In-Kind Transfers and Early Childhood Development: Evidence from Housing Assistance Lotteries*

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Abstract

Governments frequently use in-kind transfers to redistribute resources to families both to alleviate material hardship and improve children's well-being. In-kind transfers can boost low-income household consumption and may impact childhood development, though the relationship between consumption responses and child outcomes is not well understood. This paper examines how two major in-kind transfers, public housing and housing vouchers, affect families' housing consumption and their children's early skill development. Using administrative data on housing assistance applications, randomized public housing and housing voucher offers from lottery-ordered waiting lists, and linked assessments of kindergarten readiness for more than 10,000 children, this study estimates the effects of housing assistance on early childhood development. Housing voucher receipt leads to increases in children's kindergarten readiness, early literacy skills, and likelihood of meeting cognitive and noncognitive developmental benchmarks. However, public housing has no significant effect on child development outcomes. The pattern of results is largely consistent with the way each program affects housing and neighborhood quality. The results suggest that in-kind transfers *can* yield improvements in child outcomes but that the effects depend on how transfers impact consumption.

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JEL codes: I24, I38, R23

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1 Introduction

The social safety net in the United States and many other developed countries redistributes resources to low-income households and families primarily through in-kind transfers. In the United States, roughly $375 billion of annual safety net spending is directed to children, and more than 92 percent of transfer spending on children is in-kind (Aizer et al., 2022; Currie and Gahavri, 2008). The transfers alleviate material hardship, can boost low-income households' consumption contemporaneously, and may lead to improvement in children's subsequent short- and long-term outcomes.

However, predicting whether and how in-kind transfers might impact children can be difficult because the effects likely depend on consumption responses, which may vary with program design. For example, a long-standing debate in federal housing policy is whether the provision of housing vouchers or public housing is better for children (Currie and Yelowitz, 2000; Jacob, 2004; Sanbonmatsu et al., 2011). The programs have similar predicted effects on family resources, but they could generate different impacts on child outcomes if their effects on neighborhood or housing quality differ.

Understanding the link between consumption responses to different in-kind transfers and their effects on child development is important for interpreting the in-kind transfer literature broadly and informing the debate on low-income housing policy specifically. However, studying the link is complicated by several challenges. First, matching child-level information on transfer receipt, typically measured as exposure in an eligible or likely-treated group or at the individual adult or household level, to contemporaneous development measures is difficult. As a result, much of the literature features exposure designs and focuses on long-run outcomes like adult health and labor market performance (Bailey et al., 2023; Hoynes et al., 2016; Pollakowski et al., 2022). Second, a standard identification concern results from non-random exposure to in-kind transfers, their availability, or their receipt, making it challenging to establish a suitable control group. Most social safety net programs are universally available to households with children, subject to means testing, and many studies of in-kind transfers therefore rely on the programs' historical roll-out to estimate their causal effects (Bailey et al., 2023; Barr and Gibbs, 2022; Hoynes et al., 2016). Third, understanding how differences in consumption effects translate to child development requires both variation in the in-kind transfers' consumption responses and data on early skills effects.

In this paper, we overcome these challenges to provide new experimental evidence on the role of two large in-kind transfers — public housing and housing vouchers — in early childhood development, tracing how the programs' divergent effects on consumption correspond to differences in child development in the context of U.S. federal low-income housing policy. We assemble novel data linking children on waiting list applications to housing assistance records, housing and neighborhood quality data, and subsequent developmental assessments. We leverage an unusual housing assistance lottery randomly offering public housing *or* housing vouchers to a large population of low-income households. The lottery represents a unique setting in which we can *directly* observe children either receiving or not receiving assistance and estimate the impacts on housing consumption and subsequent near-term developmental outcomes drawn from assessments of all children entering public kindergarten in the state of Florida.

Using records of more than 10,000 children listed on housing assistance applications and their early childhood assessments, we estimate the causal effects of housing vouchers and public housing on children's early reading skills, kindergarten readiness, and cognitive and noncognitive skills, as demonstrated in a classroom setting. We instrument for assistance receipt with randomized initial waiting list position. To our knowledge, we provide the first large-scale experimental evidence on public housing and the first evidence of a safety net program's impact on early childhood development in the United States, a piece often missing from studies of the impact of in-kind transfers or resources. To the extent resources and early environments impact children's early skill development, these are precisely the measures on which we would expect to observe effects.

In 2008, Miami-Dade Public Housing and Community Development (PHCD) opened its waiting list for four weeks to applicants seeking any type of federal housing assistance.[1] At the end of the period, every applying household was randomly assigned two separate rankings, one on a housing voucher waiting list and another on a public housing waiting list. Housing assistance was heavily oversubscribed: more than 70,000 households applied for roughly 3,500 vouchers and 4,500 public housing apartments to be allocated over the ensuing eight years. As a result, applicants randomly assigned to the top of either list were likely to receive an offer of housing assistance, while those randomly assigned to the bottom of the list were unlikely to receive assistance. Our research design compares the outcomes of children in households that were randomly-assigned to the top of an assistance waiting list to children in families outside the top of the list.

We show that housing vouchers and public housing have different impacts on the quality of housing and neighborhood in which low-income families reside. Using address data linked to hedonic housing quality estimates and census tract measures like poverty and neighborhood crime, we estimate the effects of voucher receipt and public housing admission on the change in quality from baseline neighborhoods. Leasing-up with a voucher leads families to move to

[1]PHCD, the county's lead housing authority, is the seventh largest housing authority in the United States in terms of total households assisted at approximately 25,000.

housing with a roughly 9 percent higher hedonic quality and neighborhoods with 3 percentage points lower poverty and about 5.4 fewer violent crimes per 1,000 residents (a 40 percent reduction). In contrast, leasing-up in public housing causes families to move to neighborhoods with 8 percentage points *higher* poverty and about 8 more violent crimes per 1,000 residents (a 65 percent increase). We find no evidence of hedonic quality improvement for public housing residents.

While we find contrasting effects on housing and neighborhood quality, we find strikingly similar results for housing *instability*. Both programs substantially reduce the chances that schools flag a child as housing unstable. Vouchers and public housing reduce the probability that a child is homeless or doubled-up by roughly 7 percentage points relative to a control mean of 8 percentage points. Our results are consistent with earlier work exclusively on housing vouchers (Wood et al., 2008; Gubits et al., 2015). Uniquely, we document effects for both public housing and housing vouchers using novel McKinney-Vento data on students experiencing housing instability.

We find that voucher receipt substantially improves children's early developmental outcomes. Children whose families lease-up with a voucher score 0.2 standard deviations higher on an assessment of pre-reading skills. We estimate that voucher receipt boosts kindergarten readiness by 13 percentage points (a 22 percent increase relative to the control mean). Relative to control group children at the bottom of the waiting list, children in families receiving vouchers are considerably less likely to be identified as "not demonstrating" appropriate developmental milestones. We find treatment group children are 8.8 percentage points (72 percent) less likely to be categorized as "not emerging or demonstrating" key skills during observational classroom evaluations.

We find public housing admission has no statistically significant effect on child development outcomes. Our point estimates typically lie near zero, and we can rule out substantial positive effects. We can also rule out large negative effects, a surprising result given our finding that public housing admission generally leads children to be exposed to low quality neighborhoods. The results, along with our housing stability findings, echo earlier non-experimental work by Currie and Yelowitz (2000), which shows that admission to conventional public housing may not be as harmful for children's outcomes as suggested by public perception or implied by the *positive* effects of escaping public housing found in previous work (Chetty et al., 2016; Chyn, 2018). For families in public housing, the effects of increased resources and improved housing stability on children's development may offset those of decreased housing/neighborhood quality.

The effects of voucher receipt on child development are driven by large positive effects for boys. Boys in families randomly receiving a voucher score 0.5 standard deviations higher in evaluations of pre-reading skills, and they are 28 percentage points (49 percent) more likely to be deemed kindergarten ready. Moreover, they are less likely than those in the control group to receive the lowest categorization of skill development in classroom-based observational assessments. Girls show no consistent positive or negative effects from housing voucher receipt. Our finding of concentrated effects for boys aligns with existing literature documenting gender differences in the returns to early-life investments, and boys' heightened susceptibility to disadvantage, particularly as it affects their behavioral skill development Autor et al. (2019); Bertrand and Pan (2013).

To further explore the mechanism behind our results, we investigate heterogeneity in housing assistance effects by estimated baseline housing consumption. Due to the structure of rent rules in federal rental assistance programs, a household spending a significant portion of their income on housing *before* rental assistance receipt should experience a relatively large increase in disposable income, while a household spending relatively little on housing at baseline should have a comparatively small change in disposable income but a large improvement in housing quality. Using hedonic quality as a proxy for housing spending, we first show that housing quality increases substantially with voucher receipt for those in low quality housing at baseline and is essentially unchanged for households that were in higher quality housing. Next, we explore what the consumption changes mean for child development. We find that vouchers' positive effects are driven by the large effects for children in low quality housing at baseline. We interpret this as further evidence that child development impacts of in-kind transfers are moderated through their particular impacts on consumption.

We make several contributions to the literature. First, we show that in-kind transfers *can* yield improvements in child outcomes, but the effects are moderated by their impact on consumption. We find that housing vouchers improve child development outcomes, while public housing does not. The effects hold despite the programs operating with similar rent rules and likely leading to similar gains in disposable income. Importantly, we find that the programs impact housing and neighborhood quality differently. Voucher recipients move to significantly better quality housing in safer and somewhat less impoverished neighborhoods. Public housing recipients do not move into observably better quality housing and reside in poorer, less safe neighborhoods than they otherwise would. We examine heterogeneity in our voucher effects by observing whether the families would be expected to realize large improvements in housing and neighborhood quality or increased disposable income. We find strong evidence that the voucher effects are driven by households expected to experience the largest increases in housing consumption. Our focus on how consumption effects map to differences in child development impacts allows us to shed new light on the evidence of safety net effects on child outcomes. Researchers assessing mixed findings of safety net programs on child outcomes have emphasized the importance of pre-existing disadvantage in the populations studied (Page, 2024; Hawkins et al., 2023) or the role of parental labor supply responses (Duncan et al., 2011; Jacob et al., 2015). We think our focus on consumption responses is particularly relevant given the behavioral literature documenting how consumption patterns differ with how money or transfers are labeled (Thaler, 1990; Hastings and Shapiro, 2018).

Second, we make important contributions to the literature on low-income housing policy in the U.S., providing direct experimental evidence of the effects of both housing vouchers and public housing on policy-relevant outcomes for a large sample of low-income households. Previously, public housing and voucher debates relied primarily on evidence from the Moving to Opportunity demonstration (Kling et al., 2007; Sanbonmatsu et al., 2011; Chetty et al., 2016) or public housing demolitions (Chyn, 2018). While the studies are critical for understanding neighborhood effects, they mostly examine the effects of escaping troubled public housing, as both MTO and public housing demolitions tended to target the most distressed developments. To our knowledge, we provide the first experimental evidence of the effects of U.S. public housing programs on housing consumption, neighborhood quality, homelessness, and child development. We find that public housing reduces housing instability for participants but worsens their neighborhood quality and has no major positive or negative effects on early childhood development outcomes. Prior work has relied on a variety of research designs to estimate public housing's effect on short- and long-run child outcomes and found a mix of positive and negative effects (Currie and Yelowitz, 2000; Jacob, 2004; Pollakowski et al., 2022).

Third, we contribute to the housing voucher literature. Our results contrast with the only other large-scale experimental evidence of the effects of vouchers on children. Using a Chicago-based lottery program, Jacob et al. (2015) find vouchers have few if any meaningful long-run impacts on childhood outcomes. In section 6 of this paper, we conclude that differences in (i) housing and neighborhood quality effects, (ii) data and age-at-measurement, and (iii) outcome measures likely drive the divergent findings.

Finally, our work contributes to the literature on transfer programs' second-generation impacts. A large literature documents that investments in children can enhance both efficiency and equity because early childhood development is particularly susceptible to intervention and because early investments have a long time horizon over which to generate returns in excess of costs (Almond and Currie, 2011; Almond et al., 2018; Heckman, 2006; Heckman and Masterov, 2007; Hendren and Sprung-Keyser, 2020). To our knowledge, we provide the first experimental evidence of large U.S. safety net programs, in the field, on early childhood skill development. Our finding of housing voucher receipt's positive effects on children is consistent with a large literature using quasi-experimental methods to evaluate the effects of in-kind transfers on children's long-run outcomes (Hoynes et al., 2016; Bailey et al., 2023; Pollakowski et al., 2022). Relative to existing work, we show that children's skill development prior to entering formal schooling can be shaped by safety net programs. The results enhance our collective understanding of how childhood investments affect critical early development and may have implications in particular for closing family income-based gaps in cognitive and noncognitive skills.

The remainder of the paper is organized as follows: section 2 summarizes the literature strands to which our work contributes, section 3 discusses institution details relevant for our study and the data used, section 4 outlines our empirical strategy, section 5 reports results, section 6 discusses how the results align with related literature, and section 7 concludes.

2 Related Literature

The second-generation effects of in-kind transfers to families or households have been the subject of much policy interest and a growing body of empirical research, in part because a large and growing literature documents the potential for childhood investments, in particular, to generate large returns (Hendren and Sprung-Keyser, 2020). Researchers hypothesize that the pathway from better conditions in childhood to improved long-term outcomes may be through skill development – particularly that of noncognitive skills – in the critical early childhood years (Almond et al., 2018; Currie and Almond, 2011; Heckman et al., 2013). While direct measures of early skill development are rarely available in large-scale studies of social safety net programs, interest in the long-term and intergenerational effects derives from the conceptual and empirical foundation that investments in early childhood *can have* meaningful effects on children and their trajectories. Economists argue that early-life resources, including prenatal exposure to increased resources, may be particularly impactful because of the long time horizon for early investments to realize returns, complementarities in health and human capital investments across the life course, and the malleability of the young brain, making early development particularly susceptible to intervention (Almond and Currie, 2011; Almond et al., 2018; Heckman, 2006; Heckman and Masterov, 2007; Knudsen et al., 2006).

Studies of the role of transfers in facilitating families' consumption and improving second-generation outcomes include those investigating the impact of in-kind food assistance—the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) and the Supplemental Nutrition Assistance Program, commonly known as food stamps—on children using designs that leverage program availability across geography over time (Bailey et al., 2023; Hoynes et al., 2011, 2016). A sizable literature explores the impact of health insurance coverage on children's long-run outcomes, similarly capitalizing on program expansions and changing eligibility (Cohodes et al., 2016; East et al., 2023; Goodman-Bacon, 2021). In addition, studies have investigated the impact of receipt of housing assistance on consumption, family wellbeing, and children's medium- and long-run outcomes (Pollakowski et al., 2022; Wood et al., 2008), though only one uses a randomized design to measure the effects of housing vouchers (Jacob et al., 2015). Jacob et al. (2015) find that housing vouchers do not meaningfully shift neighborhood or housing quality in their context, and that there are correspondingly little to no effects on children's outcomes.

In addition to studies of transfers and housing assistance, in particular, a sizable literature explores the effects of changing neighborhoods through housing policies on children's short- and long-run outcomes (Kling et al., 2007; Chetty et al., 2016; Chyn, 2018; Chyn et al., 2024). While studies focused on longer-run outcomes such as earnings and college-going have consistently found positive effects of being exposed to less impoverished neighborhoods (Chetty et al., 2016; Chyn, 2018; Chyn et al., 2024), the literature finds more mixed evidence of neighborhood effects on cognitive development in childhood and adolescence (Jacob, 2004; Kling et al., 2007; Sanbonmatsu et al., 2011).

Studies of direct investments in children's early skill development — through preschool programs — demonstrate that such investments can have important, economically meaningful effects on long-run and intergenerational outcomes (Barr and Gibbs, 2022; Gray-Lobe et al., 2022). Research also suggests that early skill development, measured at school entry, is strongly related to measures of later-life wellbeing, and that improving early skills through childhood investments generates longer-run benefits in adolescence and adulthood (Chetty et al., 2011; Currie and Thomas, 1999; Duncan et al., 2007). Collectively, the evidence suggests that a channel through which transfer programs may generate lasting improvements in life chances is through the development of early noncognitive skills, which is also consistent with patterns of test-score fade out and reemergence of later-life benefits observed in studies of early childhood interventions (Chetty et al., 2011; Deming, 2009; Gibbs et al., 2011). Our paper fills an existing gap in these related literatures by using a rigorous, experimental design to measure the impact of housing assistance on children's early development, the pathway through which safety-net programs could affect long-run outcomes for the children of families exposed.

3 Institutional Details and Data

The Housing Choice Voucher program, formerly Section 8, is the largest rental assistance program administered by the U.S. Department of Housing and Urban Development (HUD).

Housing vouchers serve roughly 2.5 million households (5.3 million people) with an annual budget of roughly $20 billion (Collinson et al., 2019). Public housing serves about 1.1 million households (2.3 million people) at annual budget expense of roughly $8 billion. Both programs are typically funded exclusively using federal funds, but they are administered by local quasi-government organizations called Public Housing Authorities (PHA). Families issued housing vouchers use them to lease units on the private rental housing market, while public housing families are offered apartments in public housing developments owned by a PHA.

In Miami-Dade County, Florida, the local PHA is Miami-Dade Public Housing and Community Development (PHCD). PHCD is the seventh largest housing authority in the country in terms of total households assisted at approximately 25,000. It owns and operates 16 large public housing developments, and about 56 scattered-site properties which together total 9,000 units. The tenant-based voucher program operated by PHCD serves approximately 16,000 households per year. PHCD serves the entire county, with voucher families located in nearly all jurisdictions in Miami-Dade County.

3.1 2008 Waiting List

As a result of a lawsuit filed against PHCD, the agency agreed to open their waiting lists for all their assisted housing: housing vouchers and public housing and separately randomize wait-list positions for vouchers and public housing. In July 2008 Miami-Dade Public Housing and Community Development opened their waiting list for a four week period to applicants seeking any type of federal housing assistance. Households submitting an application were told about the eligibility for the programs and agreed to allow their submitted information to be used in determining eligibility. After four weeks the application period was closed, and in September every household that applied during the open period was randomly assigned a ranking on a housing voucher waiting list and a separate random ranking on a public housing waiting list.[2]

Households were notified about their initial position after randomization, and the initial static ranking on each list was published online. PHCD then began drawing down from each waiting list as vouchers and public housing units became available. When households approached the top of the waiting list, PHCD would attempt to contact the household to undergo a final eligibility determination. If a household wanted to learn about their current position they needed to call the leasing department. Households determined to be eligible in the final eligibility review received offers of assistance in the order that they became available.

[2]The randomized orders were created from a random number generating function in a spreadsheet.

For public housing vacancies, PHCD would attempt to try to offer one unit of the appropriate bedroom size within each of three broad geographic zones that divide the county. If households rejected all the available units they would be permanently removed from the public housing waiting list. A household that accepted the offer of a public housing unit could technically remain eligible for a voucher during the first 12 months of their public housing leases, but in practice no one was issued a voucher after leasing up in public housing. And after 12 months in their unit, families accepting public housing were removed from the voucher waiting list. For housing vouchers, households were issued housing vouchers with typically 90 days to find an acceptable unit. When a family was issued a housing voucher they were removed from both waiting lists. Finally, households that could not be contacted at follow-up or who were found ineligible at the follow-up screening were removed from the waiting list as PHCD worked through both lists. Recent work suggests that housing authorities often struggle to contact individuals on their waiting list Waldinger (2019).

3.2 Wait List Data

The primary sample is drawn from the more than 70,000 applications entered into the the 2008 housing assistance lottery. Importantly, these data contain information on both household heads and all household members at the time of application. For household heads, the data include race, ethnicity, need for an accessible unit, age, whether the household had an email address, and baseline address, as well as first name, last name, date of birth and the last four digits of the social security number. For members, it contains the unique identifiers and gender. The wait-list sample includes more than 70,000 unique households with more than 158,000 unique members. The waist-list file also includes the initial randomized position for both public housing and housing vouchers.

We determine whether a household is at the "top" of a given waiting list by running a series of first-stage regressions for receipt of a voucher (public housing) on initial voucher (public housing) position searching over all plausible cutoffs values of position rank in bins of 200 spots, and selecting the threshold which maximizes the first-stage F-statistic. This is analogous to the procedure for identifying discontinuities in RDD's with unknown cutoffs, and has been shown by Porter and Yu (2015) to yield a super-consistent estimator. [3] For public housing, we do this separately by needed bedroom size (measured at baseline) to mirror how public housing units were offered. We show in the appendix that our results are robust to a variety of alternative cut-offs to denote "top" positions. [4] Throughout, we define

[3]This approach also avoids the bias described in (De Chaisemartin and Behaghel, 2018), as it is a version of their "initial offer" estimator.

[4]Additionally, we'd note that this yields very similar cutoffs to simply choosing the 90th percentile of position ranks among the "treated" households.

treatment as leasing-up with assistance *during* the period spanned by out outcome window.[5]

We make two sample restrictions for our analysis. First, we restrict the sample to applicants living in Miami-Dade County at the time of application. This is because applicants outside of Miami-Dade were typically ineligible to receive assistance. Second, we exclude the 4 percent of applicants living at public housing at baseline, since we are primarily interested in the effects of the transfer relative to counterfactual of no housing assistance.

3.3 Address History Data

We rely on several data sources to measure address histories for waitlist families. For households that lease-up in public housing or with a housing voucher, we can directly observe their assistance addresses using housing authority records. For unassisted households, we use two data sources for address histories. The first is voter records from the state of Florida, for the years 2012 and 2015. The second is consumer reference data from Infutor Data Solutions, covering addresses from the early 2000's to 2020. Collectively, these sources allow to assign a post-lottery address for 80 percent of the families in our sample. Appendix B.2 describes how we construct address histories from this data, and includes robustness to alternative definitions.

3.4 Kindergarten Entry Assessment Data

Our primary outcomes data provide measures of children's early skill development from the Florida Kindergarten Readiness Screener, accessed through the Florida Department of Education (FDOE). Importantly, FDOE linked children listed on housing assistance applications to the individual-level readiness assessment data for 2009 through 2013. The state requires that public schools screen all kindergarten students within the first 30 days they are enrolled.[6] At the time, the school-entry screener consisted of two distinct components: an emerging literacy assessment, the Florida Assessment for Instruction in Reading (FAIR), and a classroom-embedded, observational assessment of school readiness, the Early Childhood Observation System (ECHOS). These assessments measure children's early development and school readiness relative to a number of benchmarks, including broader measures of social and behavioral development–in addition to early literacy and numeracy skills–than are typically available in assessment data.

[5] That is, some "control" households will be receive assistance years later. We explore whether this affects our results in section 5.5, and find that it does not.

[6] Children enrolled in the state's voluntary, public pre-kindergarten (VPK) program or kindergarten in non-public schools may also participate in the screening, however, nearly all of our records are from public elementary schools, including charters.

We use three measures from the readiness screener: (1) a standardized "probability of reading success" score from the FAIR literacy assessment, (2) an indicator of kindergarten readiness also derived from the FAIR literacy assessment, and (3) a categorical designation of "Demonstrating," "Emerging/Progressing," or "Not Yet Demonstrating" readiness skills as measured across the developmental benchmarks captured on the observational ECHOS assessment. While assessments of young children likely measure noncognitive development (e.g., task persistence, attention) even when the target of the assessment is a cognitive skill, such as literacy, these data importantly contain direct measures of social-behavioral development. The ECHOS evaluates individual student progress towards 19 benchmarks in seven domains: language and literacy, mathematics, social/personal skills, science, social studies, physical development, and creative arts. The data description found in Appendix B.2 provides more details on the contents of these data.

3.5 Housing Instability and Homelessness

We also draw on housing instability data from the Florida Department of Education. As a part of linking children listed on the lottery files to their student records, we also received annual information collected from local districts on students experiencing housing instability. Under the McKinney-Vento Act, as a condition of receiving funds, school districts are responsible for making every effort to identify homeless students, or students "who lack a fixed, regular, and adequate nighttime residence." Districts must employ a liaison to assist in identifying students that are homeless. Districts must use existing data to help identify homeless students and schools are encouraged to reach out to community shelters, soup kitchens, transitional living facilities, and a host of other agencies, as well as develop partnerships with programs such as Head Start and Early Intervention, to assist in identifying children who are eligible for services under the McKinney-Vento Act. The data records whether a student has experienced any of the following living situations during each academic year: Living in emergency or transitional shelters (i.e. "shelter"), awaiting foster care, sharing the housing of other persons due to loss of housing or economic hardship ("doubled-up"); living in hotels or motels, or living in a place not meant for human habitation (i.e. living in cars, parks, public spaces, abandoned buildings etc). For our linked sample, the most common housing homelessness states are living in emergency shelter and doubled-up, which comprise 34 and 58 percent of recorded instability in our sample, respectively. We focus on whether the child has been flagged as experiencing *any* of the categories of housing instability ("Homeless or Doubled-up"), and separately whether they appear as living in shelter or doubled-up.

3.6 Rent and Hedonic Quality Data

To measure housing consumption, we construct measures of hedonic quality at baseline and follow-up addresses. We assemble data on address-level rents from two sources. The first is pre-lottery data on housing voucher recipients in Miami Dade. This data provides info on the rent of the unit (before receipt of the voucher) for about 200,000 unique address from 2001-2008. We then supplement this data with more than 2 million historical rent listings on more than 300,000 unique dwellings from Altos Research, a provider of housing market data. We geocode both data to the parcel level, and link them to housing unit characteristics from the Miami Dade County Property Appraiser. These records contain detailed housing unit characteristic. We parameterize our hedonic model as follows, where j indexes parcels and n neighborhoods:

$$ln(\text{Rent}_{j,n}) = X_j\beta + \mu_n + \varepsilon_j$$

X contains attributes such as square-footage, age of home (and a cubic in age), the assessed value, bedrooms, bathrooms, number of floors, building type (i.e. townhouse, single-family attached etc) indicators, and quality codes assigned by the assessor. We include block-group median rent, which we calculate directly from our rent data. Finally, we include neighborhood subdivision fixed effects μ_n to account for other unobserved characteristics of the neighborhood (such as proximity to the ocean, downtown, or south beach). The attributes in our hedonic model can explain roughly 74 percent of the cross-sectional variation in rents in our data.

3.7 Neighborhood Data

We link address locations to neighborhood characteristics measured at the census tract-level. These include: poverty rates and rates of single-parent household from the ACS 2006-2010 five year estimates. Violent crimes per 1,000 from the National Neighborhood Crime Study (NNCS), Wave 2 (Krivo et al., 2023).[7] Finally, we link to the predicted income rank in adulthood from the Opportunity Atlas (Chetty et al., 2018).

4 Empirical Strategy

The random initial ordering of the waiting lists for vouchers and public housing provides exogenous variation in offers of housing assistance. Since some children are in households randomly placed at the top of the voucher waiting list, while others are randomly sorted to

[7] While the NNCS has multiple years of crime data for neighborhoods in several large jurisdictions in Miami-Dade County, including Miami and Hialeah, it lacks coverage in some suburban communities. To account for this, we regression impute neighborhood violent crimes for these uncovered tracts using several neighborhood characteristics from the ACS: the poverty rate, the fraction of single-parent households and the home ownership rate.

the bottom, a natural strategy is to compare those placed at the top to those at the bottom. This strategy will recover a reduced-form "Intent to Treat" (ITT)-style parameter. However, because many households on the waiting list do not receive assistance, we are also interested in estimating the effect of leasing up with housing assistance. To do so, we use random waiting list position as an instrument for assistance and estimate a Local Average Treatment Effect (LATE): the effects of leasing with assistance for those induced into assistance by their wait-list position.[8]

To estimate the reduced form effect of voucher and public housing wait-list position on child outcomes, we estimate the following equation:

$$y_i = \beta_1 \text{Top Voucher}_i + \beta_2 \text{Top Public Housing}_i + \phi_x + X_i\Gamma + \varepsilon_i \tag{4.1}$$

Where y_i is the test outcome for child i, Top Voucher$_i$ is an indicator for being randomized to the top of of the voucher waiting list, and Top Public Housing$_i$ is an indicator for being randomized to the top of the public housing waiting list. β_1 is the reduced-form effect of being at the top of the voucher list and β_2 is the reduced-form effect of being at the top of the public housing waiting list. We include fixed effects ϕ_x for two pre-lottery factors which affect the mapping of public housing position to unit offers: the predicted number of bedrooms needed and the location in one of three geographic zones delineated by PHCD. Additionally, since some individuals appear multiple times on the waiting list under different household applications, we control for the number of applications. Finally, in all specifications we control for year-quarter of birth to account for cohort differences. Throughout the analysis, we cluster the standard errors at the household level.

To estimate the effects of being admitted to public housing or leasing up with housing vouchers, we instrument for lease-up with the initial randomized position on each waiting list. Specifically, the empirical approach taken has the following form:

$$\begin{aligned} \text{Housing Voucher}_i &= \pi_1 \text{Top Voucher}_i + \pi_2 \text{Top Public Housing}_i + \phi_x + X_i\Gamma + \varepsilon_i \\ \text{Public Housing}_i &= \gamma_1 \text{Top Voucher}_i + \gamma_2 \text{Top Public Housing}_i + \phi_x + X_i\Gamma + \nu_i \end{aligned} \tag{4.2}$$

Where i indexes individuals, the outcome of the second stage is y in equation 4.3. The first stage is estimated simultaneously, jointly predicting lease-up for public housing and housing

[8] Below we discuss how this LATE does not have the usual interpretation of the effect of a single treatment relative to a "no-treatment" control group.

vouchers. Equation 4.2 is the first stage broken out by type of assistance. For both types of assistance, we instrument for leasing-up with indicators for being randomly assigned to the top of the public housing list (Top Public Housing) and top of the voucher list (Top Voucher). We include μ_x capturing the fixed effects for the factors mentioned above. We also include a vector of pre-lottery controls including: child and household demographics, neighborhood characteristics (X).[9]

$$y_i = \beta_1 \text{Housing Voucher}_i + \beta_2 \text{Public Housing}_i + \phi_x + X_i \Gamma + \varepsilon_i \tag{4.3}$$

In the simple single-treatment world, just-identified IV will recover a well-defined Local Average Treatment Effect (Imbens and Angrist (1994)) of the effect of receiving treatment relative to counterfactual of "no-treatment." However, with more than one endogenous variable standard IV does not identify the the effect of receiving either treatment relative a well-defined counterfactual. This is because an instrument draws in new households from multiple margins: those that would choose the alternative treatment if they didn't receive the instrumented treatment and those who would choose no-treatment. That is, some households may switch from public housing to vouchers if they get a voucher offer. This "switching" behavior, or substitution bias (Heckman et al., 2000), constitutes a violation of the exclusion restrictions. This issue has been considered recently by Kirkeboen et al. (2016) as well as Kline and Walters (2016) and Hull (2018), and originally by (Heckman et al., 2000). Instead, the parameters estimated in equation 4.3 correspond to the effect of vouchers (public housing) relative to a mix of "no treatment and public housing (vouchers)."

These studies (Kline and Walters, 2016; Hull, 2018; Kirkeboen et al., 2016; Mountjoy, 2022) show that in the presence of multiple treatments (or multiple counterfactual states), estimating standard just-identified IV will recover a weighted average of "subLATEs" which are counterfactual-specific treatment effects [10] For vouchers, when we estimate these weights (ω) in our full sample, about 83 percent of the weight is on the LATE for moving from no-assistance to a voucher $(LATE_{0\to v})$, and 17 percent is LATE capturing the effect of going from public housing to a voucher $(LATE_{p\to v})$. To more cleanly isolate the effects of assistance relative to a counterfactual of "no assistance," we also report the effects of receiving a voucher

[9]Our base controls include: child gender, race/ethnicity, age and gender of household head, number of kids in the household, and neighborhood poverty.

[10]In our context, the IV coefficient for vouchers βv is:

$$\beta_v^{IV} = \omega LATE_{0\to v} + (1-\omega)\, LATE_{p\to v}$$

where $LATE_{0\to v}$ is the LATE of receiving a voucher for households who would have otherwise chosen "no treatment" and $LATE_{v\to p}$ is the LATE for households who would have otherwise chosen public housing. The weight $\omega = \left(-\frac{E[D=0|Z_v(1)]-E[D=0|Z_v(0)]}{E[D=v|Z_v(1)]-E[D=v|Z_v(0)]}\right)$

(public housing) for those *not* at the top public housing (voucher) waiting list, which we refer to as the "No Assistance Alternative". Using this "No Assistance Alternative" specification, the weight on $LATE_{0\rightarrow v}$ (i.e. the effect of voucher relative to a "no assistance" counterfactual) increases to 94 percent, and the weight on $LATE_{p\rightarrow v}$ falls to 6 percent. That is, by restricting the sample to households who's voucher (public housing) position has little impact on their public housing (voucher) take-up, we can more clearly isolate the effects of vouchers (public housing) relative to a counterfactual of no assistance.[11]

4.1 Evaluating Balance and Sample Characteristics

Before turning to results, we first examine whether the initial housing voucher and public housing positions are indeed random. Table 1 reports the mean baseline sample characteristics of children and households at the top of the voucher list ("Top Vouchers") in Column 1, the top of the public housing list ("Top Public Housing") in Column 2, and those outside the top of both lists ("Control") in Column 3. We report the p-value from a joint significance test across groups in Column 4. The characteristics of children and families looks quite similar across groups, and we fail to reject that the groups are different for most covariates. This gives us confidence that the assignment process or initial waiting-list positions was indeed random.

In addition to testing balance on baseline covariates, the final two rows of Table 1 report the fraction of children who have non-missing outcome data for our child development measures across treatment arms. Since this data is from *after* random assignment, there is no ex ante guarantee that appearance in our outcome data would be balance. However, appearance in the outcome data appears to be well-balanced, which minimizes concerns about introducing bias from differential attrition in the outcome.

Our sample consists primarily of children ages 0-4, with most children being either 1 or 2 years old at the time of the lottery. Over 90 percent of children are in female-headed households, and most households include at least two minor children and only a single adult. The sample is overwhelmingly minority, with roughly 70 percent being Black or African-American, and 30 percent identifying as Hispanic. Most families reside in moderately high poverty neighborhoods as baseline, and families have high rates of homeless histories. In general, our sample is not dissimilar from prior studies of housing assistance Sanbonmatsu et al. (2011); Wood et al. (2008).

[11] Note that we focus on the weights here for the effects of vouchers, because we find in Table 2 that having a better public housing position generates almost no substitution away from vouchers

5 Results

5.1 Waiting List Positions and Housing Assistance

We start by examining the relationship between randomized initial waiting list position and receipt of housing assistance. Figure 1a provides graphical results that illustrate the relationship between randomized waiting list position for housing vouchers and the probability of voucher lease-up. Children in households near the "Top" of the voucher waiting list (those with positions less than 9,200) are about 30 percentage points more likely to receive a voucher than children with initial positions greater than 9,200.

Figure 1b provides an analogous plot for public housing waiting list position and lease-up. Take-up rates for public housing are significantly lower, reflecting the fact that families in Miami-Dade appear to prefer housing vouchers. Additional, the relationship is noisier, reflecting the fact that the housing authority would also consider bedrooms need and three geographic zones in making public hosing offers. Still, children in households near the "Top" of the public housing waiting list (those with positions less than 22,000) are about 16 percentage points more likely to lease-up in public housing than children with initial positions greater than 22,000.

The first-stage regression analog to these results appears in Table 2. In both cases, the randomized initial waiting list position has a strong statistical relationship with take-up. The 30 percentage point increase in lease-up for vouchers is somewhat lower than previous studies of vouchers (Jacob et al., 2015; Wood et al., 2008) which found take-up rates ranging from 40-55 percent. Part of this is due to the fact that voucher offers were spread out over several years, and the housing authority was working from "stale" records in later years, making it difficult to find and contact families to notify them. Public housing take-up is meaningfully lower, and this appears in part due to a preference for vouchers: having a "top" voucher position reduces public housing take-up by nearly 5 percentage points (while public housing position has no effect on voucher take-up). Nevertheless, both position have a strong direct effect on take-up, generating first stage F-statistics of 453 for vouchers, and 405 for public housing.

5.2 Effects on Housing and Neighborhood

Before exploring the effects of housing assistance on child outcomes, we examine the effects of housing assistance on the housing and neighborhood conditions experienced by applicants. Prior research has found has little to no effect of voucher receipt on measures such as neighborhood poverty rates for unassisted families living in private housing at baseline (Wood et al., 2008; Jacob and Ludwig, 2012). This contrasts with the effects of a voucher for families residing in public housing at baseline, as in Moving to Opportunity (MTO), where voucher families experienced large reductions in neighborhood poverty and crime exposure (Sanbonmatsu et al., 2011).

Table 3 presents the reduced-form and IV estimates on changes in housing and neighborhood characteristics from pre-lottery and post-lottery locations. Columns (2)-(6) reports results on several neighborhood characteristics, each measured at the census tract level. Receipt of a housing voucher causes a family to move to a neighborhood with roughly 3 percentage points lower poverty rates (relative to baseline poverty rate of 21 percent). This estimated effect is slightly larger than prior experimental estimates of vouchers for families living in private housing (Wood et al., 2008; Jacob and Ludwig, 2012), but only about one-sixth of the effect of relocating from distressed public housing with a voucher from MTO (Sanbonmatsu et al., 2011; Chetty et al., 2016). Vouchers also cause families to move to safer neighborhoods. We estimate that receipt of a voucher leads a family to move to neighborhood with 5.4 fewer violent crimes per one thousand (a 43 percent reduction relative to the baseline mean). We find no detectable effects on neighborhood school quality, the fraction of single-parents in the neighborhood, or predicted income rank in adulthood from the Opportunity Atlas (Chetty et al., 2018).

In contrast to vouchers, participation in public housing, on average, entails moves to neighborhoods that are *more* impoverished. We estimate that moving into public housing increases the neighborhood poverty rates exposure by 8 percentage points (36 percent of the baseline control mean). While it is unsurprising that tracts with public housing will have elevated poverty rates, it is striking that families in our sample are moving *from* pre-lottery neighborhoods with substantially lower poverty. Moving into public housing also increases exposure to violent crime. Families' lease-up in public housing causes them to move to neighborhoods with 8 more violent crimes per one thousand (a 65 percent increase relative to the baseline neighborhood).

In Table 3 we also estimate the effects of housing assistance on change in hedonic housing quality, as described in section 3.6. Receipt of a voucher increases hedonic quality by roughly 9 percent. We estimate that admissions to public housing causes families to move into units of lower hedonic quality, on average, however the estimate is not statistically significant. Taken together, these results suggest that vouchers and public housing have divergent effects on the housing and neighborhood conditions of applicants. Public housing induces moves to similar quality housing in neighborhoods with higher poverty and crime, and vouchers leads to moves to better quality housing in less impoverished and safer neighborhoods.

5.3 Effects on Housing Instability and Homelessness

Another important dimension of housing consumption relates to housing *instability* or homelessness. While housing instability is often especially difficult to observe for families, we're able to use the data collected by McKinney-Vento coordinators on students experiencing housing instability to capture it. This data is described in more detail in section 3.5.

Table 4 reports the impacts on three measures of housing instability. Column (1) captures the impacts on whether a child experienced any type of homelessness, including doubling-up. Columns (2) and (3) disaggregates into the two most common categories of housing instability in the data: living in a shelter, or living doubled-up due to economic hardship.

Housing vouchers and public housing both appear to substantially reduce housing instability. Children who's family receive a housing voucher are 7.4 percentage points less likely to be reported as living in shelter, hotel/motels, places not meant for habitation or doubling-up. Similarly, children in families admitted to public housing are 8 percentage points less likely to reported any housing instability. Both estimates represent large relative reductions with about 8 percent of the control group experiencing homelessness or doubling-up during that same time. The effects appear similar when focused on the types of housing instability. Receipt of a housing voucher leads to a 3 percentage reduction in living in a shelter. The estimate for public housing is nearly identical, however, it is more imprecise and not statistically significant. Similarly, both programs appear to decrease doubling-up. Receipt of a voucher reduces doubling-up by 5 percentage points, and public housing by an estimate 5 percentage points.

Our results indicating that voucher receipt reduces homelessness are similar to prior studies using more selected samples of homeless families Gubits et al. (2015) or families exiting welfare (Wood et al., 2008). To our knowledge, we are the first to provide randomized evidence of the effects of public housing on housing instability and homelessness.

5.4 Effects of Assistance on Child Development Outcomes

Having established how public housing and housing vouchers impact the housing and neighborhood conditions experienced by families, we now turn to documenting the effects of housing assistance on the developmental outcomes of children. As described in section 3.4, we draw on an assessment of early literacy skills as part of the Florida Assessment for Instruction in Reading which produces a reading score and a determination of kindergarten readiness, and from classroom observations derived from the Early Childhood Observation System (ECHOS).

We start by providing non-parametric evidence of these programs impacts on children's outcomes. Figure 2a plots binned means of Kindergarten Readiness rates for children by their randomized initial housing voucher waiting list position. The treatment group (i.e. those with a "Top HCV" position) are represented in navy circles, while the control group (those outside the top) are in gold. The average values for both groups appear separately as dashed lines. Children with a voucher wait list position in the "top" group appear to be, on average, about 4 percentage points more likely to be "Kindergarten Ready." For public housing, which appears in Appendix Table A.1a, there is no visually apparent difference in readiness for kindergarten among those children randomly assigned to the top of the public housing list as compared to those outside the top.

Table 5 features the regression analog to these reduced-form plots. The top panel (panel A) reports our reduced form estimates of the causal effects of waiting list position on early literacy skills and kindergarten readiness. The bottom panel reports our IV estimates of the effects of housing assistance. Consistent with the reduced-form plots for vouchers, we estimate that being randomly-assigned to the top of the voucher waiting list ("Top HCV") increases kindergarten readiness by 4 percentage points. The IV estimate implies that families' receipt of a housing voucher leads to a 13 percentage point increase in their children's kindergarten readiness. This effect is substantial, representing a 22 percent increase relative to mean control mean of 61 percent. The effects appear slightly large when we focus on households that were unlikely to have public housing as an alternative using our "No Assistance Alternative" specification (Column 3).

Table 5 also reports the standardized performance on the FAIR literacy assessment. Being randomly-assigned to the top of the voucher list increases literacy scores by 0.06 standard deviations. While our IV estimates imply that receipt of a voucher boosts early literacy skills by 0.22 standard deviations. Again, we find somewhat larger point estimates when we focus in on families who are unlikely to have public housing as an alternative (Column 3).

While vouchers appear to improve early literacy and kindergarten readiness, we find no similar gains for children in families who lease-up in public housing. The point estimates for public housing are consistently close to zero for kindergarten readiness and standardized early literacy skills, and none are statistically significant. For example, the IV estimate for leasing-up in public housing on early literacy scores is -0.006 standard deviations and a 1.7 percentage point increase in kindergarten readiness. The estimates for public housing are slightly less precisely estimated than for voucher. Still, if public housing produced similar-in-magnitude effects as vouchers, we would be powered to detect them.

Next, we investigate how housing vouchers and public housing impact the likelihood that children meet developmental benchmarks. As described in 3.4 and detailed in Appendix B.2, these measures come from an embedded observational assessments of children's behaviors and activities in the classroom. Again, we start with non-parametric evidence on this question. In the bottom panel of Figure 2b we plot the probability that a child is falls into the lowest category of performance, “Not Emerging or Demonstrating”, by initial randomized voucher position. There is a clear shift down among children at the top of the voucher list, indicating that they are *less* likely to be flagged as not-demonstrating the necessary skill development in classroom observations. However, we see no similar pattern for public housing, which appears in Append Figure A.1b.

Table 6 reports the regression estimates of impacts of wait list position and assistance on childrens’ likelihood of demonstrating developmental benchmarks during these assessments. Again, the top panel presents the reduced-form and the bottom panel reports the IV estimates. The reduced-form estimates indicate that children randomly-assigned to the top of the voucher list are 2.4 percentage points less likely to be flagged as not demonstrating the appropriate skills for kindergarten. This indicates that children in families that were more likely to receive a voucher also perform better in these assessments. We find that receipt of a voucher leads to a nearly 9 percentage point reduction in the likelihood of receiving the lowest category of performance, “Not Emerging or Demonstrating.” At the same time, we find a similar-sized *increase* in the likelihood of receiving the the middle category, “Emerging.” Children in voucher-receiving families are nearly 10 percentage points more likely to be classifies as showing “Emerging” developmental skills. The pattern of effects indicate that the impacts of vouchers on children are concentrated in the lower part of the skill distribution. With receipt of a voucher, children are made more likely to move out of the bottom and into the middle categorization of skills, however, we find no effects of moving into the top categorization of skill development (“Demonstrating”). Similar to our results above on kindergarten readiness and early literacy, the positive effects appear even larger when we estimate only among children in families with no alternative housing assistance.

In contrast to the voucher results, public housing does not appear to improve to children’s likelihood of reaching developmental benchmarks, as recorded in the observational assessment. None of our estimates for public housing are statistically significant. Relative to the results for the early literacy skills, the point estimates for impacts on developmental benchmark measures more consistently point towards improved outcomes. Again, if the point estimate for public housing on “Not Emerging or Demonstrating” were as large as the estimate for vouchers, we would be powered to detect such an effect. Our point estimates for public housing do allow us to rule-out even moderately sized *negative* effects on children’s skill building. This is not trivial given both the strong negative reputation public housing historically held in public conscious (Olsen, 2003; Collinson et al., 2016), and the generally negative effects we find on housing and neighborhood quality in section 5.2. It also broadly consistent with earlier quasi-experimental studies of public housing, such as Currie and Yelowitz (2000), which found non-negative impacts on child outcomes.

As additional validation of our main results, we revisit the declining pattern take-up of voucher receipt found in Figure 1a. Those at the very top of the "Top Voucher" group have noticeably higher take-up of vouchers, than those further down the "Top Voucher" group. If our estimated effects reflect the *causal* effect of a voucher, then our reduced-form results should be larger among the group with a higher take-up rate than the group with a lower take-up rate. This is exactly what we find in Appendix Table A.1. In Column 1, we show the differences in take-up, consistent with our plot, the earlier voucher waiting-list position have a higher take-up rate of 46 percent.[12] Column 2 of Panel A in the same table reports the reduced-form effects. The reduced-form point estimates appear considerably larger for those with very "Early" positions than those with "Later" positions, giving us further confidence the positive effects we find for vouchers are indeed causal. Finally, panel B of Appendix Table A.1 reports the IV estimates for those treated in the earlier group and those in the later group. We interpret the larger IV estimates as potentially reflecting an exposure effect: the early group received a voucher for an average of over 3 years before outcomes were measured, while the later group received closer to 1 year of exposure before outcomes were measured.

5.5 Robustness

This section presents results from several exercises that address potential concerns with the empirical strategy and interpretation of results. First, we evaluate the sensitivity of our results to the precise cutoffs used in the definition of being at the "top" of each waiting list. Appendix Table A.2 reports our main results varying the cutoff in either direction relative to the cutoff used in our main specifications (bolded). The point estimates are quite stable across alternative definitions of "top" and are consistently statistically significant. This exercise gives us confidence that these results are not the product of particular choices of "top" definition.

If the behavior of applicant households changes in response to waitlist position — through channels other than actual assistance receipt, including anticipatory effects — that would constitute a threat to our identification strategy, potentially violating the exclusion restriction. We evaluate whether anticipatory child investments could be affecting our results in two ways. First, we drop households with waiting-list positions outside of the "top" but *near* the top such that they could plausibly receive assistance in future years outside of our outcome window.[13] These results appear alongside our main estimates in Appendix Tables A.5 and

[12]The housing authority appeared to work through the voucher list in batches, so there is relatively small "Early" group which receipt vouchers more quickly while the "Later" group is much larger in size.

[13]Note this is similar to Jacob et al. (2015) who exclude households with lottery positions in the middle,

A.6. The reduced-form and IV coefficients are nearly identical to our main specification, suggesting that the presence of these "possibly treated" households is not influencing our estimates. Second, we restrict the sample to applicants that are unlikely to be treated (i.e. those outside of the "top" of their respective waiting-list), and evaluate whether waiting-list position impacts outcomes *within* this sample. These results appear in Appendix Table A.7. We find zero effect of waiting-list position in this sample of unlikely-to-be-treated households. This gives us confidence that anticipatory effects are not impacting our results.[14]

Another concern is that the improvements in child development for voucher recipients do not reflect improvements in cognitive and noncognitive skills, but instead reflect differences across test administrators/graders in the schools children attend for kindergarten. While we cannot directly observe graders, we can compare treatment and control children within the *same* school. In principle, post-treatment school choice is endogenous and is not desirable to condition on, however, it provides a useful check for interpreting our results. In Tables A.3 and A.4, we show that our main results are robust to including school fixed effects, providing support for the notion that differences in grading practices or grader severity are unlikely to be driving our results. It also buttresses our argument that our results are unlikely to be driven by changes in school quality.

5.6 Heterogeneity

To better understand the mechanisms through which housing assistance may or may not improve children's skill development, we explore heterogeneity in the effects of vouchers and public housing by family and child characteristics, families' housing consumption and expenditures, and baseline availability of early care and education options.

5.6.1 Family and Child Characteristics

Given a large literature documenting differences in response to changes in family resources or environmental factors (Dahl and Lochner, 2012; Milligan and Stabile, 2011; Barr et al., 2022), we first examine the effects of housing assistance separately by gender. Table 7 reports the estimates on early literacy skills for boys and girls.The positive effects of housing vouchers are driven by large, beneficial effects for boys. Boys in families randomized into receiving a voucher perform nearly 0.6 standard deviations higher on the pre-reading skills assessment, and they are 29 percentage points (49 percent) more likely to reach the kindergarten readiness threshold. The results in table 8 point to similar gender differences in the classroom-based observational assessments. "Treated" boys are also much less likely than their control group counterparts

"possibly" treated range.

[14] The lack of anticipatory effects is unsurprising given recent evidence of parent's lack of anticipatory investments in response to predictable loss of benefits (Deshpande and Dizon-Ross, 2023)

to be categorized as "not emerging or demonstrating" necessary skills for kindergarten, a nearly 15 percentage-point reduction (or 100-percent decrease relative to the control group). They are also 19 percentage points more likely to be in the "emerging" category, a 40-percent increase. Both sets of results for boys appear in as non-parametric reduced-form plots in Figure 3a and 3b. Girls, on the other hand, show no consistent effects (positive or negative) from receipt of housing vouchers.

In Figure 4, we extend our heterogeneity analysis by: number of children in the household, number of adults in the household, and race/ethnicity. The top panel of Figure 4 reports the impacts of vouchers and the bottom panel reports heterogeneity for public housing. The positive developmental effects of housing vouchers are significantly larger for children living in multiple-child households. The effects also appear larger for Black children than for Hispanic children. For public housing, we do not find any significant patterns of heterogeneity in the effects by household or family characteristics.

5.6.2 Baseline Neighborhood Conditions

A large literature documents the effects of neighborhood conditions on the long-run outcomes of children (Chetty et al., 2016; Chetty and Hendren, 2018; Chyn, 2018). At the same time, the evidence of neighborhoods impacts on cognitive development measured earlier in life are more mixed (Kling et al., 2007; Jacob, 2004; Sanbonmatsu et al., 2011). To evaluate whether changes in neighborhood conditions are driving the effects we find, we examine heterogeneity in effects by baseline neighborhood characteristics. This exercise exploits the fact that the effects of receiving assistance will depend, in part, on the type of neighborhood the household is living in at baseline. As an example, Appendix Figure A.2 plots the effects of voucher on neighborhood poverty (as in section 5.2) separately for households living in census tracts with poverty rates below 20% ("Low poverty") and households living in census tracts with poverty rates at or above 20% ("High poverty"). For families living in high poverty at baseline, receipt of a voucher leads to larger 6.6 percentage point reductions in neighborhood poverty than those already living in low-poverty at baseline who experience no meaningful reduction in neighborhood poverty exposure. For public housing, families living in low poverty at baseline experience the larger *increases* in poverty than those already living in higher poverty neighborhoods before housing assistance.[15]

Figure 5 summarizes heterogeneity in child development impacts by baseline neighborhood conditions: poverty and violent crime. Panel A reports the effects of housing vouchers. The point estimates for vouchers are 0.28SD for children in families starting in low-poverty, and

[15]We find an analogous pattern when we investigate heterogeneity in impacts on neighborhood crime in Appendix Figure A.3.

0.17SD for children in families starting in high-poverty. Taken at face-value the estimates are slightly *smaller* for children who get a larger *reduction* in neighborhood poverty exposure suggesting that the change in neighborhood poverty alone isn't responsible for the positive effects we find. However, we cannot statistically distinguish the estimates, so we caution over-interpreting this pattern. The pattern for heterogeneity by baseline neighborhood crime mirrors that of neighborhood poverty: the point estimates are similar across families in high- and low-crime neighborhoods at baseline with slightly larger estimates for those families who experience smaller reductions in crime exposure.

The effects of public housing by baseline neighborhood conditions appear in Figure 5b. While the sign of the effects of public housing differ across low-poverty and high-poverty, and low- and high-crime neighborhoods, it is unclear if this because the treatment effects are different or an artifact of sampling variance. Similar to vouchers, the patterns of heterogeneity do not seem to imply that households that experience the largest changes to neighborhood environment have fundamentally different treatment effects.

5.6.3 Baseline Early Care Options

One potential pathway through which housing assistance may affect child development could be through moves to neighborhoods with more early childhood care and education programs, and thus greater access to and more opportunity for investment in skill development outside the home. We compiled data from public databases on the location and availability of Head Start — the federally funded preschool program for children from low-income households — centers, providers offering the state's voluntary, public pre-kindergarten (VPK) program, child care and preschool settings accepting the financial assistance provided through the state's School Readiness program, and child care and preschool providers with the state's high-quality "Gold Seal" designation. Using these data, we explore variation in housing assistance impact for children in households with less exposure to ECE opportunities prior to receipt of housing assistance.

We focus on baseline proximity to Head Start as results disaggregated by proximity to the other types of early care and education options did not differ. We define proximity to Head Start based on any Head Start center within 3/4 of a mile of the household's address at time of housing assistance application. Results in Figure 5b are suggestive of more pronounced effects for children in families living farther from any Head Start center prior to housing assistance. In the top panel showing heterogeneity in the impact of housing vouchers, the point estimates for those close to Head Start at baseline and those who were not are both positive and statistically indistinguishable though the effect for those who were farther away from Head Start is 0.28 SD, nearly three times larger than that of those who were close to Head Start at baseline. The effects for public housing, in the bottom panel, are also statistically indistinguishable from one another, but the point estimate for those close to Head Start prior to public housing assistance is large and negative.

5.7 Mechanisms: Housing Consumption v. Non-Housing Consumption

To better understand whether the child development impacts above are driven by increased non-housing spending (i.e. "family resources") or improvements in housing and neighborhood conditions, we explore heterogeneity in impacts by the family's baseline housing situation. Public housing and housing vouchers operate with similar rent rules: families contribute a minimum of 30% of their adjusted income towards rent.[16] Consequently, families spending significantly more than 30% of their income towards rent *before* housing assistance receive a large increase in disposable income, while families spending 30% or less of their income on housing at baseline experience, if anything, a reduction in disposable income.

Figure 6 visualize this intuition with simple representation of the budget constraints before and after housing assistance (see Collinson et al. (2016) and Olsen (2003) for similar representations). In the top panel (a), we consider a household spending little on housing at baseline, consuming low-levels of housing: h'_0. If we assume that this household will maximize the value of the voucher, then they will consume h_v when they receive a housing voucher (this would be the quality of housing corresponding to the local "payment standard" or the maximum value of the voucher before the household pays more out-of-pocket). This household experiences a relatively large increase in housing consumption $\Delta h'$, but a comparatively small change in non-housing consumption $\Delta c'$. In contrast, panel b considers a household spending a high proportion of their budget on housing at baseline: h''_0. Assuming again that this household selects h_v with a voucher, they experience a large increase in non-housing consumption $\Delta c''$, but a small change in housing consumption, $\Delta h''$. Given this, we can assess heterogeneity in effects by baseline housing consumption to learn about the role of increased non-housing spending versus improved housing and neighborhood conditions.

While we cannot directly observe baseline housing consumption, h_0, we can use our hedonic rent estimates for the baseline addresses described in section 3.6 and used in section 5.2 to measure it. We categorize households in our sample as "low" housing consumption at baseline if the hedonic estimate: $\widehat{h_0}$ is less than the payment standard (i.e. h_v), and "high" baseline housing consumption if the hedonic estimate is greater (or equal to) the payment standard.

[16] Families with a voucher can "top-up" and spend more than 30% of their adjusted income on rent if the market rent is above the locally-defined payment standard (price ceiling), however, they must not spend more than 40% of their income towards rent at initial lease-up. The majority of voucher holders spend do not "top-up", for evidence see Appendix B4 in Collinson and Ganong (2018)

In Figure 7, we report impacts on hedonic quality (i.e. housing consumption) separately for "low" baseline housing consumption households and "high" baseline housing consumption households. Consistent with the theoretical predictions from Figure 6, we find empirically much larger effects on housing consumption of housing voucher receipt for households with "low" baseline consumption. The IV estimate for "low" baseline consumption households is a 25 percent increase that is highly significant, while the effect for "high" baseline housing consumption is slightly negative and not significant.

Having established that we can isolate a subgroup that experiences a sizeable increase in housing consumption and a subgroup that should experience a marked increase in non-housing consumption, we now turn to heterogeneity in child development impacts. Figure 8 reports the effects of housing voucher and public housing for the pooled sample, then separately by predicted baseline housing spending. We find that the effects of the voucher appear to be driven primarily by children in households with low baseline housing consumption. The IV point estimate for receipt of a voucher is 0.57 standard deviations, and highly significant. In contrast, the effects of a voucher for households with high baseline housing consumption is close to zero and not significant. These results suggest that increases housing consumption, rather than increase in non-housing consumption, are driving the observed effects.

One concern is that those households that we categorize as "low" housing consumption at baseline may just be lower income, in which case, we would be conflating heterogeneity in effects by housing consumption with heterogeneity by income. While we do not observe income at the time of the waiting list opening, we can use the income at admission for assisted households which we *do* observe, and predict income as a function of baseline covariates.[17] In Appendix Figure A.5, we plot the estimates for households by estimated baseline income. The effects appear slightly larger for households that with lower predicted income, but we cannot reject that the effects are the same. Taken at face value, these results would suggest that heterogeneity in effects by income is not driving the heterogeneity we find for baseline housing consumption.[18]

6 Discussion

Our results imply that receipt of a housing voucher significantly improves development outcomes for young children, while public housing has little detectable effect on child development.

[17]To improve this prediction, we also use baseline credit scores derived from a link to Experian records.

[18]Since we investigate heterogeneity over a many subgroups, we assess how distinct these groups are in a correlation matrix in Appendix Table A.8. We find relatively low-levels of correlation across subgroup membership, indicating that many of these splits are distinct along observable dimensions.

How do these results compare to other studies of the effects of housing assistance? The closest study to ours is Jacob et al. (2015) (hereafter "JKL"), which uses a lottery of housing vouchers linked to data on 3-8th grade test scores, high school completion, crime and hospitalizations. They conclude that there are few, if any, meaningful effects of vouchers on children. It's worth considering the reasons why our results for vouchers differ from this work. These can be broadly classified as differences in: treatment, outcome measures, population, and data.

While JKL also study housing choice vouchers, there are some differences in context, which may matter for understanding differences in the program's effects. The vouchers studied by JKL were administered by the Chicago Housing Authority, which operates almost exclusively within the City of Chicago. Voucher holders seeking to lease-up outside of the city boundaries (say in Cook County suburbs) generally need to "port-out" - meaning there needs to be an agreement with neighboring housing authority to receive the family. This is regarded as administratively cumbersome, so CHA housing voucher recipients typically lease-up in central city neighborhoods. In contrast, we study a context where the housing authority serves an entire county. In theory, this could open-up a greater range of residential options (Greenlee, 2011). Consistent with these facts, we find that families in our context do use their vouchers to lease-up in observably "better" neighborhoods: census tracts with lower crime and less poverty. Whereas JKL find no effects of voucher receipt on similar standard measures of neighborhood quality.

Another possibility is that the difference in effects of vouchers we observe is driven by differences in population served. There are two major observable dimensions across which our sample and that studied in JKL differ. The first is race and ethnicity. The sample studied in JKL is 94 percent Black and less than 4 percent Hispanic, while ours is 70 percent Black and 30 percent Hispanic. In Appendix Table A.9, we re-estimate our results reweighting the sample to match the race/ethnicity composition of JKL's sample. If anything, our estimates appear *larger* when we match the race/ethnicity of JKL, which suggests this is not the source of differences. The more salient difference might be that of average age at time of treatment. By construction, our focus on early child development means that our sample is substantially younger at the time of random assignment. JKL *do* find marginally significant positive effects for boys ages 0-6 at time of voucher offer, however, their IV point estimate for boys is still quite a bit smaller (0.06 SD) than our main estimate for boys (0.55 SD). While age at treatment and time from randomization to measurement of outcomes may explain some portion of the difference in results, those explanations are likely insufficient to fully account for the difference in estimates.

One subtle, but potentially important, difference relates to the data environment. There are several features of our data that differ from previous work. First, our waiting-list data allow us to directly observe the children associated with an application and their identifying information necessary for linking to outcomes. In contrast, JKL do not observe the children attached to assistance applications, and must use public assistance records to identify the children likely to be in the waiting-list household. As a result, our data should be less likely to suffer from measurement error than prior studies. Second, we define receipt of assistance *at the child-level*, whereas prior work primarily measures assistance at the level of the household-head. While both approaches yield similar results, defining assistance at the household-head level will tend to overstate the fraction of children assisted, and hence understate the IV estimates of receipt of housing assistance. Finally, we leverage statewide education data which allows us to measure children's outcomes even if they move across public school districts whereas prior work typically relied on data from just a single school district.

Finally, the outcomes we consider differ from previous work in two important ways: first, we examine effects upon entry into formal schooling which is proximal to children's experiences of early-life conditions and resources, and secondly, our outcomes capture both cognitive and noncognitive skills. Jacob et al. (2015) study the medium-run impacts of vouchers on math and reading test scores in third through eighth grades.[19] Measuring test scores well into formal schooling will include not just the effects of changes in early-life resources and the home environment, but typically several years worth of inputs from the educational system. In contrast, our measures of skill development upon entering kindergarten are likely shaped almost entirely by the home experiences because the children are too young to have significant contact with the school system. Our measures are inherently shorter-run in nature, compared to the longer-run test score impacts studied by JKL. Beyond this important difference, the outcomes we consider are also conceptually different measures than standardized reading and math assessments, including both early literacy skills assessment during dedicated one-on-one test administration, and also classroom observations of children's behavior and social-emotional development. These data contain direct measures of noncognitive skills such as responsible decision-making, social problem solving, and approaches to learning. Researchers have also demonstrated that cognitive assessments contain substantial noncognitive content, such as task persistence and attentiveness (Balart et al., 2018; Cornelissen and Dustmann, 2019), which could be particularly important for completing assessment tasks at young ages. Our measures would thus capture both the noncognitive skills they are intended to measure and implicit noncognitive content associated with assessment task performance.

Consistent with our finding that the impact of housing vouchers on early skill development

[19]JKL also examine impacts on high school completion, but only for children older than six at the time of lottery, limiting comparability to our setting.

is concentrated among boys — and JKL's (small) positive test-score effect for boys exposed to a housing voucher at younger ages — a growing literature documents gender differences in responsiveness to early-life resources. Both Barr et al. (2022) and Dahl and Lochner (2012) find more pronounced effects of increased household resources from the Earned Income Tax Credit (EITC) among boys. Research also documents the more pronounced deleterious effects of family disadvantage for boys (Autor et al., 2019; Bertrand and Pan, 2013), and the larger benefits of cumulative exposure to high-quality schools for boys (Autor et al., 2016). Explanations for larger intervention effects for boys include a biological argument about the vulnerability of young boys relative to young girls, and hinge on gender differences in returns, particularly for behavioral skills, rather than differential investments by gender.

7 Conclusion

In this paper, we investigate how two major in-kind housing transfers affect housing consumption and, in turn, childhood development outcomes. We link individual child records from a unique housing assistance lottery in Miami-Dade County, Florida, to housing and neighborhood quality, housing instability, and early childhood skill assessment data. We find that voucher and public housing receipt both reduce housing instability. However, vouchers cause families to move to observably improved housing and neighborhoods, while the reverse is true for public housing. We subsequently find substantial school readiness and early literacy skill improvements, likely capturing both cognitive and noncognitive skill development, among children whose households receive vouchers. The effects are only realized among voucher recipients, not public housing recipients, and are driven by benefits to boys.

Our work provides experimental evidence that social safety-net programs can improve childhood skill development. To our knowledge, this study represents the first randomized evidence on the effects of safety-net programs, in the field and at scale, on measures of children's early skill development. Our results are consistent with a large body of quasi-experimental evidence on second-generation effects of social programs and transfers (Hoynes et al., 2016; Bailey et al., 2023; Pollakowski et al., 2022). The positive effects we find on school-entry cognitive and non-cognitive skill measures help bridge two literature streams, one finding that in-kind transfers can improve children's long-run outcomes and a second on the importance of resources and investment in the critical early childhood years for generating downstream effects.

We also show that the effects of in-kind transfers on children depend on *how* the programs affect consumption. While vouchers improve childhood skill development, we find no improvements for children moving into public housing, likely because the programs impact housing and neighborhood quality differently. Furthermore, when we examine heterogeneity in voucher effects, the impacts are largely driven by families realizing the greatest housing quality improvements. The finding underscores the important role of the specific consumption changes induced by each in-kind transfer.

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Table 1: Summary Statistics and Randomization Check

	(1) Top Vouchers (HCV)	(2) Top Public Housing (PH)	(3) Control (Bottom)	(4) P-value from joint test
Child Age	2.433	2.426	2.449	0.628
Boy	0.492	0.515	0.498	0.179
Girl	0.508	0.485	0.502	0.179
Age, Head	27.690	28.039	27.947	0.373
Disabled, Head	0.054	0.064	0.064	0.386
Elderly, Head	0.003	0.006	0.005	0.565
White (Not Hispanic)	0.016	0.013	0.015	0.782
Hispanic	0.312	0.345	0.334	0.179
Black	0.704	0.680	0.686	0.377
Asian	0.002	0.001	0.002	0.703
Female, Head	0.919	0.913	0.920	0.538
Ever Homeless	0.081	0.085	0.076	0.461
Tract Poverty	0.227	0.221	0.217	0.019
Has Email	0.710	0.700	0.718	0.253
Total Adults	1.231	1.217	1.228	0.594
Total Children	2.295	2.304	2.231	0.042
Has FAIR-K	0.726	0.725	0.725	0.995
Has ECHOS	0.739	0.740	0.743	0.891
N	1439	3598	9874	

This table reports the mean characteristics for the children in the analytical sample by voucher and public housing wait-list positions. Column (1) reports the mean baseline characteristics for children who's household was randomly assigned to the top of the voucher list. Column (2) reports the means for children who's household was randomly assigned to the top of the public housing waiting list. Column (3) for children from households at the bottom of both waiting lists. Column (4) reports p-values from a joint test of the significance of difference between columns (1) -(2) and column (3), clustering standard errors at the household level.

Table 2: First Stage

	HCV (1)	PH (2)
Top HCV	0.304***	−0.048***
	(0.014)	(0.006)
Top PH	−0.002	0.160***
	(0.005)	(0.008)
Control Mean	0.007	0.021
N	10,864	10,864

Notes: This table reports the first stage results. Column 1 reports the impact of being at the top of the housing voucher waiting-list ("Top HCV") and being at the top of the public housing waiting-list ("Top PH") on lease-up with a housing vouchers. Column 2 reports the impact of being at the top of the housing voucher waiting-list ("Top HCV") and being at the top of the public housing waiting-list ("Top PH") on admissions to public housing. The sample consists of children less than 5 on the waiting list for housing assistance, who link to a developmental outcome described in section 3. Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Table 3: Impact on Housing and Neighborhood Quality

	Housing Quality	Neighborhood Quality				Opportunity Atlas
	$\Delta \ln(\hat{Rent})$	Δ Poverty Rate	Δ School Quality	Δ Crime	Δ Single Parent	Δ Income Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Reduced Form						
Top HCV	0.037***	−0.016***	0.017	−2.461***	−0.004	0.002
	(0.014)	(0.004)	(0.026)	(0.483)	(0.004)	(0.002)
Top PH	−0.012	0.016***	−0.005	1.614***	0.008***	−0.002*
	(0.010)	(0.003)	(0.019)	(0.357)	(0.003)	(0.001)
Panel B: IV (LATE)						
HCV	0.088**	−0.029***	0.042	−5.416***	−0.004	0.004
	(0.036)	(0.010)	(0.073)	(1.354)	(0.010)	(0.005)
PH	−0.063	0.080***	−0.023	8.003***	0.038***	−0.011*
	(0.052)	(0.014)	(0.097)	(1.725)	(0.014)	(0.006)
Control Mean	7.146	0.218	−0.650	12.370	0.266	0.360

Notes: This table reports the effects of wait-list position (reduced form) and housing assistance (IV) on housing consumption and neighborhood quality. All outcomes are measured as changes between post-lottery and pre-lottery addresses. All neighborhood quality outcomes are measured at the census tract-level. Column 1 reports impacts on housing quality, defined as hedonic rent, described in section 3. Column 6 impacts on neighborhood predicted income rank in adulthood from the Opportunity Atlas from Chetty et al. (2018). Panel A reports the ITT estimates from estimating equation 4.1 and Panel B reports the LATE estimates. "HCV" reports the coefficient on voucher wait-list position (reduced form) or voucher lease-up (IV). "PH" reports the coefficient on public housing wait-list position (reduced form) or public housing lease-up (IV). Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Table 4: Impact on Housing Instability and Homelessness

	Homeless or Doubled-up (1)	Shelter (2)	Doubled-up (3)
Panel A: Reduced Form			
Top HCV	-0.018^{**}	-0.008^{**}	-0.013^{*}
	(0.008)	(0.004)	(0.007)
Top PH	-0.014^{**}	-0.005	-0.008^{*}
	(0.006)	(0.004)	(0.005)
Panel B: IV (LATE)			
HCV	-0.074^{***}	-0.033^{**}	-0.052^{**}
	(0.028)	(0.015)	(0.023)
PH	-0.085^{**}	-0.032	-0.053^{*}
	(0.039)	(0.023)	(0.031)
Control Mean	0.084	0.027	0.057
Controls	Y	Y	Y
N	11,131	11,131	11,131

Notes: This table reports the effects of wait-list position (reduced form) and housing assistance (IV) on homelessness and housing instability. Column 1 reports impacts on any form of homelessness and doubling-up as described in 3.5. Columns 2 and 3 report impacts on the two common housing instability statuses in the McKinney Vento data, which are living in shelter or transitional housing (i.e "Shelter") and living doubled-up ("Doubled-up").Panel A reports the ITT estimates from estimating equation 4.1 and Panel B reports the LATE estimates. "HCV" reports the coefficient on voucher wait-list position (reduced form) or voucher lease-up (IV). "PH" reports the coefficient on public housing wait-list position (reduced form) or public housing lease-up (IV). Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Table 5: Impacts on Early Literacy and Kindergarten Readiness

	Control Mean (1)	Full Sample (2)	No Assistance Alternative (3)
Panel A: Reduced Form			
Kindergarten Readiness			
Top HCV	0.608	0.040***	0.046***
	(0.488)	(0.014)	(0.017)
Top PH	0.612	0.002	0.004
	(0.487)	(0.010)	(0.011)
Std. Reading Score			
Top HCV	−0.005	0.066**	0.082**
	(1.004)	(0.029)	(0.034)
Top PH	0.004	−0.001	0.004
	(0.999)	(0.021)	(0.023)
Panel B: IV (LATE)			
Kindergarten Readiness			
HCV	0.608	0.135***	0.150***
	(0.488)	(0.048)	(0.056)
PH	0.612	0.017	0.026
	(0.487)	(0.064)	(0.064)
Std. Reading Score			
HCV	−0.005	0.218**	0.269**
	(1.004)	(0.100)	(0.115)
PH	0.004	−0.006	0.025
	(0.999)	(0.134)	(0.134)
Controls	N	Y	Y
N	9,456	10,863	9,454

Notes: This table reports the effects of wait-list position (reduced form) and housing assistance (IV) on early child outcomes. "Kindergarten Readiness" is an indicator for having scored at or above the threshold to be determined ready for kindergarten. "Std. Reading Score" is a standardized score from evaluation of reading success. Column 1 reports the mean among those randomized to the bottom of the waiting list ("Control Mean"). Column 2 reports the ITT estimates from equation 4.1 in Panel A and LATE estimates in Panel B. "HCV" reports the coefficient on voucher wait-list position (reduced form) or voucher lease-up (IV). "PH" reports the coefficient on public housing wait-list position (reduced form) or public housing lease-up (IV). Column 3 reports the effects of voucher (public housing) waiting list position and voucher (public housing) lease-up, but restricts the sample to households with bottom public housing (voucher) waiting list positions. This restrictions limits the degree to which voucher (public housing) control group families are receiving public housing (vouchers). Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Table 6: Impacts on Children's Observed Developmental Benchmarks

	Control Mean (1)	Full Sample (2)	No Assistance Alternative (3)
Panel A: Reduced Form			
Not Emerging or Demonstrating			
Top HCV	0.122	−0.024***	−0.029***
	(0.307)	(0.008)	(0.010)
Top PH	0.121	−0.007	−0.008
	(0.305)	(0.006)	(0.007)
Emerging			
Top HCV	0.472	0.031**	0.063***
	(0.471)	(0.013)	(0.016)
Top PH	0.477	−0.006	0.007
	(0.471)	(0.010)	(0.010)
Demonstrating			
Top HCV	0.312	0.004	−0.011
	(0.438)	(0.012)	(0.015)
Top PH	0.308	0.014	0.008
	(0.437)	(0.009)	(0.010)
Panel B: IV (LATE)			
Not Emerging or Demonstrating			
HCV	0.122	−0.088***	−0.094***
	(0.307)	(0.028)	(0.032)
PH	0.121	−0.044	−0.048
	(0.305)	(0.039)	(0.039)
Emerging			
HCV	0.472	0.097**	0.208***
	(0.471)	(0.045)	(0.053)
PH	0.477	−0.034	0.040
	(0.471)	(0.060)	(0.061)
Demonstrating			
HCV	0.312	0.029	−0.037
	(0.438)	(0.043)	(0.049)
PH	0.308	0.088	0.049
	(0.437)	(0.057)	(0.057)
Controls	N	Y	Y
N	9,697	11,131	9,694

Notes:This table reports the effects of wait-list position (reduced form) and housing assistance (IV) on developmental assessments from embedded classroom observations. "Not Emerging or Demonstrating" is an indicator for the lowest categorization of development during classroom observations, taking the value zero otherwise. "Emerging" is an indicator if the child is coded as showing some indication of the necessary skill development. "Demonstrating" is an indicator which takes the value 1 if the child shows the highest level of skill-development, and zero otherwise. Column 1 reports the mean among those randomized to the bottom of the waiting list. Column 2 reports the ITT estimates from estimating equation 4.1 in Panel A and LATE estimates in Panel B. "HCV" reports the coefficient on voucher wait-list position (reduced form) or voucher lease-up (IV). "PH" reports the coefficient on public housing wait-list position (reduced form) or public housing lease-up (IV). Column 3 reports the effects of voucher (public housing) waiting list position and voucher (public housing) lease-up, but restricts the sample to households with bottom public housing (voucher) waiting list positions. This restrictions limits the degree to which voucher (public housing) control group families are receiving public housing (vouchers). Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Table 7: Impacts on Early Literacy and Kindergarten Readiness by Gender

	Boys		Girls	
	Control Mean (1)	Full Sample (2)	Control Mean (3)	Full Sample (4)
Panel A: Reduced Form				
Kindergarten Readiness				
Top HCV	0.571	0.083***	0.645	−0.005
	(0.495)	(0.020)	(0.479)	(0.019)
Top PH	0.581	−0.002	0.641	0.008
	(0.493)	(0.015)	(0.480)	(0.014)
Std. Reading Score				
Top HCV	−0.093	0.156***	0.080	−0.026
	(1.038)	(0.040)	(0.962)	(0.041)
Top PH	−0.078	0.015	0.082	−0.017
	(1.033)	(0.030)	(0.959)	(0.029)
Panel B: IV (LATE)				
Kindergarten Readiness				
HCV	0.571	0.285***	0.645	−0.008
	(0.495)	(0.074)	(0.479)	(0.063)
PH	0.581	−0.016	0.641	0.049
	(0.493)	(0.089)	(0.480)	(0.090)
Std. Reading Score				
HCV	−0.093	0.554***	0.080	−0.098
	(1.038)	(0.152)	(0.962)	(0.132)
PH	−0.078	0.083	0.082	−0.111
	(1.033)	(0.186)	(0.959)	(0.185)
Controls	N	Y	N	Y
N	4,671	5,351	4,785	5,510

Notes:This table reports the effects of wait-list position (reduced form) and housing assistance (IV) on the same outcomes considered in Table 5, separately by child gender. Column 1 reports the mean among those randomized to the bottom of the waiting list ("Control Mean"). Column 2 reports the ITT estimates from estimating equation 4.1 in Panel A and LATE estimates in Panel B. "HCV" reports the coefficient on voucher wait-list position (reduced form) or voucher lease-up (IV). "PH" reports the coefficient on public housing wait-list position (reduced form) or public housing lease-up (IV). Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

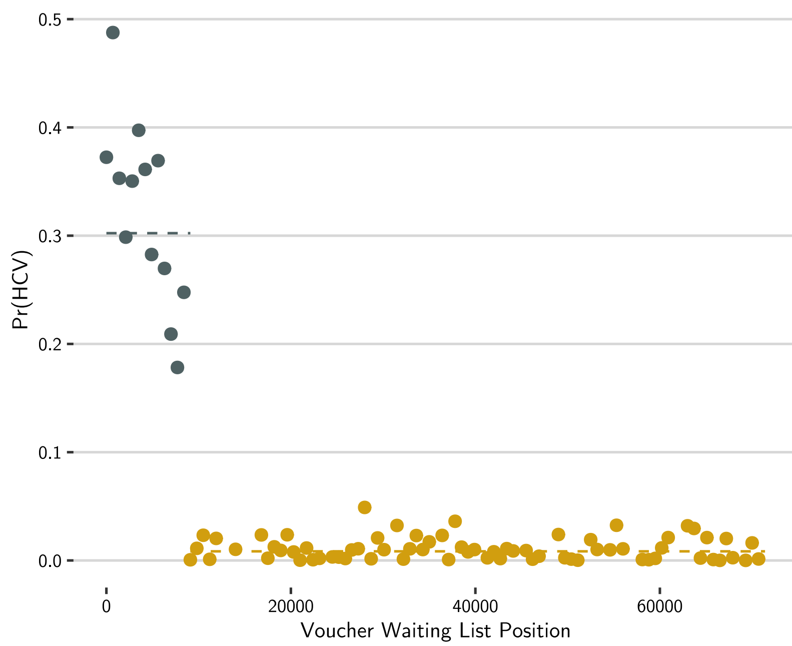
Table 8: Impacts on Children's Observed Developmental Benchmarks by Gender

	Boys		Girls	
	Control Mean (1)	Full Sample (2)	Control Mean (3)	Full Sample (4)
Panel A: Reduced Form				
Not Emerging or Demonstrating				
Top HCV	0.139	−0.041***	0.105	−0.009
	(0.325)	(0.012)	(0.287)	(0.011)
Top PH	0.136	−0.005	0.106	−0.010
	(0.322)	(0.009)	(0.288)	(0.008)
Emerging				
Top HCV	0.470	0.059***	0.474	0.005
	(0.472)	(0.019)	(0.470)	(0.018)
Top PH	0.480	−0.011	0.474	−0.000
	(0.472)	(0.014)	(0.470)	(0.014)
Demonstrating				
Top HCV	0.282	0.014	0.340	−0.004
	(0.425)	(0.018)	(0.449)	(0.018)
Top PH	0.279	0.016	0.336	0.013
	(0.422)	(0.013)	(0.448)	(0.013)
Panel B: IV (LATE)				
Not Emerging or Demonstrating				
HCV	0.139	−0.147***	0.105	−0.039
	(0.325)	(0.044)	(0.287)	(0.036)
PH	0.136	−0.032	0.106	−0.068
	(0.322)	(0.056)	(0.288)	(0.053)
Emerging				
HCV	0.470	0.190***	0.474	0.015
	(0.472)	(0.069)	(0.470)	(0.060)
PH	0.480	−0.066	0.474	0.000
	(0.472)	(0.084)	(0.470)	(0.087)
Demonstrating				
HCV	0.282	0.066	0.340	−0.002
	(0.425)	(0.063)	(0.449)	(0.058)
PH	0.279	0.095	0.336	0.081
	(0.422)	(0.077)	(0.448)	(0.084)
Controls	N	Y	N	Y
N	4,810	5,506	4,887	5,623

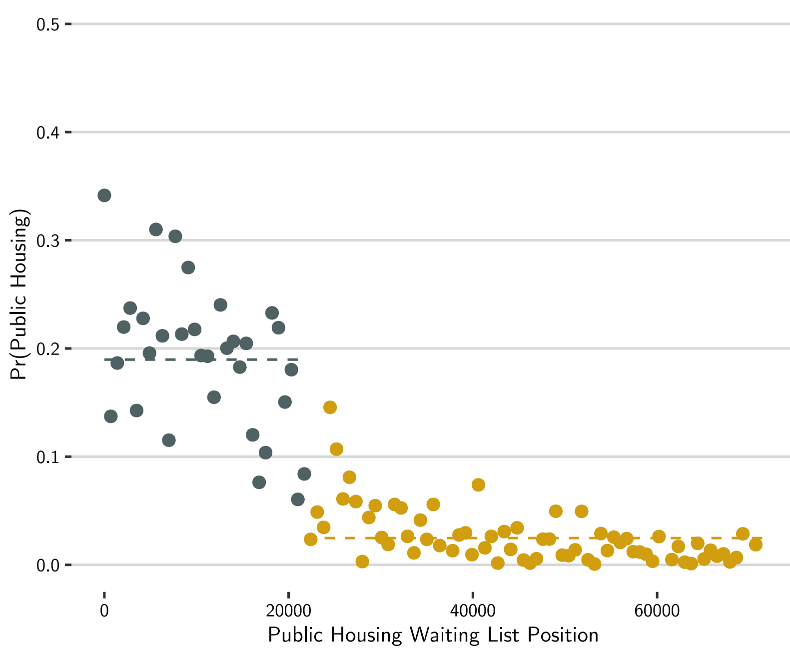
Notes: :This table reports the effects of wait-list position (reduced form) and housing assistance (IV) on the same outcomes considered in Table 6, separately by child gender. Column 1 reports the mean among those randomized to the bottom of the waiting list ("Control Mean"). Column 2 reports the ITT estimates from estimating equation 4.1 in Panel A and LATE estimates in Panel B. "HCV" reports the coefficient on voucher wait-list position (reduced form) or voucher lease-up (IV). "PH" reports the coefficient on public housing wait-list position (reduced form) or public housing lease-up (IV). Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Figure 1: Housing Assistance Receipt by Waiting-List Positions

(a) Housing Vouchers (HCV)



(b) Public Housing (PH)

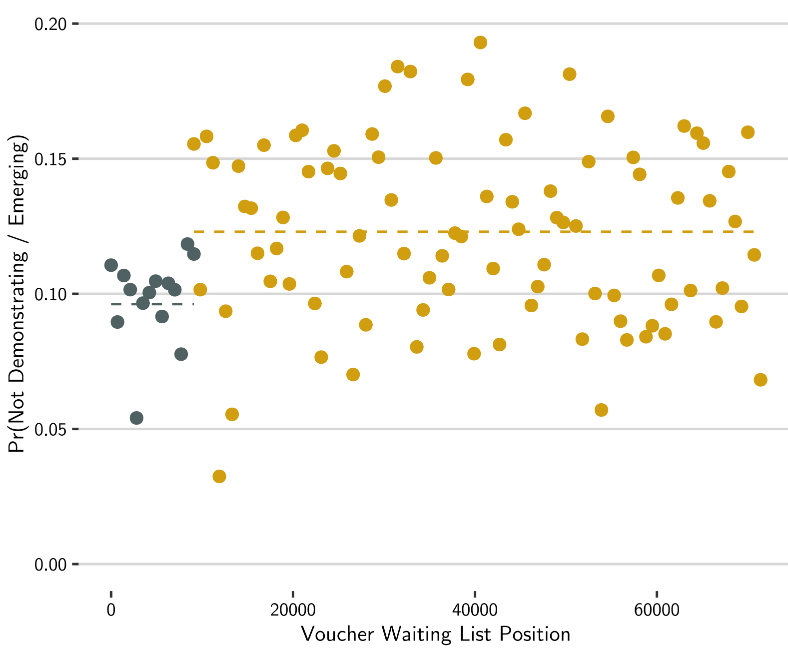


Notes: This figure plots the probability of leasing-up with a voucher by voucher waiting list position (panel a), and the probability of leasing-up in public housing by public housing waiting list position (panel b). Each dot is a 700 positions bin of the analytical sample.

Figure 2: Child Development Outcomes by Voucher Lottery Position

(a) Kindergarten Readiness

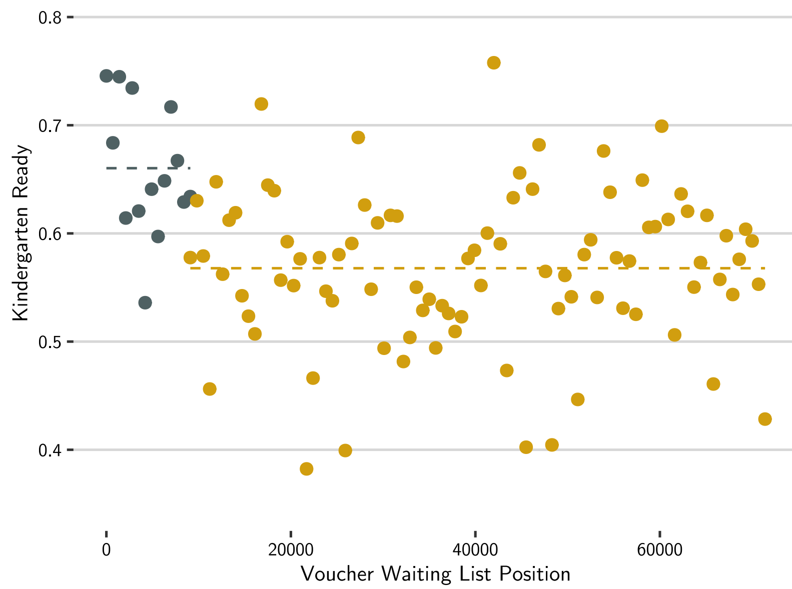
(b) Not Emerging or Demonstrating Skills



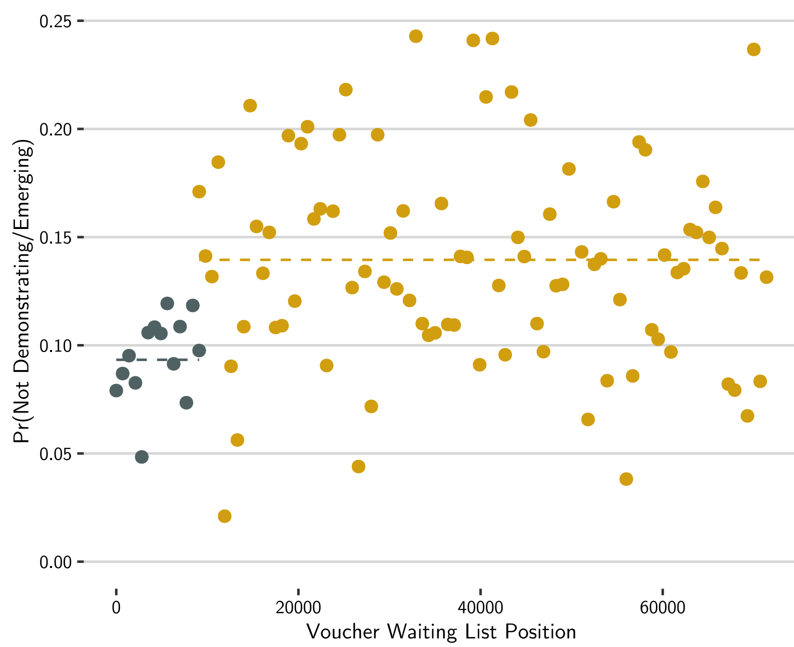
Notes: This figure plots mean child development outcomes by bins of the randomized initial positions on the voucher list for the sample. In panel A the outcome is kindergarten readiness. In panel B the outcome is whether the child is determined to be "Not Emerging or Demonstrating" in the ECHOS assessment. Each dot represents 700 slots of voucher positions. The dashed blue line is the average of the y-variable for children with positions less than 9,200 on the voucher list, the dashed gold line is the average y-variable for children in positions greater than 9,200.

Figure 3: Child Development Outcomes by Voucher Lottery Position (Boys)

(a) Kindergarten Readiness



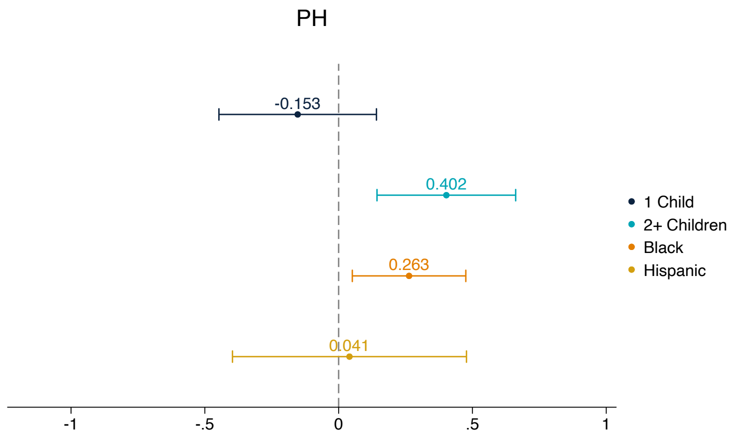
(b) Not Emerging or Demonstrating Skills



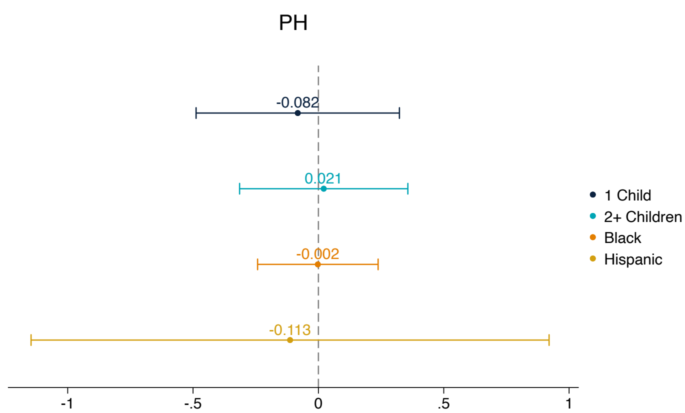
Notes: This figure plots mean child development outcomes by bins of the randomized initial positions on the voucher list for the sample of boys. Each dot represents 700 slots of voucher positions. The dashed blue line is the average of the y-variable for children with positions less than 9,200 on the voucher list, the dashed gold line is the average y-variable for children in positions greater than 9,200.

Figure 4: Heterogeneity by Demographics

(a) Impacts of Vouchers



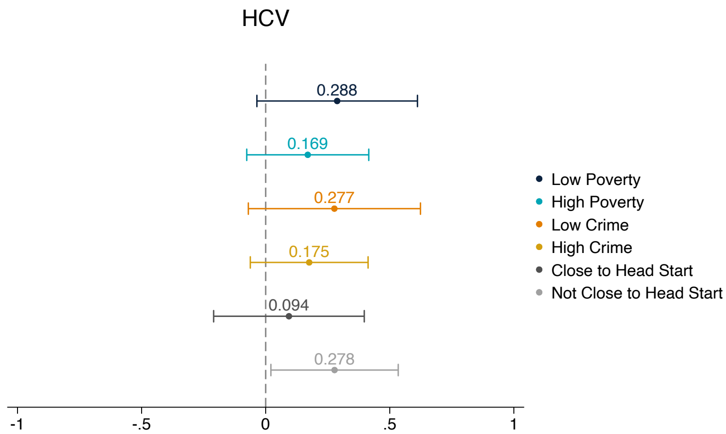
(b) Impacts of Public Housing



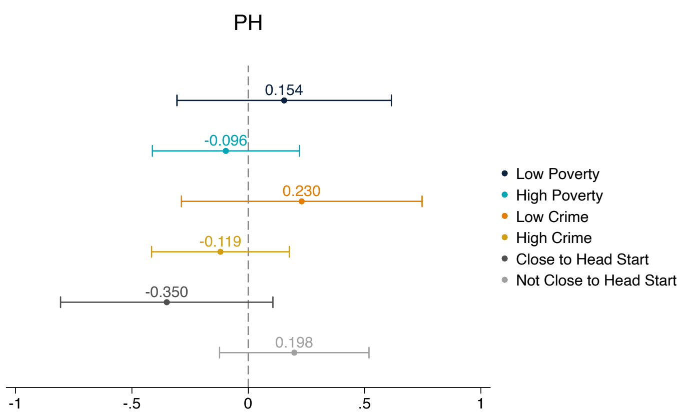
Notes: This figure plots the IV estimates for the effects of housing vouchers ("HCV") and public housing ("PH") on children's standardized early literacy skills for several subgroups based on baseline characteristics. Whiskers are 95% confidence intervals.

Figure 5: Heterogeneity by Neighborhood Environment

(a) Impacts of Vouchers

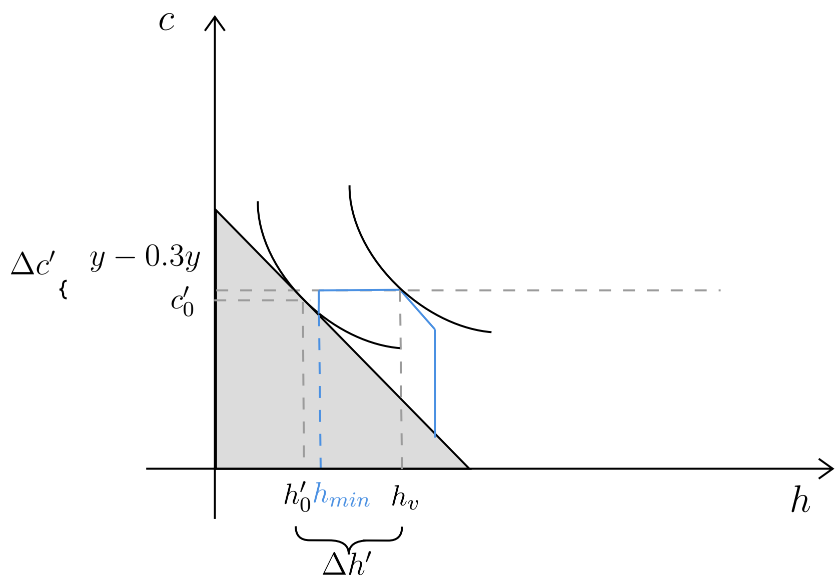


(b) Impacts of Public Housing

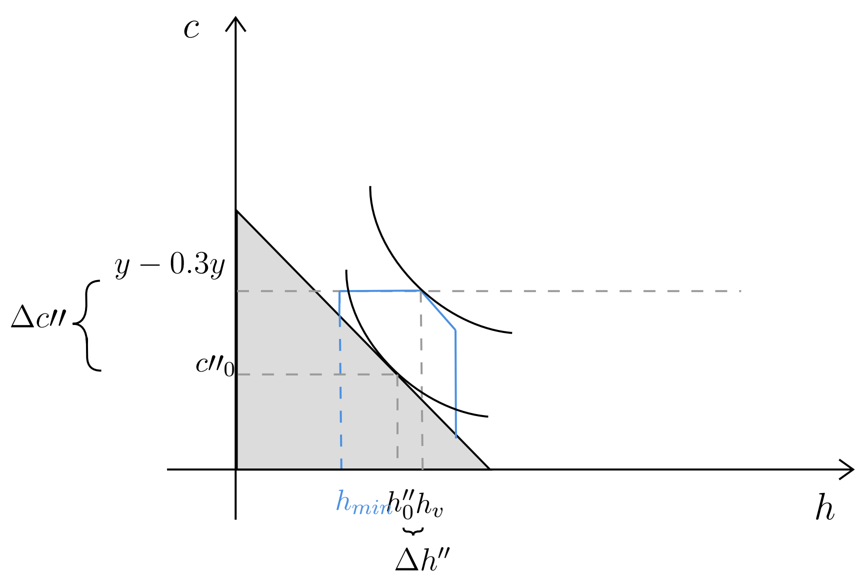


Notes: This figure plots the IV estimates for the effects of housing vouchers ("HCV") and public housing ("PH") on children's standardized early literacy skills for several subgroups of baseline neighborhood conditions. Whiskers are 95% confidence intervals.

Figure 6: Theoretical Impacts of Voucher on Consumption



(a) Low Baseline Housing Consumption

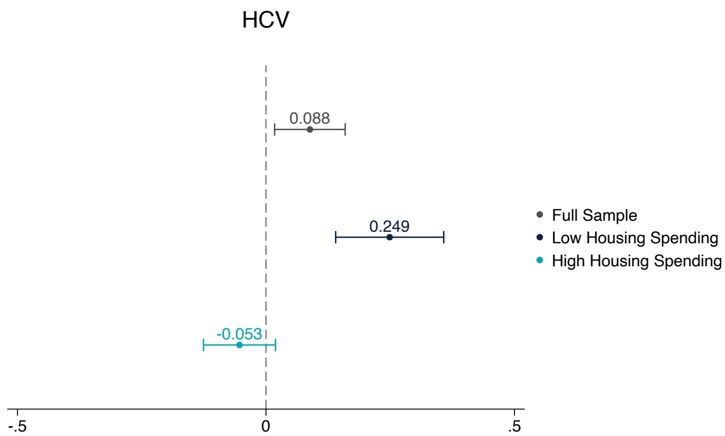


(b) High Baseline Housing Consumption

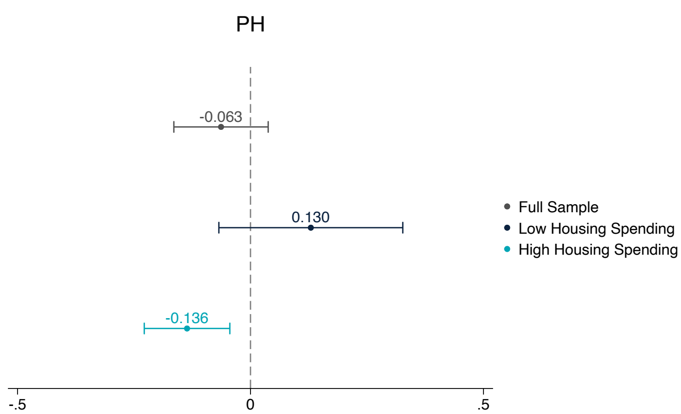
Notes: This figure characterizes the theoretical consumption trade-off facing a household with and without a housing voucher. The grey shaded area is the budget region. The top panel illustrates the changes in housing and non-housing consumption for a household with low spending on housing before voucher receipt. The bottom illustrates the changes in non-housing spending for a household with high initial spending on housing.

Figure 7: Heterogeneity in Housing Consumption Impacts

(a) Impacts of Vouchers



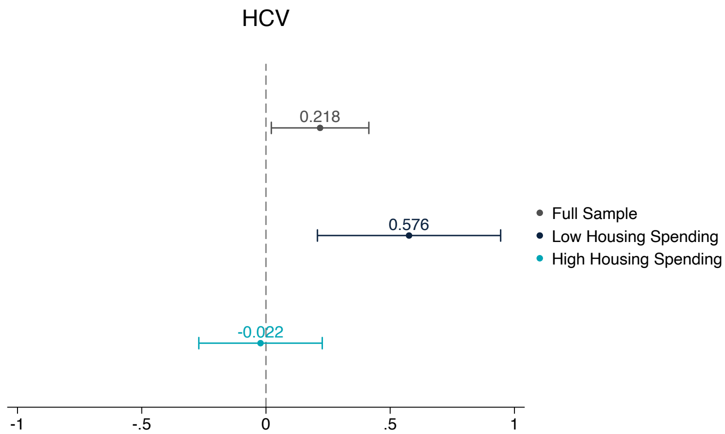
(b) Impacts of Public Housing



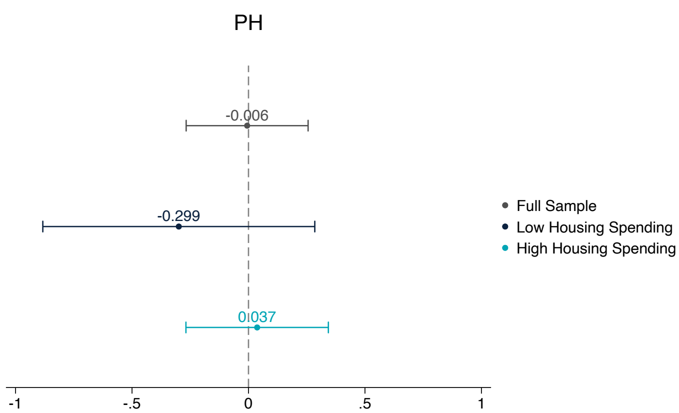
Notes: This figure plots the IV estimates for the effects of housing vouchers ("HCV") and public housing ("PH") on hedonic housing consumption (described in 3.6) separately for households who spend relatively less on housing at baseline (hedonic rent less than FMR), and those that spend more relatively more on housing at baseline (hedonic rent greater than FMR). Whiskers are 95% confidence intervals

Figure 8: Heterogeneity in Child Development Impacts by Baseline Housing Spending

(a) Impacts of Vouchers



(b) Impacts of Public Housing



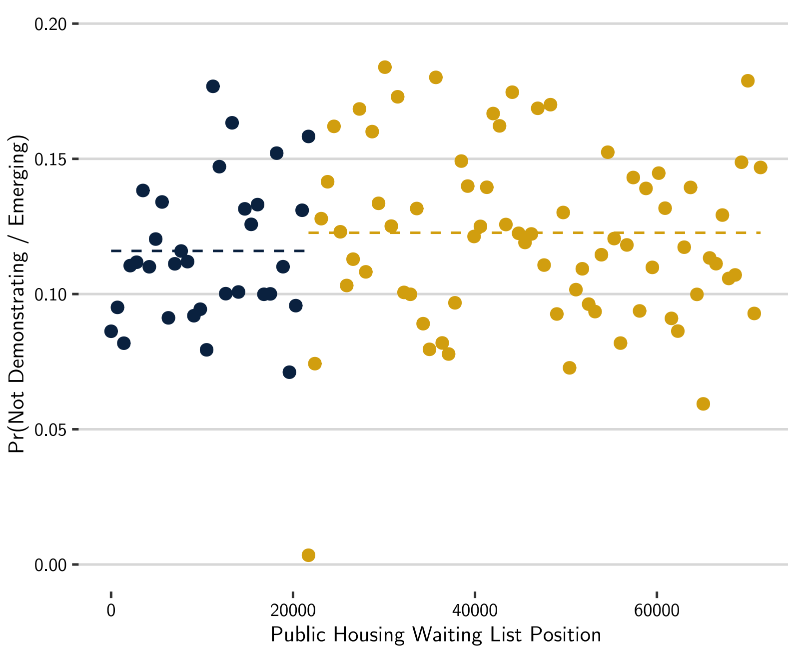
Notes: This figure plots the IV estimates for the effects of housing vouchers ("HCV") and public housing ("PH") on children's standardized early literacy skills for households who spend relatively less on housing at baseline (hedonic rent less than FMR), and those that spend more relatively more on housing at baseline (hedonic rent greater than FMR). Whiskers are 95% confidence intervals

Appendix

Appendix Figure A.1: Child Development Outcomes by Public Housing Lottery Position

(a) Kindergarten Readiness

(b) Not Emerging or Demonstrating Skills



Notes: This figure plots mean child development outcomes by bins of the randomized initial positions on the public housing list for the sample. In panel A the outcome is kindergarten readiness. In panel B the outcome is whether the child is determined to be "Not Emerging or Demonstrating" in the ECHOS assessment. Each dot represents 700 slots of public housing positions. The dashed blue line is the average of the y-variable for children below position 22,000 on the public housing list, the dashed gold line is the average y-variable for children in positions above 22,000.

Appendix Table A.1: Impacts of Vouchers on Early Literacy and Kindergarten Readiness for Early Positions v. Later Positions

	HCV (1)	Treatment Effect (2)
Panel A: Reduced Form		
Kindergarten Readiness		
Early HCV Position	0.463***	0.137***
	(0.062)	(0.045)
Later HCV Position	0.293***	0.034**
	(0.015)	(0.014)
Std. Reading Score		
Early HCV Position	0.463***	0.190*
	(0.062)	(0.100)
Later HCV Position	0.293***	0.058*
	(0.015)	(0.030)
Panel B: IV (LATE)		
Kindergarten Readiness		
Early HCV		0.301***
		(0.113)
Late HCV		0.119**
		(0.051)
Std. Reading Score		
Early HCV		0.421*
		(0.231)
Late HCV		0.199*
		(0.106)
Controls	Y	Y
N	10,863	10,863

Notes: This table examines impacts of voucher waiting list position on voucher take-up (Column 1), as well as the reduced-form impact of voucher waiting list position on kindergarten readiness and early literacy skills (Column 2). In particular, this table compares the effect of having a position near the very top ("Early HCV Position") to the effect of having a position within the top, but outside the very top ("Late HCV Position"). Panel A reports the reduced-form. Panel B reports IV effects, where we separately estimate the effects of getting a Voucher earlier and later. Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table A.2: Robustness to Top Voucher Cutoff

	< 8600	<8800	<9000	**<9200**	<9400	<9600	<9800
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Reduced Form							
Kindergarten Readiness	0.041***	0.037***	0.039***	**0.040*** **	0.040***	0.038***	0.041***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Std Reading	0.063**	0.055*	0.062**	**0.066** **	0.063**	0.063**	0.069**
	(0.030)	(0.030)	(0.029)	(0.029)	(0.029)	(0.029)	(0.028)
Not Emerging/ Demonstrating	−0.025***	−0.025***	−0.025***	**-0.024*** **	−0.024***	−0.022***	−0.022***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Emerging	0.035**	0.035***	0.034**	**0.031** **	0.032**	0.033**	0.031**
	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Demonstrating	0.002	−0.001	0.002	**0.004**	0.004	0.002	0.001
	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)
Panel B: IV (LATE)							
Kindergarten Readiness	0.132***	0.118**	0.130***	**0.135*** **	0.138***	0.134***	0.147***
	(0.047)	(0.047)	(0.047)	(0.048)	(0.049)	(0.049)	(0.050)
Std Reading	0.197**	0.173*	0.199**	**0.218** **	0.211**	0.215**	0.242**
	(0.098)	(0.098)	(0.099)	(0.100)	(0.102)	(0.103)	(0.105)
Not Emerging/ Demonstrating	−0.085***	−0.085***	−0.089***	**-0.088*** **	−0.088***	−0.085***	−0.085***
	(0.027)	(0.027)	(0.027)	(0.028)	(0.029)	(0.029)	(0.029)
Emerging	0.103**	0.106**	0.104**	**0.097** **	0.103**	0.107**	0.105**
	(0.044)	(0.044)	(0.045)	(0.045)	(0.046)	(0.047)	(0.047)
Demonstrating	0.021	0.011	0.020	**0.029**	0.027	0.022	0.020
	(0.042)	(0.041)	(0.042)	(0.043)	(0.043)	(0.044)	(0.045)

Notes: This table reports our main reduced-form (Panel A) and IV (Panel B) results for all child development outcomes under a variety of alternative "top" definitions for voucher position. Our main estimates are in **bold** in column (4). We then vary the cutoff by 200 spots above and below our selected cutoff. Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table A.3: Robustness: Including School Fixed Effects (FAIR Outcomes)

	Control Mean (1)	Full Sample (2)	No Assistance Alternative (3)
Panel A: Reduced Form			
Kindergarten Readiness			
Top HCV	0.608	0.035**	0.038**
	(0.488)	(0.014)	(0.018)
Top PH	0.612	−0.000	−0.001
	(0.487)	(0.010)	(0.011)
Std. Reading Score			
Top HCV	−0.005	0.059**	0.071**
	(1.004)	(0.030)	(0.035)
Top PH	0.004	−0.004	−0.002
	(0.999)	(0.022)	(0.024)
Panel B: IV (LATE)			
Kindergarten Readiness			
HCV	0.608	0.112**	0.118**
	(0.488)	(0.047)	(0.056)
PH	0.612	−0.001	−0.004
	(0.487)	(0.064)	(0.065)
Std. Reading Score			
HCV	−0.005	0.184*	0.222**
	(1.004)	(0.097)	(0.113)
PH	0.004	−0.020	−0.011
	(0.999)	(0.134)	(0.135)
Controls	N	Y	Y
School FE	Y	Y	Y
N	9,456	10,546	9,163

Notes: This table repeats the analysis in Table 5, but includes school fixed effects This table reports the effects of wait-list position (reduced form) and housing assistance (IV) on Kindergarten Readiness and Standardized Early Literacy scores. Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table A.4: Robustness: Including School Fixed Effects (ECHOS Outcomes)

	Control Mean (1)	Full Sample (2)	No Assistance Alternative (3)
Panel A: Reduced Form			
Not Emerging or Demonstrating			
Top HCV	0.122	−0.024***	−0.030***
	(0.307)	(0.008)	(0.010)
Top PH	0.121	−0.003	−0.005
	(0.305)	(0.006)	(0.007)
Emerging			
Top HCV	0.472	0.028**	0.065***
	(0.471)	(0.013)	(0.016)
Top PH	0.477	−0.006	0.009
	(0.471)	(0.010)	(0.010)
Demonstrating			
Top HCV	0.312	0.009	−0.007
	(0.438)	(0.012)	(0.015)
Top PH	0.308	0.010	0.001
	(0.437)	(0.009)	(0.010)
Panel B: IV (LATE)			
Not Emerging or Demonstrating			
HCV	0.122	−0.079***	−0.093***
	(0.307)	(0.027)	(0.032)
PH	0.121	−0.021	−0.028
	(0.305)	(0.039)	(0.040)
Emerging			
HCV	0.472	0.084*	0.203***
	(0.471)	(0.044)	(0.051)
PH	0.477	−0.033	0.052
	(0.471)	(0.060)	(0.060)
Demonstrating			
HCV	0.312	0.037	−0.023
	(0.438)	(0.041)	(0.047)
PH	0.308	0.060	0.007
	(0.437)	(0.055)	(0.055)
Controls	N	Y	Y
School FE	Y	Y	Y
N	9,697	10,805	9,393

Notes: This table repeats the analysis in Table 6, but includes school fixed effects This table reports the effects of wait-list position (reduced form) and housing assistance (IV) on observation-based development benchmarks. Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table A.5: Robustness: Effects of Vouchers on Early Literacy & Kindergarten Readiness, Exclude "Possibly Treated" Voucher Households

	Control Mean (1)	Full Sample (2)	Excluding Near Top (3)
Panel A: Reduced Form			
Kindergarten Readiness			
Top HCV	0.608	0.040***	0.043***
	(0.488)	(0.014)	(0.014)
Std. Reading Score			
Top HCV	−0.005	0.066**	0.067**
	(1.004)	(0.029)	(0.029)
Panel B: IV (LATE)			
Kindergarten Readiness			
HCV	0.608	0.135***	0.147***
	(0.488)	(0.048)	(0.049)
Std. Reading Score			
HCV	−0.005	0.218**	0.235**
	(1.004)	(0.100)	(0.102)
Controls	N	Y	Y
N	9,456	10,863	9,513

Notes: This table repeats the analysis in Table 5, but drops children in families with voucher lottery positions such that they are "possibly treated" (i.e. that could receive voucher offers in the years following our outcome window period. This table reports the effects of wait-list position (reduced form) and housing assistance (IV) on kindergarten readiness and early literacy skills. Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Table A.6: Robustness: Effects of Vouchers on Developmental Benchmarks, Exclude "Possibly Treated" Voucher Households

	Control Mean (1)	Full Sample (2)	Excluding Near Top (3)
Panel A: Reduced Form			
Not Emerging or Demonstrating			
Top HCV	0.122	−0.024***	−0.026***
	(0.307)	(0.008)	(0.008)
Emerging			
Top HCV	0.472	0.031**	0.035***
	(0.471)	(0.013)	(0.013)
Demonstrating			
Top HCV	0.312	0.004	0.003
	(0.438)	(0.012)	(0.013)
Panel B: IV (LATE)			
Not Emerging or Demonstrating			
HCV	0.122	−0.088***	−0.096***
	(0.307)	(0.028)	(0.029)
Emerging			
HCV	0.472	0.097**	0.104**
	(0.471)	(0.045)	(0.046)
Demonstrating			
HCV	0.312	0.029	0.033
	(0.438)	(0.043)	(0.044)
Controls	N	Y	Y
N	9,697	11,131	9,755

Notes: This table repeats the analysis in Table 6, but drops children in families with voucher lottery positions such that they are "possibly treated" (i.e. that could receive voucher offers in the years following our outcome window period. This table reports the effects of wait-list position (reduced form) and housing assistance (IV) on observation-based development benchmarks. Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

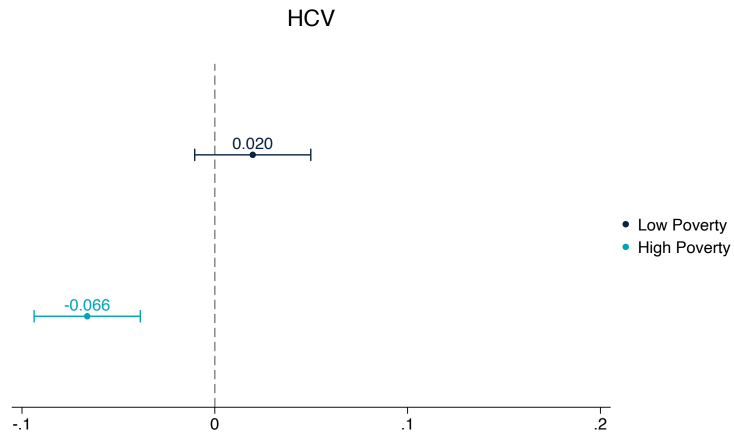
Appendix Table A.7: Robustness: Testing for Anticipation Effects on Early Literacy Skills and Kindergarten Readiness

	Control Mean (1)	Placebo Sample (2)
Reduced Form		
Kindergarten Readiness		
Near Top HCV	0.608	0.019
	(0.488)	(0.014)
Near Top PH	0.612	−0.004
	(0.487)	(0.014)
Std. Reading Score		
Near Top HCV	−0.005	0.008
	(1.004)	(0.031)
Near Top PH	0.004	0.005
	(0.999)	(0.029)
Controls	N	Y
N	9,456	7,499

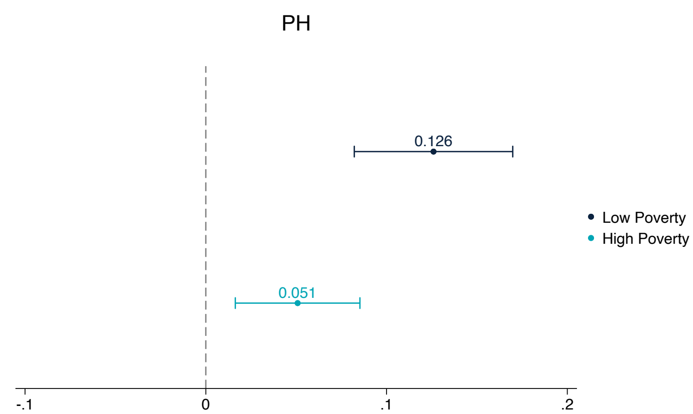
Notes: This table repeats the analysis in Table 5, but excludes households at the top of the voucher and public housing waiting lists, and instead looks at the effects being "near the top" of the the voucher list and the public housing list respectively. Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Figure A.2: Impacts on Neighborhood Poverty by Baseline Neighborhood Poverty

(a) Impacts of Vouchers



(b) Impacts of Public Housing



Notes: This figure plots the IV estimates for the effects of housing vouchers ("HCV") and public housing ("PH") on the change in neighborhood poverty rates by baseline poverty exposure. Whiskers are 95% confidence intervals

Appendix Table A.8: Correlation of Sub-groups for Heterogeneity

	1 Adult	2+ Kids	Black	Hispanic	High Pov.	Any	Gold-Seal	Head-Start	Low Housing Spend	Low Inc.
1 Adult	1	-.042	.228	-.219	.022	-.012	-.016	.005	-.101	-.095
2+ Kids	-.042	1	-.047	.048	.04	.008	.029	0	.251	.065
Black	.228	-.047	1	-.864	.093	-.064	-.097	-.042	.012	-.053
Hispanic	-.219	.048	-.864	1	-.095	.072	.088	.039	-.007	.039
High Pov.	.022	.04	.093	-.095	1	.101	.15	.231	.242	.025
Any	-.012	.008	-.064	.072	.101	1	.626	.504	.025	.01
Gold-Seal	-.016	.029	-.097	.088	.15	.626	1	.588	.026	-.008
Head-Start	.005	0	-.042	.039	.231	.504	.588	1	.038	.011
Low Housing Spend	-.101	.251	.012	-.007	.242	.025	.026	.038	1	.088
Low Inc.	-.095	.065	-.053	.039	.025	.01	-.008	.011	.088	1

Notes: This table is a correlation matrix of subgroups we consider in our heterogeneity analysis. Importantly, none of the major sub-groups are particularly correlated, suggesting that the heterogeneity in treatment effects that we find are more likely to reflect differences among the group in-question and not a mix of different groups.

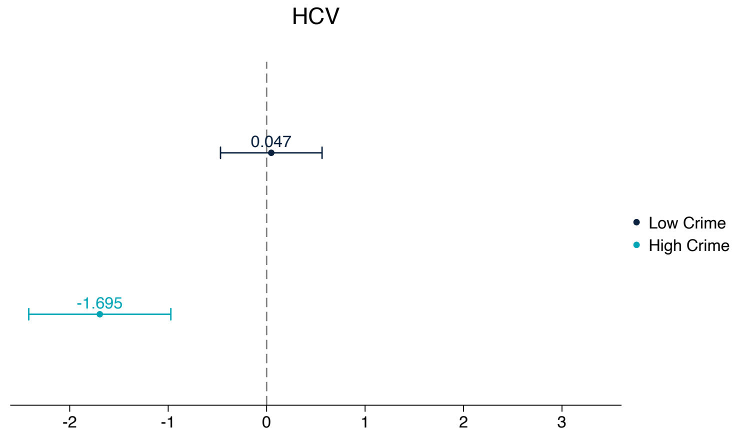
Appendix Table A.9: Effects of Vouchers on Early Literacy Skills and Kindergarten Readiness, Re-weighted to match the Race/Ethnic Composition of Jacob et al. (2015)

	Control Mean (1)	Full Sample (2)	No Assistance Alternative (3)
Panel A: Reduced Form			
Kindergarten Readiness			
Top HCV	0.608	0.042***	0.051***
	(0.488)	(0.016)	(0.019)
Top PH	0.612	0.002	0.006
	(0.487)	(0.012)	(0.013)
Std. Reading Score			
Top HCV	−0.005	0.082**	0.108***
	(1.004)	(0.033)	(0.038)
Top PH	0.004	−0.001	0.012
	(0.999)	(0.025)	(0.027)
Panel B: IV (LATE)			
Kindergarten Readiness			
HCV	0.608	0.134**	0.155***
	(0.488)	(0.052)	(0.059)
PH	0.612	0.015	0.029
	(0.487)	(0.060)	(0.061)
Std. Reading Score			
HCV	−0.005	0.253**	0.328***
	(1.004)	(0.107)	(0.119)
PH	0.004	0.003	0.056
	(0.999)	(0.124)	(0.124)
Controls	N	Y	Y
JKL Weights	N	Y	Y
N	9,456	10,814	9,407

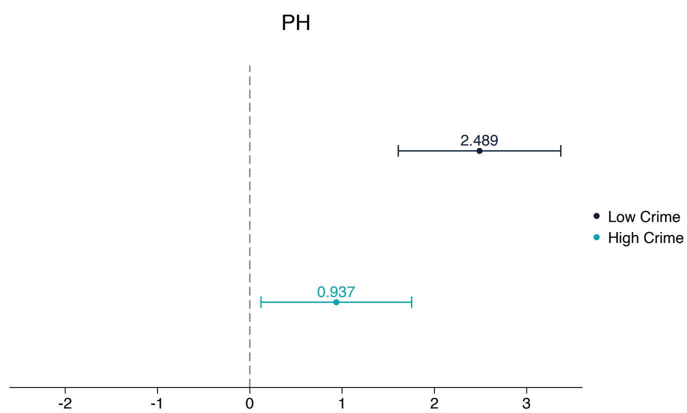
Notes: This table repeats the analysis in Table 5, but re-weights the sample to match the racial and ethnic compostion of the lottery study in Jacob et al. (2015). Robust standard errors are reported in parenthesis, clustered at the household-level. Statistical significance is denoted by: * $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$.

Appendix Figure A.3: Impacts on Neighborhood Crime by Baseline Neighborhood Crime

(a) Impacts of Vouchers

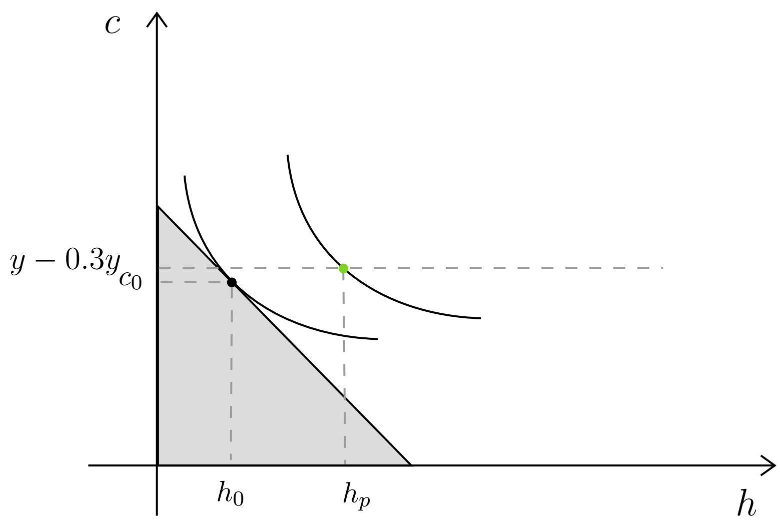


(b) Impacts of Public Housing

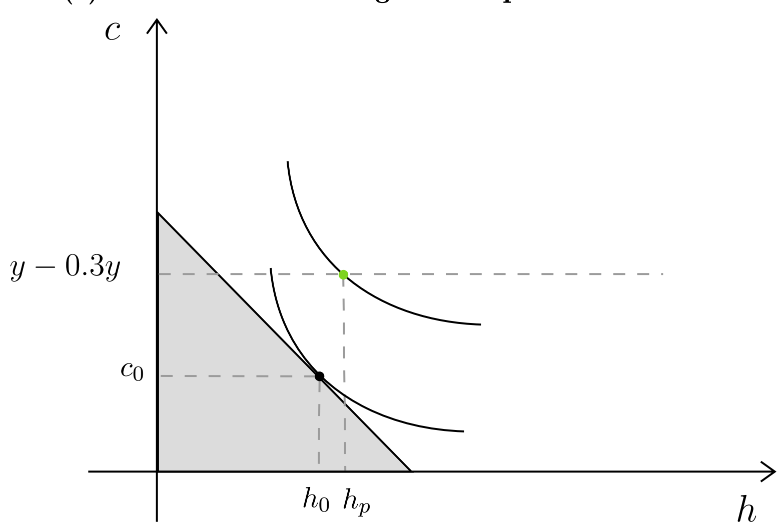


Notes: This figure plots the IV estimates for the effects of housing vouchers ("HCV") and public housing ("PH") on the change in neighborhood violent crime exposure by baseline neighborhood violent crime. Whiskers are 95% confidence intervals

Appendix Figure A.4: Theoretical Impacts of Public Housing on Consumption



(a) Low Baseline Housing Consumption

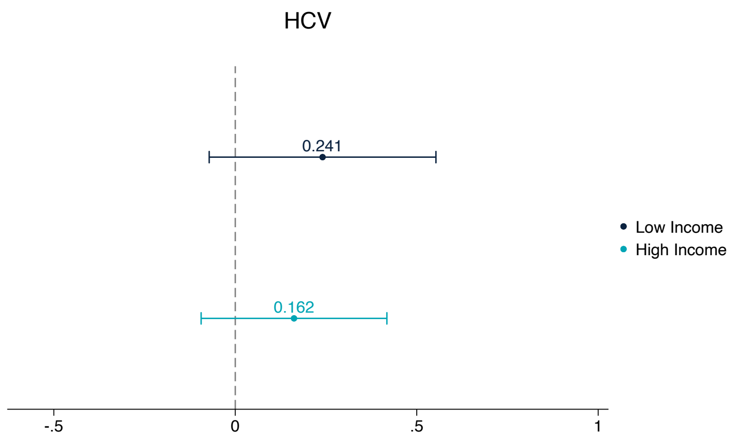


(b) High Baseline Housing Consumption

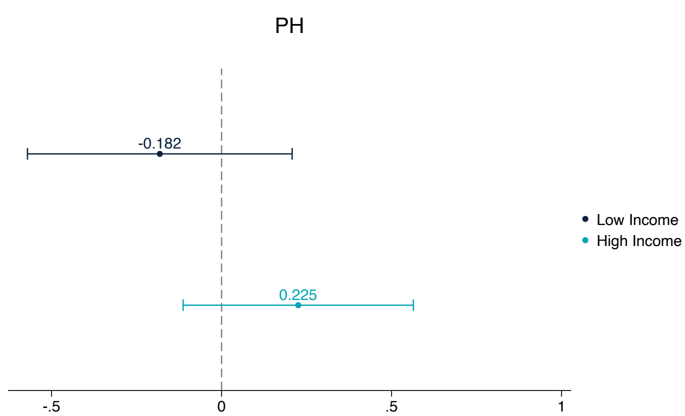
Notes: This figure characterizes the theoretical consumption trade-off facing a household with and without public housing. The grey shaded area is the budget region. The quality of public housing (h_p) is placed to be similar to the voucher maximum quality level in 6. Empirically, we find that h_p is probably quite a bit lower than what we indicate here. The top panel illustrates the changes in housing and non-housing consumption for a household with low spending on housing before public housing admission. The bottom illustrates the changes in non-housing spending for a household with high initial spending on housing.

Appendix Figure A.5: Impacts on Early Literacy by Estimated Baseline Income

(a) Impacts of Vouchers



(b) Impacts of Public Housing



Notes: This figure plots the IV estimates for the effects of housing vouchers ("HCV") and public housing ("PH") on the standardized measure of reading success separately by estimated baseline income. Whiskers are 95% confidence intervals

A Address History and Housing Data Details

We rely on two sources of information on follow-up addresses. The first is Florida voter registration records for 2012 and 2015. We link these to the household heads of our waiting list records using name and date of birth. In our sample of families with young children, we link 65 percent of household heads to a voter registration record. Our second data source of address information is Infutor data, which we also link to the household heads in our sample using name and date of birth. Similarly, we link 68 percent of household heads to a post-lottery Infutor record. Across both sources, we can link 80 percent of our household heads to a post-lottery address.

To determine which source of address information to prioritize, we evaluate whether the voter records or Infutor records do a better job of correctly assigning the address *for assisted households*, which we can validate using the housing assistance records. We find that the voter registration records are 1.5 times more likely to place the household at the correct address than the Infutor records. Therefore, we first assign a household the parcel and census tract on the voter registration file, if available, and otherwise use the parcel data and census tracts of the Infutor addressses.

B Kindergarten Readiness Data Details

The Florida Department of Education linked children listed on the the housing authority applications to all school readiness assessments for the years 2009-2013 using fuzzy matching on first name, last name, and date of birth. Among children listed on housing applications living in Miami-Dade County who were younger than four years old at the time of random assignment, we estimate that approximately 85% match to an assessment. At the time, the Florida Kindergarten Readiness Screener was comprised of two parts: the Florida Assessment for Instruction in Reading (FAIR), measuring early literacy development, and the Early Childhood Observation System (ECHOS), a classroom-embedded, observational assessment of school readiness across a broad range of cognitive and noncognitive skills.

B.1 Early Literacy Assessment

The FAIR screening is administered by schools. The "Broad Screen" evaluates progress on two dimensions of early literacy, letter naming and phonemic awareness. Performance on the screener is then used to calculate a "Probability of Reading Success" score. This score ranges from 1 to 100, and a student is judged to be "Kindergarten Ready" if they score a 67 or above. The probability of reading success scores are calibrated to reflect the likelihood of the child scoring at or above proficiency on the reading section of the Stanford Early School Achievement Test if taken at the end of the year.

We use two outcome measures from the linked FAIR screening: first, an indicator for whether the child scored as being "Kindergarten Ready," and second, we standardize the raw probability of reading success score to facilitate comparisons with other studies ("Standardized Score").

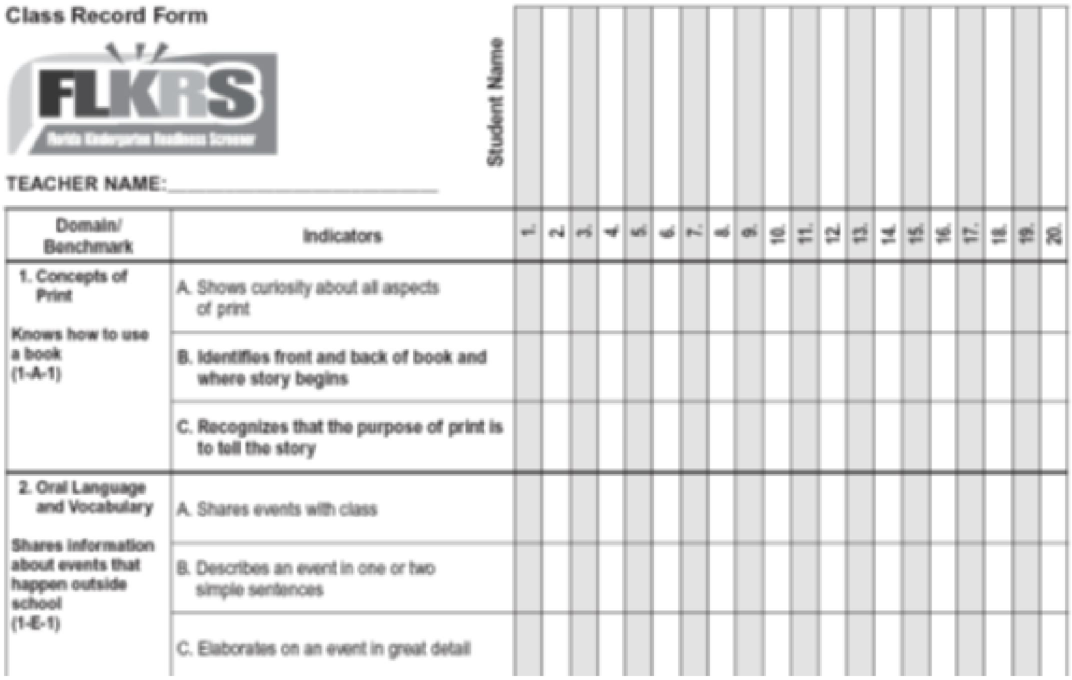
AP1	Kindergarten - Assessment Period 1 - Probability of Reading Success											
	Phonemic Awareness - Total Correct											
Letter Naming - Total Correct	Total Correct	0	1	2	3	4	5	6	7	8	9	10
	0	.09	.11	.14	.17	.20	.24	.29	.34	.39	.45	.51
	1	.12	.14	.17	.21	.25	.30	.35	.40	.46	.52	.57
	2	.15	.18	.22	.26	.31	.36	.41	.47	.53	.59	.64
	3	.19	.22	.27	.31	.37	.42	.48	.54	.60	.65	.70
	4	.23	.28	.32	.38	.43	.49	.55	.61	.66	.71	.76
	5	.28	.33	.39	.45	.50	.56	.62	.67	.72	.77	.80
	6	.34	.40	.46	.51	.57	.63	.68	.73	.77	.81	.84
	7	.41	.47	.53	.58	.64	.69	.74	.78	.82	.85	.88
	8	.48	.54	.59	.65	.70	.75	.79	.83	.86	.88	.90
	9	.55	.61	.66	.71	.76	.80	.83	.86	.89	.91	.93
	10	.62	.67	.72	.76	.80	.84	.87	.89	.91	.93	.94

Appendix Figure A.6: FAIR Assessment: Probability of Reading Success Scorecard

B.2 Observational Assessment

The ECHOS is also administered in schools within 30 days of kindergarten entry, but is an embedded assessment, intended to be administered during the course of natural classroom activities. The ECHOS evaluates progress on 19 benchmarks across seven domains: Language and Literacy, Mathematics, Social and Personal Skills, Science, Social Studies, Physical Development and Fitness, and Creative Arts. Teachers or assessment administrators use a matrix to code student progress along each item.

From the ECHOS data, we receive the overall readiness designation which falls into one of the three categories: “Demonstrating” – the child clearly demonstrated skills appropriate for beginning kindergarten; “Emerging/Progressing” – the child demonstrated some of the skills appropriate; or “Not Yet Demonstrating” – the child did not demonstrate appropriate skill development during the screening. With these data, we examine effects of housing assistance on a child’s inclusion in each of the three distinct categories.



Class Record Form

FLKRS

TEACHER NAME:____________________

Domain/ Benchmark	Indicators	Student Name 1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.
1. Concepts of Print Knows how to use a book (1-A-1)	A. Shows curiosity about all aspects of print																				
	B. Identifies front and back of book and where story begins																				
	C. Recognizes that the purpose of print is to tell the story																				
2. Oral Language and Vocabulary Shares information about events that happen outside school (1-E-1)	A. Shares events with class																				
	B. Describes an event in one or two simple sentences																				
	C. Elaborates on an event in great detail																				

Appendix Figure A.7: Example of ECHOS Assessment Matrix

Below is a list of the 19 ECHOS benchmarks, their indicators, and the assignment of children's ratings based on which indicators they demonstrate in classroom observation.

1. **Concepts of Print**

   Benchmark: Knows how to use a book

   Indicators:

   A. Shows curiosity about all aspects of print
   B. Identifies front and back of book and where story begins
   C. Recognizes that the purpose of print is to tell a story

   Ratings:

   Emerging/Progressing: A or B or C
   Demonstrating: B and C

2. **Oral Language and Vocabulary**

   Benchmark: Shares information about events that happen outside of school

   Indicators:

   A. Shares events with class
   B. Describes an event in one or two simple sentences

C. Elaborates on an event in great detail

Ratings:

Emerging/Progressing: A or B

Demonstrating: C

3. **Comprehension**

Benchmark: Retells a story or part of a story that has been read to the class

Indicators:

A. Listens attentively when the teacher reads books in class
B. Retells one part of the story accurately
C. Retells whole story or event experienced in class

Ratings:

Emerging/Progressing: A or B

Demonstrating: C

4. **Comprehension**

Benchmark: Demonstrates understanding of story elements

Indicators:

A. Answers literal questions about story elements including character, setting, and plot
B. Makes predictions based on illustrations or portions of stories
C. Answers questions (e.g., inferential, cause and effect) about stories read by teacher

Ratings:

Emerging/Progressing: A

Demonstrating: B or C

5. **Writing**

Benchmark: Demonstrates awareness of distinction between "kid's writing" and conventional writing

Indicators:

A. Rereads own scribble writing
B. Recognizes difference between scribbling/drawing pictures and conventional writing
C. Uses phonetic spellings mixed with conventional spellings when writing

Ratings:

Emerging/Progressing: A

Demonstrating: B or C

6. **Number Sense and Operations**

   Benchmark: Counts objects in a collection by creating one-to-one correspondence between each number, word, and each object

   Indicators:

   A. Counts five objects and associates last counting word with "how many"
   B. Provides correct number of objects (up to 10) upon request
   C. Creates a collection of one to 20 items by counting them out

   Ratings:

   Emerging/Progressing: A

   Demonstrating: B or C

7. **Geometry**

   Benchmark: Identifies two-dimensional shapes and their uses

   Indicators:

   A. Recognizes and names circle, square, triangle, rectangle, in size or orientation
   B. Identifies, builds, and draws various types of triangles and rectangles
   C. Describes two-dimensional shapes (e.g., by identifying and counting sides)

   Ratings:

   Emerging/Progressing: A

   Demonstrating: B or C

8. **Algebraic Thinking**

   Benchmark: Recognizes, creates, and analyzes patterns

   Indicators:

   A. Recognizes and creates repeating pattern (e.g., red, blue, red, blue)
   B. Copies patterns with blocks, cubes, colors, and shapes
   C. Recognizes and iterates the unit or core of a pattern

   Ratings:

Emerging/Progressing: A or B

Demonstrating: C

9. **Data Analysis**

   Benchmark: Analyzes data by classifying, organizing, representing, and using information to ask and answer questions

   Indicators:

   A. Sorts objects and counts number in each group
   B. Creates a chart of play choices of classmates
   C. Answers simple questions by reading a chart or graph

   Ratings:

   Emerging/Progressing: A or B

   Demonstrating: C

10. **Responsible Decision Making**

    Benchmark: Uses classroom materials purposefully, safely, and respectfully

    Indicators:

    A. Takes care of personal property
    B. Asks permission to use someone else's property
    C. Uses and stores classroom equipment and supplies

    Ratings:

    Emerging/Progressing: A or B or C

    Demonstrating: B and C

11. **Social Problem Solving**

    Benchmark: Talks to, and plays cooperatively with, other students

    Indicators:

    A. Joins play activity when invited by adults or peers
    B. Asks students to join in activity; engages in extended conversation about an event with teacher or peers
    C. Takes turns and shares; plans roles before entering dramatic play

    Ratings:

    Emerging/Progressing: A or B

Demonstrating: C

12. **Approaches to Learning**

    Benchmark: Shows eagerness and curiosity about new topics and ideas

    Indicators:

    A. Observes and comments on new topics and ideas
    B. Asks questions (e.g., How can the caterpillar live in the cocoon with no food or water)
    C. Willingly discusses a new idea with teacher or another student

    Ratings:

    Emerging/Progressing: A or B

    Demonstrating: C

13. **Scientific Inquiry**

    Benchmark: Uses the techniques of scientific inquiry, problem solving, questioning, and reasoning

    Indicators:

    A. Asks questions about objects and events
    B. Observes an object (e.g., flower, rock) and names its properties
    C. Conducts experiments and explorations during active play

    Ratings:

    Emerging/Progressing: A or B

    Demonstrating: C

14. **Production, Distribution, Consumption**

    Benchmark: Explores the kinds of work that people do and how that work benefits family and community

    Indicators:

    A. Identifies different jobs that people (community helpers) do
    B. Role plays different jobs with costumes/hats
    C. Draws picture of, or writes about, people who provide services

    Ratings:

    Emerging/Progressing: A or B

Demonstrating: C

15. **Civic Ideals and Participation**

    Benchmark: Identifies the need for rules and authority figures and the consequences of breaking the rules

    Indicators:

    A. Identifies classroom rules
    B. Takes turns and shares responsibility for classroom chores
    C. Discusses how school rules and consequences are for the safety of all students

    Ratings:

    Emerging/Progressing: A or B or C

    Demonstrating: B and C

16. **Fitness**

    Benchmark: Engages voluntarily in large-muscle activity

    Indicators:

    A. Runs while on playground
    B. Plays on climber for long periods with repetition of motor movements
    C. Incorporates jumping, skipping, and throwing in play

    Ratings:

    Emerging/Progressing: A or B

    Demonstrating: C

17. **Fine Motor Skills**

    Benchmark: Demonstrates increasing ability to use hands and fingers to perform tasks

    Indicators:

    A. Uses some computer keys accurately
    B. Uses scissors to cut paper unassisted
    C. Uses tools such as a stapler, tape, scissors, or markers

    Ratings:

    Emerging/Progressing: A

    Demonstrating: B or C

18. **Dance**

    Benchmark: Creates movement that corresponds to different types of music

    Indicators:

    A. Claps or marches to beat of music
    B. Moves body to indicate different musical beats, tempos, and dynamics (e.g., loudness and softness)
    C. Responds through purposeful movement (e.g., swaying, skipping, dramatic play) to different types of music

    Ratings:

    Emerging/Progressing: A or B

    Demonstrating: C

19. **Visual Arts**

    Benchmark: Applies media, techniques, and processes to create original art

    Indicators:

    A. Mixes colors and media in creations
    B. Selects purposefully different techniques for different projects
    C. Plans and creates original art with varying media, processes, and techniques

    Ratings:

    Emerging/Progressing: A

    Demonstrating: B or C