Housing and Human Capital: Condominiums in Ethiopia*

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Abstract

Rapid urbanization has led to an urgent need for new housing in cities throughout the developing world. We use randomized evidence from the largest expansion of public housing in Africa to estimate the effects of housing on children's human capital. Children in households winning a random lottery for the ownership of a condominium unit in urban Ethiopia experience large gains in educational enrollment (4.5-11%), secondary school completion rates (10.5%), tertiary attendance rates (16%), and in measures of cognitive and socioemotional development. Heads of winning households experience an 8p.p. increase in formal sector employment rates, which increases household income. To unpack mechanisms, we use instruments derived from spatial variation in randomly assigned condominium locations and show that treatment effects are concentrated amongst households that own and occupy the unit that they win. A structural model allows us to characterize selection into condominium occupation, ruling out that policy impacts can be explained through a wealth effect alone. Our results suggest that effects of neighborhood residence are an important channel through which housing policy can improve children's outcomes and that in-situ development or an allocation mechanism accounting for household residential location would increase policy impacts.

JEL Classifications: O10, O12, O18, D04, J24, I25, I38, R21, R23, R31
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1 Introduction

Modern urbanization is concentrated in low- and middle-income countries (LMICs): in the past two decades, they have been urbanizing 4-8 times faster than North America and Europe (UN-HABITAT, 2022). This is particularly true in Sub-Saharan Africa, where structural transformation away from agriculture has rapidly increased the shares of populations living in urban settings. By 2050, 1.5 billion Africans will live in cities, nearly triple the number of urban Africans today (Haas et al., 2023). As urban populations grow, so too does demand for all types of urban infrastructure, foremost amongst which is housing. But housing construction has struggled to keep up with rapid city growth, leaving tens of millions of urban residents living in slums and informal housing (Marx et al., 2013; Laros and Jones, 2014).

Governments across the globe have responded to this housing crisis with large investments in public housing, often located in the peripheries of major cities. While these investments will play a major role in determining the shape and function of developing cities, the real allure of these programs lies in their potential to provide a stable foundation for families who otherwise would have been living in low-quality, "slum" housing.[1] However, evaluations of housing lotteries and rental subsidies in low-income settings generally conclude that they fail to be transformative, with null or negative impacts on most household-level economic outcomes, (Galiani et al., 2017; Barnhardt et al., 2017; Franklin, 2020b; Rojas Ampuero and Carrera, 2022; Belchior et al., 2023) echoing findings from the United States and Europe (Kling et al., 2007; Van Dijk, 2019).

How could policies that address a need as fundamental as housing be so inconsequential? One potential explanation is that by focusing on adults, much of the previous literature misses policy impacts on the population that has been shown to be most sensitive to changes in home quality and neighborhood of residence: children (Chetty et al., 2016; Chyn, 2018;

[1] We use the term slum in a manner consistent with the UN-HABITAT definition (UN-HABITAT, 2002). Households are said to live in a slum if their residence lacks one or more of the following five elements: 1) access to adequate drinking water; 2) access to adequate sanitation; 3) housing with adequate space; 4) housing with adequate structure to protect against climatic conditions; 5) secured tenure.

Kumar, 2020; Rojas Ampuero and Carrera, 2022; Camacho et al., 2022). Particularly in LMICs, where administrative data is scarce, the impacts of housing policy on children, and long-run policy impacts more generally, remain understudied.

An alternative proposed in the literature is that decreases in social cohesion, labor market access, and public service quality associated with relocation to far-flung neighborhoods, which depress policy adoption, ultimately outweigh improvements in home quality. Testing this hypothesis is confounded by policy environments that leave households with large choice sets, even conditional on a randomized program offer: households first choose whether to take up a program, then choose the neighborhood in which to live (Heckman and Pinto, 2018). In cases where policies involve home ownership, as opposed to a rental subsidy, households further choose whether to occupy, rent out, or sell their unit. This sequence of endogenous choices implies that the typical reduced form, intent-to-treat analysis employed in the literature may disguise heterogeneity across "hidden treatments" (Rothstein and Von Wachter, 2017) that depend on the full set of household choices.[2] Disentangling this heterogeneity is essential for understanding mechanisms and estimating policy counterfactuals. We provide evidence supporting both of these explanations.

In this paper, we use a natural experiment associated with the largest expansion of public housing on the African continent to answer two questions: (1) How do shocks to neighborhoods of residence and parental wealth impact the human capital development of children? (2) What are the relative contributions of changes in neighborhood versus changes in wealth? Our project combines new survey data, matched administrative data, reduced form and policy-derived instrumental variable impact analysis, and a structural selection model to understand the policy's medium-run impact on children and families. Through a partnership with the Policy Studies Institute in Ethiopia (PSI) and the Addis Ababa Housing Development Agency (AAHDA), we conducted an extensive household survey with 2,987 households, drawn from the universe of applicants for subsidized condominium units

[2]In our setting, households can own and occupy, rent out, or sell their unit. The causal effects of these outcomes are pooled in ITT analysis.

in urban Addis Ababa, Ethiopia. We combine our household surveys with supplementary data on wages, firms, neighborhood amenities, and administrative budgets. With these data, we are able to study a battery of outcomes typically unavailable to researchers relying on administrative data alone (Chetty et al., 2016; Chyn, 2018).

Since its inception in 2005, the policy has been massively oversubscribed; an estimated 50% of all households in Addis Ababa have registered for the program, with more than 900,000 applications to date. These applicants were all urban residents at the time of their application, generally living in low-quality homes near the city center. Through 2023, approximately 210,000 units were completed and transferred to residents via random lottery in 14 lottery rounds. Lottery winning households have the opportunity to purchase a subsidized unit, paired with a low-interest mortgage through the city administration, provided that they are able to make a 20% down payment upfront. These condominiums are spread throughout the city; the majority are located 8-12 kilometers from the city center, while others are located in Addis Ababa's urban core.

Our analysis relies on the lottery mechanism employed by the AAHDA to assign subsidized condominium units to applicants. The lotteries in our study are for home *ownership*, not rental, which distinguishes it from most policies studied in high-income settings (Van Dijk, 2019; Chyn, 2018; Chetty et al., 2016; Pinto, 2021). This common feature of housing policy in lower-income settings expands household decision sets – they can occupy, rent, or sell their unit (Barnhardt et al., 2017; Kumar, 2020; Belchior et al., 2023). We compare children in lottery winning households to those in similar households that remain on the waitlist for a condominium unit. Critically, the location of the winning households' units and the lottery round in which they win are exogenous, allowing us to use spatial and temporal variation to disentangle impacts and understand mechanisms.[3]

We show that nearly all winning households purchase the unit that they won. Nearly perfect take-up, conditional on winning, is due to the substantial subsidy associated with

[3]While households can choose the number of bedrooms in their unit, the lottery round in which they win and the unit's location are random. This policy approximates "double randomization" (Graham, 2018), whereby households are randomly grouped and randomly assigned to a neighborhood.

winning a unit (Franklin, 2020b). In our sample, 82% of winning households still own the unit that they won an average of 8 years after winning. However, many winning households chose not to move into their unit: 35% rent out their unit, 17% sell (often before the 5-year embargo had elapsed, suggesting it was not enforced), and a small share either leave the unit unoccupied or allow it to be used rent-free by friends and family. The remaining 40% of the winning households own and occupy the unit they won. We expect treatment effects to vary with this decision: only households that move into condominiums will experience the change in housing quality and neighborhood characteristics attributable to the policy, but all winning households experience an increase in wealth via a government-subsidized asset. Consequently, our reduced form estimates of the average treatment effects of winning a lottery pool impacts driven by direct exposure to condominiums and their associated neighborhoods with impacts due to increases in familial wealth – a "neighborhood" effect and a wealth effect. To separate treatment channels, we develop a structural model adapted from the policy evaluation literature (Kline and Walters, 2016; Mountjoy, 2019) to account for the fact that, conditional on winning, households make an endogenous choice of whether to occupy the unit that they win.

An average of 8 years after their lottery, winning households live in better neighborhoods in terms of public infrastructure and in higher quality homes. However, these neighborhoods are farther from the city center, relatives, and close friends. This result is consistent with previous work that highlights the potential for housing policy to disrupt social networks (Barnhardt et al., 2017; Harding et al., 2023; Rojas Ampuero and Carrera, 2022). [4]

We next show that winning a condominium lottery meaningfully improves child outcomes across a range of measures associated with children's human capital: school enrollment, educational attainment, cognitive skills, aspirations, and socioemotional development. The policy increases active educational enrollment for children of winning households by 4.5-11%, secondary school completion rates by 10.5%, and post-secondary attendance rates by 16%.

[4] However, we find no evidence of thinner social networks for lottery winners in measures of neighborhood social connectivity and trust, which may be explained by measurement error in social connectivity or the capacity of social networks to develop in new sites.

Increases in attendance rates are greater for older children, for whom school attendance is no longer compulsory, and are increasing in years of childhood exposure to the policy. These impacts on educational attainment are greater than many school expansion programs, Head Start in the United States (Bailey et al., 2021), and are about half as large as some of the most generous scholarship programs (Duflo et al., 2021). Despite large increases in educational attainment, we find no evidence that children of winning households are attending schools of differential quality.

In the sample of 6-17 year old children that we interviewed directly, we see substantial gains in measures of cognition and aspirations an average of eight years post-lottery. Specifically, we see that children in winning households score significantly better on Raven's matrix tests and complete a numerical Stroop exercise faster, and more accurately. Children in lottery winning households are additionally more likely to aspire to an advanced degree or an occupation that requires an advanced degree, are more confident in their academic performance, are more optimistic about their future, and more satisfied with the neighborhood in which they live. Finally, we find small improvements in socioemotional development for male children as measured by the Strengths and Difficulties Questionnaire (SDQ) asked about children and administered to children's parents. These results highlight policy impacts that may be missed when looking only at traditional economic outcomes.

Previous research on this policy has found that it had limited short-term impacts on adult economic outcomes (Franklin, 2020b; Andersen et al., 2022) despite increases in household wealth. At the household-level, we consider many of the same outcomes an average of eight years after the lottery. While increases in household wealth and job transitions rates are similar to those found in Franklin (2020b), we find that lottery winning households have higher incomes, driven by heads of winning households being eight percentage points more likely to be formally employed. This increase in formal sector employment rates is caused by household heads leaving casual employment, not by changes in overall rates of employment. We show that the formalization and household impacts documented in our survey are increasing in years since winning the lottery, implying that the policy's impacts on these outcomes may only accrue over longer time horizons. This implies that short-term evaluations may miss policy-induced changes in household welfare. This paper focuses on the potential externality to children and outcomes in the medium-run, neither of which have been previously studied in this setting.

Our results for children may be unsurprising if they simply represent a wealth effect: winning a condominium bequeaths households with a valuable, subsidized asset, dramatically increasing familial wealth. Understanding the extent to which our results can be explained solely through changes in parental wealth motivates two empirical approaches that move beyond the intent-to-treat effects estimated in our reduced form analysis.

To separate mechanisms – a wealth effect due to a randomly allocated subsidized asset versus an effect driven by exposure to improved housing and condominium neighborhoods – we first turn to an instrumental variables (IV) approach. The temporal and spatial variation in our setting allows for the creation of rich sets of instruments that influence the household's decision of whether to *own and occupy* or capital (*rent out or sell*) the units that they win. Using these instrument sets, interacted with the lottery offer, enables us to identify a model with multiple endogenous treatment states under an assumption of constant complier effects (Hull, 2018; Kline and Walters, 2016; Kirkeboen et al., 2016; Pinto, 2021).[5]

In our preferred specification, using the difference between the realized distance to the winning condominium from the expected distance to all condominiums as an instrument (Borusyak and Hull, 2020), we show that the positive effect on educational outcomes for children are driven almost entirely by children in households which choose to *own and occupy* the unit that they win. Households winning condominiums that are closer to their current residence than expected are significantly more likely to own and occupy their units, consistent with evidence on the preference for maintaining employment and social ties (Barnhardt et al., 2017; Franklin, 2020b), and suggests that variation in the quality of a match between a household and their assigned unit, reflecting household preferences over maintaining local

[5]Variation in multiple instruments creates different complier groups into each treatment. Constant complier effects assumes that treatment effects are identical across these complier groups.

connections, may lead to heterogeneous treatment effects and selection. These results imply that the intergenerational impacts of this policy cannot be explained by a wealth effect alone.

To relax the assumption of constant complier effects and characterize the nature of household selection into treatment states, we adapt a structural selection model with multiple, unordered treatments from the policy evaluation literature (Kline and Walters, 2016; Mountjoy, 2019; Heckman and Pinto, 2018; Kamat and Norris, 2020; Heinesen et al., 2022; Stevenson et al., 2023). Results from this model support the IV model, with treatment effects for children concentrated amongst those in families that own and occupy the unit that they win. We additionally document Roy-style selection: if anything, children in households with higher tastes for occupying their unit are less likely to attain secondary and tertiary education, but children in lottery winning households with high propensities for unit occupation experience larger gains in educational attainment.

The results of our study make contributions across three strands of literature. First, we contribute to the literature on the impacts of public housing and slum redevelopment. Polices to improve housing quality and remove slums are ubiquitous in low-income countries, (Franklin, 2020a; Michaels et al., 2021; Camacho et al., 2022) just as they were historically in the United States and Europe (LaVoice, 2013; Collins and Shester, 2013). We are the first to show large, positive impacts of a housing policy in a low-income setting (Barnhardt et al., 2017; Franklin, 2020b; Hoagland, 2020). Evaluating a policy that focuses on *ownership* rather than rental subsidies distinguishes us from more commonly studied programs (Kling et al., 2007; Oreopoulos, 2003; Chetty et al., 2016; Chyn, 2018; Van Dijk, 2019; Pinto, 2021) or those that limit a household's ability to sell or rent out the unit that they own (Kumar, 2020; Camacho et al., 2022; Belchior et al., 2023). By documenting heterogeneity in treatment effects based on household choices, conditional on winning a unit, we are able to separate a wealth effect from one related to ownership and neighborhoods. Pooling these two have confounded policy evaluation of other programs (Pinto, 2021).

The relationship between unit proximity and occupation rates implies that in-site slum redevelopment policies may have much larger impacts on beneficiaries than redevelopment policies that expand housing in the city periphery (Lall et al., 2008; Camacho et al., 2022). Our results help to explain the negative effects found in evaluations of other slum redevelopment policies that mandate relocation to suburban neighborhoods, and support the findings of Camacho et al. (2022) that emphasize the importance of housing placement. They further suggest that an allocation mechanism that incorporates household residential location, matching households to close developments, would likely improve outcomes for lottery participants and their children. Beyond location, by considering outcomes in the medium-term, we are able to document household-level impacts that were not observed in a short-run evaluation of the same policy (Franklin, 2020b).[6] We therefore add to the small set of papers on the long-run impacts of slum redevelopment policies in low-income countries (Picarelli, 2019; Michaels et al., 2021; Rojas Ampuero and Carrera, 2022; Belchior et al., 2023).

Second, we contribute to the literature on the intergenerational impacts of public policy. Failure to consider longer-term effects on children may dramatically underestimate a policy's impact, (Bailey et al., 2020, 2021; Nakamura et al., 2021; Duflo et al., 2023) and previous studies on housing have generally focused on adults. A critical exception is the long-term analysis of the Moving to Opportunity (MtO) experiment, in Chetty et al. (2016) where the authors find large impacts of housing rental subsidies on income and educational attainment for children. The results from lower-income settings are mixed: Kumar (2020) shows that a housing lottery in India leads to only modest increases in measures of housing quality and assets, but children in winning households have higher employment and educational attainment; Camacho et al. (2022) shows substantial gains in educational attainment for children whose parents win houses in desirable Chilean neighborhoods; Rojas Ampuero and Carrera (2022) finds decreases in employment for children effected by a slum clearance program in Brazil. We offer the first long-term evaluation of a lottery for full homeownership on children, and consider measures of human capital that are unavailable in administrative data.

Finally, studies on the impacts of public housing generally emphasize intent-to-treat

[6] Other than Franklin (2020b), the only other papers to study the Ethiopian housing lotteries are Andersen et al. (2020) and Andersen et al. (2022) which document how winning a lottery changes household preferences for redistribution and subjective well-being.

results that do not account for households' endogenous response to treatment. Adapting new methods from the policy evaluation literature (Kline and Walters, 2016; Kirkeboen et al., 2016; Kamat and Norris, 2020; Lee and Salanié, 2018; Stevenson et al., 2023), we make use of the full set of information embedded in ex-post household responses to treatment in order to disentangle potentially competing mechanisms (Pinto, 2021). Our study represents one of the first applications of these methods to an evaluation of a policy in a low-income setting, and in doing so is one of the first to characterize the importance of neighborhoods and homeownership in a developing city (Michaels et al., 2021; Belchior et al., 2023).

2 Context

Ethiopia is one of the fastest urbanizing countries in the world; Addis Ababa, the capital, has doubled in size since 2000 and is expected to nearly double again by 2035 (Koroso et al., 2021). Rapid urban population growth has stressed the existing housing stock in Ethiopian cities, raising rental prices, and private sector development has not kept up with demand – over 70% of households in Addis Ababa live in slums or informal settlements (Franklin, 2020b). Beginning in 2005, with the rate of construction increasing rapidly since 2015, the Ethiopian government launched an ambitious public housing policy to build hundreds of thousands of residential units for urban dwellers in Addis Ababa. Through 2022, approximately 200,000 units have been built and occupied, with thousands more expected to be completed in 2023. Appendix Figure A.1 shows the total units built over time. The stated goals of the program were to provide housing for low- and middle-income urban dwellers and to support the domestic construction industry.

There were two rounds of registration for the lotteries, which took place in 2005 and 2013. An estimated 50% of all households in Addis Ababa registered for the program, with over 900,000 applications in total.[7] Only one application was allowed per household, and

[7]Approximately 300,000 households registered in the 2005 registration round and the remaining 600,000 registered in 2013.

eligibility required that the heads of household could not own property in Addis Ababa. Registrants were also required to have been residing in the city for at least six months at the time of their registration. Households were free to choose the size of the desired unit – studios, 1-bedroom, 2-bedroom, and 3-bedroom units – but not the unit's location.

Critically for our research design, condominiums were allocated via random lottery. Due to over-subscription and limited construction capacity, the lottery was conducted in rounds as units were completed. There were 14 lottery rounds through 2022. The lotteries were random within pre-determined strata for female-headed households, government employees, and disabled households. Lottery winners were announced publicly in the media with substantial fanfare. The city government went to great lengths to ensure that lotteries were viewed as fair by the community, and there is no evidence of corruption in the lottery implementation for the rounds considered in this paper(Franklin, 2020b).[8] The policy has not been without controversy, however, as condominium sites built in the city outskirts have spilled into land in the surrounding Oromia region, aggravating issues related to Addis Ababa's urban sprawl.

In order to be eligible for the lotteries, after submitting an application, the households were required to open a tagged bank account with the Central Bank of Ethiopia (CBE) and to make deposits towards a down-payment. The required payments corresponded to the unit's size and the down-payment program to which the household belonged.[9] The households were not required to have completed the full down-payment at the time of the lottery, but needed to have made consistent deposits. Only after making the entirety of the down payment were households given the keys to their unit. The remainder of the total unit cost was financed via a low-interest mortgage at CBE. During the 11th round in 2019, the total condominium unit price was \$6,400 for a one-bedroom, \$8,800 for a two-bedroom, and \$11,700 for a three-bedroom. These prices represent, on average, a 40% subsidy relative to

[8]There has been an accusation of lottery corruption in the 14th lottery round (Borkena, 2022). We do not sample winners from this round in our analysis.

[9]There are three program types: 10/90, 20/80, 40/60, where the first number is the percentage of the unit's total cost that must be paid via down-payment. Households were mapped into down-payment programs via rough means-testing, with lower down-payments required for low-income households. All registrants from the first registration, and were part of our sampling frame, were in the 20/80 program.

the cost of production per unit (Franklin, 2020b).

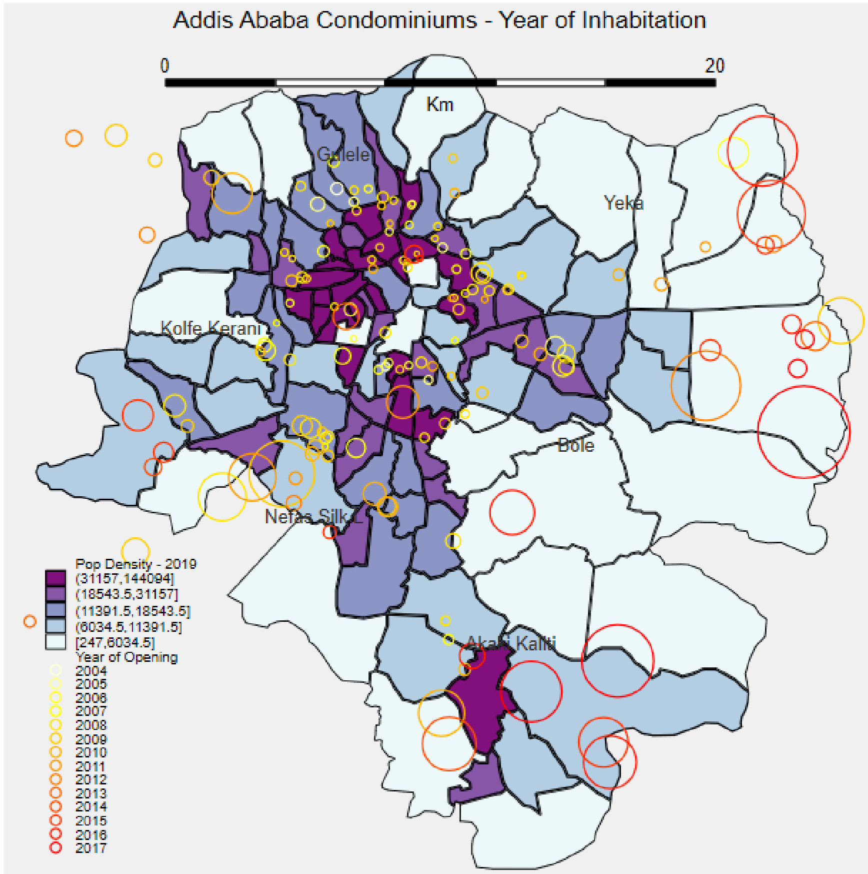
While early lottery rounds included more centrally located units, the condominium policy functionally reallocated families from low-quality, dense housing in the city center to higher-quality housing on the outskirts of the city. Due to their peripheral location, many condominium neighborhoods had worse labor market access, sparser social networks, and lower quality schools and infrastructure. Figure 1 shows the spatial distribution and timing of condominium openings in Addis Ababa through 2017. Recent developments are increasingly located in peripheral locations and are substantially larger than early developments, often consisting of 30,000 or more condominium units. Condominiums were virtually identical in size, quality, and appearance across sites. Anecdotal evidence suggests that some winning households were unsatisfied with the construction, but winning households were free to invest in upgrading their units.

Relative to public housing programs in North America that focus on moving families from "bad" neighborhoods to "good" ones (Kling et al., 2007; Oreopoulos, 2003; Chetty et al., 2016), and a similar policy in Colombia (Camacho et al., 2022), condominium neighborhood exhibit substantial heterogeneity along multiple dimensions of neighborhood quality. This heterogeneity makes the expected effects on households and child welfare unclear ex ante.

The design of the policy corrects for key margins of selection that confound the estimation of neighborhood effects. Typically, households make an endogenous choice of where to live, matching a household to a neighborhood. They similarly choose with whom they wish to live, leading to residential sorting across neighborhoods. In our case, the scope for neighborhood matching and residential sorting are diminished. Since households could be assigned to any condominium unit, those who choose to move into their unit are exogenously matched to a neighborhood. Similarly, a household's neighbors in their new units are also randomly assigned, at least amongst the set of winners who occupy their unit.

However, after winning, there is no requirement that the household move into the unit that they win. That is, they are free to rent it out or leave it unoccupied. Although there

Figure 1: Condominiums in Addis Ababa



The map divides Addis Ababa into woredas, the smallest formal administrative unit within the city. Woreda color represents population density; denser woredas have darker shading. Circles represent the location of condominium sites. The size of the circle represents the number of units in the site, and the color of the circle indicates the year the site opened, with darker colors being more recent.

was a technical requirement that households not sell their unit for five years after winning, this requirement was unenforced and often ignored (Andersen et al., 2020). Thus, there remains the potential for residential matching and sorting, such that the policy falls short of the ideal “double randomization” experiment as described in Graham (2018). This ideal experiment would only be possible under mandated relocation, which is infeasible in most settings.

3 Data

Through our partnership with the AAHDA we obtained the universe of condominium applicants, both winners and "waitlist" households who had yet to win a unit as of 2019. This administrative data was used as our sampling frame from which we sampled households to participate in our survey.

3.1 Sampling Frame

Before sampling the households for our survey, some cleaning of the sampling frame was required. We first excluded lottery rounds for which no winner contact information was available. We further excluded Round 13, which took place in 2020, as we believed this to have been too short a period to observe changes in key outcomes of interest. Round 14 was not included in the survey as it occurred after the project had started. We were left to draw our sample from 9 of the 14 completed lottery rounds.

We further limited the sample to the subset of households who had applied during the first registration period in 2005. The 2005 registrants were prioritized during the first 14 lottery rounds, and few of the 2013 registrants had won a unit by 2022. Finally, we excluded all households who applied for a 3-bedroom unit since nearly all of these households from the 2005 registration had won before the 13th lottery round, leaving few comparable controls. After these restrictions, we were left with 171,183 lottery winning households and 48,932 waitlist households in the sampling frame. Appendix Table A.1 shows the totals by lottery round.

3.2 Household Sampling

We used a two-step stratified sampling strategy to sample winning households for our survey. We first sampled condominium site-by-round pairs from across the 9 eligible rounds, oversampling from early lottery rounds, and stratifying by the round-specific median condominium site size. Since some condominium sites were allotted over multiple rounds, we allowed single sites to be sampled multiple times. In order to ensure that we could characterize neighborhood characteristics for winning households, we targeted approximately 50 households per condominium site. This left us with 32 site-by-round units in our sample.

Households were selected within these site-by-round pairs via stratified random sampling. The strata were the interactions of the gender of the head of household, the number of bedrooms applied for [10], and the sub-city where the household resided at the time of its registration. In total, there were 60 strata. We sampled a total of 1,648 lottery winning households.

The waitlist households were selected using simple stratified random sampling, relying on the same strata as the above winners. Since waitlist households have not yet been assigned a condominium unit, this sampling did not include the first site sampling step. Waitlist households had been eligible during each of the 12 rounds through 2019, but had not won. A small share of these households won during rounds 13 and 14 and were included in the survey. In total, we sampled 1,500 waitlist households to participate in the survey.

Our sample of waitlist and condominium winning households is balanced across the baseline covariates used in the strata, as seen in Appendix **??**. To achieve balance, households with female leaders were over-sampled from the wait list. This reflects the fact that, on average, female headed households were 10p.p. more likely to win a unit due to specific quotas. This left relatively fewer female headed households in the remaining waitlist.

Site Re-sampling Due to security issues, six of these sites were re-sampled and replaced with alternative sites drawn from the same round and strata. Details on this process can be found in Appendix **??**.

[10] These were either a studio, 1-bedroom, or 2-bedroom unit.

3.3 Household Survey

Households were first screened via phone before being surveyed by our team of trained enumerators. Since a primary focus of our study was labor market and educational outcomes for children, we required households to have a child who was less than 35 years old to be eligible. Since our survey took place in-person, we required that the household still be living in Addis Ababa. Of contacted households, 96% of contacted respondents still lived in Addis Ababa and amongst these, 89% had a child under 35, with balance across the treatment and waitlist groups. In total, 85% of contacted households were eligible for the survey. Our primary sample consists of 2,236 households, which include over 6,000 children. We supplement this primary sample with a sample of 661 households sampled through a separate, in-person sampling strategy in four condominium sites. We use this supplementary sample to validate our primary sampling strategy, as describe in further detail in Appendix ??.

Summary Statistics Basic summary statistics for our surveyed households are displayed in Table 1. Column (1) is the waitlist households, while columns (3) and (4) show statistics based on the decision of winning households. These figures suggest that our population is positively selected relative to the entirety of Addis Ababa, as approximately 50% of all household heads were formally employed at the time of the condominium registration and over 60% of household heads have obtained at secondary level of education. Our sample is comparable to the sample drawn from a single lottery round in Franklin (2020b), though more likely to be married and have a female head of household. When we compare households in our sample drawn from the same round as those in Franklin (2020b), the two samples looks similar.

Attrition We successfully contacted 65% of sampled households. This is comparable to similar phone surveys conducted in this setting, and attrition was largely due to our reliance on dated administrative data. Attrition is balanced across lottery winners and the waitlist,

Table 1: Summary Statistics and Balance

	DV Mean (DV SD) (1)	Lotto coef. (SE) (2)	Own and Occupy Condo (3)	Rent Out Sold Condo (4)
HHH Age	49.775	1.001	51.946	48.822
	(9.637)	(0.684)	(9.784)	(8.903)
HHH Years in Addis	38.713	-0.340	37.509	39.215
	(11.547)	(1.031)	(12.217)	(11.001)
HHH Married	0.686	-0.025	0.686	0.683
	(0.464)	(0.040)	(0.465)	(0.466)
HHH Years Ed	10.094	0.928**	10.901	10.232
	(4.244)	(0.389)	(4.569)	(4.153)
Orthodox	0.679	0.032	0.695	0.686
	(0.467)	(0.036)	(0.461)	(0.464)
Amharic	0.700	0.020	0.672	0.732
	(0.458)	(0.037)	(0.470)	(0.443)
BL: HHH Wage Emp	0.488	0.056	0.575	0.469
	(0.500)	(0.036)	(0.495)	(0.499)
BL: HHH No Income	0.251	0.031	0.221	0.287
	(0.434)	(0.033)	(0.415)	(0.453)
HHH Father Wage Emp	0.393	0.027	0.315	0.435
	(0.488)	(0.046)	(0.465)	(0.496)
HHH Father Casual/Self Emp	0.571	-0.025	0.671	0.512
	(0.495)	(0.048)	(0.471)	(0.500)
HHH Mother Wage Emp	0.103	-0.017	0.071	0.109
	(0.304)	(0.024)	(0.257)	(0.311)
HHH Mother Casual/Self Emp	0.211	-0.024	0.170	0.232
	(0.408)	(0.035)	(0.376)	(0.423)
BL: HH Size	3.619	-0.042	3.786	3.519
	(2.216)	(0.199)	(2.130)	(2.070)
Observations	2326	1176	480	622
Samp Weights	X	X	X	X
Joint F-Stat		1.423		

Household-level OLS regressions of time-invariant household head (HHH) characteristics on an indicator for whether the household won a condominium lottery. Standard errors are clustered at the household level. The joint F-stat in column (2) is from a test that all lottery winner coefficients are equal to zero. Columns (3) and (4) present summary statistics for winning households that alternatively own and occupy or have rented out or sold the unit that they won. Orthodox is an indicator for the HHH belonging to the Ethiopian Orthodox Church. Amharic is an indicator equal to one when the HHH's mother tongue is the Amharic language. Baseline (BL) employment measures reflect household head employment states at the time of the condominium registration in 2005. HHH [Father/Mother] Wage Emp and HHH[Father/Mother] Casual/Self Emp are indicators equal to one when the parent of the HHH was primarily employed in a given sector. BL HH size is the number of members in the household at the time of lottery registration in 2005. ***, **, * indicates significance at 1, 5, and 10%.

though higher for early lottery round winners. These higher rates of attrition in early rounds are due to households not updating their phone numbers with the AAHDA after winning. Additionally, female-headed households and two-bedroom applicants are slightly less likely to be contacted. We believe that these attrition rates are reasonable given that we are tracking households up to 17 years after they win a lottery in an urban setting where relocation and phone number changes are common. Of those contacted, 11% refused to participate in the survey. While this refusal rate was unusually high, recently political instability in Ethiopia had led to significant tension amongst respondents.

Upon receiving consent, households were asked to respond to an extensive survey which included information on education, employment, and residential history for all household members and any of the respondent's children who may be living outside the household. We additionally surveyed a subset of children directly. The survey with children covered aspirations, education, measures of cognition, and basic numeracy and literacy exercises.

3.4 Administrative Data

We supplement our household survey with administrative and survey data on administrative budgets, school quality, wages, roads, and neighborhood characteristics.

Administrative Budgets In partnership with the Addis Ababa City Administration, we have collected line-item administrative budgets for each woreda within Addis Ababa between 2014 and 2018. These data are used to build measures of per capita spending on education and public services.

School Quality The Ministry of Education tracks school quality for all primary, secondary, and tertiary educational institutions throughout the country. For primary and secondary schools, we obtained school quality data collected in 2018 and 2019. These data rank primary schools along 26 distinct standards and five aggregate performance measures. We use these data in our analysis of school quality.

We also obtained a comprehensive list of all tertiary institutions – universities, colleges, and technical training institutes – from the Ministry of Education. These data include school location, year of establishment, and the institution's ownership status. We combine this list with data from the Ethiopian Higher Education Relevance and Quality Agency (HERQA) which monitors school accreditation. These data are used to build measures of postsecondary school quality.

Firms We collected matched employer-employee from the Private Sector Employer's Social Security Agency (POESSA) to build measures of firm density and average wages. With quarterly observations between 2011 and 2021, we observe firm location, sector, employment, and wages for the set of private sector firms which contribute to social security. While Addis Ababa has a large informal sector, this data represents the most comprehensive data on formal sector wages and employment.

Roads We build a biannual road network panel of all roads built in Addis Ababa. This data has been used in prior work in Ethiopia (Adamopoulos et al., 2019), and includes measures of road quality, allowing us to construct measures of and document changes in neighborhood-level market access.

Neighborhood Characteristics We also use survey data collected by the Central Statistics Agency and the Stanford University African Urbanization and Development Research Initiative (AUDRI) (Abebe et al., 2018). For the former, we use the Urban Employment and Unemployment Survey collected in 2012, 2014, 2016, and 2018 to build neighborhood-level measures of unemployment and poverty rates. We separately use a survey of all woreda-level administrators from the AUDRI project, which asks specifically about public services, spending, and local population changes.

4 Impacts of Condominium Lotteries

4.1 Policy Uptake

Before showing how the policy affects households and their children, we begin by documenting how the policy was used. First, 99% of winning households purchased the unit that they won. Nearly perfect take-up, conditional on winning, is due to the substantial subsidy associated with winning a unit. Households who won a condominium are given full ownership, able to rent out or sell their unit immediately. In our sample, 82% of winning households still own the unit that they won, even up to 17 years after winning. However, many winning households chose not to move into their unit. In Table 2 we see that of winning households, 35% rent out their unit, 17% sell, and a small share either leave the unit unoccupied or allow it to be used rent-free by friends and family. The remaining 41% of the winning households live in and own the unit they won.

Table 2: Condominium Usage

	(1) All	(2) Winners	(3) Waitlist
Own Lottery Condo	0.82	0.82	.
	(0.38)	(0.38)	(.)
Own Any Condo	0.44	0.85	0.01
	(0.50)	(0.36)	(0.10)
Sold - Lottery Condo	0.17	0.17	.
	(0.37)	(0.37)	(.)
Occupy - Lottery Condo	0.41	0.41	.
	(0.49)	(0.49)	(.)
Rent Out - Lottery Condo	0.35	0.35	.
	(0.48)	(0.48)	(.)
Rent In Condo	0.04	0.02	0.07
	(0.21)	(0.13)	(0.26)
Observations	2326	1176	1150

Variables defined over any condominium or lottery condominium. Lottery condominium refers to the particular unit that winners obtained via the lottery. Any condominium is the unit won via lottery or any other condominium unit.

4.2 Changing Neighborhoods

Having shown how households interact with the policy, we now turn to documenting policy-induced changes in neighborhood characteristics that may impact household and child welfare. In Table 3 we show that winning a condominium leads to significant changes in neighborhood quality. Lottery winners live, on average, 3.3 kilometers further from the city center than waitlist households, a 53% increase. This effect is much larger for winning households that occupy the unit that they win. These households live an average of 10.3 kilometers from the city center, compared to 5.9 kilometers for waitlist households not living in condominiums (column (2)). Despite living in more peripheral neighborhoods, we show that a neighborhood quality index, composed of measures of household satisfaction with their current neighborhood, is 0.86 SDs larger for winning households. Gains in this index are again concentrated amongst households that occupy their units, as shown in column (4). Appendix Figure A.2 displays how each of the index components varies based on lottery winner and condominium occupation status.

Table 3: Neighborhood Quality and Proximity

	Km to City Center		Qual Index		Prox Index		1(Feel Secure)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Won Condo)	3.331***	1.704***	0.863***	0.609***	-0.310*	-0.317	0.075***	0.085***
	(0.323)	(0.351)	(0.220)	(0.232)	(0.185)	(0.221)	(0.021)	(0.024)
1(Occupy)		3.978***		1.151***		-0.986***		0.020
		(0.619)		(0.294)		(0.232)		(0.049)
Winner X Occupy		0.919		-0.266		0.780**		-0.041
		(0.717)		(0.420)		(0.332)		(0.058)
Constant	6.319***	6.948***	-0.299	-0.236	2.209***	2.278***	0.740***	0.733***
	(1.306)	(1.067)	(0.840)	(0.821)	(0.721)	(0.717)	(0.074)	(0.073)
N	2269	2269	2269	2269	2269	2269	2269	2269
Wait/Non-Dwell Mean	5.758	5.866	-0.455	-0.380	0.141	0.151	0.831	0.859
Samp Weights	X	X	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X	X	X

Household-level regressions. Standard errors are clustered at the household level. Regressions include controls for household head characteristics. The quality and proximity indices are the first principal component from the amenities and infrastructure show in Appendix Figures A.2 and A.3. Quality is measured on a 1-5 scale. Proximity is measured by one-way travel time. ***, **, * indicates significance at 1, 5, and 10%.

We similarly construct an index of neighborhood proximity to key amenities and social venues in columns (5) and (6) of Table 3. We do not find evidence that condominium households live significantly further from these amenities; if anything, column (3) implies that winning households live marginally closer to these amenities. Looking at the components of the index in Appendix Figure A.3 we observe that there is substantial heterogeneity based on the household's decision to occupy their unit. Condominium dwellers do live further from close friends and family members, but not public services or other infrastructure. This echos findings from Barnhardt et al. (2017) and Franklin (2020b) that suggest that moving to new housing may disrupt social networks. We discuss additional results on household social networks and neighborhood cohesion in Section 4.5. In column (7) of Table 3 we show that winning a condominium increases respondent's feelings of security in their neighborhood by 7.5 percentage points.

Using administrative data on school inspections and woreda-level budgets, we build measures of average neighborhood school quality and per capita spending. These results are presented in Appendix Tables A.2 and A.3. These results imply that while condominium winning households live in neighborhoods with slightly lower quality schools, school density is higher, driven by an increase in the number of (lower-quality) proximate private schools. Further, winning households live in neighborhoods with lower per capita spending on education, health, and women's and children's affairs. Variation in these measures are displayed visually in Appendix Figure A.4.

Together, these results show that winning a condominium lottery impacts household neighborhoods of residence. These new neighborhoods are not clearly of lower quality: there is substantial variation in neighborhood characteristics across outcome measures and within the set of winning households, reflecting the varied locations of condominium units across the city. Differences in quality measures, conditional on the household's decision to move into their unit, previews the key margin of heterogeneity that we will explore further in Section 5.

4.3 Empirical Strategy

Having documented that the policy was utilized and meaningfully changed neighborhoods of residence for the winning households, we turn to estimating the impacts of the policy on children's human capital and household welfare. Equation 1 is our primary reduced form specification, which estimates an intent-to-treat (ITT) effect for households and families who win condominiums in their youth. Specifically, we estimate:

$$y_i = \alpha + \beta_1 T_i + \mathbf{X}_i \gamma + \epsilon_i \tag{1}$$

$$y_i = \alpha + \kappa_1 Exp_i + \mathbf{X}_i \gamma + \epsilon_i \tag{2}$$

where T is a treatment indicator for winning households for child or household i. $\mathbf{X}$ is a vector of child and household covariates, including the child's birth cohort. Given that there was nearly perfect take-up for lottery winners, and almost zero waitlist households acquired a unit, β_1 approximates the ATE in this setting.[11]

Our setting allows for an additional reduced form specification for estimating policy impacts for children. Drawing on the literature on intergenerational mobility and housing in the United States (Oreopoulos, 2003; Chetty and Hendren, 2018), and leveraging exogenous temporal variation in the timing of condominium lotteries, we can separately estimate a linear exposure design, show in Equation 2. Here, the treatment variable *Exp* is defined as the number of years after a child's household wins a condominium lottery before they turn 18 years old. Children in waitlist households are defined as having zero years of childhood exposure to the policy. We can additionally include the primary treatment indicator T from specification 1 to control for the main effect of winning a lottery, such that κ_1 is identified by exogenous variation in policy timing within the subset of winning households. We show the variation in *Exp* for children in winning households in Appendix Figure A.6.

[11] In the parlance of instrumental variables (Imbens and Angrist, 1994), our setting has almost no never-takers or always-takers. We return to this point in Section 5.

Balance We use specification 1 to test for balance in our sample of lottery winning and waitlist households. As we lack a true baseline survey for respondents, prior to condominium application or lotteries, we examine whether time-invariant characteristics of winning and waitlist heads of household are comparable. In Table 1 we regress various baseline household head characteristics on an indicator for whether the household won a lottery. We include sampling weights that reflect the sampling probability due to our household sampling strategy. We additionally include two controls: an indicator for the condominium site sample group and an indicator for households who had already adopted a cell phone at the time of the 2005 registration. We discuss the inclusion of these controls and robustness in Appendix **??**. We obtain similar balance in unweighted regressions controlling for strata fixed effects. We believe the weighted regressions to be more conservative and thus preferred for estimation.

In column (2) we observe that we obtain balance on all but one of the included covariates. However, we cannot reject the joint balance test across all baseline covariates. The imbalanced characteristic, years of education of the household head, is included along with sampling weights and the controls discussed above in all subsequent analysis.

4.4 Children's Human Capital

We now turn to our primary research question, how winning a housing lottery during childhood affects human capital. First, we focus on educational attainment and school quality, two of the principal components of human capital. Next, we consider direct measures of learning and cognition using exercises on cognition, literacy, and numeracy. We then use our household and child surveys to consider outcomes that are unavailable in administrative data. Specifically, we focus on measures of soft skills and children's aspirations that may be associated with human capital, but are infrequently measured. Finally, we present results on children's downstream employment and income.

Educational Attainment The results on child educational attainment are presented in Table 4. In column (1) we show that the policy increases active educational enrollment for children of winning households by 4.5% amongst 5-30 year-olds and 11% amongst 14-30 year-olds, shown in column (2). The effects are larger for the older cohort for whom school attendance is no longer compulsory. These results are consistent with visual evidence in Figure 2 which estimates marginal effects on enrollment by age group. We see that increases in school enrollment are largest for children between 15 and 20 years old, which coincides with the period in which children are completing secondary schooling and enrolling in post-secondary education. These results are robust to the inclusion of additional household and child controls, the consideration of different age-restricted subsamples, and unweighted specifications with strata fixed effects.

Similarly, in the upper-left panel of Appendix Figure A.5 we observe that trends in enrollment rates between lottery winning and waitlist households until age 15 at which point children in lottery winning households are consistently more likely to be enrolled in school. In contrast, we find no impact on primary school completion rates. Primary school is mandatory and free in Ethiopia, leading to high completion rates of over 90%. We would not expect the lottery to significantly impact these rates and consider this a placebo test our estimation strategy.

In column (3) of Table 4 we show that treatment increases secondary school completion rates by 10.5%. Post-secondary attendance rates – defined as attendance at any college, university, or technical training program – increase by 16% for children in lottery winning households. These impacts on post-secondary attendance are larger than many school expansion programs, and about half as large as some of the most generous scholarship programs (Snilstveit et al., 2015; Duflo et al., 2021).

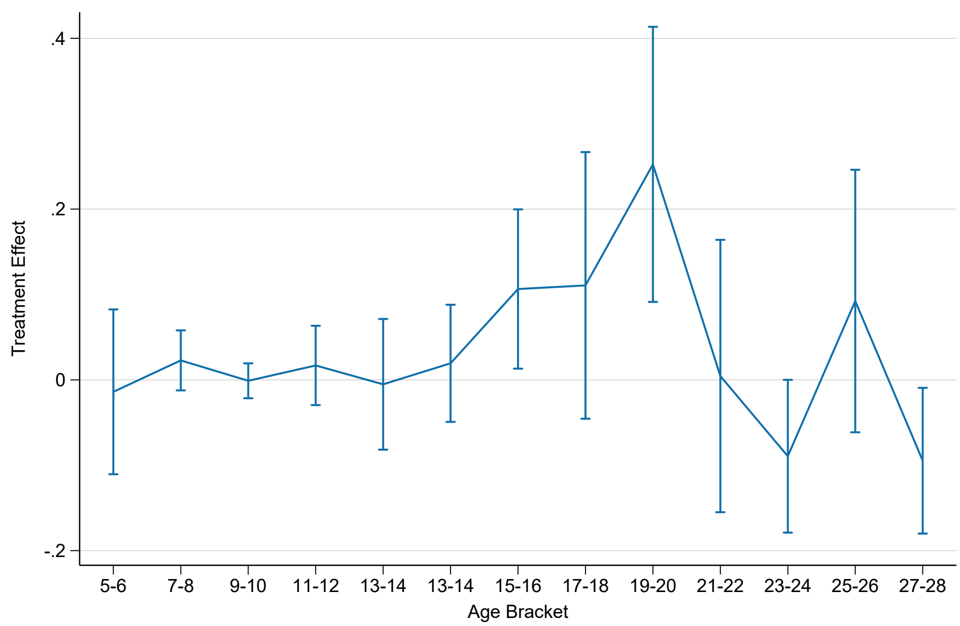
Second, in Table 5 we estimate the linear exposure model from specification 2. Binning children's age into 3-year groups to improve power, as in Chetty et al. (2016), and we find evidence of large exposure effects for children in winning households. Even after controlling

Table 4: Children's Educational Attainment

	(1) 1(Enrolled)	(2) 1(Enrolled)	(3) Primary	(4) Secondary	(5) Post-Sec Att
1(Won Lottery)	0.030*	0.056**	0.011	0.070**	0.079**
	(0.016)	(0.024)	(0.025)	(0.031)	(0.039)
1(Male)	-0.011	-0.022	0.042**	-0.035	-0.122***
	(0.015)	(0.024)	(0.017)	(0.028)	(0.036)
Constant	0.632***	0.420***	0.851***	0.630***	0.344***
	(0.030)	(0.039)	(0.027)	(0.037)	(0.062)
N	4558	2614	2210	1812	1812
Waitlist Mean	0.742	0.558	0.913	0.620	0.406
Birth Cohort FEs	X	X	X	X	X
Sample	5-30	14-30	14-30	18-30	18-30

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. ***, **, * indicates significance at 1, 5, and 10%.

Figure 2: Children's Enrollment By Age



Weighted child-level OLS regressions with 95% confidence intervals. Standard errors are clustered at the household level. Each estimate in the figure represents the marginal effect from an OLS regression of a treatment indicator interacted with a child age group at the time of the survey, controlling for base age-cohort effects.

for the main effect of winning a condominium, three additional years of childhood exposure to the policy increases school enrollment rates by 1-4% percentage points. This result makes an important point: lottery winning children of the same age experience larger gains in enrollment rates when their parents win a lottery when they are younger.

In contrast, columns (4) and (5) show that the entirety of policy's effects on secondary school completion and post-secondary attendance are attributable to a level effect due to winning a lottery. We expand on our enrollment results in Appendix Table A.14 and estimate

Table 5: Children's Education - Exposure Design

	(1) 1(Enrolled)	(2) 1(Enrolled)	(3) Primary	(4) Secondary	(5) Post-Sec Att
Exposure (Years)	0.007*	0.017***	-0.006	-0.016	-0.005
	(0.004)	(0.005)	(0.005)	(0.010)	(0.011)
1(Won Lottery)	-0.009	-0.021	0.050*	0.119***	0.094*
	(0.029)	(0.035)	(0.027)	(0.039)	(0.053)
1(Male)	-0.009	-0.018	0.040**	-0.030	-0.114***
	(0.016)	(0.025)	(0.017)	(0.029)	(0.038)
Constant	0.618***	0.398***	0.859***	0.646***	0.344***
	(0.030)	(0.041)	(0.025)	(0.041)	(0.065)
N	4558	2614	2210	1812	1812
No Exposure Mean	0.638	0.434	0.912	0.715	0.521
Birth Cohort FEs	X	X	X	X	X
Sample	5-30	14-30	14-30	18-30	18-30

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. Exposure is defined as years of treatment before the child turned 18 years old. ***, **, * indicates significance at 1, 5, and 10%.

a "sibling design"" (Oreopoulos, 2003). We include household fixed effects, such that treatment effects are estimated off of within-household variation in years of childhood exposure to the policy. While we lose power, our results are remarkably stable. Comparing children within the same household who had varying years of childhood exposure, an additional 3 years of childhood exposure increases school attendance rates by 2-5%. These results are consistent with evidence from the United States that the impacts of housing interventions are concentrated among younger individuals (Chetty et al., 2016; Chyn and Katz, 2021).

School Quality While we have shown that the policy leads to large increases in educational *attainment*, one may also believe that the quality of education received may also change based on treatment. On the one hand, children who live on the city's periphery may have access to lower quality schools. On the other, an increase in parental wealth may allow parents to enroll their children in better schools.

To make progress on this, we first use administrative data on primary school quality from HERQA. These data are collected by the agency annually and rates schools from 0 to 100 along 26 standards ranging from facility quality to curriculum and testing performance. These 26 standards are aggregated into 4 indices – Performance, Input, Process, Output. Finally, these indices are combined to assign each school a quality level between 1 and 4; a grade of 1 corresponds to very low quality and a grade of 4 corresponds to very high quality.

We match the children with their most recently attended primary school. In Table 6 we show that treatment does not change the quality of primary schools attended by children. We find consistently null effects across all measures used by HERQA and a quality index derived from the first principal component of HERQA's 4 aggregate indices. Furthermore, children in lottery winning households are no more likely to attend a private primary school, which is generally considered to be of higher quality in Addis Ababa.

Next, we used data on all colleges, universities and technical institutions in Ethiopia from the Ministry of Education to match children with the post-secondary schools they attended. The results are presented in Table 7. Although treatment effects are positive in all specifications, our results are imprecise. As with primary school quality, we find no significant differences in the quality of lottery winning children's post-secondary institutions. In column (1) we show that children of winning households are no more likely to attend post-secondary school in Addis Ababa. The results in columns (2) and (3) show that they also do not attend Addis Ababa University, the country's top university, or any of the flagship public universities. Reconciling these results with the significant increase in post-secondary attendance, we see in columns (3) and (5) that children in winning households are marginally

Table 6: Primary School Quality

	(1) Standard Avg	(2) Performance	(3) Input	(4) Process	(5) Output	(6) Quality Index	(7) 1(Private)	(8) Level
1(Won Lottery)	-0.047	-0.051	0.032	-0.070	-0.063	-0.076	0.008	0.029
	(0.095)	(0.096)	(0.092)	(0.094)	(0.094)	(0.177)	(0.042)	(0.053)
1(Male)	-0.007	-0.006	-0.017	-0.002	0.003	-0.011	0.056*	0.011
	(0.069)	(0.068)	(0.071)	(0.067)	(0.066)	(0.127)	(0.030)	(0.042)
Constant	-0.135	-0.152	-0.139	-0.103	-0.177	-0.284	0.378***	2.292***
	(0.132)	(0.129)	(0.128)	(0.132)	(0.112)	(0.243)	(0.059)	(0.087)
N	2872	2872	2872	2872	2872	2872	2872	2872
Waitlist Mean	0.020	0.013	-0.009	0.038	0.003	0.023	0.378	2.423
Sampling Weights	X	X	X	X	X	X	X	X
Sample Controls	X	X	X	X	X	X	X	X
Birth Cohort FEs	X	X	X	X	X	X	X	X
Sample	5-30	5-30	5-30	5-30	5-30	5-30	5-30	5-30

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. Children are matched to their most recently attended primary school. "Standard Avg" is the unweighted, normalized average of 26 quality components. Performance, Input, Process, and Output are normalized, aggregate quality measures. Quality index is the first principal component of the 4 aggregate quality measures. Level is measured on a scale of 1-4 with 1 representing significant failures and 4 representing exemplary performance. ***, **, * indicates significance at 1, 5, and 10%.

more likely to attend public universities or technical training institutes (TVETs). This result holds if we additionally condition on any post-secondary attendance.

Cognitive Skills, Literacy, and Numeracy Our educational results are not limited to attainment. In a sample of 6-17 year old children who we interviewed directly, we see substantial gains in measures of fluid intelligence. These results are presented in Table 8 and are consistent with household wealth and residential stability being important drivers of human capital (Bursztyn and Jensen, 2015; Card and Giuliano, 2013). Specifically, in column (1) we see that children in winning households complete a numerical Stroop exercise 28% faster, and more 88% more accurately. In column (5) we show that winning a condominium lottery improves children's performance on a Raven's matrix exercise by 24%. These measures are commonly used in economics and the child development literature and have been previously validated in Ethiopia (Mekonnen et al., 2020; Abebe et al., 2021). Details on the imple-

Table 7: Post-Secondary School Quality

	(1) 1(Post-Sec in AA)	(2) 1(AAU)	(3) 1(Public Uni)	(4) 1(Private Uni)	(5) 1(TVET)
1(Won Lottery)	0.037	0.002	0.032	0.007	0.023
	(0.038)	(0.014)	(0.029)	(0.031)	(0.032)
1(Male)	-0.060	-0.038**	-0.083**	-0.068*	0.040
	(0.040)	(0.017)	(0.033)	(0.037)	(0.032)
Constant	0.326***	0.027***	0.146***	0.204***	0.095**
	(0.048)	(0.010)	(0.034)	(0.043)	(0.045)
N	1814	1731	1731	1731	1731
Waitlist Mean	0.322	0.024	0.108	0.157	0.127
Sampling Weights	X	X	X	X	X
Sample Controls	X	X	X	X	X
Birth Cohort FEs	X	X	X	X	X
Sample	18-30	18-30	18-30	18-30	18-30

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. Children are matched to their most recently attended post-secondary institution, if any. Post-Sec in AA is an indicator for attending tertiary education in Addis Ababa. AAU is an indicator for attending Addis Ababa University. TVET is an indicator for attending a technical or vocational training institute. ***, **, * indicates significance at 1, 5, and 10%.

mentation of these tests can be found in Appendix **??**. Columns (2), (4), and (6) of Table 8 replicate the exposure design in Equation (2). Columns (2) and (6) provide evidence of an exposure effect on Stroop exercise timing and Raven's matrix performance, though the effect for the Stroop test is imprecisely estimated. Focusing on Raven's matrix performance for which we have a larger sample, after controlling for the main effect of the lottery, an additional three years of childhood exposure to the housing policy increases Raven's scores by 15%.

In Appendix **??**, we discuss additional results for additional literacy and numeracy exercises. While each of the results across a numeracy index, a literacy index, and a combined testing index are positive, the effects are insignificant. We show results for each of the index components in the Appendix Table A.4 and note that the effects are positive for 5 of the 7 components. Children in winning households score significantly better on the math component, which is also the component in which we successfully induce significant variation in scores – students generally scored very well on the tests, with many getting perfect scores.

Table 8: Stroop Test & Raven's Matrices

	Stroop Time (Sec)		Stroop Num Mistakes		Raven Score	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Won Lottery)	-19.042**	-10.681	-2.420*	-2.430*	1.370**	-1.064
	(9.492)	(6.668)	(1.400)	(1.355)	(0.615)	(0.963)
Exposure [3 Yr]		-2.878		0.004		0.840**
		(3.744)		(0.555)		(0.342)
1(Male)	-9.289*	-9.405*	0.142	0.142	0.795*	0.820**
	(5.440)	(5.571)	(1.005)	(1.012)	(0.420)	(0.411)
Constant	70.613***	71.468***	1.501	1.500	5.699***	5.559***
	(7.803)	(8.633)	(1.025)	(1.114)	(0.489)	(0.493)
N	98	98	98	98	223	223
Waitlist Mean	67.378	67.378	2.705	2.705	5.594	5.594
Birth Cohort FEs	X	X	X	X	X	X
Test FEs					X	X
Sample	13-17	13-17	13-17	13-17	6-17	6-17

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. Stroop Time is the number of seconds to complete the Stroop exercise. Stroop Num mistakes is the count of mistakes on the Stroop exercise. Raven Score is the numerical score on the Raven's matrix exercise. Test fixed effects reflect control for average scores on age-specific sets of Raven's matrices. ***, **, * indicates significance at 1, 5, and 10%.

We view these results as suggestive of improved learning, although our failure to generate significant variation in components and our relatively small sample of children hinder our ability to detect differences.

Soft Skills & Aspirations Outside of education and learning, we find broadly positive impacts of winning a lottery on children's soft skills, aspirations, and general well-being. First, we conduct the Strengths and Difficulties Questionnaire (SDQ) for a random 50% sample of male and female children. This questionnaire, standard in the literature on child development and validated in Ethiopia (Hoosen et al., 2018; Mekonnen et al., 2020), is administered to parents, asking about their children, and is designed to measure the child's emotional and behavioral development. More details on its implementation can be found in Appendix **??**.

Table 9: Strengths & Difficulties

	SDQ Scores (2-18)			
	(1)	(2)	(3)	(4)
Winner	0.028	-0.062	-0.017	-0.131
	(0.088)	(0.101)	(0.151)	(0.198)
Winner × 1(Male)=1		0.179*	0.179*	0.385*
		(0.104)	(0.104)	(0.217)
Exposure (Years)			-0.007	0.009
			(0.021)	(0.028)
Exposure (Years) x Male				-0.029
				(0.027)
1(Male)=1	0.035	-0.073	-0.073	-0.073
	(0.056)	(0.058)	(0.058)	(0.058)
Constant	-0.291**	-0.224*	-0.222*	-0.225*
	(0.122)	(0.123)	(0.124)	(0.124)
N	2443	2443	2443	2443
Waitlist Mean	-0.038	-0.038	-0.038	-0.038
Birth Cohort FEs	X	X	X	X
Resp Controls	X	X	X	X

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. SDQ score is the normalized score out of 40 SDQ questions. Scores are reversed such that higher values indicate fewer behavioral issues. ***, **, * indicates significance at 1, 5, and 10%.

The results for the SDQs are presented in Table 9. First, we show in column (1) that there is no distinguishable difference between the SDQ scores of lottery winning and waitlist children. However, as seen in columns (3)-(5), we observe marginally significant positive effects amongst male children in lottery winning households. While the results are relatively imprecise, they provide further suggestive evidence of policy impacts on soft-skills.

In our survey with 6-17 year old children, we follow the literature on aspiration measurement (Bernard and Seyoum Taffesse, 2014) to ask about educational and occupational goals as well as children's general well-being. The results are presented in Table 10 where we aggregate educational goals, occupational goals, and well-being into indices, and build a composite index covering all measures. More information on aspiration measurement can be found in Appendix ??. Treatment effects of for lottery winner are positive across all measures, with statistically significant impacts for educational aspirations show in column (1). These increases in educational aspirations, combined with marginal increases in the other index measures, drives the significant increase in the composite index measure in column (4).

Table 10: Aspirations

	(1) Ed Aspir Index	(2) Occ Aspir Index	(3) WB Index	(4) Tot Index 1	(5) Tot Index 2
1(Won Lottery)	1.363**	0.333	0.461	1.359***	0.112
	(0.566)	(0.327)	(0.516)	(0.496)	(0.307)
1(Male)	-0.774	0.023	-0.152	-1.235***	0.704***
	(0.486)	(0.350)	(0.442)	(0.429)	(0.257)
Constant	0.414	-0.259	0.668	0.856	-0.015
	(0.576)	(0.304)	(0.560)	(0.524)	(0.300)
N	98	98	98	98	225
Waitlist Mean	-0.236	-0.071	-0.163	-0.217	-0.168
Birth Cohort FEs	X	X	X	X	X
Sample	13-17	13-17	13-17	13-17	6-17

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. Educational and Occupational Aspirations, and the well-being indices are the first principal components of the outcomes presented in Appendix Tables A.5, A.6, and A.7 respectively. The first Total Index is the first principal component of all education and occupation aspiration measures. The second Total Index additionally includes the well-being outcome measures. ***, **, * indicates significance at 1, 5, and 10%.

Income and Employment We do not find significant evidence that the policy increases children's earnings or employment. Focusing on children 17-35 years of age who are not currently enrolled in school, we show in Table 11 that treatment is not associated with higher employment rates, higher earnings, or more days worked in the past month. We believe two features of our setting may explain these findings. First, children in winning households stay in school longer, such that those who are not currently enrolled may be negatively selected within the sample of winning children. We expect that these results may change once more of the children impacted by the policy finish schooling and enter the labor market. Second, we note the extremely high levels of unemployment among young entrants into the labor market. The unemployment rate among 17-35-year-olds in our sample is over 50%, which matches recent reports in Ethiopian media (Sahlu, 2023). The lingering impacts of the recent civil war in Ethiopia and macroeconomic instability are likely causes. One interpretation of our results is that the condominium policy is insufficient to overcome these macro-conditions. Knowledge of these poor labor market conditions may also partially explain why children stay in school longer.

Table 11: Children's Income

	(1) Any Income	(2) Formal Emp	(3) Self Emp	(4) Days Worked	(5) Asinh(Prim Inc)	(6) Asinh(All Inc)
1(Won Lottery)	-0.012	0.020	-0.008	0.239	-0.077	-0.260
	(0.031)	(0.030)	(0.017)	(0.762)	(0.276)	(0.332)
1(Male)	0.061**	-0.067***	0.074***	1.415**	0.551**	0.548**
	(0.026)	(0.025)	(0.015)	(0.648)	(0.231)	(0.272)
Constant	0.430***	0.343***	0.042***	9.975***	3.736***	4.310***
	(0.031)	(0.030)	(0.015)	(0.770)	(0.275)	(0.326)
N	1515	1515	1515	1515	1515	1515
Waitlist Mean	0.480	0.330	0.086	11.297	4.250	4.898
Birth Cohort FEs	X	X	X	X	X	X
Sample	17-35	17-35	17-35	17-35	17-35	17-35

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. All outcome measures are for the past 30 days. Primary income is income from the individual's main income source. All income includes the primary source and any other income sources. ***, **, * indicates significance at 1, 5, and 10%.

4.5 Household-Level Impacts

Our results for children may be unsurprising if they simply represent a wealth effect: winning a condominium bequeaths households with a valuable, subsidized asset, dramatically increasing familial wealth. As was similarly found in Franklin (2020b), we observe large increases in family assets (Appendix Table A.8), home quality (Appendix Table A.9), and estimated home value (Appendix Table A.10). That is, regardless of whether the family occupies the unit that they win, condominium winning households are wealthier and live in higher quality homes.

As in Franklin (2020b), we find no impacts on overall adult employment rates, but observe 21% increases in formal sector employment for household heads driven (Appendix Table A.11). This formalization represents a specific type of job-switching, where adult heads of household are moving from causal to formal sector employment. Given the wage premium associated with formal sector employment, it is unsurprising that we also see substantial increases in household income (Appendix Table A.12), which Franklin (2020b) does not find. Specifically, in column (1) we see 10% increases in household head employment, 11% increases in household head and spouse income (column (2)), and 13% increases in total household income per capita.

We believe that the differences between our results and those in Franklin (2020b), which only studies short-term impacts of a particular lottery round, is likely due to our study looking at longer-term impacts. Consistent with this, we find that the impact on formal sector employment is increasing in years since winning the lottery. By looking across various lottery rounds, we may pick up on average treatment effects that might be missed by only considering a single lottery round. Furthermore, we believe that our results on household income are plausible given the wage premium associated with formal sector employment. In Appendix **??** we document changes in additional household-level outcomes.

5 Model & Mechanisms

As was previously noted, only 41% of lottery winners move into their unit after winning, while the remainder sell or rent out their unit to others. Thus, our reduced form estimates reflect the combined treatment effect for the two groups: movers and non-movers. The decision to move into a unit, conditional on winning, is an endogenous choice made by the household. Although we can focus only on the subset of households that move, we expect that these households differ from non-moving households in meaningful ways. Since we do not know, ex-ante, which of the waitlist households would have moved if they had won the lottery, treatment effect estimates in this reduced sample of movers may be contaminated by selection.[12]

Taking into account selection into condominium occupation presents an empirical challenge, but also allows for the opportunity to disentangle the channels through which the condominium policy may operate. All households who decide to purchase a government-subsidized condominium through the housing lottery experience a large increase in wealth due to the associated subsidy. Only households who move in, however, will experience the changes in neighborhood and peer characteristics that are the focus of the literature on neighborhood effects (Kling et al., 2007). In order to disentangle the effect of a parental wealth shock from the effects of moving into a condominium unit during childhood, we take two empirical approaches. In the first, we consider an interacted two-stage least squares (2SLS) model with multiple endogenous treatment states. To relax assumptions required for the estimation of causal effects in the 2SLS model, we extend the structural selection model developed in Kline and Walters (2016).

5.1 Interacted 2SLS

Before introducing our selection model, we first outline a multivariate 2SLS estimation strategy and consider its limitations.

Let *owning and occupying* (**O**) and *selling or renting out* the condominium (**W**) to be the treatment incentivized by the lotteries. We consider a single fall-back state (**S**) that represents the outside option of the household absent the winning a lottery.[13] Thus, households choose between three mutually exclusive treatments $k \in \{\mathbf{S}, \mathbf{W}, \mathbf{O}\}$. Let D_{ki} represent the binary indicator corresponding to each treatment, $D_{ki} = \mathbb{1}[D_i = k]$ for each

[12]Consider, for instance, a scenario where only financially vulnerable households sell their unit. Then a model considering the full set of waitlist households and only winners who move into their unit would be biased. The sign of the bias is unclear ex-ante.

[13]The model can be extended to separately consider selling versus renting out, or including rental in as an additional fall-back state. One simply needs instruments that differentially predict these choices.

household i such that:

$$D_{Si} + D_{Wi} + D_{Oi} = 1$$

We're interested in the impact of the causal effect of each treatment on a child's later life outcome Y (e.g. college attendance, income):

$$Y = Y_{Si}D_{Si} + Y_{Wi}D_{Wi} + Y_{Oi}D_{Oi}$$

where Y_{ki} is the potential outcome for household i if assigned to treatment k. This implies that $Y_{Wi} - Y_{Si}$ represents the effect of a shock to parental wealth, $Y_{Oi} - Y_{Si}$ represents the combined neighborhood and wealth effects, and under an assumption that the wealth effect associated with ownership is the same as the wealth effect associated with renting or selling the unit, $Y_{Oi} - Y_{Wi}$ represents the effect of condominium occupation net of the wealth effect.

This setup would suggest an OLS regression with two endogenous treatments, $\mathbf{W}_i = D_{Wi}$ and $\mathbf{O}_i = D_{Oi}$:

$$y_{h(i)} = \alpha_0 + \alpha_1\mathbf{O}_i + \alpha_2\mathbf{W}_i + \mathbf{X}_i\gamma + \epsilon_i \tag{3}$$

The lottery offer, as used in the reduced form analysis, $z_1 \in \{0, 1\}$, can be used as a first instrument. Fortunately, our setting allows for the creation of multiple sets of exogenous instruments leveraging temporal variation in lottery rounds and spatial variation in condominium locations. In our main specifications, we interact the lottery round indicator with the difference between realized and expected distance of the household to their condominium unit:

$$\mathbf{z}_2 = \mathbb{1}\,(\text{Won Lottery}) \times (\text{Distance to Unit} - \mathbb{E}\,[\text{Distance to Unit}])$$

The instrument z_2 is re-centered, as in Borusyak and Hull (2020), accounting for the fact that household residential location, and therefore the distance to condominium lottery units, is endogenous. Borusyak and Hull (2020) establish how re-centered instruments can avoid omitted variable bias when shocks (i.e. distance to condominium units) are exogenous but actor characteristics (i.e. household residential location) that influence the exposure to such shocks are not. The instrument z_2 is constructed by calculating, for each household, the distance to all lotteries that they could have won based on their residential history since the time of lottery registration. These distances are then weighted by the number of units in the condominium site that were dispersed. For waitlist households, this expectation is calculated for all units transferred through the first 13 rounds. For the lottery winning households, we calculate the expected distance in two ways: (1) the expected distance to all units transferred in years before a household wins; (2) the expected distance to all units transferred both before and after a household wins. We prefer the first approach, as household location after winning is impacted by the lottery offer. In practice, results look similar using either version of the instrument.

Here, z_2 can be thought of as a measure of whether the unit that was won was closer to or farther away from the household than expected, given its location of residence. Negative values indicate that a unit was close, while positive values indicate the opposite. Notably, the realized distance to a lottery condominium is only defined for lottery winning households, not waitlist households. By interacting the instrument with the lottery offer, all waitlist households are necessarily assigned a value of zero. We consider alternative constructions where waitlist households are assigned their expected distance, their maximum distance, or a fixed large value. These alternative instruments can be used directly, without an interaction with the lottery offer. Results are robust to these alternative instrument constructions.

With two instruments, the lottery indicator and the interacted deviated distance measure, we then estimate Equation (3) instrumenting $\mathbf{W}$ and $\mathbf{O}$ with z_1, z_2 controlling for the main effects of the interacting variables. Following Kline and Walters (2016) we can build additional instruments by further interacting z_1 and z_2 with exogenous (time-invariant) household characteristics. In the following, we outline the assumptions required for identification of causal effects in this model.

Let Z be an L vector of instruments with support $\mathcal{Z} \subseteq \mathbb{R}^{\mathbb{L}}$. Let $Y(k, z)$ denote latent potential outcomes for children or households with $k \in \{S, W, O\}$ and $z \in Z$. We adapt assumptions E.1-E.3 from Mogstad et al. (2020) and suppress household characteristics **X** for notational simplicity. Consequently, all assumptions should be thought of as holding conditional on **X**.

Exclusion

$$Y(k, z) = Y(k, z') \equiv Y(k) \text{ for all } k \in \{S, W, O\} \text{ and } z, z' \in \mathcal{Z} \tag{4}$$

Equation 4 is the traditional exclusion restriction as in Imbens and Angrist (1994), that instruments have no direct causal effects on outcomes except through choices, extended to a setting with multiple choices and instruments. Our instruments are functions of a random lottery offer, so violations of this assumption would require that lottery offers themselves, not choices influenced by the offer, have direct impacts on outcomes.

Independence

$$\mathbb{E}\left[Y(k)|Z, \{D(z)\}_{z\in\mathcal{Z}}\right] = \mathbb{E}\left[Y(k)|\{D(z)\}_{z\in\mathcal{Z}}\right] \text{ and } \mathbb{E}\left[Y(k)^2\right] < \infty \text{ for all } k \in \{S, W, O\} \tag{5}$$

$$\{D(z)\}_{z\in\mathcal{Z}} \text{ is statistically independent of Z} \tag{6}$$

The mean independence assumptions in Equation 5 are weaker than the full independence assumption of Imbens and Angrist (1994) and are common in the marginal treatment effect (MTE) literature (Mogstad et al., 2020; Borusyak and Hull, 2020). We observe the full set of information used by the AAHDA and the lottery is believed to have been implemented correctly.

Partial Unordered Monotonicity Divide $z \in \mathcal{Z} \subseteq \mathbb{R}^L$ into its lth component and all other $(L-1)$ components, z_{-l}.

For any $l \in L$ let (z_l, z_{-l}) and (z_l', z_{-l}) be any two points in $\mathcal{Z}$.Then either $D_k(z_l, z_{-l}) \geq D_k(z_l', z_{-l})$ or $D_d(z_l, z_{-l}) \leq D_d(z_l', z_{-l})$ almost surely for all $k \in \{S, W, O\}$. (7)

In a setting with multiple instruments and multiple treatments, the standard notion of monotonicity from the binary treatment, binary instrument case is insufficient to achieve identification. We adapt the notion of partial unordered monotonicity (PUM) from Mountjoy (2019) and Mogstad et al. (2020). The standard monotonicity assumption, when $L = 1$, implies partial monotonicity, and PUM is strictly weaker. While maintaining the "no defiers" condition from the binary case, this assumption requires that each shift in the instrument render each treatment state either weakly more or less attractive for all households.

Consider the case of the lottery offer, that greatly subsidizes treatment **W** and **O**. While this instrument is not targeted directly at either treatment state, PUM implies that households may only flow into one of these two treatment states in response to a winning a lottery.[14] For the deviated distance measure, conditional on the lottery offer, PUM requires that a household winning a condominium closer (further) from their residence only be more or less likely to *own and occupy* their unit. That is, winning a closer unit cannot induce some compliers into **W** and others into **O**. We believe this assumption to be reasonable in this setting: the deviated distance instrument can be thought of as a cost shifter for unit occupation, but not necessarily for unit rental or sale. Thus, we would expect that winning a lottery closer to one's home than expected increases the probability of occupation, which we confirm in the first stage of the analysis below.

According to the discussion in Kline and Walters (2016), Hull (2018), and Heinesen et al. (2022), this model can be characterized by the assumption of constant complier effects. In

[14]The assumption that the inequality in PUM holds strictly for at least some households implies an instrument relevance condition.

a setting with multiple unordered treatments, the local average treatment effect (LATE) estimated through an interacted 2SLS model is a weighted average of "subLATEs" reflecting compliers drawn from alternative treatment states. We can write the composite LATE_O as:

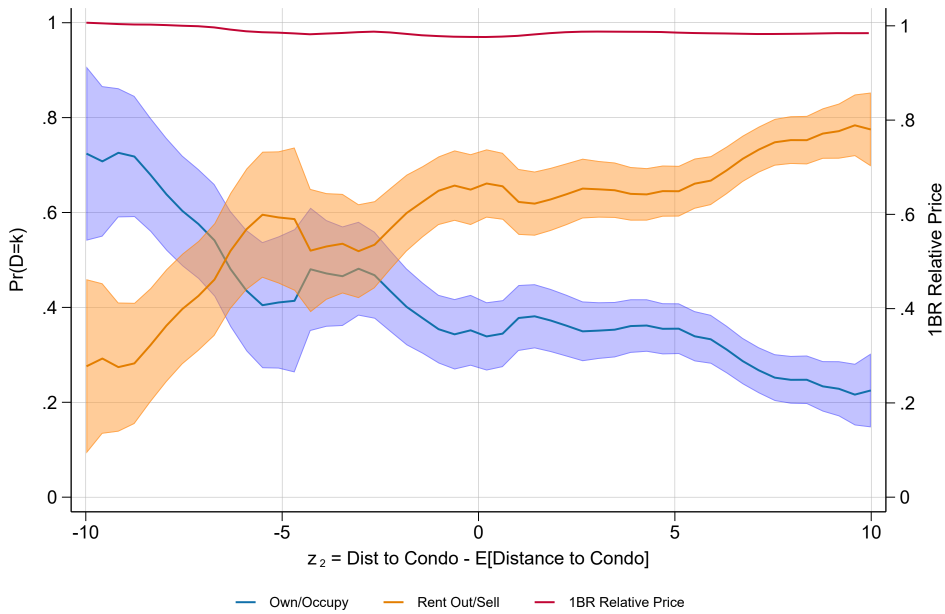
$$\text{LATE}_O = \omega_S \text{LATE}_{SO} + (1 - \omega_S)\text{LATE}_{WO}$$

where ω_S is the share of compliers would have remained in the base residential state absent the lotteries. The subLATE terms, LATE_{SO} and LATE_{WO}, each represent a LATE for a separate complier margin – those drawn from **S** to **O** and those drawn from **W** to **O** given their realization of the instruments, $z \in \mathcal{Z}$. These two complier groups can be loosely thought of as always takers of either **W** or **O**, conditional on winning a lottery, and marginal takers whose treatment is influenced by the cost of **O** versus **W**. The composite LATE_W is defined analogously. In general, the subLATEs are not separately identified, such that we can only interpret the composite LATE_O as a causal estimate of a given treatment state under an assumption of constant complier effects, $\text{LATE}_{SO} = \text{LATE}_{WO}$.

Based on the construction of our 2SLS model, we think of the coefficients for *ownership and occupation*(**O**) and *renting out or selling* (**W**) as representing average effects of condominium occupation, rental, or sale across all condominium neighborhoods. We are not modeling heterogeneity in outcomes based on variation in condominium neighborhood amenities like those documented in Section 4.2. One might be concerned that winning a condominium closer than expected implies that the unit is located near the city center, and consequently worth more. Then changes in occupation, rental, and sale rates estimated in the first stage of our 2SLS model will reflect not just distance but also the value of the unit that was won.

Fortunately, the spatial dispersion in the lottery condominium sites induces exogenous variation in these condominium neighborhood characteristics that we can control for or stratify by. If we assume that condominium rental value represents an index measure of neighborhood quality, which is observed for all condominium sites, then we can control for exogenous neighborhood quality directly.[15] But as we show in the red line in Figure 3, condominium value is only weakly correlated with z_2. This implies that variation in neighborhood and condominium quality cannot be driving our first stage results.

Figure 3: Recentered Distance (z_2) First Stage and Condominium Prices



This figure graphs local polynomial regressions of the predicted treatment states **O** and **W** for lottery winners as a function of the deviated distance instrument, z_2. These predictions are based on the first stage of the 2SLS regressions presented in Table 12 and use the left axis. Using the right axis, the red line graphs the average rental price of a 1BR condominium unit as a function of z_2 with $z_2 = -10$ normalized to 1.

Results from the interacted 2SLS model are presented in Table 12. We have a strong first stage, with Angrist-Pischke partial F's between 45 and 175. The first stage results imply that households winning a condominium one kilometer closer to their residence are 1.2-2.6 percentage points more likely to occupy those units. We depict this relationship graphically in Figure 3. In the second stage, we show that nearly all the positive treatment effects in educational enrollment and attainment accrue to children in households that own and occupy

[15] A similar assumption is often made in research studying neighborhoods in the United States and the MtO policy (see e.g. Kling et al. (2007)). These papers use neighborhood-level poverty rates as their quality index measure.

the condominium unit that they win. Columns (1)-(2) and (4)-(5) of Table 12 show positive and significant increases in active educational enrollment, secondary, and post-secondary attainment for children in lottery winning households. While positive, the coefficient on **W** is close to and indistinguishable from zero in all specifications. Together, these results imply that the positive intergenerational effects of lottery winning on education cannot be explained solely through increases in familial wealth and that the occupation of condominium units, in their associated neighborhoods, plays a key role in the intergenerational transmission of policy impacts.

Table 12: Interacted 2SLS

	(1) Enrolled	(2) Enrolled	(3) Primary	(4) Secondary	(5) Any Tertiary
Own/Occupy - **O**	0.090*	0.147*	-0.007	0.167**	0.161
	(0.047)	(0.077)	(0.074)	(0.085)	(0.124)
Rent Out/Sell - **W**	0.002	0.015	0.036	0.028	0.014
	(0.033)	(0.058)	(0.048)	(0.072)	(0.090)
N	4558	2614	2210	1812	1812
Waitlist Mean	0.742	0.558	0.913	0.620	0.406
Birth Cohort FEs	X	X	X	X	X
Sample	5-30	14-30	14-30	18-30	18-30
O First-stage F	78.30	63.67	58.43	44.94	44.94
W First-stage F	175.02	97.53	100.20	54.92	54.92

IVs: 1(Lotto Winner) ; 1(Lotto Winner) × (Distance to Unit - $\mathbb{E}$[Distance to Unit]) Weighted 2SLS regressions. Standard errors are clustered at the household level. The excluded instruments are z_1 and z_2. The first stage includes household head and sampling controls. The second stage includes child birth cohort, birth order, and gender fixed effects. First-stage F's are Angrist-Pischke partial F's. ***, **, * indicates significance at 1, 5, and 10%.

5.2 Selection Model

To relax the assumption of constant complier effects, we adapt the model developed in Kline and Walters (2016) to our context. We incorporate household preferences and potential outcomes over three treatment states: *ownership and occupation* (**O**), *renting out or selling* (**W**), and an outside option of *staying* outside condominium housing (**S**). Like the interacted

2SLS approach, we use the lottery instrument interacted with household and condominium site covariates (e.g. z_2) to identify causal effects for each treatment. The model allows for different margin-specific treatment effects, which was the primary limitation of the 2SLS approach.

Model Setup There is a population of households, indexed by h, each of which has one or more children, indexed by i, who have applied to the condominium lotteries. Assume that households have preferences over choices given by:

$$\begin{aligned} U_{h(i)}(S) &= 0 \\ U_{h(i)}\left(W, Z_{h(i)}\right) &= \Psi_W\left(Z_{h(i)}, X_{h(i)}\right) + \nu_{h(i)W} \\ U_{h(i)}\left(O, Z_{h(i)}\right) &= \Psi_{bm}\left(Z_{h(i)}, X_{h(i)}\right) + \nu_{h(i)O} \end{aligned}$$

where we normalize the value of staying in non-condominium housing to zero. Here, Ψ_k is the mean treatment-level utility for treatment k while ν_k are unobserved idiosyncratic components that vary across households. Households maximize state-specific utility:

$$D_{h(i)}(X, z) = \underset{k \in \{S,W,O\}}{\operatorname{argmax}} U_{h(i)}(k, z_1, z_2, X)$$

where $D_{h(i)}(X, z) = k$ represents the observed outcome. We further assume that the stochastic components are distributed multinomial probit:

$$\left(\nu_{h(i)O}, \nu_{h(i)W}\right) | X_{h(i)}, Z_{h(i)} \sim N\left(0, \begin{bmatrix} 1 & \rho(X_{h(i)}) \\ \rho(X_{h(i)}) & 1 \end{bmatrix}\right)$$

Following Heckman (1979), we can write potential outcomes for each treatment as:

$$E\left[Y_{h(i)k} | X_{h(i)}, Z_{h(i)}, \nu_{h(i)O}, \nu_{h(i)W}\right] = \mu_k\left(X_{h(i)}\right) + \gamma_{k,O}\nu_{h(i)O} + \gamma_{k,W}\nu_{h(i)W}$$

such that the γ terms govern selection on unobservables. They are assumed to enter into the potential outcome framework linearly and to be additively separable from observables.

Using the law of iterated expectations, we can write the conditional expectation of realized outcomes as:

$$
\begin{aligned}
E\left[Y_{h(i)}|X_{h(i)}, Z_{h(i)}, D_{h(i)} = k\right] = \mu_k\left(X_{h(i)}\right) + \gamma_{k,W}\lambda_W\left(X_{h(i)}, Z_{h(i)}, k\right) \\
+ \gamma_{k,O}\lambda_O\left(X_{h(i)}, Z_{h(i)}, k\right)
\end{aligned}
$$

where $\lambda_k\left(X_{h(i)}, Z_{h(i)}, D_{h(i)}\right) = E\left[\nu_{h(i)k}|X_{h(i)}, Z_{h(i)}, D_{h(i)}\right]\ \forall\, k \in \{O, W\}$ are variations of the Mills ratio terms from a two-step Heckman selection model.

In our setting, with almost zero never-takers (i.e. lottery winners who do not purchase their unit) and few always takers (i.e. waitlist households who purchase a unit) our model reduces to a single index model. To see this, we show in Figure 3 that conditional on winning a lottery, $Pr(\mathbf{O}) \approx 1 - Pr(\mathbf{W})$. Relatedly, less than 1% of waitlist households are in treatment state **O** and none are in treatment state **W**. This implies that there is a single threshold governing the selection of **O** versus **W** conditional on z_1. Consequently, we estimate a single Mills ratio term, λ_O that governs section into the occupation treatment state.[16]

Kline and Walters (2016) describe identification of this model using a two-step procedure. Following their work, in a first step we estimate the multinomial probit model using simulated maximum likelihood, relying on the GHK probability simulator. We then use the parameters from our probit model to build our single control function estimate, which is included in a second step regression to estimate treatment effects for compliers and selection-adjusted average treatment effects.

Identification is obtained under a few critical criteria. First, we require the additive separability of potential outcomes in observables and unobservables, as is common in this

[16] An alternative framing of this model would be as a sequential choice, where households only select between **O** and **W** conditional on an exogenous lottery offer.

literature (Heckman et al., 2006). This rules out selection coefficients (γ) depending on household characteristics, which is testable by comparing selection coefficients across different subsets of households. Further, we require that (1) the instruments shift choice probabilities across the support of z_2 conditional on winning the lottery; (2) the instruments must shift the conditional mean values of $\nu_{h(i)k}$ in a non-proportional manner for all $k \in \{O, W\}$.

Model Results The results of our second step estimates are presented in Table 13. Children in households that own and occupy their unit are 6-12pp more likely to be actively enrolled in school, 14pp more likely to finish secondary school, and 16pp more likely to start post-secondary education relative to children in households in the outside option state **S**. The control function term, λ_O exploits experimental variation in the lottery assignment and

Table 13: Control Function Model Estimates

	(1)	(2)	(3)	(4)	(5)
	1(Enrolled)	1(Enrolled)	Primary	Secondary	Post-Sec Att
O	0.060**	0.116***	0.027	0.137***	0.158***
	(0.028)	(0.041)	(0.039)	(0.050)	(0.060)
W	-0.015	-0.004	0.025	0.076	0.042
	(0.054)	(0.089)	(0.082)	(0.104)	(0.126)
λ_O	0.027	0.052	-0.033*	-0.085**	-0.048
	(0.027)	(0.047)	(0.017)	(0.033)	(0.042)
$\mathbf{O} \times \lambda_O$	-0.003	0.018	-0.009	0.128***	0.169***
	(0.038)	(0.049)	(0.023)	(0.048)	(0.057)
$\mathbf{W} \times \lambda_O$	-0.081	-0.098	0.046	0.016	-0.066
	(0.091)	(0.153)	(0.144)	(0.173)	(0.215)
Constant	0.669***	0.474***	0.874***	0.681***	0.464***
	(0.025)	(0.032)	(0.022)	(0.033)	(0.047)
N	4558	2614	2210	1812	1812
Waitlist Mean	0.742	0.558	0.913	0.620	0.406
Birth Cohort FEs	X	X	X	X	X
Sample	5-30	14-30	14-30	18-30	18-30

Weighted child-level OLS regressions. Standard errors are clustered at the household level. The first-stage multinomial probit specification is estimated using simulated maximum likelihood and includes sampling weights and household head controls. λ_O is the generalized Mills Ratio estimated in the first-stage multinomial probit regression. ***, **, * indicates significance at 1, 5, and 10%.

the recentered distance to the condominium unit. Adjusting for selection on unobservables decreases the estimated average impact of **O** relative to **S** when compared to the results in the 2SLS model. The results of the control function estimates are somewhat imprecise, however we reject the hypothesis of no selection on gains for secondary and post-secondary education in columns (4) and (5). In these cases, we document Roy-style selection in which children whose families are more likely to own and occupy the unit that they win achieve larger gains in educational attainment when shifted from the outside option to occupation of the condominium unit. This suggests smaller gains for households with unobservables that make them less likely to own and occupy their unit. The results for renting out or selling the unit are similar consistent with the 2SLS model, with all estimates indistinguishable from zero. It is worth noting, however, that the coefficients on **W** in columns (4) and (5) are positive and larger than their 2SLS counterparts.

6 Conclusion

The expansion of public housing and slum redevelopment are two of the primary policy levers used by policymakers in low-income countries to manage urban growth. This paper has empirically explored the largest of these policies on the African continent to understand how it impacts children's human capital and household welfare.

By interviewing households an average of 8 years after winning a condominium, we are able to observe outcomes that may be missed in short-run followups. Further, through extensive surveys with households and children, we are able to document changes in key human capital outcomes that are unavailable in administrative data. The empirical results show that winning a condominium meaningfully changes neighborhoods of residence and significantly increases children's educational attainment, cognitive performance, and aspirations. These impacts are concentrated amongst children in households that own and occupy the unit that they win. This suggests that the policy's impacts cannot be explained through a wealth effect alone.

Using an instrumental variables approach and a structural model, we show that households winning a condominium relatively close to their existing residence is an important driver of their decision to occupy the unit, but this is mediated by selection. Taken together, these results suggest that in-site housing redevelopment, as opposed to peripheral construction, would substantially increase the policy's impacts on children and households.

We believe that our results may help inform policy in other contexts. Housing upgrading and slum redevelopment in Ethiopia is not an outlier, as it shares many characteristics with policies that are actively implemented in low-income, urbanizing countries around the world. We hope that based on our findings, deeper consideration may be given to long-run policy effects on children and the importance of homeownership and neighborhoods in developing cities.

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A Tables and Figures

Figure A.1: Condominium Openings

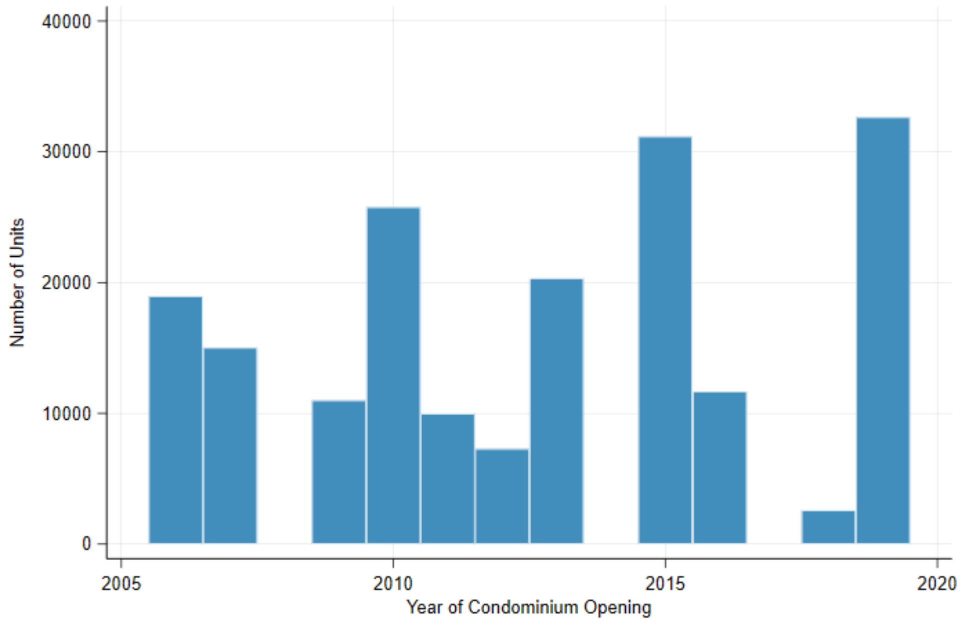

Table A.1: Condominium Openings

	Total	Studio	1 BR	2 BR	3 BR
2006	18972	4087	5677	8091	1117
2007	15031	2592	5070	6263	1106
2009	11005	2965	3679	3626	735
2010	25775	5882	11459	6131	2303
2011	9981	1255	4457	2742	1527
2012	7300	2952	3594	433	321
2013	4991	-	-	-	-
2015	31178	6573	14293	6695	3617
2016	11695	2103	5392	2723	1477
2018	2602	246	1041	123	1192
2019	32653	1248	18823	7127	5455
Total	171183	29903	73485	43954	18850

Figure A.2: Neighborhood Quality

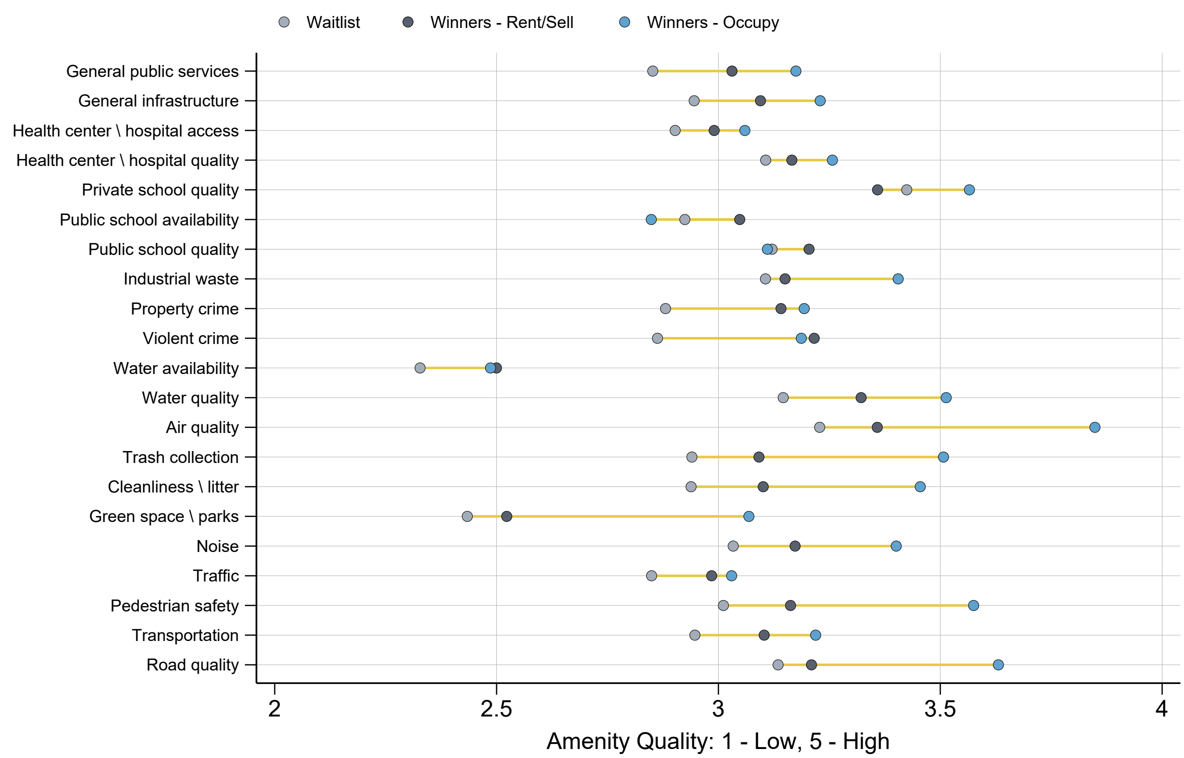

Table A.2: Neighborhood School Quality and Density

	Avg School Quality		Num Primary Schools		Num Secondary Schools		Num Public Schools		Num Private Schools	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1(Won Condo)	-0.008***	-0.002	1.322***	0.567	0.080	0.281*	0.074	0.043	1.266***	0.716
	(0.003)	(0.003)	(0.344)	(0.409)	(0.128)	(0.164)	(0.085)	(0.112)	(0.399)	(0.492)
1(Occupy)		0.010		0.218		-0.504***		-0.242*		-0.083
		(0.007)		(0.580)		(0.192)		(0.145)		(0.709)
Winner X Occupy		-0.022***		1.717**		-0.113		0.264		1.441
		(0.008)		(0.744)		(0.263)		(0.178)		(0.903)
Constant	0.637***	0.634***	6.771***	7.139***	1.955***	1.885***	2.707***	2.738***	5.874***	6.158***
	(0.011)	(0.011)	(1.296)	(1.257)	(0.480)	(0.474)	(0.321)	(0.321)	(1.443)	(1.442)
N	2234	2234	2234	2234	2234	2234	2234	2234	2234	2234
Wait/Non-Dwell Mean	0.648	0.645	6.574	6.803	1.685	1.831	2.636	2.627	5.631	6.000
Samp Weights	X	X	X	X	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X	X	X	X	X

Figure A.3: Neighborhood Distance

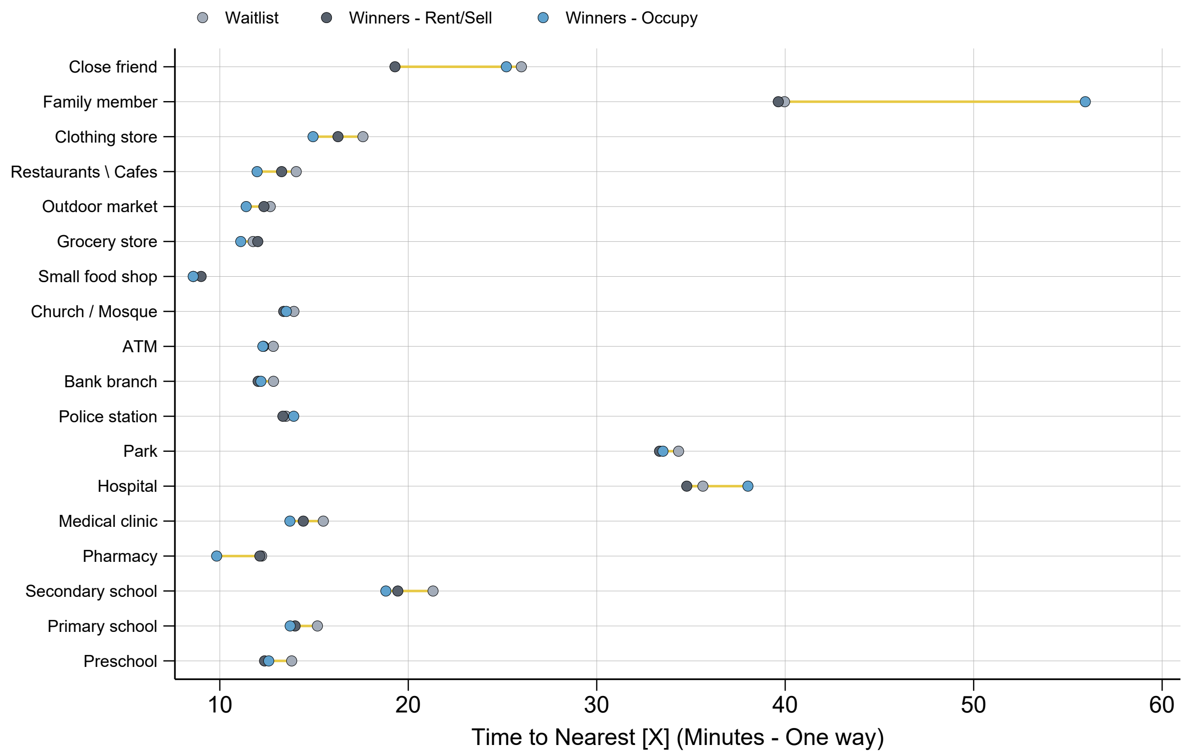

Table A.3: Neighborhood Spending Per Capita

	Education Spending Pc		Women/Children Spending Pc		Health Spending Pc	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Won Condo)	-8.002***	-4.623***	-7.899***	-4.439***	-4.501***	-2.880***
	(1.333)	(1.251)	(1.340)	(1.329)	(1.043)	(0.978)
1(Occupy)		-4.691		-4.808		-1.911
		(3.698)		(3.117)		(2.128)
Winner X Occupy		-4.806		-4.918		-2.569
		(3.940)		(3.486)		(2.431)
Constant	34.671***	33.254***	32.490***	31.040***	19.964***	19.263***
	(4.458)	(4.185)	(4.889)	(4.504)	(3.595)	(3.547)
N	2234	2234	2234	2234	2234	2234
Wait/Non-Dwell Mean	35.271	34.993	31.664	31.569	20.824	20.691
Samp Weights	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X

Figure A.4: Variation in Neighborhood Characteristics

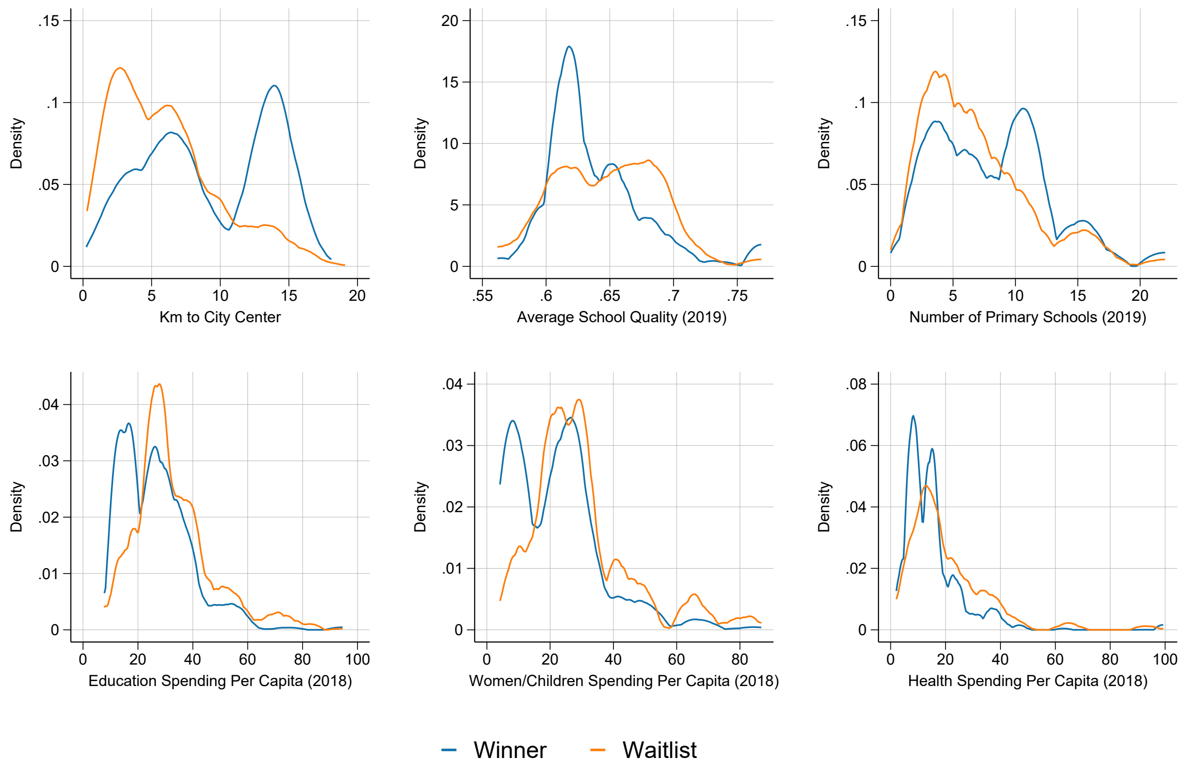

Table A.4: Cognitive Tests - Components

	(1) Math	(2) Num Patterns	(3) Counting	(4) Math Index	(5) Writing	(6) Sentences	(7) Words	(8) Letters	(9) Literacy Index
1(Won Lottery)	0.812**	-0.109	0.213	0.331	-0.002	0.240	0.319	0.069	0.164
	(0.394)	(0.326)	(0.281)	(0.364)	(0.336)	(0.263)	(0.208)	(0.150)	(0.385)
1(Male)	0.741**	0.272	0.282	0.609**	0.521*	0.272	0.349	0.316*	0.522
	(0.309)	(0.274)	(0.258)	(0.302)	(0.289)	(0.251)	(0.216)	(0.178)	(0.349)
Constant	2.391***	1.933***	4.890***	-0.049	1.792***	1.644***	2.115***	2.324***	-0.780*
	(0.422)	(0.294)	(0.309)	(0.337)	(0.336)	(0.326)	(0.244)	(0.239)	(0.432)
N	127	127	127	127	127	127	127	127	127
Waitlist Mean	2.513	1.600	5.350	-0.261	2.100	2.050	2.406	2.688	-0.303
Birth Cohort FEs	X	X	X	X	X	X	X	X	X
Sample	6-12	6-12	6-12	6-12	6-12	6-12	6-12	6-12	6-12

Figure A.5: Educational Attainment

Table A.5: Child Educational Aspirations

	(1) School Satis	(2) School Dissat	(3) Aspire Adv Deg	(4) Ed Aspir Likely	(5) Read Better than most	(6) Math Better than most	(7) English Better than most	(8) Science Better than most
1(Won Lottery)	-0.238	-0.005	0.101	-0.350**	0.373***	0.329*	0.340***	0.393**
	(0.346)	(0.233)	(0.107)	(0.143)	(0.123)	(0.186)	(0.117)	(0.192)
1(Male)	-0.162	0.491**	-0.347***	-0.016	-0.203	0.007	-0.358***	-0.068
	(0.283)	(0.204)	(0.112)	(0.123)	(0.142)	(0.166)	(0.107)	(0.167)
Constant	3.631***	0.989***	0.748***	1.143***	0.496***	0.161	0.541***	0.209
	(0.385)	(0.231)	(0.130)	(0.128)	(0.172)	(0.187)	(0.115)	(0.215)
N	225	220	98	98	98	98	98	98
Waitlist Mean	2.545	1.058	0.762	0.778	0.300	0.253	0.237	0.170
Birth Cohort FEs	X	X	X	X	X	X	X	X

Figure A.6: Years of Childhood Exposure

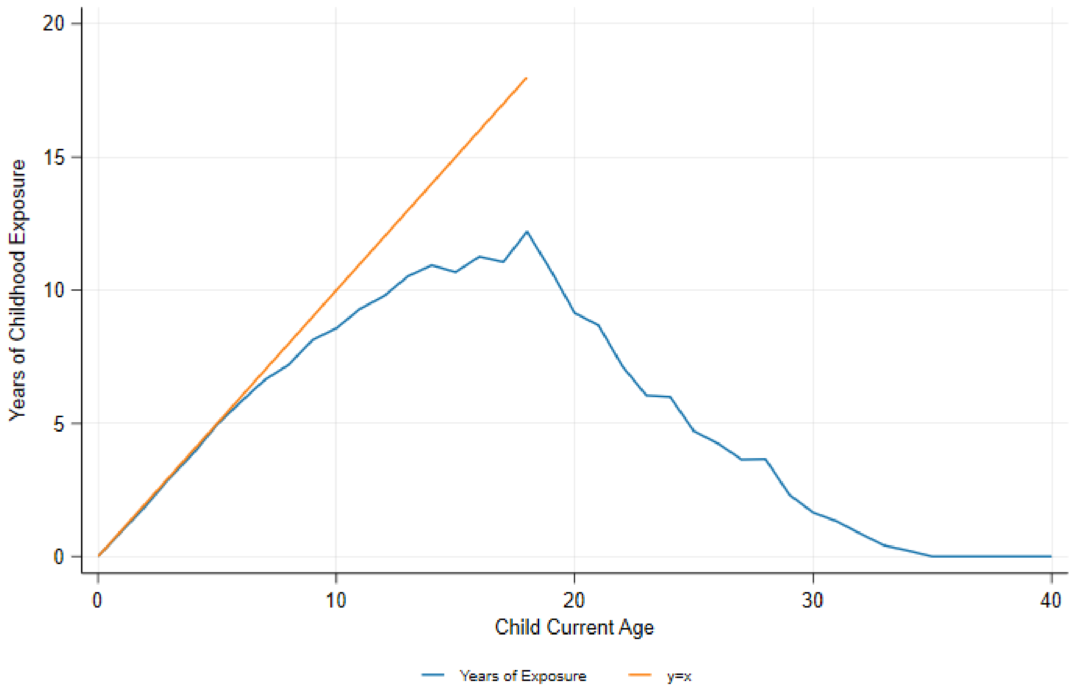

Table A.6: Child Occupational Aspirations

	(1) Aspire Adv Occ	(2) Occ Aspir Likely	(3) Aspir Occ Edu Constraint	(4) Apsir Occ Fam Constraint	(5) Likely Adv Occ
1(Won Lottery)	0.115	0.022	-0.241**	-0.064	0.083
	(0.078)	(0.060)	(0.107)	(0.108)	(0.098)
1(Male)	-0.183**	-0.040	0.347***	-0.083	-0.035
	(0.071)	(0.057)	(0.110)	(0.110)	(0.101)
Constant	0.827***	0.945***	0.216	0.256**	0.787***
	(0.075)	(0.052)	(0.130)	(0.112)	(0.083)
N	225	98	98	98	98
Waitlist Mean	0.741	0.857	0.571	0.190	0.794
Birth Cohort FEs	X	X	X	X	X

Table A.7: Children's Well-Being

	(1) Ladder Current	(2) Ladder Future	(3) 1(Future Better)	(4) Future Diff	(5) Nhood Satis	(6) Nhood Dissat	(7) Num Friends	(8) Num Friends School	(9) 1(Treated Well)
1(Won Lottery)	-0.485	0.132	0.187**	0.617	0.178	-0.292**	-0.077	1.520	0.102
	(0.470)	(0.631)	(0.092)	(0.647)	(0.403)	(0.124)	(0.737)	(1.595)	(0.088)
1(Male)	0.044	-0.125	-0.137	-0.169	0.007	-0.118	2.914***	3.012**	0.027
	(0.431)	(0.637)	(0.110)	(0.711)	(0.292)	(0.117)	(0.672)	(1.383)	(0.061)
Constant	4.989***	7.511***	0.694***	2.521***	2.540***	0.806***	2.322***	3.755***	0.822***
	(0.423)	(0.752)	(0.144)	(0.863)	(0.450)	(0.146)	(0.713)	(1.102)	(0.061)
N	97	97	98	97	225	225	225	225	225
Waitlist Mean	4.903	8.194	0.902	3.290	2.042	0.811	4.281	4.536	0.776
Birth Cohort FEs	X	X	X	X	X	X	X	X	X

Table A.8: Household Assets

	(1) As Index 1	(2) Com Bldgs	(3) Houses	(4) Apts	(5) Ag Land	(6) As Index 2
1(Won Condo)	0.398***	0.015*	0.055**	0.300***	-0.005	0.420***
	(0.109)	(0.008)	(0.026)	(0.028)	(0.009)	(0.109)
Constant	-1.054***	-0.055	0.113	0.120	-0.033	-1.059***
	(0.357)	(0.040)	(0.147)	(0.123)	(0.031)	(0.357)
N	2269	2269	2269	2267	2269	2267
Waitlist Mean	-0.527	0.000	0.004	0.004	0.019	-0.544
Samp Weights	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X

Table A.9: House Quality

	(1) Imp Floor	(2) Iron Roof	(3) Imp Walls	(4) Qual Index 1	(5) Area PP	(6) Qual Index 2
1(Won Condo)	0.083***	-0.103***	0.285***	1.051***	3.834***	1.219***
	(0.023)	(0.032)	(0.035)	(0.137)	(0.691)	(0.142)
Constant	0.742***	0.866***	0.197	-1.972***	3.755	-2.099***
	(0.080)	(0.132)	(0.138)	(0.519)	(2.430)	(0.522)
N	2269	2269	2269	2269	2269	2269
Waitlist Mean	0.784	0.831	0.275	-0.914	8.196	-1.042
Samp Weights	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X

Table A.10: House Value

	(1) Ln(Rent) Sim	(2) Ln(Rent) Est	(3) Ln(Rent)	(4) Rent Val All	(5) Ln(Sale) Sim	(6) Ln(Sale) Est
1(Won Condo)	0.494***	0.324	1.294***	1.672***	-0.510*	0.383
	(0.088)	(0.197)	(0.163)	(0.135)	(0.290)	(0.566)
Constant	7.445***	8.339***	8.576***	7.583***	14.140***	11.329***
	(0.250)	(0.452)	(0.493)	(0.401)	(1.360)	(2.158)
N	703	359	1268	1627	235	390
Waitlist Mean	7.909	8.833	6.310	6.365	15.015	14.512
Samp Weights	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: Household Head Employment

	(1) Any Work	(2) Formal Emp	(3) Self Emp	(4) Casual Emp	(5) Unemployed
1(Won Condo)	-0.009	0.086***	-0.047	-0.044***	-0.012
	(0.026)	(0.031)	(0.029)	(0.014)	(0.022)
Constant	1.147***	0.319**	0.567***	0.270***	0.190**
	(0.120)	(0.129)	(0.102)	(0.062)	(0.096)
N	2267	2267	2267	2267	2267
Waitlist Mean	0.813	0.419	0.297	0.083	0.117
Samp Weights	X	X	X	X	X
HHH Controls	X	X	X	X	X

Table A.12: Household Income

	(1) HHH Tot Inc	(2) HHH + Spouse Tot Inc	(3) HH Tot Inc Pc
1(Won Condo)	0.595***	0.747***	0.794***
	(0.226)	(0.238)	(0.215)
Constant	4.193***	4.433***	3.670***
	(0.899)	(0.996)	(0.879)
N	2265	2269	2269
Waitlist Mean	6.824	6.783	6.320
Samp Weights	X	X	X
HHH Controls	X	X	X

Table A.13: Household Savings

	(1) 1(Bank Acct)	(2) Any Sav 12 Mo	(3) Asinh(Sav 12 Mo)	(4) Asinh(Tot Sav)	(5) Sav Index
1(Won Condo)	-0.000	-0.038	-0.378	-1.116***	-0.219*
	(0.011)	(0.034)	(0.334)	(0.387)	(0.120)
Constant	1.015***	0.398***	4.306***	5.541***	0.516
	(0.033)	(0.119)	(1.164)	(1.486)	(0.425)
N	2269	2269	2213	2213	2213
Waitlist Mean	0.983	0.315	2.872	5.109	0.088
Samp Weights	X	X	X	X	X
HHH Controls	X	X	X	X	X

Standard errors in parentheses
Index 1 is the first principal component from the other outcome variables in the table.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.14: Children's Education - Sibling Design

	(1) 1(Enrolled)	(2) 1(Enrolled)	(3) Primary	(4) Secondary	(5) Post-Sec Att	(6) Post-Sec Att
Exposure (Years)	0.011*	0.018*	-0.004	-0.020	0.007	0.003
	(0.007)	(0.010)	(0.008)	(0.020)	(0.021)	(0.016)
1(Male)	-0.024	-0.041	0.040	0.058	-0.056	-0.059
	(0.021)	(0.036)	(0.029)	(0.039)	(0.063)	(0.051)
Constant	0.701***	0.485***	0.909***	0.734***	0.494***	0.505***
	(0.028)	(0.039)	(0.025)	(0.050)	(0.054)	(0.038)
N	3892	1858	1471	1200	1200	1571
No Exposure Mean	0.639	0.387	0.916	0.718	0.500	0.490
HH FEs	X	X	X	X	X	X
Birth Cohort FEs	X	X	X	X	X	X
Sample	5-30	14-30	14-30	18-30	18-30	18-35