equal to the reduction in government expenditures. Overall, providing an individual with one year of public housing saves the government $1,114 in the long run.

The net cost to the government of providing public housing to one person for one year comes to $3,768. If we repeat these calculations separately for each income group, we find the net cost to the government is $3,340 for children from the most disadvantaged families and $4,087 for children from the less disadvantaged families.

Willingness to Pay

The numerator for the MVPF represents individuals' willingness to pay for one additional year of public housing in childhood. We separately calculate individuals' willingness to pay for the increase in earnings and the changes in benefit receipt. We report estimates in row 4 of each panel.

We assume individuals value the increase in earnings at the net-of-tax rate $1 - .129$. For the full sample, the increase in discounted, lifetime, after-tax earnings is equal to $5,601.

Given the absence of estimates on willingness to pay for HUD rental assistance programs, we assume rental assistance spending is valued dollar-for-dollar to produce conserva-

[23] This may be an overestimate for several reasons. Other studies that analyze housing voucher receipt over a slightly longer horizon find that negative effects on earnings fade out (Carlson et al., 2009; Abt Associates et al., 2006). Additionally, Bloom (2008) documents that NYCHA has, historically, been less aggressive in evicting tenants above the income limit compared to other housing authorities in other cities, including Chicago (where the Jacob and Ludwig (2012) estimates comes from). He reports that, more recently, tenants with incomes above the limit are never evicted. If families are aware of NYCHA's enforcement policies, then the program may only have minimum work disincentives.

tive MVPFs.[24] For SNAP, we follow Bailey et al. (2023) and assume individuals value \$1 of SNAP benefits at 80 cents, implying that individuals value the lost benefits at \$43. We assume cash assistance through TANF is valued dollar-for-dollar and, therefore, that their willingness to pay declines by \$10 due to reduction in cash assistance.

Adding the willingness to pay for the increase in earnings to the change in willingness for the change in benefit receipt, we find that overall individuals are willing to pay \$5,327 for one additional year of public housing. Children from the most disadvantaged families are willing to pay \$7,707, while children from less disadvantaged families are willing to pay \$4,157.

Calculating the MVPF

To calculate the MVPF, we divide the willingness to pay by the net cost to the government, separately for each sample.

For every dollar in government spending, children receive a little more than \$1.40 in benefits. A dollar in government spending benefits children from the most disadvantaged families roughly \$2.30 and children from less disadvantaged families \$1.10.

8 Conclusion

This paper uses linked administrative data to examine the effects of growing up in public housing on children's long-run outcomes. We exploit the variation in when children move into public housing and find that spending more time in public housing during childhood leads to improvements in economic outcomes. In particular, we find that children who spend more years in public housing see increased earnings, higher employment rates, and lower participation in safety net programs in early adulthood. Notably, children from the most disadvantaged families experience the largest benefits from residence in public housing.

We find that there is some heterogeneity in effects across public housing developments. Among children from the most disadvantaged families, for example, developments that produce outcomes that are one standard deviation higher have effects about one quarter greater than the mean development effect. We investigate this heterogeneity and find that developments that are better for children tend to be in neighborhoods with a lower share of renters, higher household incomes, lower poverty rates, and relatively lower concentrations of immigrants.

[24] If we instead assume that individuals value \$1 of government spending on rental assistance at \$0, the ratio of benefits to costs rises by at most .1.

Using our estimates of the impacts on earnings and program participation, we estimate that for every \$1 the government spends on public housing, children receive \$1.40 in benefits overall, including \$2.30 for children from the most disadvantaged families.

Our findings add to the growing body of evidence on the importance of childhood investments on children's long-run outcomes. Much of the existing literature focuses on the impact of direct cash transfers, or on other programs that are explicitly intended to target individuals' physical well-being, such as SNAP or Medicaid. We instead add to a smaller literature on the importance of programs that help families secure housing for children's long-term success. Furthermore, our results reinforce the importance of neighborhood environment in shaping the life trajectories of the most disadvantaged children. This work highlights how public policy can influence individuals' outcomes by shaping their physical environments in childhood.

While this paper makes progress on key questions, one of the study's limitations is that the findings remain a black box. Future research should investigate the mechanisms that drive increases in earnings. Additionally, future work might extend the analysis to other public housing authorities. Examining impacts across cities would allow researchers to identify how differences between places and in the public housing programs themselves impact children's long-run outcomes.

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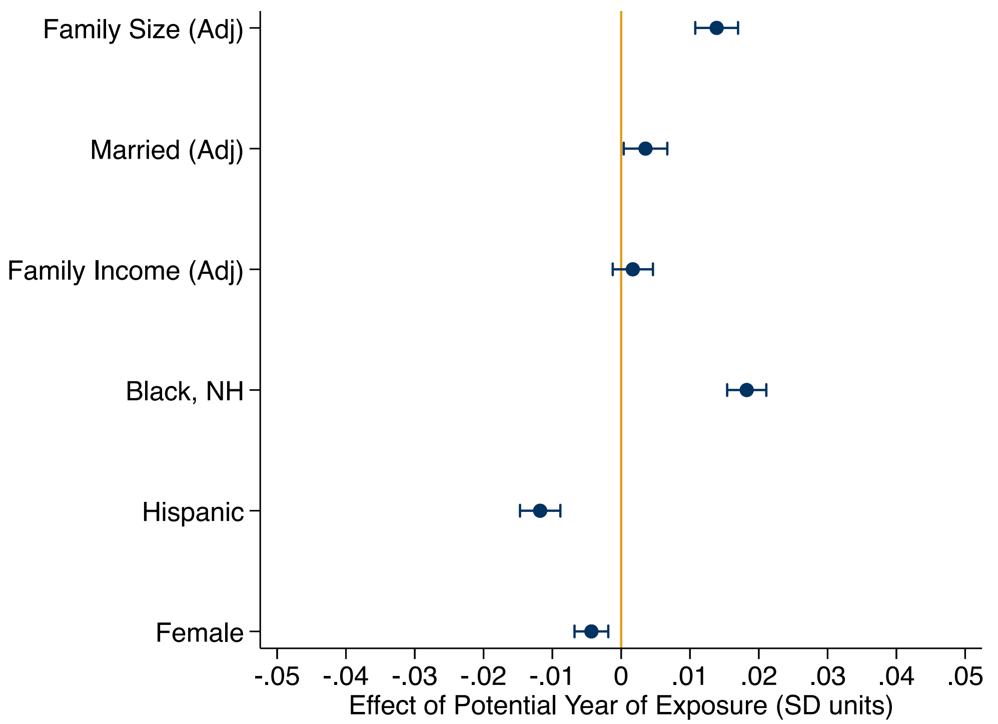
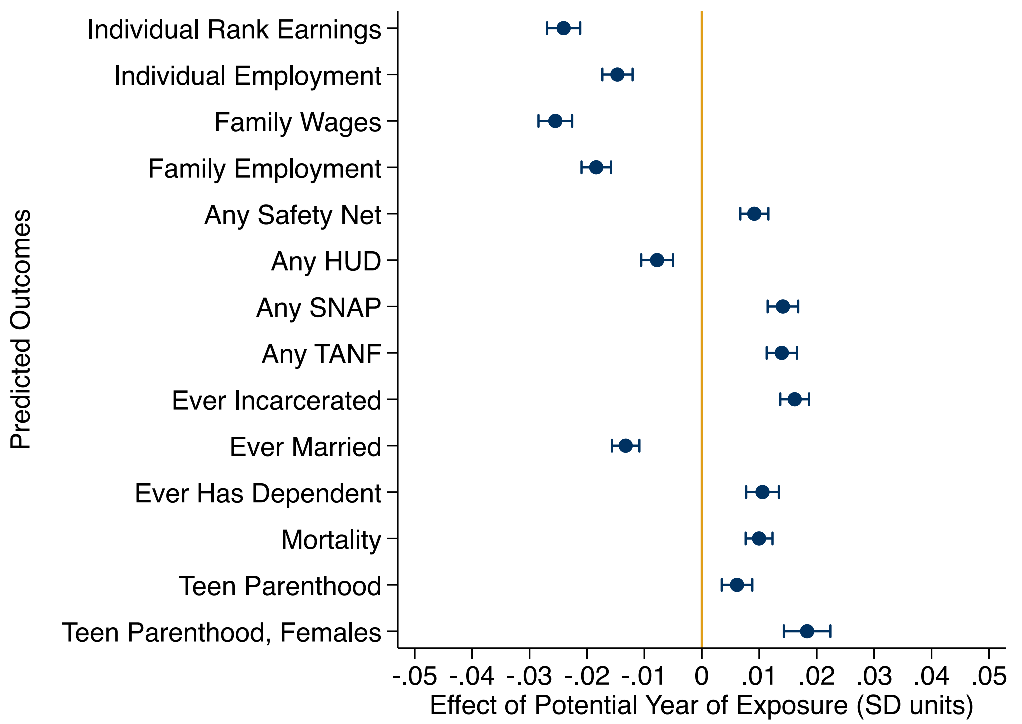
9 Figures and Tables

Figure 1: Balance Tests Using Baseline Characteristics, Controlling for Nativity



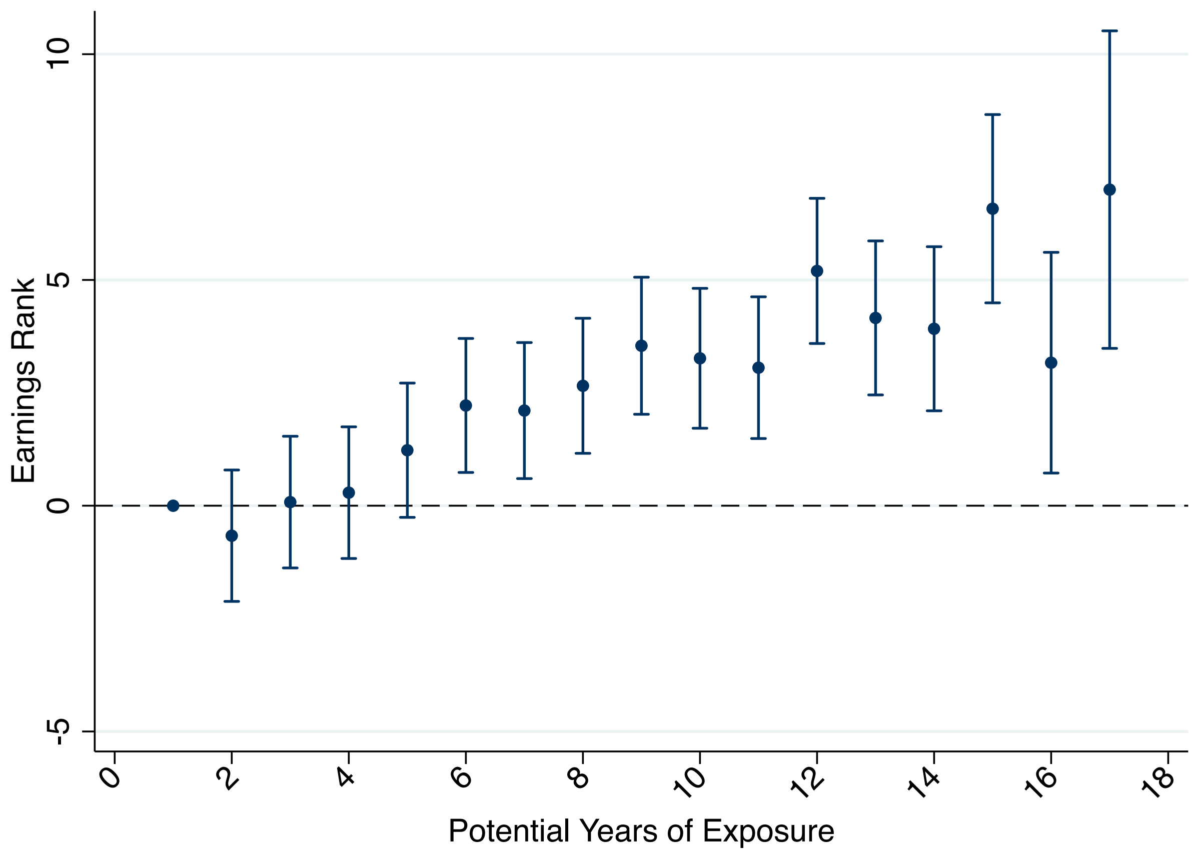
Notes: This figure displays results from tests for balance on baseline characteristics. Family size, family marital status, and family income are residualized to account for parent lifecycle effects and macroeconomic conditions. We transform all characteristics by subtracting the sample mean and dividing by the sample standard deviation. The coefficients and confidence intervals are constructed from regressions of the standardized characteristic on potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, birth year fixed effects, and an indicator for whether a child was born in the US.

Figure 2: Balance Tests Using Predicted Outcomes, Controlling for Nativity



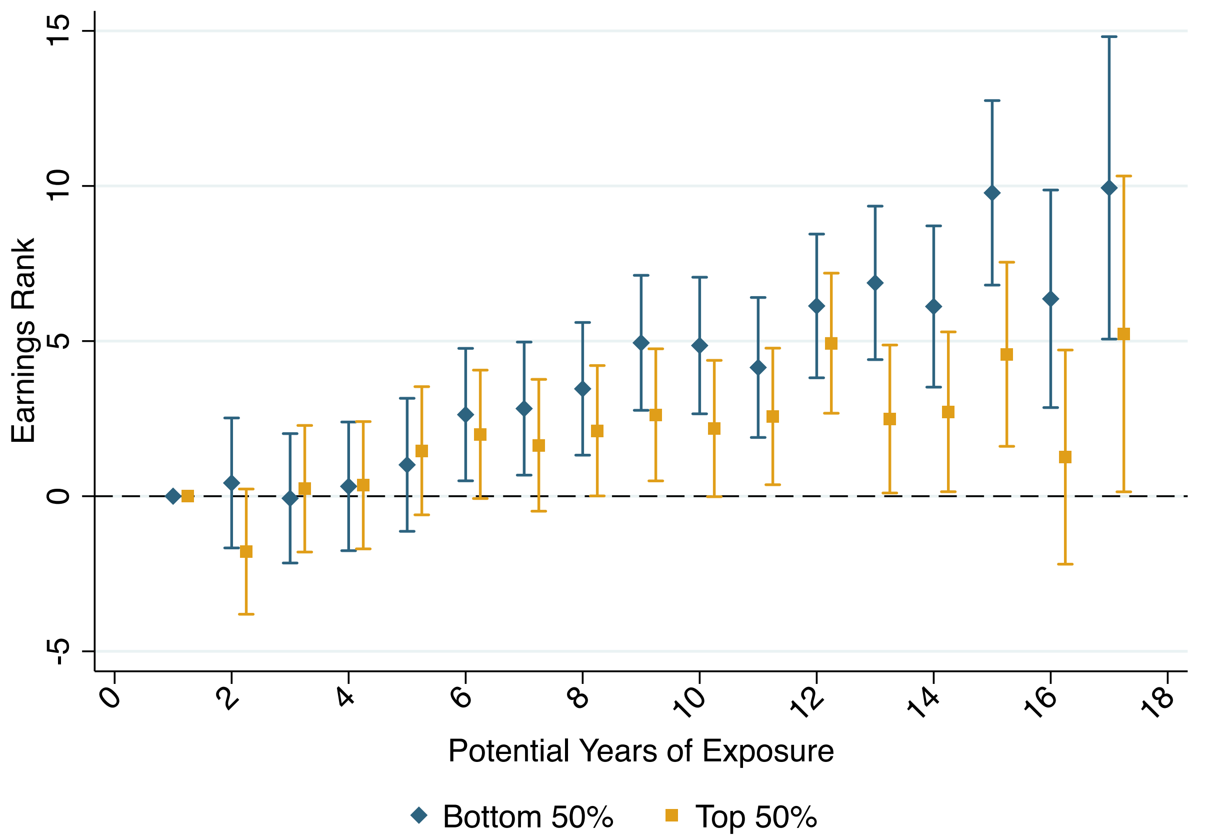
Notes: This figure displays the results of tests for balance on predicted outcomes. Predicted outcomes are generated from regressions of outcomes on birth year fixed effects and baseline characteristics, including residualized family size, residualized family marital status, residualized family income, and indicators for race/ethnicity, sex, and whether a child was born in the US. We transform all predicted outcomes by subtracting the mean and dividing by the standard deviation. The coefficients and confidence intervals are constructed from regressions of the standardized predicted outcomes on potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, and all covariates used in generating the predicted outcomes.

Figure 3: Impacts of Potential Years of Exposure on Earnings Rank



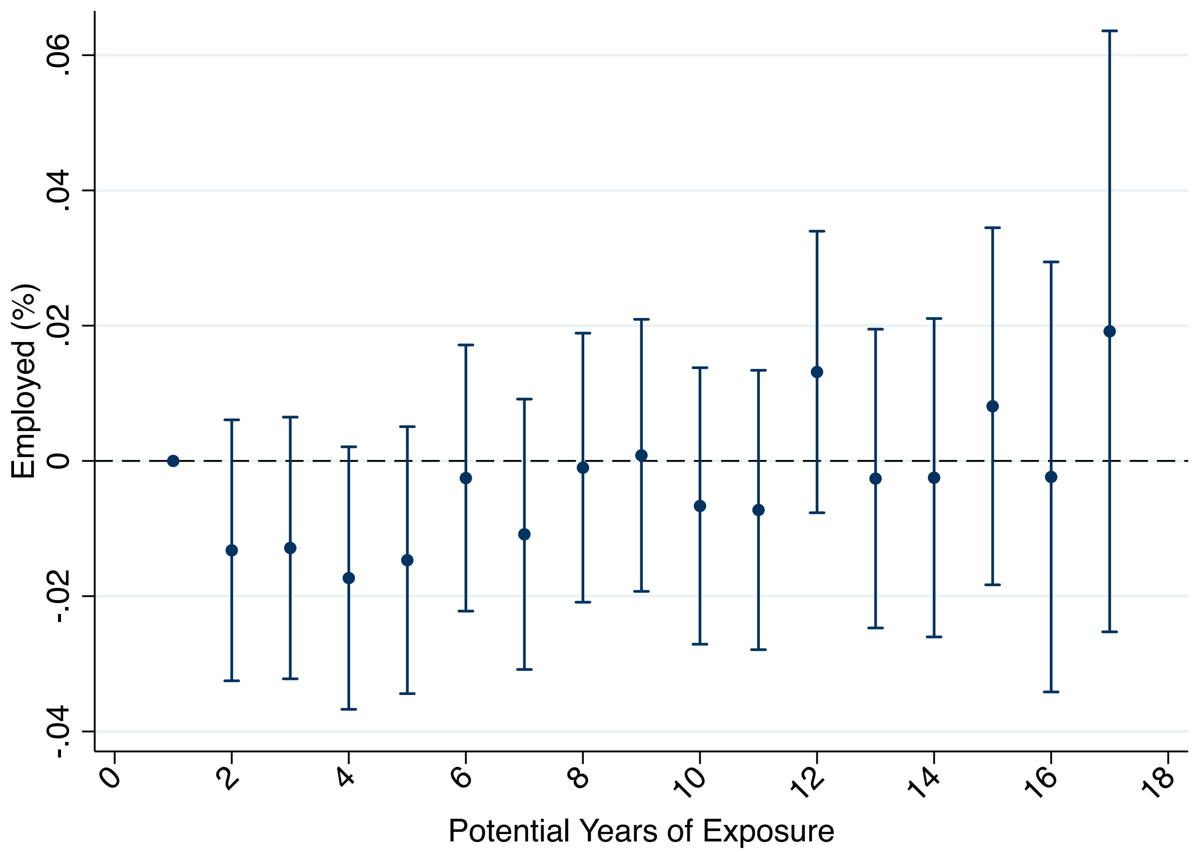
Notes: This figure displays the effects of potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, on earnings. The outcome is the percentile rank of an individual's W-2 earnings at age 26, calculated within their birth cohort. The coefficients are estimated by replacing the continuous potential years of exposure variable in Equation 1 with dummies for each potential year of exposure.

Figure 4: Impacts of Potential Years of Exposure on Earnings Rank, by Income Group



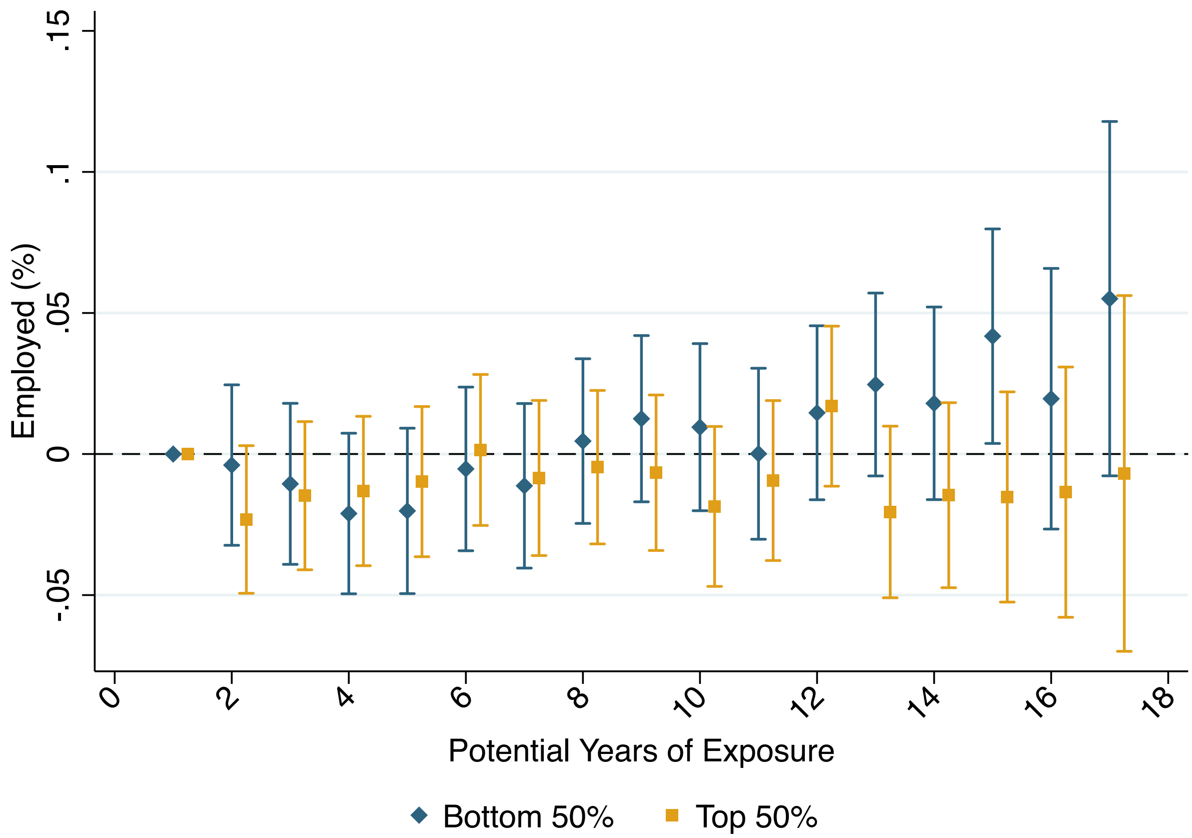
Notes: This figures displays the effects of potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, on earnings. The outcome is the percentile rank of an individual's W-2 earnings at age 26, calculated within their birth cohort. The coefficients are estimated by replacing the continuous potential years of exposure variable in Equation 1 with dummies for each potential year of exposure and estimating the model separately for individuals from families with incomes below or above the median for their admission year.

Figure 5: Impacts of Potential Years of Exposure on Employment



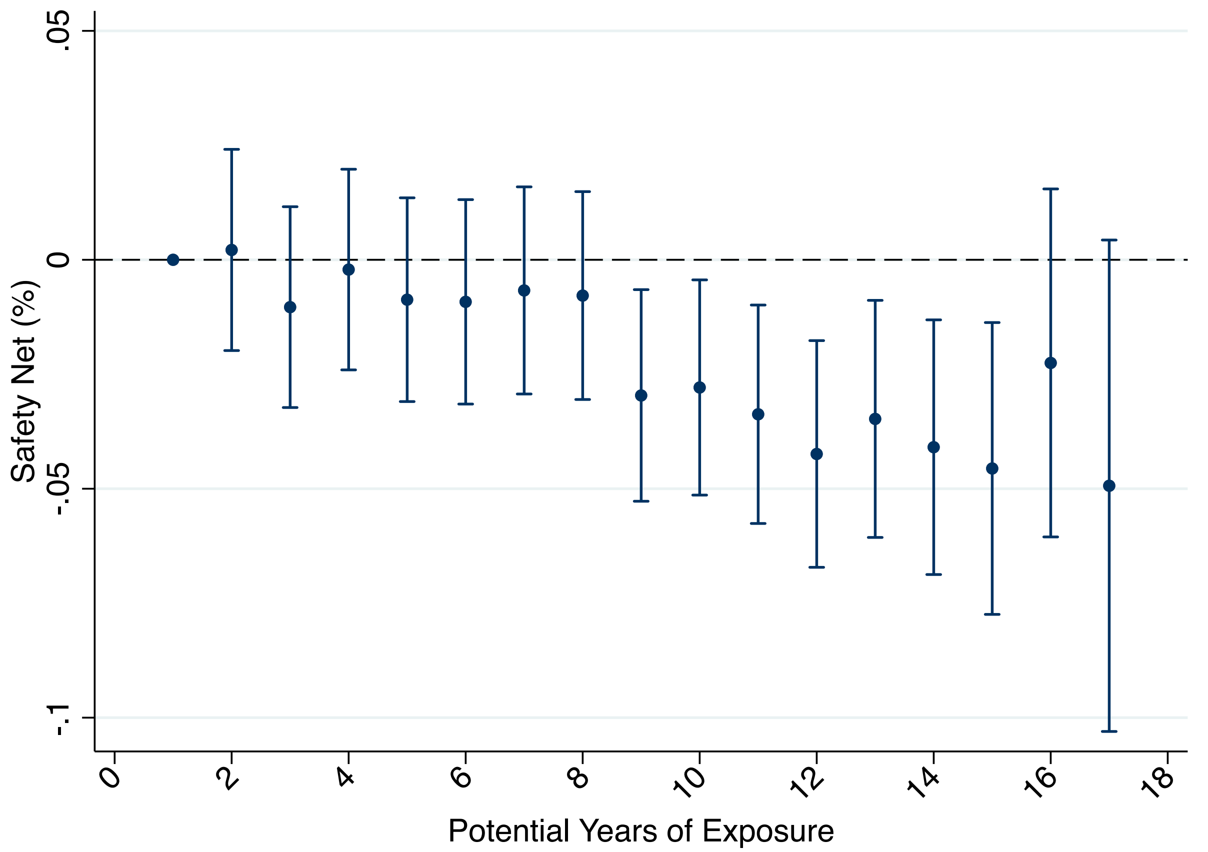
Notes: This figure displays the effects of potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, on employment. The outcome is an indicator for whether an individual had positive W-2 earnings in the year they turned 26. The coefficients are estimated by replacing the continuous potential years of exposure variable in Equation 1 with dummies for each potential year of exposure.

Figure 6: Impacts of Potential Years of Exposure on Employment, by Income Group



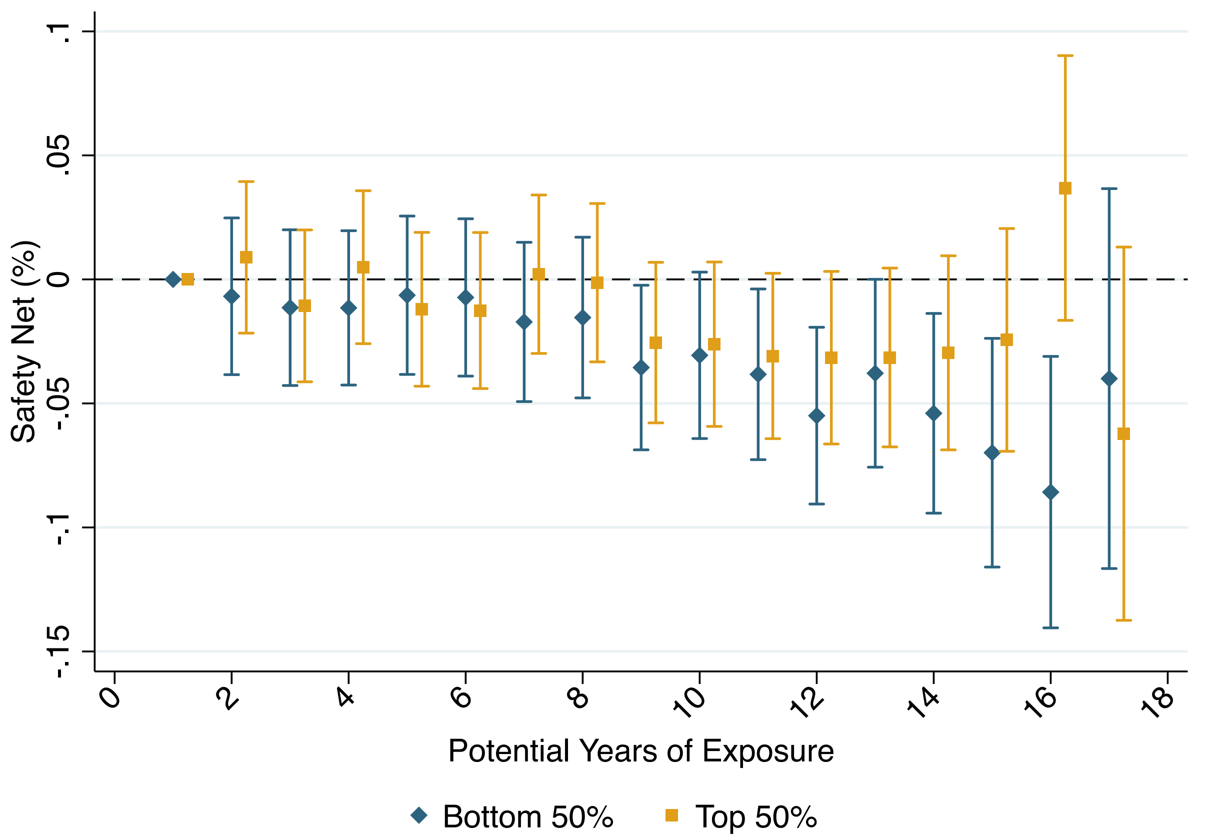
Notes: This figure displays the effects of potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, on employment. The outcome is an indicator for whether an individual had positive W-2 earnings in the year they turned 26. The coefficients are estimated by replacing the continuous potential years of exposure variable in Equation 1 with dummies for each potential year of exposure and estimating the model separately for individuals from families with incomes below or above the median for their admission year.

Figure 7: Impacts of Potential Years of Exposure on Safety Net Participation



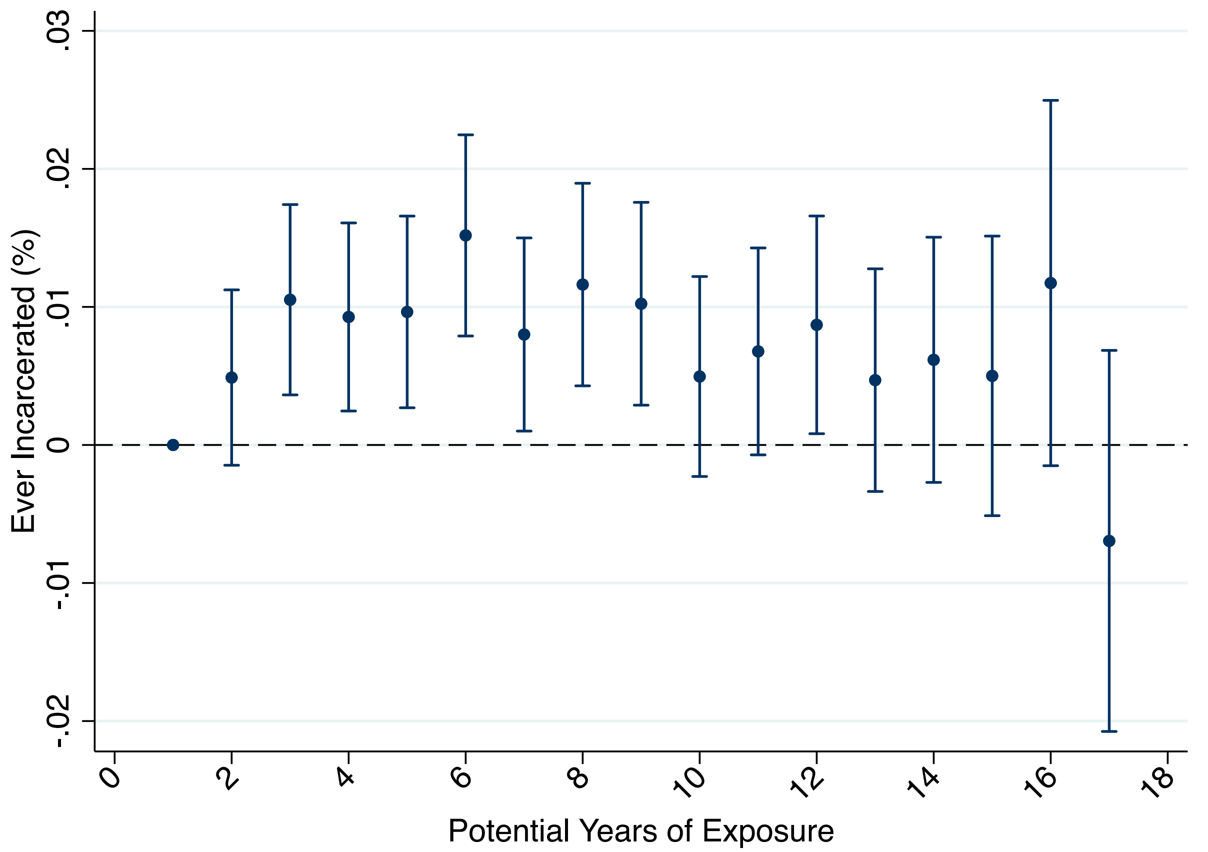
Notes: This figure displays the effects of potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, on safety net participation. The outcome is an indicator for whether an individual received rental assistance through HUD, participated in SNAP, or participated in TANF in the year the individual turned 26. The coefficients are estimated by replacing the continuous potential years of exposure variable in Equation 1 with dummies for each potential year of exposure.

Figure 8: Impacts of Potential Years of Exposure on Safety Net Participation, by Income Group



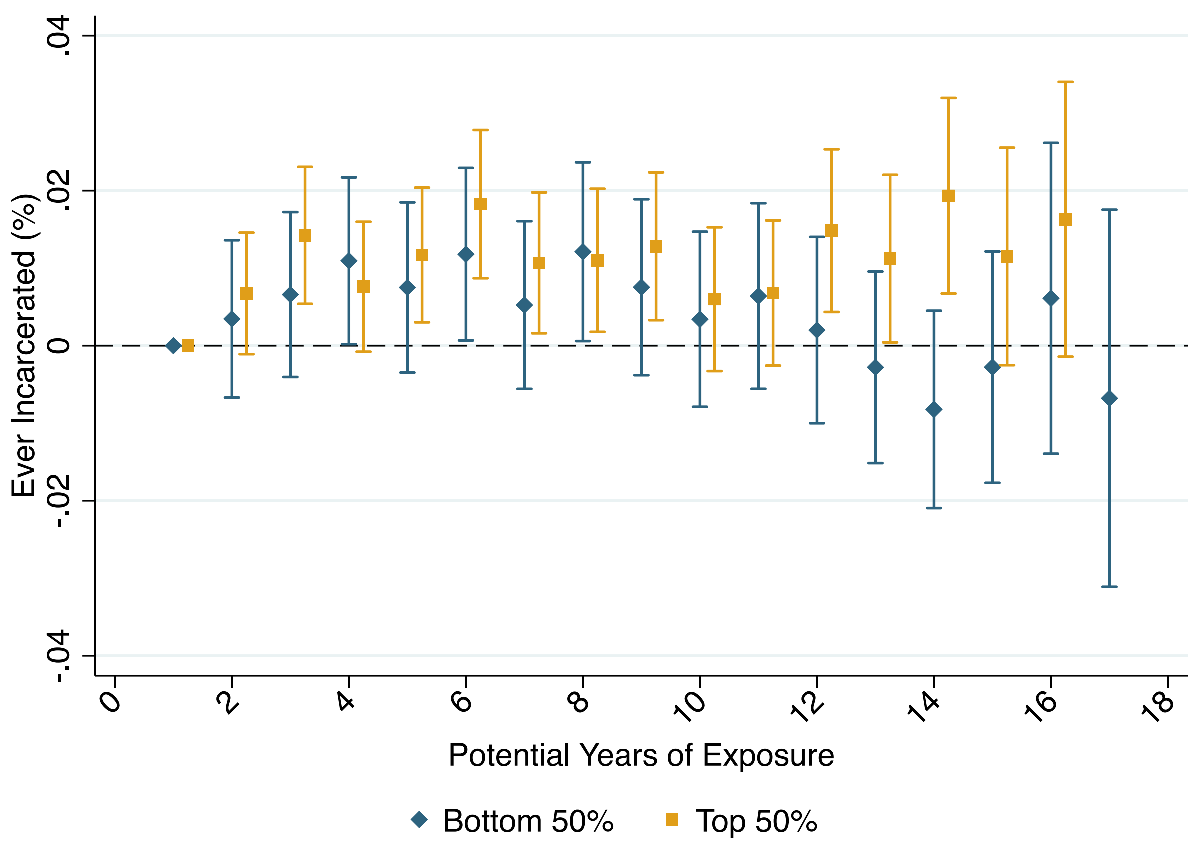
Notes: This figure displays the effects of potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, on safety net participation. The outcome is an indicator for whether an individual received rental assistance through HUD, participated in SNAP, or participated in TANF in the year the individual turned 26. The coefficients are estimated by replacing the continuous potential years of exposure variable in Equation 1 with dummies for each potential year of exposure and estimating the model separately for individuals from families with incomes below or above the median for their admission year.

Figure 9: Impacts of Potential Years of Exposure on Incarceration



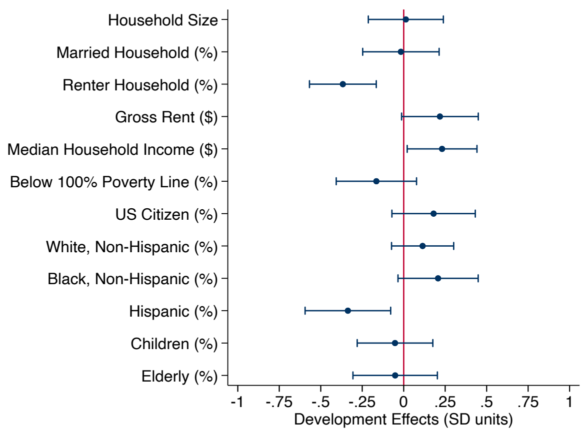
Notes: This figure displays the effects of potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, on incarceration. The outcome is an indicator for whether an individual was ever incarcerated by the time they turned 26. The coefficients are estimated by replacing the continuous potential years of exposure variable in Equation 1 with dummies for each potential year of exposure.

Figure 10: Impacts of Potential Years of Exposure on Incarceration, by Income Group

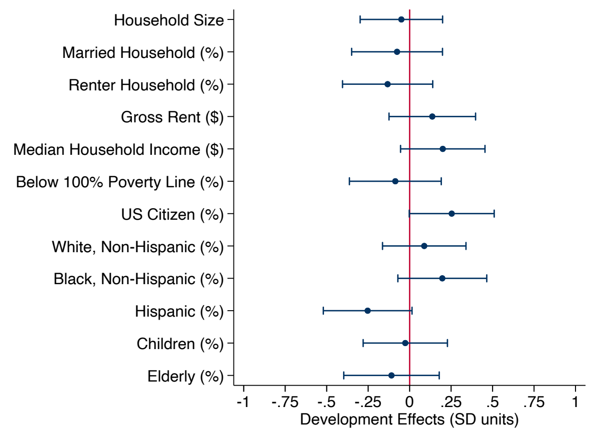


Notes: This figure displays the effects of potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, on incarceration. The outcome is an indicator for whether an individual was ever incarcerated by the time they turned 26. The coefficients are estimated by replacing the continuous potential years of exposure variable in Equation 1 with dummies for each potential year of exposure and estimating the model separately for individuals from families with incomes below or above the median for their admission year.

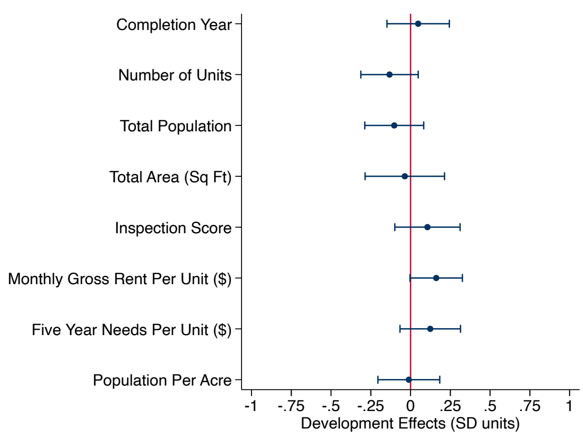
Figure 11: Relationship Between Developments' Effects and Characteristics



(a) Own Tract Characteristics



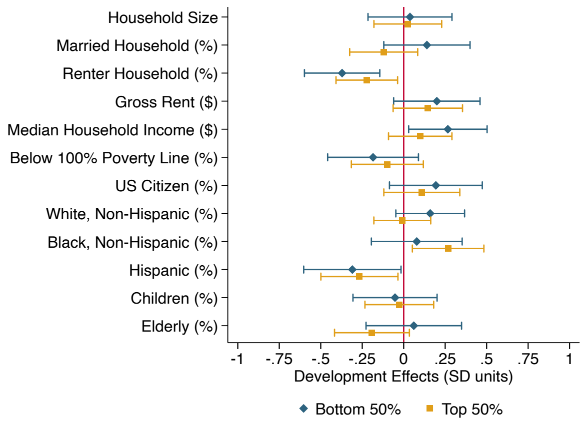
(b) Neighboring Tract Characteristics



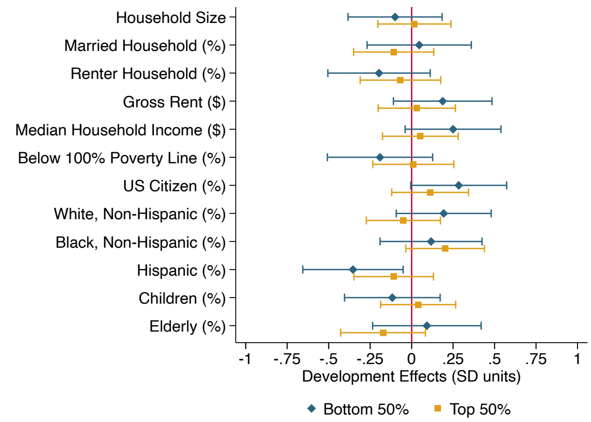
(c) Development Characteristics

Notes: This figure displays the relationship between development characteristics and development effects. We transform all development effects and development characteristics by subtracting the sample mean and dividing by the sample standard deviation. Each estimate and its confidence interval come from a regression of development effects on a single development characteristic.

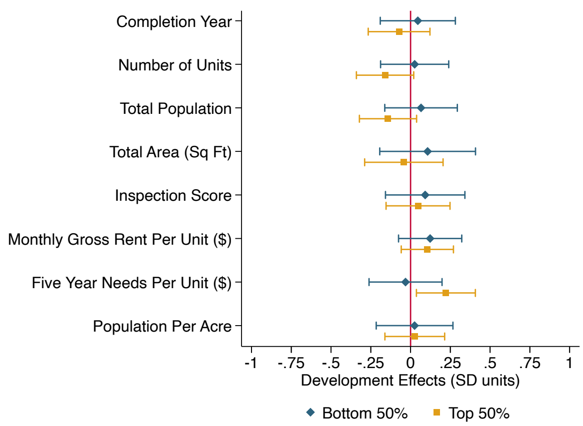
Figure 12: Relationship Between Developments' Effects and Characteristics, by Income Group



(a) Own Tract Characteristics



(b) Neighboring Tract Characteristics



(c) Development Characteristics

Notes: This figure displays the relationship between development characteristics and development effects. We transform all development effects and development characteristics by subtracting the sample mean and dividing by the sample standard deviation. Each estimate and its confidence interval come from a regression of development effects on a single development characteristic, estimated on a particular subsample.

Table 1: Summary Statistics for Main Sample

	Mean	SD
Age at Entry	10.52	4.17
Years in PH	7.02	4.06
Admission Year	2000	3.88
Birth Year	1989	3.82
White, NH (%)	1.25	11.11
Black, NH (%)	40.42	49.07
Asian, NH (%)	3.15	17.47
Hispanic, NH (%)	53.25	49.89
Other Race (%)	1.93	13.74
Native Born (%)	83.71	36.93
Head Native Born (%)	54.09	49.83
Female (%)	49.21	49.99
Family Income ($)	24,040.00	17,550.00
Bottom 50%	12,570.00	5,509.00
Top 50%	34,970.00	18,120.00
Family Size	4.12	1.58
Head Age at Entry	38.24	9.94
Head Married (%)	15.73	36.41
6+ Yrs of Pot Exp (%)	63.22	48.22
Observations	55,500	

Notes: This table presents summary statistics for the main sample. The table contains information on baseline demographic and family characteristics.

Table 2: Effect of Potential Years of Exposure on Observed Years of Childhood Exposure

	All	Bottom 50%	Top 50%
Potential Years of Exposure	0.9223***	0.9252***	0.9200***
	(0.0019)	(0.0027)	(0.0027)
Observations	55,500	27,000	28,500

Notes: This table presents estimates from regressions of the number of years an individual lived in public housing during childhood on their potential years of exposure, which is defined as 18 minus the age they first moved into public housing. Effects are estimated on the main sample by replacing the dependent variable in Equation 1 with the observed years of exposure. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

Table 3: Impacts on Labor Market Outcomes

Outcome	Mean	All	Bottom 50%	Top 50%
Earnings Rank, Individual	18780.00	0.4482***	0.6430***	0.3448***
		(0.0453)	(0.0664)	(0.0650)
Employment, Individual	0.7554	0.0012**	0.0030***	0.0002
		(0.0006)	(0.0009)	(0.0008)
Wages Rank, Family	19200.00	0.3333***	0.5135***	0.2281***
		(0.0481)	(0.0707)	(0.0688)
Employment, Family	0.6693	0.0007	0.0026***	-0.0006
		(0.0006)	(0.0010)	(0.0009)
Observations		55,500	27,000	28,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on labor market outcomes. The outcomes are based on W-2s and 1040s from the year an individual turned 26. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the main sample using Equation 2. The first column presents the means for the (untransformed) dependent variables, calculated over the full sample. The second column presents estimates for the full sample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

$^{*}p < .1, ^{**}p < .05, ^{***}p < .01$

Table 4: Heterogeneity in Impacts on Labor Market Outcomes, by Potential Years of Exposure

	All	Bottom 50%	Top 50%
Earnings Rank, Individual			
Exp Yrs $\times$ Age of Entry ≥ 13	0.3708**	0.2037	0.5574**
	(0.1810)	(0.2611)	(0.2514)
Exp Yrs $\times$ Age of Entry < 13	0.3547***	0.6151***	0.2124**
	(0.0741)	(0.1072)	(0.1053)
Employment, Individual			
Exp Yrs $\times$ Age of Entry ≥ 13	-0.0035	-0.0062*	-0.0008
	(0.0024)	(0.0036)	(0.0033)
Exp Yrs $\times$ Age of Entry < 13	0.0011	0.0043***	-0.0010
	(0.0010)	(0.0014)	(0.0013)
Observations	55,500	27,000	28,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on labor market outcomes by potential years of exposure. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual had at least six years of potential exposure (i.e., moved into public housing before age 13). The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual had at least six years of potential exposure. Effects are estimated on the main sample, adapting Equation 2 by adding the interaction term. The first column presents estimates for the full sample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

*p $< .1$,** p $< .05$,*** p $< .01$

Table 5: Impacts on Safety Net Participation

Outcome	Mean	All	Bottom 50%	Top 50%
Any Safety Net	0.5478	-0.0036***	-0.0046***	-0.0026***
		(0.0007)	(0.0010)	(0.0010)
HUD	0.3816	-0.0027***	-0.0023**	-0.0019**
		(0.0007)	(0.0010)	(0.0010)
SNAP	0.3305	-0.0018***	-0.0039***	-0.0007
		(0.0006)	(0.0010)	(0.0009)
TANF	0.0945	-0.0003	-0.0015**	0.0006
		(0.0004)	(0.0006)	(0.0005)
Observations		55,500	27,000	28,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on participation in safety net programs. The outcomes are all indicators for whether an individual participated in a program in the year an individual turned 26. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the main sample using Equation 2. The first column presents the means for the (untransformed) dependent variables, calculated over the full sample. The second column presents estimates for the full sample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.
*p $<$ .1,** p $<$ .05,*** p $<$ .01

Table 6: Heterogeneity in Impacts on Safety Net Participation, by Potential Years of Exposure

	All	Bottom 50%	Top 50%
Any Safety Net			
Exp Yrs × Age of Entry ≥ 13	-0.0023	-0.0018	-0.0031
	(0.0027)	(0.0039)	(0.0038)
Exp Yrs × Age of Entry < 13	-0.0045***	-0.0064***	-0.0026
	(0.0011)	(0.0017)	(0.0016)
Observations	55,500	27,000	28,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on participation in safety net programs by potential years of exposure. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual had at least six years of potential exposure (i.e., moved into public housing before age 13). The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual had at least six years of potential exposure. Effects are estimated on the main sample, adapting Equation 2 by adding the interaction term. The first column presents estimates for the full sample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.
*p < .1,** p < .05,*** p < .01

Table 7: Impacts on Incarceration

Outcome	Mean	All	Bottom 50%	Top 50%
Ever Incarcerated	0.0310	-0.0000	-0.0005	0.0004
		(0.0002)	(0.0003)	(0.0003)
Observations		55,500	27,000	28,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on criminal justice outcomes. The outcome is an indicator for whether an individual was ever incarcerated by age 26. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the main sample using Equation 2. The first column presents the means for the (untransformed) dependent variables, calculated over the full sample. The second column presents estimates for the full sample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.
$^{*}p < .1, ^{**}p < .05, ^{***}p < .01$

Table 8: Heterogeneity in Impacts on Incarceration, by Potential Years of Exposure

	All	Bottom 50%	Top 50%
Ever Incarcerated			
Exp Yrs $\times$ Age of Entry ≥ 13	0.0025***	0.0024*	0.0025**
	(0.0009)	(0.0013)	(0.0011)
Exp Yrs $\times$ Age of Entry < 13	-0.0009**	-0.0017***	-0.0003
	(0.0004)	(0.0006)	(0.0005)
Observations	55,500	27,000	28,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on criminal justice outcomes by potential years of exposure. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual had at least six years of potential exposure (i.e., moved into public housing before age 13). The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual had at least six years of potential exposure. Effects are estimated on the main sample, adapting Equation 2 by adding the interaction term. The first column presents estimates for the full sample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

$^{*}p < .1, ^{**}p < .05, ^{***}p < .01$

Table 9: Impacts on Marriage and Fertility

Outcome	Mean	All	Bottom 50%	Top 50%
Ever Married	0.1154	-0.0009**	-0.0004	-0.0015**
		(0.0004)	(0.0006)	(0.0006)
Ever Has Dependent	0.4878	-0.0018***	-0.0030***	-0.0012
		(0.0007)	(0.0010)	(0.0009)
Observations		55,500	27,000	28,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on family structure. The outcomes are indicators for whether an individual ever filed a 1040 and either reported being married or having a dependent by age 26. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the main sample using Equation 2. The first column presents the means for the (untransformed) dependent variables, calculated over the full sample. The second column presents estimates for the full sample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

$^{*}p < .1,^{**}p < .05,^{***}p < .01$

Table 10: Impacts on Teen Parenthood Among Women

Outcome	Mean	All	Bottom 50%	Top 50%
Teen Parenthood, Females	0.2048	-0.0005	-0.0030***	0.0016
		(0.0008)	(0.0012)	(0.0010)
Observations		27,500	13,500	14,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on teen parenthood among women in our sample. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the main sample using Equation 2. The first column presents the means for the (untransformed) dependent variables, calculated over the full sample. The second column presents estimates for the full sample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.
$^{*}p < .1, ^{**} p < .05, ^{***} p < .01$

Table 11: Heterogeneity in Impacts on Teen Parenthood, by Potential Years of Exposure

	All	Bottom 50%	Top 50%
Teen Pregnancy, Females			
Exp Yrs $\times$ Age of Entry ≥ 13	-0.0019	-0.0016	-0.0018
	(0.0032)	(0.0048)	(0.0042)
Exp Yrs $\times$ Age of Entry < 13	0.0001	-0.0030	0.0026
	(0.0013)	(0.0019)	(0.0017)
Observations	27,500	13,500	14,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on teen parenthood among women in our sample by potential years of exposure. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual had at least six years of potential exposure (i.e., moved into public housing before age 13). The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual had at least six years of potential exposure. Effects are estimated on the main sample, adapting Equation 2 by adding the interaction term. The first column presents estimates for the full sample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

$^{*}p < .1, ^{**} p < .05, ^{***} p < .01$

Table 12: Impacts on Mortality

Outcome	Mean	All	Bottom 50%	Top 50%
Mortality	0.0098	0.0000	-0.0001	0.0001
		(0.0001)	(0.0002)	(0.0002)
Observations		55,500	27,000	28,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on mortality. The outcomes are indicators for whether an individual ever filed a 1040 and either reported being married or having a dependent by age 26. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the main sample using Equation 2. The first column presents the means for the (untransformed) dependent variables, calculated over the full sample. The second column presents estimates for the full sample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level. $^{*}p < .1, ^{**}p < .05, ^{***}p < .01$

Table 13: Heterogeneity in Impacts on Mortality, by Potential Years of Exposure

	All	Bottom 50%	Top 50%
Mortality			
Exp Yrs $\times$ Age of Entry ≥ 13	0.0003	0.0008	0.0000
	(0.0005)	(0.0008)	(0.0007)
Exp Yrs $\times$ Age of Entry < 13	-0.0002	-0.0005	-0.0001
	(0.0002)	(0.0003)	(0.0003)
Observations	55,500	27,000	28,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on mortality by potential years of exposure. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual had at least six years of potential exposure (i.e., moved into public housing before age 13). The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual had at least six years of potential exposure. Effects are estimated on the main sample, adapting Equation 2 by adding the interaction term. The first column presents estimates for the full sample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.
$^{*}p < .1, ^{**} p < .05, ^{***} p < .01$

Table 14: Heterogeneity in Impacts by Race/Ethnicity

	All	Bottom 50%	Top 50%
Earnings Rank, Individual			
Exp Yrs	0.4425***	0.6087***	0.3559***
	(0.0581)	(0.0834)	(0.0831)
Exp Yrs × Black, NH	0.0033	0.0998	-0.0439
	(0.0768)	(0.1084)	(0.1089)
Employment, Individual			
Exp Yrs	0.0012*	0.0027**	0.0004
	(0.0007)	(0.0011)	(0.0010)
Exp Yrs × Black, NH	-0.0000	0.0012	-0.0008
	(0.0010)	(0.0015)	(0.0014)
Any Safety Net			
Exp Yrs	-0.0030***	-0.0037***	-0.0022*
	(0.0009)	(0.0013)	(0.0013)
Exp Yrs × Black, NH	-0.0015	-0.0024	-0.0009
	(0.0012)	(0.0017)	(0.0017)
Ever Incarcerated			
Exp Yrs	0.0002	-0.0002	0.0005
	(0.0003)	(0.0004)	(0.0004)
Exp Yrs × Black, NH	-0.0004	-0.0007	-0.0002
	(0.0004)	(0.0006)	(0.0005)
Mortality			
Exp Yrs	0.0001	-0.0001	0.0002
	(0.0001)	(0.0002)	(0.0002)
Exp Yrs × Black, NH	-0.0000	0.0001	-0.0002
	(0.0002)	(0.0003)	(0.0003)
Observations	52,000	25,500	26,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on some key outcomes by race and ethnicity. The sample in this table is restricted to individuals who are non-Hispanic Black or Hispanic. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual is non-Hispanic Black. The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual is non-Hispanic Black. Effects are estimated on the main sample, adapting Equation 2 by adding the interaction term. The first column presents estimates for the full sample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.
$^{*}p < .1, ^{**}p < .05, ^{***}p < .01$

Table 15: Heterogeneity in Impacts by Gender

	All	Bottom 50%	Top 50%
Earnings Rank, Individual			
Exp Yrs	0.2953***	0.3922***	0.2861***
	(0.0597)	(0.0863)	(0.0851)
Exp Yrs × Female	0.3095***	0.5014***	0.1198
	(0.0722)	(0.1022)	(0.1018)
Employment, Individual			
Exp Yrs	-0.0003	0.0012	-0.0011
	(0.0008)	(0.0012)	(0.0011)
Exp Yrs × Female	0.0031***	0.0035***	0.0026**
	(0.0009)	(0.0014)	(0.0013)
Any Safety Net			
Exp Yrs	-0.0025***	-0.0017	-0.0032**
	(0.0009)	(0.0013)	(0.0013)
Exp Yrs × Female	-0.0021*	-0.0058***	0.0012
	(0.0011)	(0.0016)	(0.0015)
Ever Incarcerated			
Exp Yrs	0.0003	-0.0006	0.0011**
	(0.0004)	(0.0006)	(0.0005)
Exp Yrs × Female	-0.0007**	0.0001	-0.0015***
	(0.0004)	(0.0005)	(0.0005)
Mortality			
Exp Yrs	0.0001	-0.0001	0.0002
	(0.0002)	(0.0003)	(0.0003)
Exp Yrs × Female	-0.0001	-0.0000	-0.0002
	(0.0002)	(0.0003)	(0.0003)
Observations	55,500	27,000	28,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on some key outcomes by gender. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual is female. The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual is female. Effects are estimated on the main sample, adapting Equation 2 by adding the interaction term. The first column presents estimates for the full sample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level. Standard errors are clustered at the household level.

*p $< .1$, ** p $< .05$, *** p $< .01$

Table 16: Summary Statistics for Public Housing Developments

	Mean	SD
A. Own Tract Characteristics		
Household Size	2.81	0.31
Married Household (%)	22.08	6.90
Renter Household (%)	92.81	8.41
Gross Rent ($)	539.91	183.24
Median Household Income ($)	26,015.05	10,807.18
Below 100% Poverty Line (%)	43.88	10.50
US Citizen (%)	71.37	9.99
White, Non-Hispanic (%)	6.98	11.87
Black, Non-Hispanic (%)	48.57	20.92
Hispanic (%)	41.36	18.41
Children (%)	33.83	5.75
Elderly (%)	9.57	3.44
B. Neighboring Tract Characteristics		
Household Size	2.76	0.38
Married Household (%)	29.83	10.14
Renter Household (%)	80.67	14.87
Gross Rent ($)	855.56	258.19
Median Household Income ($)	41,669.32	17,841.05
Below 100% Poverty Line (%)	31.81	12.58
US Citizen (%)	64.10	10.19
White, Non-Hispanic (%)	17.05	21.47
Black, Non-Hispanic (%)	40.81	26.02
Hispanic (%)	36.89	20.43
Children (%)	29.00	7.04
Elderly (%)	10.04	3.58
C. Development Characteristics		
Completion Year	1956	9.69
Number of Units	1,384.00	493.80
Total Population	2,888.00	1,033.00
Total Area (Sq Ft)	856,400.00	504,900.00
Inspection Score	50.35	22.23
Monthly Gross Rent Per Unit ($)	473.61	38.50
Five Year Needs Per Unit ($)	319,692.32	41,436.26
Population Per Acre	299.10	138.60
J	150	

Notes: This table presents summary statistics for public housing developments. Panel A reports the average characteristics, measured in the 2000 decennial census, of Census tracts where developments are located. Panel B does the same, but uses the characteristics of neighboring tracts instead of the tract where the development is located. To construct Panel B, we first average the characteristics of the five tracts closest to the one where the development is located. Panel C summarizes characteristics of public housing developments themselves.

Table 17: Balance Tests Based on Predicted Outcomes

	All		Bottom 50%		Top 50%	
	Intercept (1)	Slope (2)	Intercept (3)	Slope (4)	Intercept (5)	Slope (6)
Earnings Rank, Individual						
Mean Development Effect	0.1902	-0.0255	0.1532	-0.0206	0.2458	-0.0328
Std Dev	0.2788	0.0174	0.2934	0.0229	0.3294	0.0238
Bias-Corrected Std Dev	0.2454	0.0090	0.2294	0.0101	0.2766	0.0132
Standard Error of Bias-Corrected Std Dev	0.0157	0.0037	0.0201	0.0043	0.0225	0.0041
Any Safety Net						
Mean Development Effect	-0.1930	0.0259	-0.1731	0.0231	-0.2189	0.0296
Std Dev	0.2706	0.0166	0.2899	0.0196	0.3021	0.0229
Bias-Corrected Std Dev	0.2421	0.0086	0.2354	-	0.2524	0.0132
Standard Error of Bias-Corrected Std Dev	0.0100	0.0025	0.0155	0.0043	0.0143	0.0024
Ever Incarcerated						
Mean Development Effect	-0.1518	0.0203	-0.1489	0.0198	-0.1576	0.0211
Std Dev	0.1743	0.0151	0.2231	0.0198	0.1970	0.0217
Bias-Corrected Std Dev	0.1304	0.0063	0.1504	0.0041	0.1158	0.0112
Standard Error of Bias-Corrected Std Dev	0.0130	0.0030	0.0184	0.0041	0.0209	0.0032
N	55,500	55,500	27,000	27,000	28,500	28,500
J	150	150	150	150	150	150

Notes: This table presents the results of tests for balance on predicted outcomes. Predicted outcomes are generated from regressions of outcomes on birth cohort fixed effects and baseline characteristics, including residualized family size, residualized marital status, residualized family income, and indicators for race/ethnicity and sex. We transform all predicted outcomes by subtracting the mean and dividing by the standard deviation. Each panel is associated with estimating Equation 3 using a different predicted outcome. The first set of columns are associated with regressions including the full sample, while the second and third set of columns are associated with children from families with below and above median incomes among families in their admission year, respectively. With each set of columns, the two columns analyze the intercepts (η_j) and slopes (θ_j), respectively. The first row reports the mean of the estimated coefficients. The second row reports the raw standard deviation of the coefficients. The third produces bias-corrected estimates of the standard deviation. The fourth row presents standard errors on the bias-corrected standard deviations, constructed using a development-clustered bootstrap.

Table 18: Effects of Public Housing Developments

	All		Bottom 50%		Top 50%	
	Intercept (1)	Slope (2)	Intercept (3)	Slope (4)	Intercept (5)	Slope (6)
Mean Development Effect	49.7100	0.4273	44.9200	0.6108	52.4800	0.3288
Std Dev	3.9070	0.4604	5.9040	0.6778	5.6170	0.6762
Bias-Corrected Std Dev	1.2040	0.1254	2.8130	0.2562	2.1270	0.2885
Standard Error of Bias-Corrected Std Dev	0.8665	0.1009	0.9028	0.1263	1.0770	0.1167
N	55,500	55,500	27,000	27,000	28,500	28,500
J	150	150	150	150	150	150

Notes: This table presents the results of tests for balance on predicted outcomes. Predicted outcomes are generated from regressions of outcomes on birth cohort fixed effects and baseline characteristics, including residualized family size, residualized marital status, residualized family income, and indicators for race/ethnicity and sex. We transform all predicted outcomes by subtracting the mean and dividing by the standard deviation. Each panel is associated with estimating Equation 3 using a different predicted outcome. The first set of columns are associated with regressions including the full sample, while the second and third set of columns are associated with children from families with below and above median incomes among families in their admission year, respectively. With each set of columns, the two columns analyze the intercepts (η_j) and slopes (θ_j), respectively. The first row reports the mean of the estimated coefficients. The second row reports the raw standard deviation of the coefficients. The third produces bias-corrected estimates of the standard deviation. The fourth row presents standard errors on the bias-corrected standard deviations, constructed using a development-clustered bootstrap.

Table 19: Explanatory Power of Predictors

Set of Characteristics	All (1)	Bottom 50% (2)	Top 50% (3)
All	0.3397	0.2763	0.2850
Own Tract	0.1251	0.0422	0.1166
Neighboring Tract	0.1703	0.0672	0.1202
Development	0.0740	0.0402	0.0687
Observations	150	150	150

Notes: This table presents the R^2 on regressions of development effects on different sets of characteristics. In the first column, we report results from regressions using development effects estimated on the full sample. The second and third set of columns are associated with children from families with below and above median incomes among families in their admission year, respectively. Estimates in the first row come from regressions of development effects on all the characteristics in Table 16. The second row uses just the tract characteristics as predictors. The third row reports results from regressions using just the characteristics of a development's neighboring tracts. The last row focuses just on the development's own characteristics.

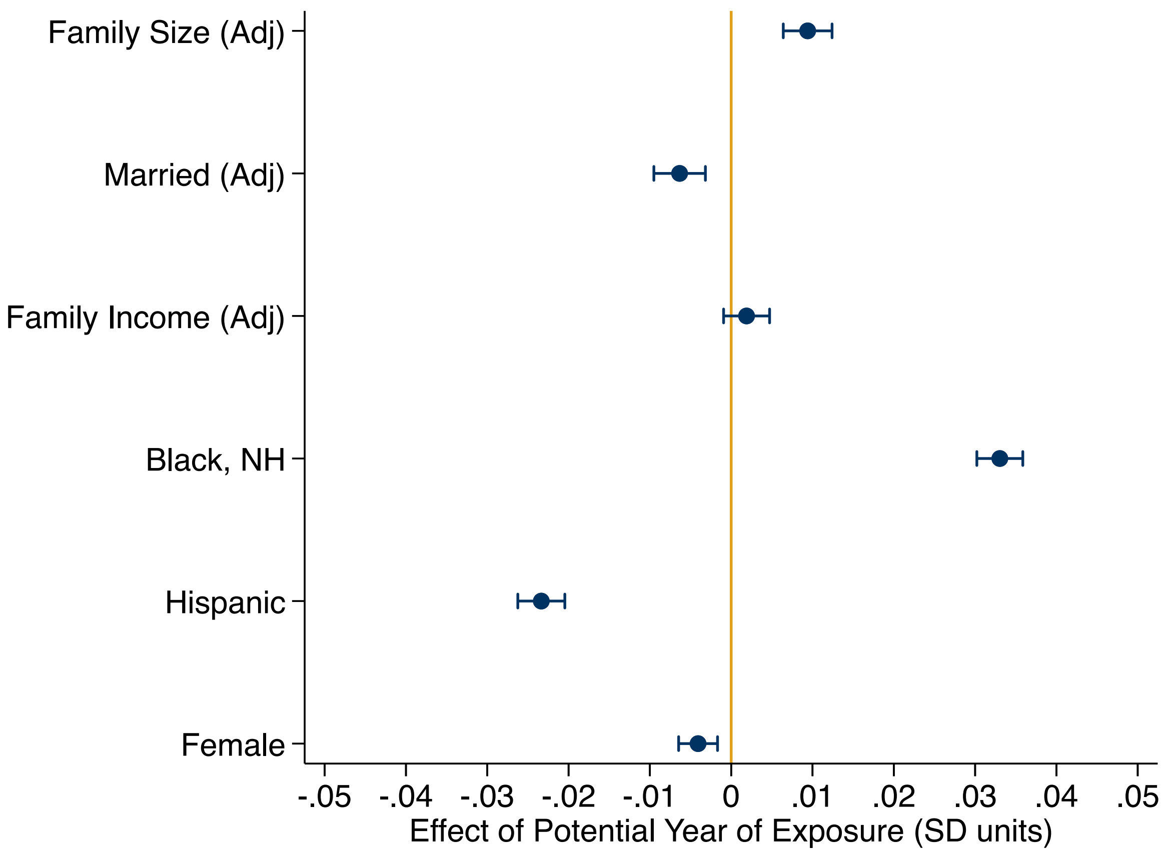
Table 20: Components of MVPF

	Earnings	HUD	SNAP	TANF	Total
PV at Age 10	865,127	2,427	4,994	285	
PV at Age 10 for Sample	489,080	31,043	9,846	3,093	
Panel A. All					
Est Impact (%)	1.31	-0.71	-0.55	-0.31	
Change in PV	6,430.27	-220.95	-53.89	-9.51	6,145.93
Cost to Govt	-829.51	-220.95	-53.89	-9.51	-1113.85
WTP	5,600.77	-220.95	-43.11	-9.51	5,327.20
Panel B. Bottom 50%					
Est Impact (%)	1.89	-0.60	-1.19	-1.57	
Change in PV	9,225.04	-185.48	-117.37	-48.68	8,873.51
Cost to Govt	-1190.03	-185.48	-117.37	-48.68	-1541.56
WTP	8,035.01	-185.48	-93.90	-48.68	7,706.95
Panel C. Top 50%					
Est Impact (%)	1.01	-0.50	-0.21	0.63	
Change in PV	4,946.80	-155.22	-20.27	19.53	4,790.85
Cost to Govt	-638.14	-155.22	-20.27	19.53	-794.09
WTP	4,308.67	-155.22	-16.22	19.53	4,156.77

Notes: This table reports the components used to calculate the marginal value of public funds. The first row estimates the present value of lifetime earnings and benefit receipt nationally, measured at age 10. The second row calculates this quantity for our sample. In Panel A, we reports components for the full sample. In the first row, we report the impacts of spending a year in public housing on earnings and program participation, rescaled to percent impacts. In the second row, we calculate the change in the present value of earnings and program participation due to spending an additional year in childhood in public housing. In the third row, we report how these changes affect the government's budget in the long run. In the fourth row, we present individuals' willingness to pay for the changes in earnings and program benefits. In Panels B and C, we report the same calculations for individuals from families with below or above median incomes among families in their admission year, respectively.

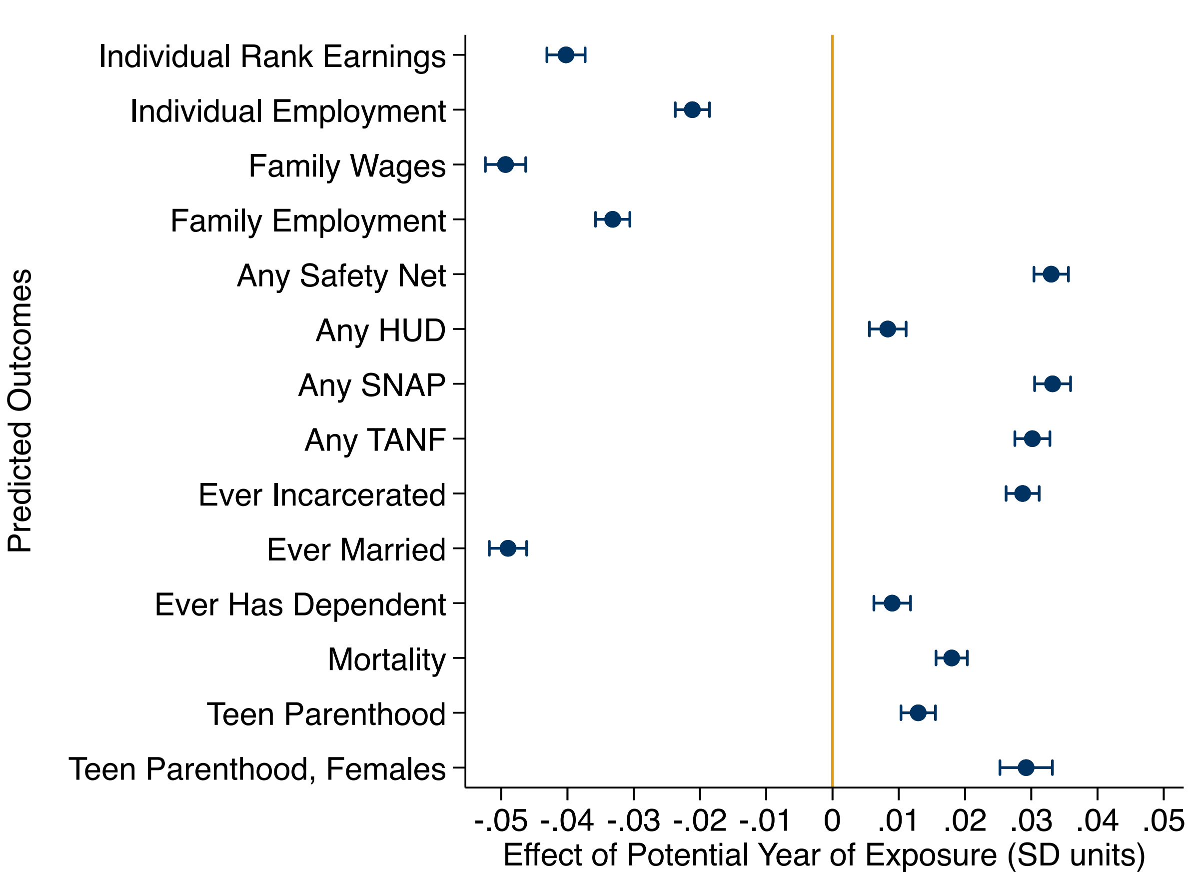
A Additional Figures and Tables

Figure A.1: Balance Tests Using Baseline Characteristics, Excluding Control for Nativity



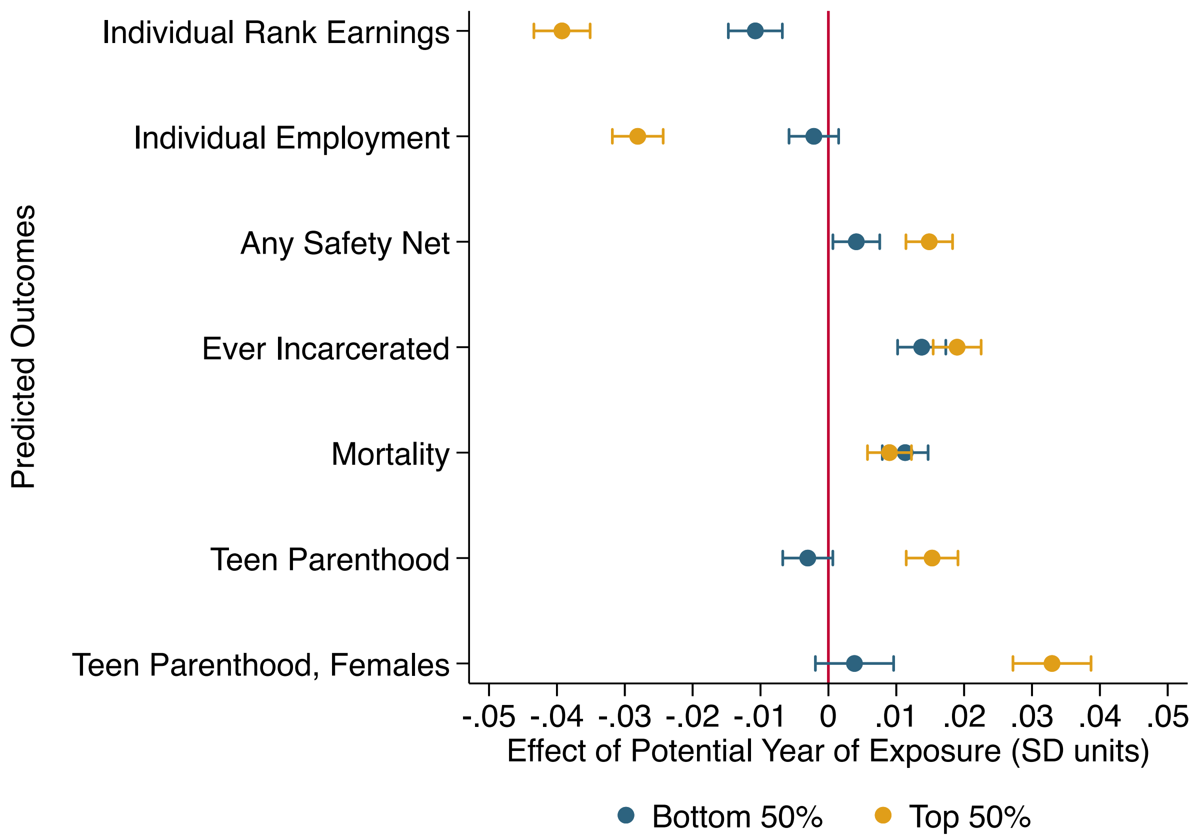
Notes: This figure displays results from tests for balance on baseline characteristics. Family size, family marital status, and family income are residualized to account for parent lifecycle effects and macroeconomic conditions. We transform all characteristics by subtracting the sample mean and dividing by the sample standard deviation. The coefficients and confidence intervals are constructed from regressions of the standardized characteristic on potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood and birth cohort fixed effects.

Figure A.2: Balance Tests Using Predicted Outcomes, Excluding Control for Nativity



Notes: This figure displays the results of tests for balance on predicted outcomes. Predicted outcomes are generated from regressions of outcomes on birth cohort fixed effects and baseline characteristics, including residualized family size, residualized family marital status, residualized family income, and indicators for race/ethnicity and sex. We transform all predicted outcomes by subtracting the mean and dividing by the standard deviation. The coefficients and confidence intervals are constructed from regressions of the standardized predicted outcomes on potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, and all covariates used in generating the predicted outcomes.

Figure A.3: Balance Tests Using Predicted Outcomes, by Income Group



Notes: This figure displays the results of tests for balance on predicted outcomes. Predicted outcomes are generated from regressions of outcomes on birth cohort fixed effects and baseline characteristics, including residualized family size, residualized family marital status, residualized family income, and indicators for race/ethnicity and sex. We transform all predicted outcomes by subtracting the mean and dividing by the standard deviation. The coefficients and confidence intervals are constructed from regressions of the standardized predicted outcomes on potential years of exposure, defined as 18 minus the age an individual entered public housing in childhood, and all covariates used in generating the predicted outcomes.

Table A.1: Distribution of Admission Year Among Sample

Admission Year	Count
1994	900
1995	6600
1996	6000
1997	5700
1998	7900
1999	5100
2000	4300
2001	3200
2002	3200
2003	2400
2004	2400
2005	2400
2006	1900
2007	1400
2008	900
2009	650
2010	500
2011	200

Notes: This table presents the distribution of sample members' admission year.

Table A.2: Distribution of Birth Year Among Sample

Birth Year	Count
1979	500
1980	750
1981	1200
1982	1600
1983	1900
1984	2200
1985	2600
1986	2900
1987	3400
1988	4100
1989	4800
1990	5200
1991	5600
1992	6000
1993	6300
1994	6500

Notes: This table presents the distribution of sample members' birth year.

Table A.3: Summary Statistics for Siblings Analysis

	Mean	SD
Age at Entry	10.26	4.08
Years in PH	7.29	4.00
Admission Year	1999	3.45
Birth Year	1989	3.75
White, NH (%)	1.14	10.59
Black, NH (%)	38.89	48.75
Asian, NH (%)	2.98	17.01
Hispanic, NH (%)	55.16	49.73
Other Race (%)	1.82	13.39
Native Born (%)	82.70	37.83
Head Native Born (%)	52.06	49.96
Female (%)	49.37	50.00
Family Income ($)	23,650.00	16,800.00
Bottom 50%	12,830.00	5,699.00
Top 50%	33,220.00	17,560.00
Family Size	4.66	1.53
Head Age at Entry	37.33	8.80
Head Married (%)	16.22	36.86
6+ Yrs of Pot Exp (%)	66.51	47.20
Observations	32,000	

Notes: This table presents summary statistics for the siblings subsample. The table contains information on baseline demographic and family characteristics.

Table A.4: Effect of Potential Years of Exposure on Observed Years of Childhood Exposure, Siblings Analysis

	All	Bottom 50%	Top 50%
Potential Years of Exposure	0.9299***	0.9272***	0.9311***
	(0.0105)	(0.0150)	(0.0147)
Observations	31,500	15,000	17,000

Notes: This table presents estimates from regressions of the number of years an individual lived in public housing during childhood on their potential years of exposure, which is defined as 18 minus the age they first moved into public housing. Effects are estimated on the siblings subsample by replacing the dependent variable in Equation 1 with the observed years of exposure, adding household fixed effects and a control for birth order, and dropping controls with values common within households. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

Table A.5: Impacts on Labor Market Outcomes, Siblings Analysis

Outcome	Mean	All	Bottom 50%	Top 50%
Earnings Rank, Individual	18390.00	0.3326	0.5554	0.0699
		(0.2964)	(0.3997)	(0.4418)
Employment, Individual	0.7516	-0.0034	0.0025	-0.0100*
		(0.0040)	(0.0055)	(0.0059)
Wages Rank, Family	19030.00	0.5466*	0.8130**	0.2499
		(0.3006)	(0.3998)	(0.4520)
Employment, Family	0.6686	0.0035	0.0082	-0.0017
		(0.0043)	(0.0058)	(0.0064)
Observations		31,500	15,000	17,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on labor market outcomes. The outcomes are based on W-2s and 1040s from the year an individual turned 26. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the siblings subsample, adapting Equation 2 by adding household fixed effects and a control for birth order, and dropping controls with values common within households. The first column presents the means for the (untransformed) dependent variables, calculated over the full siblings subsample. The second column presents estimates for the full siblings subsample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

*p $< .1$,** p $< .05$,*** p $< .01$

Table A.6: Heterogeneity in Impacts on Labor Market Outcomes, by Potential Years of Exposure, Siblings Analysis

	All	Bottom 50%	Top 50%
Earnings Rank, Individual			
Exp Yrs $\times$ Age of Entry ≥ 13	0.3126	0.7043	-0.0937
	(0.4217)	(0.6128)	(0.5871)
Exp Yrs $\times$ Age of Entry < 13	0.3639	0.5047	0.1805
	(0.3107)	(0.4226)	(0.4597)
Employment, Individual			
Exp Yrs $\times$ Age of Entry ≥ 13	-0.0080	-0.0037	-0.0136*
	(0.0057)	(0.0085)	(0.0078)
Exp Yrs $\times$ Age of Entry < 13	-0.0021	0.0045	-0.0092
	(0.0042)	(0.0058)	(0.0061)
Observations	31,500	15,000	17,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on labor market outcomes by potential years of exposure. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual had at least six years of potential exposure (i.e., moved into public housing before age 13). The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual had at least six years of potential exposure. Effects are estimated on the siblings subsample, adapting Equation 2 by adding the interaction term, household fixed effects and a control for birth order, and dropping controls with values common within households. The first column presents estimates for the full sample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

*p $< .1$,** p $< .05$,*** p $< .01$

Table A.7: Impacts on Safety Net Participation, Siblings Analysis

Outcome	Mean	All	Bottom 50%	Top 50%
Any Safety Net	0.5440	-0.0057	-0.0113*	0.0002
		(0.0045)	(0.0064)	(0.0064)
HUD	0.3664	0.0011	-0.0049	0.0075
		(0.0044)	(0.0061)	(0.0065)
SNAP	0.3380	-0.0091**	-0.0067	-0.0116**
		(0.0041)	(0.0058)	(0.0059)
TANF	0.0979	-0.0082***	-0.0065	-0.0099**
		(0.0030)	(0.0042)	(0.0043)
Observations		31,500	15,000	17,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on participation in safety net programs. The outcomes are all indicators for whether an individual participated in a program in the year an individual turned 26. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the siblings subsample, adapting Equation 2 by adding household fixed effects and a control for birth order, and dropping controls with values common within households. The first column presents the means for the (untransformed) dependent variables, calculated over the full siblings subsample. The second column presents estimates for the full siblings subsample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.
*p < .1,** p < .05,*** p < .01

Table A.8: Heterogeneity in Impacts on Safety Net Participation, by Potential Years of Exposure, Siblings Analysis

	All	Bottom 50%	Top 50%
Any Safety Net			
Exp Yrs $\times$ Age of Entry $\geq$ 13	-0.0067	-0.0041	-0.0075
	(0.0065)	(0.0095)	(0.0090)
Exp Yrs $\times$ Age of Entry $<$ 13	-0.0073	-0.0149**	0.0004
	(0.0047)	(0.0066)	(0.0066)
Observations	31,500	15,000	17,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on safety net participation by potential years of exposure. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual had at least six years of potential exposure (i.e., moved into public housing before age 13). The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual had at least six years of potential exposure. Effects are estimated on the siblings subsample, adapting Equation 2 by adding the interaction term, household fixed effects and a control for birth order, and dropping controls with values common within households. The first column presents estimates for the full sample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

*p $<$ .1,** p $<$ .05,*** p $<$ .01

Table A.9: Impacts on Incarceration, Siblings Analysis

Outcome	Mean	All	Bottom 50%	Top 50%
Ever Incarcerated	0.0335	0.0008	0.0010	0.0009
		(0.0016)	(0.0024)	(0.0021)
Observations		31,500	15,000	17,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on criminal justice outcomes. The outcome is an indicator for whether an individual was ever incarcerated by age 26. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the siblings subsample, adapting Equation 2 by adding household fixed effects and a control for birth order, and dropping controls with values common within households. The first column presents the means for the (untransformed) dependent variables, calculated over the full siblings subsample. The second column presents estimates for the full siblings subsample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

$^{*}p < .1,^{**} p < .05,^{***} p < .01$

Table A.10: Heterogeneity in Impacts on Incarceration, by Potential Years of Exposure, Siblings Analysis

	All	Bottom 50%	Top 50%
Ever Incarcerated			
Exp Yrs $\times$ Age of Entry ≥ 13	0.0024	0.0001	0.0043
	(0.0022)	(0.0034)	(0.0028)
Exp Yrs $\times$ Age of Entry < 13	-0.0004	-0.0002	-0.0003
	(0.0017)	(0.0025)	(0.0022)
Observations	31,500	15,000	17,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on criminal justice outcomes, by potential years of exposure. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual had at least six years of potential exposure (i.e., moved into public housing before age 13). The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual had at least six years of potential exposure. Effects are estimated on the siblings subsample, adapting Equation 2 by adding the interaction term, household fixed effects and a control for birth order, and dropping controls with values common within households. The first column presents estimates for the full sample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

*p $< .1$,** p $< .05$,*** p $< .01$

Table A.11: Impacts on Marriage and Fertility, Siblings Analysis

Outcome	Mean	All	Bottom 50%	Top 50%
Ever Married	0.1232	-0.0017	0.0025	-0.0063
		(0.0028)	(0.0038)	(0.0042)
Ever Has Dependent	0.5151	-0.0029	-0.0001	-0.0057
		(0.0044)	(0.0062)	(0.0064)
Observations		31,500	15,000	17,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on family structure. The outcomes are indicators for whether an individual ever filed a 1040 and either reported being married or having a dependent by age 26. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the siblings subsample, adapting Equation 2 by adding household fixed effects and a control for birth order, and dropping controls with values common within households. The first column presents the means for the (untransformed) dependent variables, calculated over the full siblings subsample. The second column presents estimates for the full siblings subsample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

$^{*}p < .1, ^{**}p < .05, ^{***}p < .01$

Table A.12: Impacts on Teen Parenthood Among Women, Siblings Analysis

Outcome	Mean	All	Bottom 50%	Top 50%
Teen Parenthood, Females	0.2239	-0.0037	-0.0043	-0.0040
		(0.0066)	(0.0088)	(0.0098)
Observations		10,000	4,700	5,300

Notes: This table presents the impact of spending one additional year in public housing during childhood on teen parenthood among women in our sample. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the siblings subsample, adapting Equation 2 by adding household fixed effects and a control for birth order, and dropping controls with values common within households. The first column presents the means for the (untransformed) dependent variables, calculated over the full siblings subsample. The second column presents estimates for the full siblings subsample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

*p $< .1$,** p $< .05$,*** p $< .01$

Table A.13: Heterogeneity in Impacts on Teen Parenthood Among Women, by Potential Years of Exposure, Siblings Analysis

	All	Bottom 50%	Top 50%
Teen Pregnancy, Females			
Exp Yrs × Age of Entry ≥ 13	-0.0143	-0.0094	-0.0190
	(0.0097)	(0.0139)	(0.0135)
Exp Yrs × Age of Entry < 13	-0.0002	-0.0030	0.0010
	(0.0070)	(0.0095)	(0.0102)
Observations	10,000	4,700	5,300

Notes: This table presents the impact of spending one additional year in public housing during childhood on teen parenthood among women in our sample, by potential years of exposure. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual had at least six years of potential exposure (i.e., moved into public housing before age 13). The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual had at least six years of potential exposure. Effects are estimated on the siblings subsample, adapting Equation 2 by adding the interaction term, household fixed effects and a control for birth order, and dropping controls with values common within households. The first column presents estimates for the full sample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

*p $<$ .1,** p $<$ .05,*** p $<$ .01

Table A.14: Impacts on Mortality, Siblings Analysis

Outcome	Mean	All	Bottom 50%	Top 50%
Mortality	0.0094	-0.0006	-0.0004	-0.0011
		(0.0010)	(0.0015)	(0.0011)
Observations		31,500	15,000	17,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on mortality. The outcomes are indicators for whether an individual ever filed a 1040 and either reported being married or having a dependent by age 26. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. Effects are estimated on the siblings subsample, adapting Equation 2 by adding household fixed effects and a control for birth order, and dropping controls with values common within households. The first column presents estimates for the full sample. The third and fourth columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

*p $<$ .1,** p $<$ .05,*** p $<$ .01

Table A.15: Heterogeneity in Impacts on Mortality, by Potential Years of Exposure, Siblings Analysis

	All	Bottom 50%	Top 50%
Mortality			
Exp Yrs $\times$ Age of Entry ≥ 13	-0.0010	-0.0013	-0.0010
	(0.0012)	(0.0018)	(0.0016)
Exp Yrs $\times$ Age of Entry < 13	-0.0006	0.0001	-0.0015
	(0.0010)	(0.0016)	(0.0012)
Observations	31,500	15,000	17,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on mortality, by potential years of exposure. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual had at least six years of potential exposure (i.e., moved into public housing before age 13). The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual had at least six years of potential exposure. Effects are estimated on the siblings subsample, adapting Equation 2 by adding the interaction term, household fixed effects and a control for birth order, and dropping controls with values common within households. The first column presents the means for the (untransformed) dependent variables, calculated over the full siblings subsample. The second column presents estimates for the full siblings subsample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.
*p $< .1$,** p $< .05$,*** p $< .01$

Table A.16: Heterogeneity in Impacts by Race/Ethnicity, Siblings Analysis

	All	Bottom 50%	Top 50%
Earnings Rank, Individual			
Exp Yrs	0.2792	0.3446	0.1607
	(0.3094)	(0.4166)	(0.4652)
Exp Yrs × Black, NH	-0.0252	0.1485	-0.1438
	(0.1556)	(0.2266)	(0.2141)
Employment, Individual			
Exp Yrs	-0.0042	-0.0000	-0.0092
	(0.0042)	(0.0057)	(0.0061)
Exp Yrs × Black, NH	0.0010	0.0021	0.0005
	(0.0022)	(0.0032)	(0.0029)
Any Safety Net			
Exp Yrs	-0.0024	-0.0115*	0.0076
	(0.0047)	(0.0066)	(0.0067)
Exp Yrs × Black, NH	-0.0034	-0.0035	-0.0035
	(0.0024)	(0.0035)	(0.0033)
Ever Incarcerated			
Exp Yrs	0.0017	0.0019	0.0017
	(0.0017)	(0.0025)	(0.0023)
Exp Yrs × Black, NH	-0.0018*	-0.0024	-0.0013
	(0.0010)	(0.0015)	(0.0012)
Mortality			
Exp Yrs	-0.0000	0.0007	-0.0010
	(0.0009)	(0.0013)	(0.0012)
Exp Yrs × Black, NH	-0.0000	0.0002	-0.0002
	(0.0005)	(0.0007)	(0.0007)
Observations	29,500	14,000	15,500

Notes: This table presents the impact of spending one additional year in public housing during childhood on some key outcomes by race and ethnicity. The sample in this table is restricted to individuals who are non-Hispanic Black or Hispanic. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual is non-Hispanic Black. The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual is non-Hispanic Black. Effects are estimated on the siblings subsample, adapting Equation 2 by adding the interaction term, household fixed effects, and a control for birth order, and dropping controls with values common within households. The first column presents estimates for the full siblings subsample. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

$^{*}p < .1, ^{**}p < .05, ^{***}p < .01$

Table A.17: Heterogeneity in Impacts by Gender, Siblings Analysis

	All	Bottom 50%	Top 50%
Earnings Rank, Individual			
Exp Yrs	0.2632	0.4815	0.0077
	(0.3029)	(0.4092)	(0.4508)
Exp Yrs × Female	0.1451	0.1542	0.1311
	(0.1187)	(0.1748)	(0.1618)
Employment, Individual			
Exp Yrs	-0.0044	0.0020	-0.0114*
	(0.0041)	(0.0057)	(0.0060)
Exp Yrs × Female	0.0020	0.0010	0.0028
	(0.0016)	(0.0025)	(0.0022)
Any Safety Net			
Exp Yrs	-0.0055	-0.0092	-0.0011
	(0.0046)	(0.0065)	(0.0066)
Exp Yrs × Female	-0.0005	-0.0044	0.0026
	(0.0018)	(0.0027)	(0.0025)
Ever Incarcerated			
Exp Yrs	0.0007	-0.0001	0.0016
	(0.0017)	(0.0025)	(0.0022)
Exp Yrs × Female	0.0002	0.0023**	-0.0014
	(0.0007)	(0.0010)	(0.0010)
Mortality			
Exp Yrs	-0.0005	-0.0003	-0.0010
	(0.0010)	(0.0016)	(0.0012)
Exp Yrs × Female	-0.0002	-0.0001	-0.0003
	(0.0004)	(0.0005)	(0.0005)
Observations	31,500	15,000	17,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on some key outcomes by gender. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, interacted with an indicator for whether an individual is female. The independent variables are instrumented by the age an individual first moved into public housing, interacted with an indicator for whether an individual is female. Effects are estimated on the siblings subsample, adapting Equation 2 by adding the interaction term, household fixed effects, and a control for birth order, and dropping controls with values common within households. The second and third columns show results for individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level. Standard errors are clustered at the household level.

*p $< .1$,** p $< .05$,*** p $< .01$

Table A.18: Comparison of Impacts Across Alternative Specifications

	Main		Siblings	
Outcome	Birth Yr FE (1)	+Covars (2)	HHFE (3)	+Birth Yr FE (4)
Panel A. All				
Earnings Rank, Individual	0.1870***	0.4482***	0.7286***	0.3326
	(0.0448)	(0.0453)	(0.1357)	(0.2964)
Employment, Individual	-0.0001	0.0012**	0.0070***	-0.0034
	(0.0006)	(0.0006)	(0.0018)	(0.0040)
Any Safety Net	-0.0002	-0.0036***	0.0010	-0.0057
	(0.0007)	(0.0007)	(0.0021)	(0.0045)
Ever Incarcerated	0.0010***	-0.0000	-0.0020**	0.0008
	(0.0002)	(0.0002)	(0.0008)	(0.0016)
Observations	55,500	55,500	31,500	31,500
Panel B. Bottom 50%				
Earnings Rank, Individual	0.4038***	0.6430***	0.8675***	0.5554
	(0.0624)	(0.0664)	(0.1997)	(0.3997)
Employment, Individual	0.0020**	0.0030***	0.0089***	0.0025
	(0.0008)	(0.0009)	(0.0027)	(0.0055)
Any Safety Net	-0.0014	-0.0046***	0.0037	-0.0113*
	(0.0010)	(0.0010)	(0.0031)	(0.0064)
Ever Incarcerated	0.0006*	-0.0005	-0.0031**	0.0010
	(0.0003)	(0.0003)	(0.0012)	(0.0024)
Observations	27,000	27,000	15,000	15,000
Panel C. Top 50%				
Earnings Rank, Individual	-0.0288	0.3448***	0.6005***	0.0699
	(0.0640)	(0.0650)	(0.1852)	(0.4418)
Employment, Individual	-0.0023***	0.0002	0.0054**	-0.0100*
	(0.0008)	(0.0008)	(0.0025)	(0.0059)
Any Safety Net	0.0010	-0.0026***	-0.0015	0.0002
	(0.0010)	(0.0010)	(0.0029)	(0.0064)
Ever Incarcerated	0.0014***	0.0004	-0.0010	0.0009
	(0.0003)	(0.0003)	(0.0010)	(0.0021)
Observations	28,500	28,500	17,000	17,000

Notes: This table presents the impact of spending one additional year in public housing during childhood on some key outcomes. Estimates come from regressions of the dependent variable on the number of years an individual lived in public housing during childhood, instrumented by the age an individual first moved into public housing. The first set of columns are associated with our main sample. Column 1 includes birth year fixed effects. The second column additionally includes all our baseline controls. The second set of columns are associated with our siblings subsample. Column 3 includes only household fixed effects. Column 4 adds the full set of controls (except characteristics common within households) and an additional control for birth order. The first panel is associated with the full sample, while the second and third panels are associated with individuals from families with incomes below and above the median income for their admission year, respectively. Standard errors are clustered at the household level.

$^{*}p < .1, ^{**}p < .05, ^{***}p < .01$

B Data Appendix

Sample Construction

In this section, we detail how we use the HUD longitudinal data to identify baseline family characteristics and to construct the main sample used in this paper.

Baseline Family Characteristics

To calculate baseline family characteristics, we start by concatenating all years of the HUD longitudinal data, which is available from 1995 to the present and sort the data so it is ascending by year.

Note that, within a single year of the HUD longitudinal data (a "vintage"), there may be rare instances where PIKs are not unique. This may occur for several reasons. We may see a PIK multiple times if an individual moves between assisted households or if a household moves across jurisdictions and continues to participate in HUD rental assistance programs. We may also see duplicate PIKs within a year if Census happens to assign multiple individuals a single PIK, though this is rare.

We focus first on PIKs where the PIK is always unique within a vintage. For this subset of observations, we identify the first non-missing value for all key variables by PIK. For each variable, we create a flag to document the HUD vintage year associated with the first non-missing value. Then, we collapse the dataset so that we have one row per PIK.[25]

To deal with PIKs that may have duplicates within a year, we develop an alternative routine. Crucially, we start with the assumption that a unique PIK represents a single individual. If the PIK is unique in the first year, we simply take baseline outcomes from the first year and we have no conflicts. If, however, the PIK appears multiple times in the first year, then we may have multiple sets of baseline outcomes. If this is the case, we keep all sets of baseline outcomes in the raw data. We count the number of matches the PIK has in the first year, and we re-weight all analysis in this paper by the inverse of the number of matches a PIK has (equivalently, we could simply average across baseline outcomes).

[25] Alternatively, we could simply identify the first year a PIK appears in the HUD longitudinal data and keep data from just that year. However, there are occasional instances where a key variable is missing in the first year individuals show up in the data, but does show up in subsequent years. If we keep data only from the first year a PIK is present, we will not be able to identify a baseline value. Keeping the first non-missing value is equivalent to imputing values backwards. Below, we discuss adjustments we make to variables when the year of entry and the year the baseline outcomes is measured do not match.

Assigning Admission Year

Next, we need to identify the year individuals first participate in a HUD program. This is complicated by the fact that the HUD data quality varies over time and by PHA. For example, in some PHAs, data on admission dates may be missing or incorrect in the earliest vintages. Similarly, there may be a huge influx of individuals appearing in the data in some years – which sometimes reflects changes to IT systems – hinting that the PHA did not report these individuals in earlier years though they were likely living in public housing.

To assign each individual an admission year, we implement the following routine. We assign individuals the admission year that is reported in the HUD longitudinal file if it agrees with when individuals first appear in the data. If the reported admission date and the observed date of entry do not agree, we assign individuals an admission year equal to the year we first see them in the data, as long as (1) the year they first enter is not left censored (that is, as long as the first year they appear is not 1995, the first year we have available data) and (2) as long as they do not appear at an address associated with a public housing unit in the tax data in an earlier year. If individuals do appear in public housing in the tax data before they appear in the HUD data, we assign them an admission year equal to the earliest tax year when they live in public housing, as long as the first year they appear is not left censored (as long as the first year is not the first year the individual is in the tax data).

Main Analysis Sample

To construct the final sample, we limit our attention to those individuals whose first appearance in the HUD data is in New York City public housing. This excludes individuals who previously benefit from another program (e.g., receive housing vouchers) or live in public housing in a different jurisdiction before moving into New York City public housing. We focus on individuals who would have been children when they first entered public housing, where we define children as individuals who are 17 years old or younger and are not the head of household when they move into public housing.

We implement two further restrictions. First, we keep only individuals for whom we are able to assign a year of entry. This restriction will primarily filter out individuals who were living in households receiving assistance through one of HUD's programs before 1995, the year when HUD data are first available. Second, our final sample includes only individuals for whom all outcomes are defined at age 26.

Adjustments to Controls

One issue is that the admission year we assign individuals may not be the same year we use to impute baseline characteristics. This may happen, for instance, if we first observe individuals in HUD data in 2000, but family income is missing until 2001. Or we may impute an admission date of 1995 based on tax records, but they do not show up in HUD data until later years.

We use baseline characteristics in two ways. First, we use them as controls in our main analysis. Second, we use income in admission year to identify our income group subsamples. If baseline characteristics are endogenous, then incongruence between admission year and year the baseline characteristics are measured may introduce bias. This may be a problem for household income, household size, and household marital status, which may vary due to parent lifecycle or, for income, macroeconomic conditions.

We deal with this by residualizing key covariates before using them in the analysis. We regress household size or household marital status (a dummy for whether the head is married) on head age at baseline. We use the estimates from this model to predict our control variables, and then subtract the raw value from the predicted one. The result is a residualized version of the control that represents the portion of the control variable that is not predicted using head age. For income, we do the same, but further control for a measure of wage growth. To construct our measure of wage growth, we start with data on median hourly wages by year.[26] We index each value to median wages in 2000. Then, we assign each individual a value of indexed wages based on the year that their baseline household income is measured.

To generate our income group subsamples, we need to compare families' incomes in the year they were admitted. In this case, we would not want to adjust for parent age. Therefore, when determining income group, we residualize household income only on wage growth.

[26] "State of Working America Data Library," https://www.epi.org/data/; We use this data source because we explore using wage growth for different percentiles, which are not readily available from other sources.

C Appendix on Empirical Strategy

C.1 Interpretation of Results

Our sample includes only individuals who lived in public housing at some point during childhood. We restrict our sample in this way in part to address concerns about selection bias, since families who eventually live in public housing may differ from those who never live in public housing, even if they appear similar on observable characteristics, such as family income.

To interpret results, it is important to consider where individuals in our sample may live if not in New York City public housing. Many families face considerable residential instability before moving into New York City public housing, and may access shelters, live with other relatives, or benefit from other HUD rental assistance programs.

First, New York City guarantees a right to shelter. In practice, this means that individuals or families that are homeless are guaranteed the right to temporary shelter. While we do not know what share of individuals in our sample move in from a shelter, we do have some suggestive evidence that families that move into public housing are at risk of homelessness and are in contact with city services. In 2023, over 80% of all new admissions to New York City public housing were referred to NYCHA by the Department of Homeless Services or from some other city agency on the basis that the family was homeless, or at risk of becoming homeless.

A large fraction of individuals in our sample likely live with other relatives before moving into New York City public housing. We can identify the living situations of individuals in our sample at the time of the 2000 decennial census. We find that almost 21.7% of sample members who had not yet moved into public housing at the time of the 2000 decennial census lived in a household where the head was someone other than their parent (typically another relative) compared to just 11.5% of children who had already moved into public housing. Additionally, these children lived in households that were 4.5% larger on average. These facts suggest that a substantial fraction of individuals in our sample live with other relatives or in families that were doubling up before receiving admission into public housing.

A small share of families also benefit from other HUD rental assistance programs. Among all individuals who ever lived in New York City public housing as children between 1995 and 2014 and who turned 26 between 2005 and 2020 (and are therefore are eligible to be in our sample), 4.2% lived in public housing elsewhere or received rental assistance through another HUD program (such as through housing vouchers) before moving into New York City public housing.

C.2 Alternative Specifications

In Table A.18, we present results from alternative specifications and discuss the implications from these models below.

In Column 1, we present results from estimates using Equation 2, excluding baseline controls (but keeping birth year fixed effects). For the full sample, estimates on earnings are considerably smaller than those from the main results (for reference, we present results including baseline controls, which are identical to those found in earlier tables in this paper, in Column 2). We find no effects on employment or safety net participation, and *positive* effects on incarceration. The results for children from families with above median incomes. For children from families with above median income, we find no effects on earnings, negative effects on employment, and positive effects on incarceration. The results show that children who spend more time in public housing have worse outcomes. These results are perhaps not surprising given that our balance tests showed that children with more exposure (i.e., those that enter at younger ages) tend to be more disadvantaged. This exercise highlights the importance of including baseline controls when estimating exposure effects.

In the second set of columns, we focus on analysis of our siblings subsample. In Column 3, we report the results of regressions that include only the treatment indicators and household fixed effects (for reference, we present results that include birth year fixed effects and a control for birth order, which are identical to those found in earlier tables in this paper, in Column 4). The magnitude of the impacts on earnings and employment are much larger than any other specification, and standard errors are much tighter than those found in our main siblings analysis (with birth year fixed effects). We find no effect on safety net participation. However, we find large declines in incarceration (the point estimate is -.002, roughly 6% of the mean), though the results are not significant at the .05 level.

The concern with this specification is that results may be driven by circumstances when we measure outcomes, rather than public housing exposure. Since we measure ages at a fixed point in the lifecycle (age 26) outcomes of older siblings are measured earlier in calendar time compared to when we measure outcomes for younger siblings. Note, furthermore, that during the 2010s, when the vast majority of our sample turned age 26, real earnings and the employment rate were rising, while incarceration rates were falling (participation in major safety net programs increased during the first half of the decade, and fell thereafter). Holding age fixed, real earnings measured later in calendar time would tend to be higher than earnings measured earlier.

This is a major issue since, for our siblings design, our within-household comparisons are between older children who spend fewer years of their childhood in public housing and younger children who spend more of their childhood in public housing. In other words, the children with more exposure within a household will also tend to have their outcomes measured in periods with relatively higher earnings and employment, and lower incarceration rates. For this reason, we find it important to control for birth year fixed effects, which double as fixed effects for the year an outcome is measured, even though including them causes standard errors to increase substantially.

D Estimating Variance Components

In this section, we describe how we estimate bias-corrected standard deviations for the distribution of development effects and their standard errors. The exposition is informed by Kline et al. (2020) and Aloni and Avivi (2024).

Consider the simple model

$$y_i = D_i'\beta + \epsilon_i$$

where D_i' is a vector of development dummies.

Suppose we are interested in the distribution of the individual elements of β, which we may interpret as development effects. The variance-covariance matrix is given by $\theta = \beta' A \beta$, where

$$A = \frac{N}{N-1}\left(I_J - \pi_J \pi_J'\right)$$

where N is the total number of individuals who live in the developments and J is the number of developments. The vector π_J is a $J \times 1$ vector where the j^{th} element is the total share of all individuals who live in development j.

The plug-in estimator $\widehat{\theta}_{\text{PI}} = \widehat{\beta}' A \widehat{\beta}$ will overestimate the true dispersion of the components of β. To see this, suppose the true development effect was the sum of the estimated development effect and some sampling error

$$\widehat{\beta}_j = \beta_j + \omega_j$$

Assuming the sampling error is orthogonal to the true effect, the variance of the sum will be greater than the variance of the true effects.

It can be shown that an unbiased estimate for the variance component can be given by

$$\widehat{\theta} = \widehat{\beta}' A \widehat{\beta} - \text{trace}(A \mathbb{V}(\widehat{\beta}))$$

In our setting, we do not estimate a single set of development effects. Rather, we estimate coefficients for intercepts and slopes associated with each development, as well as for a set of baseline controls. To deal with this, we replace the matrix A with a second matrix B, which may vary depending on the variance component we seek to estimate.

To calculate, for example, the variance on the intercepts, we let

$$B = \begin{pmatrix} A & 0 & \cdots & 0 \\ 0 & \ddots & & \vdots \\ \vdots & & \ddots & \vdots \\ 0 & \cdots & \cdots & 0 \end{pmatrix}$$

E Appendix for Calculating the Marginal Value of Public Funds

In this section, we detail the calculations used in estimating the marginal value of public funds (MVPF) in Section 7.

Change in Lifetime Earnings and Benefits

We start by calculating the present value of lifetime earnings and benefits nationally.

Using the 2016 Current Population Survey Annual Social and Economic Supplement (CPS ASEC), we calculate the average earnings for all individuals 18-65 years old by age. We adjust these values to 2020 dollars and apply a real wage growth of .5 percent to generate a profile of earnings over the lifecycle. We apply a 3% discount rate and find that the present value of earnings over the lifecycle, estimated at age 10 (the median age children from our sample move into public housing), is about $865,127 nationally.

Calculating the present value for program participation is slightly more involved. First, we calculate expenditures for each program.

To calculate SNAP expenditures, we take SNAP spending per beneficiary in fiscal year 2015, reported by the US Department of Agriculture (USDA).[27] We then annualize this value and inflate it to 2020 dollars using the CPI-U-RS. This gives us our per beneficiary spending of $1,663.

Calculating TANF benefits is more complicated because only about a quarter of the program's spending is on direct cash assistance. To calculate benefits, we first take total government spending on basic assistance in fiscal year 2015, as reported by HHS, and adjust it for inflation, yielding a value of $8.67 billion.[28]

We then take the total number of families the Department of Health and Human Services (HHS) reports participating in the program and multiply that figure by the average family size to estimate the total number of beneficiaries at 4.00 million individuals.[29] Dividing the two figures gives us our estimated spending per beneficiary of $2,168.

We calculate annual rental assistance per beneficiary using the 2015 Picture of Subsidized Households.[30] We take the average HUD monthly expenditure on families in public housing

[27] "Supplemental Nutrition Assistance Program Participation and Costs," https://fns-prod.azureedge.us/sites/default/files/resource-files/snap-annualsummary-8.pdf

[28] "FY 2015 Federal TANF & State MOE Financial Data," https://www.acf.hhs.gov/sites/default/files/documents/ofa/tanf_financial_data_fy_2015_8217.pdf

[29] "Characteristics and Financial Circumstances of TANF Recipients, Fiscal Year (FY) 2015", https://www.acf.hhs.gov/sites/default/files/documents/ofa/characteristics_and_financial_circumstances_of_tanf_recipients.pdf

[30] "Picture of Subsidized Households," https://www.huduser.gov/portal/datasets/assthsg.html

or with a housing voucher and divide this by the average number of individuals per unit. We adjust this figure for inflation and annualize it to yield an average spending per beneficiary of $4,218.

Next, we need to estimate lifetime participation in each program. One challenge is that participation varies over the lifecycle. Using the 2016 CPS ASEC, we calculate the share of individuals who participate in SNAP or TANF, or who receive rental assistance by age at any point in calendar year 2015. Since benefits are underreported in survey data, we merge in information on program participation from the Urban Institute's Transfer Income Model (TRIM), a microsimulation model that corrects for underreporting in the CPS. To calculate the expected expenditure per person at each age, we multiply their estimated participation rate by the government's per beneficiary expenditure. We apply a discount rate of 3% to calculate the present value, estimated at age 10.

Our sample comprises individuals that are relatively more disadvantaged. For this reason, lifetime earnings tend to be lower and program participation higher than the national averages. We next calculate the present values for earnings and federal benefits for our sample.

To get the present value of lifecycle earnings for our sample, we rescale this figure by the ratio of mean earnings for our sample to mean earnings for the national sample at age 26 ($\frac{18780}{33220} = .57$). This yields a present value at age 10 of $865127 \times .57 = 489080$.

In general, individuals in our sample are more likely to participate in federal safety net programs than the overall population. For each program, we rescale the participation rate at each age calculated using the CPS ASEC-TRIM by the ratio of mean participation at age 26 for our sample to that of the overall population. For example, we find that 33.05% of our sample participate in SNAP at age 26. Using the 2016 CPS ASEC and TRIM, we find that almost 16.76% of 26 years olds participate in SNAP nationally. So, we multiply the participation rate at each age by $\frac{33.05}{16.76} = 1.97$.

Finally, we need to calculate how spending a year in public housing changes the present value of lifetime earnings and benefits. To do this, we need to convert the impacts to percent impacts. For earnings, we multiply the percentile effects to dollars using the same method we use in Section 5. That is, we multiply effects by $550.90. For the full sample, this implies that one year of public housing leads to an additional $247 in earnings. We divide this by the mean age 26 earnings to recover a percent impact of 1.3%.

For program participation, calculating percent impacts is more straightforward. We simply divide the impacts in Table 5 by the mean age 26 outcomes found in the same table.

We multiply the present value of each quantity with the appropriate percent impact to calculate the change in present value for each quantity.

Net Cost to the Government

To calculate per person expenditures, we use the Picture of Subsidized Households (PSH) data for 2000.[31] The PSH is a dataset compiled by the Department of Housing and Urban Development (HUD) based on reports submitted by individual public housing authorities (PHAs) and landlords. These data contain a variety of information on a PHA, broken out by program. We use the 2000 data since this is the median year individuals in our sample moved into public housing. From this data, we take the average spending per unit per month, annualize it, and adjust to 2020 dollars. There are roughly 2.2 people per unit in New York City, so we assume that the average unit has 2 bedrooms. Individuals in our sample have families with more than four people on average, and so likely have at least three rooms in their unit. Therefore, we rescale annual spending by 1.25, the ratio of the fair market rents HUD reports for three bedroom units to two bedroom units in New York City, resulting in an average spending per unit of $13,120.63. Dividing this estimate by 4.12, the average family size at baseline for children in our sample, yields estimated spending of $3,184.62 per individual per year.

For our sample, note that baseline household income is 24,040 and the average age of the head at entry is just over 38 years old. We assume a .5% growth rate to estimate household income for the household head at every age from 38-65. We apply a discount rate of 3% to get a present value of household income. Previous research estimates that housing vouchers decrease earnings by about 11% (Jacob and Ludwig, 2012). Given research from MTO that finds no labor market effects of switching from public housing to vouchers (Ludwig et al., 2013), we multiply the present value of household income by this factor to calculate the present value of lost income. We follow Bailey et al. (2023) and assume households face an average tax rate of 12.9%, estimating the government will lose $6,991 in revenue, or $1,696.83 per person using the same calculations above.

We sum the direct costs and the revenue lost to negative labor supply effects and estimate that the government spent $4,881.45 per individual in our sample in 2000.

Methods describing how to calculate how the program affects government revenues and spending in the long run can be found in the body of the text.

Willingness to Pay

Methods to calculate willingness to pay can be found in the main body of the text.

[31] https://www.huduser.gov/portal/datasets/assthsg.html