# 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 a natural experiment from the largest expansion of public housing on the African continent to estimate the effects of housing on the development of human capital of children. Random lotteries for ownership of condominium houses in Ethiopia lead to large gains in educational attainment for children in winning families: the policy increases active educational enrollment by 4.5-11%, secondary school completion rates by 10.5% and post-secondary attendance rates by 16%. The policy additionally leads to significant increases in measures of children's cognitive and socio-emotional development, but does not change rates of employment or income. Winning heads of household experience 8p.p. increases in formal sector employment rates, which increases household income, but these effects only arise in the medium-run. Using instruments derived from spatial variation in condominium locations and temporal variation in their dates of completion, we show that treatment effects are concentrated amongst households which own and occupy the unit that they win. A structural model allows us to characterize selection across treatment states, ruling out that our results can be explained through a wealth effect alone.

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

Modern urbanization is concentrated in the Global South: in the past two decades low-income countries have been urbanizing 4-8 times faster than North America and Europe (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, 1.3 billion Africans will live in urban settings, nearly triple the number of urban Africans today (Laros and Jones, 2014). 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 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 policies in low-income settings generally conclude that they fail to be transformative, with null or negative impacts on most household-level economic outcomes (Barnhardt et al., 2017; Franklin, 2020; Belchior et al., 2023; Rojas Ampuero, 2022) 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? Researchers have proposed that the decreases in social cohesion, labor market access, and public service quality associated with relocation to far-flung neighborhoods which depress policy take-up ultimately outweigh improvements in home quality. We consider these and two alternative explanations. First, by focusing on household-level outcomes, much of the

[1] We use the term "slum" in a manner consistent with the UN-HABITAT definition (UN-HABITAT, 2002). Households are said to be living in 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.

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; Kumar, 2020; Rojas Ampuero, 2022). Second, the typical reduced form analysis of these policies may disguise heterogeneity across "hidden treatments" (Rothstein and Von Wachter, 2017) due to households' making endogenous choices over their neighborhood of residence and *how* they interact with the policy.

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 parental wealth and neighborhoods of residence impact the human capital development of children? (2) What are the relative contributions of these channels? 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 condominium lottery applicants in Addis Ababa, Ethiopia. We combine these 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).

Our analysis relies on the lottery mechanism employed by the AAHDA to assign subsidized condominium units to applicants. Since its inception, 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. Through 2023, more than 200,000 units were completed and transferred to residents via random lottery in 15 lottery rounds. We compare children in lottery winning households to those in similar households that remain on the waitlist for a condominium unit. Critically, the physical location of winning households' units and the lottery round in which they win are exogenous, allowing us to use spatial and temporal variation to disentangle mechanisms.[2]

Our setting diverges from most that have been previously studied (Van Dijk, 2019; Chyn, 2018; Chetty et al., 2016; Pinto, 2021), as the lotteries in our study are for home *ownership*, not rental. This common feature of housing policy in lower-income settings expands household decision sets – they can live in the unit, sell it, or rent it out (Barnhardt et al., 2017; Kumar, 2020; Belchior et al., 2023). 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 pool impacts driven by direct exposure to condominiums and their associated neighborhoods with impacts due to increases in parental 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 move into the unit that they win.

First, 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 winning a unit (Franklin, 2020). Households that win a condominium are given full ownership, able to rent out their unit immediately and with the opportunity to sell their unit after a 5-year embargo.[3] In our sample, 82% of winning households still own the unit that they won, even up to 14 years after winning. However, many winning households chose not to move into their unit: 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 40% of the winning households own and occupy the unit they won.

Lottery winning households live in better neighborhoods and in higher quality homes,

[2]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.

[3]In practice, this embargo was often ignored.

albeit 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, 2022). However, we find no evidence of thinner social networks for lottery winners in measures of neighborhood social connectivity and trust. We take this as evidence of the capacity of social networks to develop in the new condominium sites.

We next show that the 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 socio-emotional 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%. 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. The impacts on educational attainment are larger than many school expansion programs, Head Start in the United States (Bailey et al., 2021), and 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 – primary schools attended by lottery winning and waitlist children look similar across a battery of measures, and while lottery winning children are slightly more likely to attend flagship public universities, these results are imprecise. The increase in post-secondary attendance is distributed evenly across these public universities and technical training institutes.

In a small sample of children that we interviewed directly, we see substantial gains in measures of cognition and precision. These results are consistent with household wealth and residential stability being important drivers of human capital beyond their impacts on household wealth (Bursztyn and Jensen, 2015; Card and Giuliano, 2013; Billings and Hoekstra, 2023). 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.

Outside of education and cognition, we provide suggestive evidence of improved soft skills and socio-emotional development for children in lottery winning households. We find small improvements in socio-emotional development in the Strengths and Difficulties Questionnaire (SDQ) administered to children's parents, but only for male children. Results from the survey with children further imply that the policy changes educational and occupational aspirations. While we are weakly powered to detect significant differences, children in lottery winning households are more likely to aspire to an advanced degree or an occupation that requires an advanced degree, and are more confident in their academic performance. They are also more optimistic about their future and more satisfied with the neighborhood in which they live.

We do not find significant evidence that the policy increases children's earnings or employment. 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. Consequently, 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 document extremely high levels of unemployment among young entrants into the labor market in Addis Ababa. The unemployment rate among 17-35 year-olds in our sample is 52%, which matches recent reports in Ethiopian media. The lingering impacts of the recent civil war in Ethiopia, rapid inflation, and macroeconomic instability are likely causes.

Household-level impacts of the Ethiopian condominium policy were previously studied in Franklin (2020). The author shows that households that win a condominium lottery have increased assets, are more likely to change jobs, and have longer commute times, but there are no impacts on household labor income or employment. We replicate the results in Franklin (2020) with one exception: we find that lottery winning households have higher incomes, driven by heads of household moving from casual to formal sector employment. Our study functions as a medium-run follow-up to Franklin (2020) which focused on short-run outcomes. 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.

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 rich sets of instruments that influence the household's decision of whether to *own and occupy, 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). 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 on average, households winning condominiums that are closer to their current residence than expected are more likely to own and occupy their units. This is consistent with evidence on the preference for maintaining social ties being an important determinant of housing policy response (Barnhardt et al., 2017; Franklin, 2020) and suggests that "match effects" may lead to heterogeneous treatment effects and selection. In our IV models, 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. This implies that the inter-generational impacts of this policy cannot be explained by a wealth effect alone.

[**TO BE FINISHED**] 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).

This paper contributes to three strands of literature. First, we contribute to the literature on the impacts of public housing and slum re-development. We are the first to show large, positive impacts of a housing policy in a low-income setting (Barnhardt et al., 2017; Franklin, 2020; Hoagland, 2020). Evaluating a policy that focuses on *ownership* rather than rental subsidies distinguishes us from more commonly studied programs (Kling et al., 2007; Pinto, 2021; Van Dijk, 2019; Chetty et al., 2016) or those that limit household's ability to sell or rent out the unit that they own (Belchior et al., 2023; Kumar, 2020). By considering outcomes in the medium-term, we are able to document treatment effects on household employment formalization and income that were not observed in short-run evaluations of the same policy (Franklin, 2020).[4]

Second, we contribute to the literature on the inter-generational impacts of public policy. Failure to consider longer-term effects on children may dramatically underestimate a policy's impact (Bailey et al., 2020, 2021) 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; (Rojas Ampuero, 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 home ownership on children and consider measures of human capital that are unavailable in administrative data.

Finally, studies on the impacts of public housing generally emphasize reduced form, intent-to-treat results that do not account for households' endogenous response to treat-

[4]Other than Franklin (2020), the only other paper to study the Ethiopian housing lotteries is Andersen et al. (2020) which aims to document how winning a lottery changes household preferences for redistribution.

ment. By incorporating an explicit model of selection, drawing on 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). We are the first to bring these methods to an evaluation of a policy in a low-income setting and in doing so are one of the first characterize the importance of neighborhoods in a developing city (Belchior et al., 2023).

We believe that our results may help inform policy in other contexts. The urbanization movement in Ethiopia is not an outlier, but rather representative of changes across the Global South. We hope that based on our findings, deeper consideration may be given to long-run policy effects on children and the importance of neighborhoods in developing cities.

## 2 Context

Rapid urban population growth has stressed the existing housing stock in Addis Ababa and raised rental prices. Private sector development has not kept up with demand – over 70% of households in Addis Ababa live in slums or informal settlements (Franklin, 2020). Beginning in the early 2000's, 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 lottery in 2005 and 2013. An estimated 50% of all households in Addis Ababa registered for the program, with over 900,000 applications in total. Only one application was allowed per household, and to be eligible 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[5] but not the 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 government employees, female-headed households, 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 (Franklin, 2020).

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.[6] 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 downpayment 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 the cost of production per unit (Franklin, 2020)

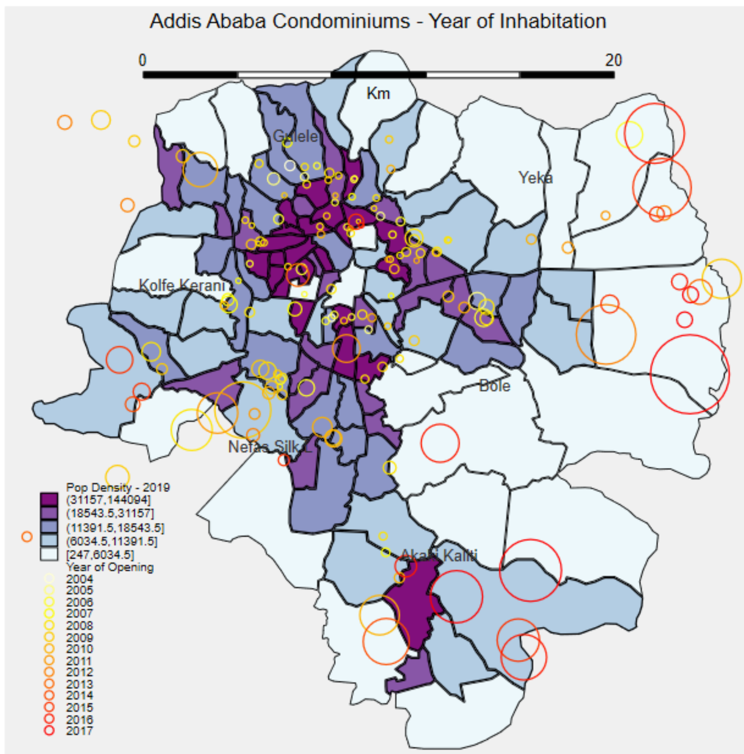
While early lottery rounds included more centrally located units, the condominium policy functionally reallocated families from low-quality, dense housing in the city center to higher-quality housing on the outskirts of the city. Due to their peripheral location, many condominium neighborhoods had worse labor market access, sparser social networks, and

[5]There are studios, 1-bedroom, 2-bedroom, and 3-bedroom units.

[6]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.

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.

**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.

Relative to public housing programs in North America that focus on moving families from "bad" neighborhoods to "good" ones (Kling et al., 2007; Chetty et al., 2016; Oreopoulos, 2003), heterogeneity of neighborhood quality along multiple dimensions in our context makes the 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, with residents sorting amongst neighborhoods. In our case, the scope of 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.

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. Winning households may only sell their condominium unit after an embargo period of five years. There is some evidence that a small share of households illegally sell their units before the five year embargo (Andersen et al., 2020). There remains the potential for sorting and the policy consequently 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 household to participate in our survey.

## 3.1 Sampling Frame Restrictions

Before sampling households, 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 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 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 [7], and the sub-city where the household resided at the time of their registration. In total, there were 60 strata. We sampled a total of 1,648 lottery winning households.

Waitlist households were selected using simple stratified random sampling, relying on the same strata as the winners above. Since waitlist households have not yet been assigned

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

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, yet had failed to win. 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.

**Site Re-sampling** Due to security issues, 10 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 B.

### 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, 97% of contacted respondents still lived in Addis Ababa and amongst these, 92% had a child under 35, with balance across the treatment and waitlist groups. In total, 86% of contacted households were eligible for the survey.

**Summary Statistics** Basic summary statistics for our surveyed households are displayed in Table 4. 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.

**Attrition** We successfully contacted 64% of households. This is comparable to similar phone surveys conducted in this setting and attrition was largely due to our reliance on dated administrative data. 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 of the household. We additionally surveyed a subset of children directly. The survey with children covered aspirations, education, measures of fluid intelligence, 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. This data is used to build measures of post-secondary 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 bi-annual road network panel of all built roads 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 combine survey data collected by the Central Statistics Agency and Stanford University's African Urbanization and Development Research Initiative (AUDRI). 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 impacts households and their children, we begin by documenting how the policy was utilized. 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 their unit immediately and with the opportunity to sell their unit after a 5-year embargo.[8] In our sample, 82% of winning households still own the unit that they won, even up to 14 years after winning. However, many winning households chose not to move into their unit. In Table 1 we see that of winning households, 35% rent out their unit, 17% sell, and a small share either leave the unit unoccupied or allow to be used rent-free by friends and family. The remaining 40% of the winning households live in and own the unit they won.

**Table 1:** Condominium Usage

| | (1)<br>All | (2)<br>Winners | (3)<br>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 |

Next, we show that winning a condominium is associated with meaningful changes in neighborhood quality. In Table 2 we show that an index composed of household perceptions of their current neighborhood is 0.54 SDs larger for winning households. However, column (2) implies that the perceived increases in neighborhood quality are largest amongst waitlist households who rent in a condominium and winners who also dwell in their unit. Appendix Figure A.2 displays how each of the index components varies based on lottery winner and

**Table 2:** Neighborhood Quality and Proximity

| | Qual Index | | Prox Index | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 1(Won Condo) | 0.863*** | 0.609*** | -0.310* | -0.317 |
| | (0.220) | (0.232) | (0.185) | (0.221) |
| 1(Dweller) | | 1.151*** | | -0.986*** |
| | | (0.294) | | (0.232) |
| Winner X Dweller | | -0.266 | | 0.780** |
| | | (0.420) | | (0.332) |
| Constant | -0.299 | -0.236 | 2.209*** | 2.278*** |
| | (0.840) | (0.821) | (0.721) | (0.717) |
| N | 2269 | 2269 | 2269 | 2269 |
| Wait/Non-Dwell Mean | -0.455 | -0.380 | 0.141 | 0.151 |
| Samp Weights | X | X | X | X |
| HHH Controls | X | X | X | X |

condominium dweller status.

We similarly construct an index of perceived neighborhood proximity to key amenities and social venues in columns (3) and (4) of Table 2. We see that condominium winners live significantly further from these key amenities. Looking at the components of the index in Appendix Figure A.3 we see that nearly all of this effect can be explained by condominium dwellers living further from close friends and family members, not public services or other amenities. This echos findings from Barnhardt et al. (2017) and Franklin (2020) that suggest that moving to new housing may disrupt social networks. We discuss additional results on household social networks and neighborhood cohesion in Section 4.4.

## 4.2 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. Our primary reduced form specification estimates an intent-to-treat (ITT) effect for households and families who win condominiums

[8]This embargo was often ignored.

in their youth. Specifically we estimate:

$$y_{h(i)} = \alpha + \beta_1 T_{h(i)} + \mathbf{X}_{h(i)}\gamma + \epsilon_{h(i)} \tag{1}$$

where $T$ is a treatment indicator for winning households for child $i$ in household $h$. $\mathbf{X}$ is a vector of child and household covariates, including the child birth cohort.[9]

Our setting allows for two additional reduced form specifications for estimating impacts on children. Drawing on the literature on inter-generational 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 linear exposure and sibling designs.

$$y_{h(i)} = \alpha + \beta_1 Exp_{h(i)} + \mathbf{X}_{h(i)}\gamma + \epsilon_{h(i)} \tag{2}$$

$$y_{h(i)} = \alpha + \beta_1 Exp_{h(i)} + \mathbf{X}_{h(i)}\gamma + \kappa_h + \epsilon_{h(i)} \tag{3}$$

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. In (3) we include household fixed effects, $\kappa_h$, such that treatment effects are estimated based on differential exposure to treatment across children in the same household. In each of these specifications, 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 $\beta_1$ is identified by exogenous variation in policy timing within the subset of winning households. We show visually the variation in *Exp* for children in winning households in Appendix Figure **??**.

**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 winning, we examine whether time-invariant characteristics of winning and

[9]For household-level outcomes and balance tests, we do not include the $i$ subscript.

waitlist heads of household are comparable. In Table 4 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 three controls: an indicator for the condominium site sample group, an indicator for winners who won before round 10, 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 C.

In column (2) we observe that we obtain balance on all but one of the included covariates. The imbalanced characteristic, the share of household heads with at least a secondary education, is included along with sampling weights and the controls discussed above in all subsequent analysis.

## 4.3 Children's Human Capital

We now turn to our primary research question, how winning a housing lottery during childhood affects individual welfare. 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 fluid intelligence, 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 well-being but not reflected in traditional outcomes. Finally, we present results on down-stream employment and income.

**Educational Attainment** The results on child educational attainment are presented in Table 5. 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 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

**Table 3:** Summary Statistics and Balance

| | DV Mean (DV SD) [Observations] | Lotto coef. (SE) | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| HHH Age | 49.775 | 1.246*** | 1.181* | 1.001 | 0.453 | 1.291* | 1.047 |
| | (9.637) | (0.482) | (0.617) | (0.684) | (1.167) | (0.728) | (0.852) |
| | [2326] | | | | | | |
| HHH Years in Addis | 38.713 | 0.360 | -0.173 | -0.340 | 0.752 | 0.051 | -0.534 |
| | (11.547) | (0.563) | (0.864) | (1.031) | (1.659) | (0.881) | (1.068) |
| | [2315] | | | | | | |
| HHH Married | 0.686 | 0.074*** | -0.040 | -0.025 | -0.012 | -0.060 | -0.061 |
| | (0.464) | (0.026) | (0.034) | (0.040) | (0.068) | (0.038) | (0.047) |
| | [2326] | | | | | | |
| HHH Years Ed | 10.094 | 1.621*** | 1.027*** | 0.928** | 1.173* | 0.906** | 0.657 |
| | (4.244) | (0.225) | (0.346) | (0.389) | (0.667) | (0.373) | (0.437) |
| | [2326] | | | | | | |
| Orthodox | 0.679 | 0.031 | 0.052 | 0.032 | -0.026 | 0.091** | 0.077* |
| | (0.467) | (0.023) | (0.032) | (0.036) | (0.061) | (0.036) | (0.043) |
| | [2326] | | | | | | |
| Amharic | 0.700 | 0.023 | 0.027 | 0.020 | 0.044 | 0.029 | 0.030 |
| | (0.458) | (0.021) | (0.033) | (0.037) | (0.059) | (0.038) | (0.041) |
| | [2326] | | | | | | |
| BL: HHH Wage Emp | 0.488 | 0.069*** | 0.055 | 0.056 | 0.002 | 0.035 | 0.041 |
| | (0.500) | (0.026) | (0.033) | (0.036) | (0.065) | (0.039) | (0.045) |
| | [2269] | | | | | | |
| BL: HHH No Income | 0.251 | 0.016 | 0.024 | 0.031 | 0.096 | 0.040 | 0.039 |
| | (0.434) | (0.021) | (0.030) | (0.033) | (0.058) | (0.036) | (0.041) |
| | [2269] | | | | | | |
| HHH Father Wage Emp | 0.394 | 0.003 | 0.007 | 0.025 | 0.144* | 0.040 | 0.053 |
| | (0.489) | (0.026) | (0.040) | (0.046) | (0.077) | (0.045) | (0.051) |
| | [1859] | | | | | | |
| HHH Mother Wage Emp | 0.103 | -0.012 | -0.006 | -0.017 | 0.020 | 0.019 | 0.007 |
| | (0.304) | (0.013) | (0.020) | (0.024) | (0.047) | (0.026) | (0.032) |
| | [1900] | | | | | | |
| N Winners | | 1176 | 1176 | 1176 | 378 | 1038 | 1038 |
| Samp Weights | | | X | X | X | X | X |
| Controls | | | | X | X | | X |
| Only Main Sample | | | | | X | | |
| Drop Outlier Sites | | | | | | X | X |

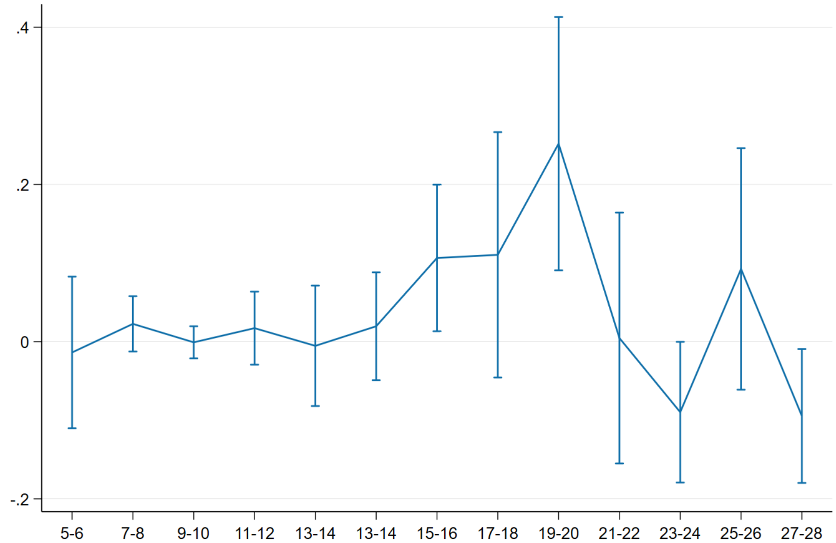
estimates marginal effects on enrollment for particular age groups. We see that increases in school enrollment are largest for children between 15 and 20 years old which coincides with the period in which children are completing secondary schooling and enrolling in post-secondary education. Similarly, in the upper-left panel of Appendix Figure A.4 we observe similar 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%. Consequently, we would not expect the lottery to significantly impact these rates.

In column (3) we show that treatment increases secondary school completion rates by 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 6 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 for the main effect of winning a condominium, three additional years of childhood exposure to the policy increases school enrollment rates by 2.4-5.2 percentage points.

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 lottery winning. We expand on our enrollment results in Appendix Table A.14 and estimate the sibling design from specification 3. 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

**Figure 2:** Children's Enrollment - By Age




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 group effects.

3.4-4.8 percentage points. These results are consistent with evidence from the United States that the impacts of housing interventions are concentrated among young individuals (Chyn and Katz, 2021; Chetty et al., 2016).

**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, while 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 children to their most recently attended primary school. In Table 7 we show that treatment does not change the quality of the 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. Additionally, children in lottery winning households are no more likely to attend a private school which are generally considered to be of higher quality in Addis Ababa.

Next, we use a list of all colleges, universities, and technical institutions in Ethiopia from the Ministry of Education to match children to any post-secondary school that they attended. The results are presented in Table 8. As with primary school quality, we find no evidence that children of winning households go to *better* post-secondary institutions. This results holds regardless of whether we condition on post-secondary attendance. 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 more likely to attend either public universities or technical training institutes (TVETs).

**Cognitive Skills, Literacy, and Numeracy** Our educational outcomes are not limited to attainment. In a small sample of children who we interviewed directly, we see substantial gains in measures of fluid intelligence. These results are presented in Table 9 and are consistent with household wealth and residential stability being important drivers of human capital (Bursztyn and Jensen, 2015; Card and Giuliano, 2013). Specifically, we see that children in winning households score substantially better on Raven's matrix tests and complete a numerical Stroop exercise faster and more accurately, although these latter results are imprecisely estimated. These measures are commonly used in economics and the child development literature and had been previously validated in Ethiopia. Details on the implementation of these tests can be found in Appendix D.

We find evidence of an exposure effect in cognitive tests and SDQs. Columns (2), (4), and (6) of Table 9 replicate the exposure design in Equation (2). Each supports longer childhood exposure improving cognitive ability, though the effects in columns (2) and (4) are imprecisely estimated.

In contrast, we find no significant impacts on literacy or numeracy in Table 10. 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 Appendix Table A.3 and note that 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. We view these results as suggestive of improved learning, though our failure to generate significant variation in components and our relatively small sample of children hinder our power to detect differences.

**Soft Skills & Aspirations** Outside of education and learning, we find mixed results on child 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 and is designed to measure the emotional and behavioral development of children. More details on its implementation can be found in Appendix E. In Appendix Table A.7 we show that children in winning households do not score any better on this measure of soft skill development.

From our child survey, we follow the literature on aspiration measurement to ask directly about educational and occupational goals. We are under-powered to detect significant differences in most measures, however children in winning households are generally more likely to desire an advanced degree, desire an occupation that requires an advanced degree, and are less likely to mention educational constraints to attaining this occupation. In contrast, they are more pessimistic about their chance of reaching their academic goals, likely due to their goals being more ambitious. Additionally, children in winning households are more confident in their academic performance, believe that their life in the future will be better than their current standing, and are more satisfied with the neighborhood in which they live.

**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. 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.

## 4.4 Household Welfare

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 (2020), we observe large increases in family assets (Appendix Table A.8) and indexes of home quality (Appendix Table A.9). Again confirming the results in Franklin (2020), we find no impacts on adult employment rates, but large increases in formal sector employment for household heads driven by a move away from casual employment (Appendix Table A.11). This, in turn, is associated with substantial increases in household income (Appendix Table A.12), which Franklin (2020) does not find. We believe that the differences between our results and those in Franklin (2020) may be due to our study looking at longer-term impacts: Franklin (2020) only considers outcomes within a year of one particular condominium round. Additionally, we believe that our results on household income are plausible given the wage premium associated with formal sector employment.

# 5 Model & Mechanisms

As was previously noted, only a subset of lottery winners move into their unit after winning while the remainder 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 losing households would have moved if they had won the lottery, treatment effect estimates in this reduced sample will be biased due to selection.

Taking into account selection into moving presents an empirical challenge, but also allows for the opportunity to disentangle the channels through which treatment 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 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 two-stage least squares (2SLS) estimation strategy. We consider owning and occupying (**O**) and selling or renting out the condominium (**W**) to be the "treatments" incentivized by the lotteries. We consider a single fall-back state (**S**) that represents the household's outside option absent winning a lottery.[10] Thus, households choose between three mutually exclusive treatments $k \in \{\mathbf{S}, \mathbf{W}, \mathbf{O}\}$. Let $D_k$ represent the binary indicator corresponding to each treatment, $D_k = \mathbb{1}[D = k]$ such that:

$$D_S + D_W + D_O = 1$$

We're interested in the impact of the causal effect of each treatment on a later life outcome Y:

$$Y = Y_S D_S + Y_W D_W + Y_O D_O$$

where $Y_k$ is the counterfactual outcome if assigned to treatment $k$. This implies that $Y_W - Y_S$ recovers the effect of a shock to parental wealth while $Y_O - Y_S$ recovers the combined neighborhood and wealth effects.

With this setup, we are interested in estimating an OLS regression with two endogenous treatments (**W**, **O**):

$$y_{h(i)} = \alpha_0 + \alpha_1 R_{h(i)} + \alpha_2 W_{h(i)} + \alpha_3 O_{h(i)} + \mathbf{X}_{h(i)}\gamma + \epsilon_{h(i)} \tag{4}$$

[10] We will extend the model to include an additional fall-back option of renting in a condominium (**R**).

However, until now we have been using a binary instrument for whether a household won a lottery, $Z \in \{0, 1\}$, leaving our equation underidentified. Fortunately, our setting allows for the creation of multiple sets of exogenous instruments leveraging temporal variation in lottery winning and spatial variation in condominium locations. Specifically, we interact indicators for (grouped) lottery rounds with the difference between realized and expected distance of the household to their condominium unit:

$$\mathbf{Z} = \text{(Grouped) Lottery Round} \times (\text{Distance to Unit} - \mathbb{E}\,[\text{Distance to Unit}])$$

We then estimate Equation (4) instrumenting **W** and **O** with **Z** controlling for the main effects of the interacting variables. The model is identified off of variation in complier shares across Z-strata. We estimate "sub-LATES", $LATE_W$ and $LATE_O$.

Per the discussion in Kline and Walters (2016) and Hull (2018), this model can secure identification under an assumption of constant effects. Put differently, one must assume that "subLATEs" do not vary with the interacted instruments; constant effects in this environment amount to the assumption that

$$E\,[y_O - y_S | O_1 > O_0, S_1 = S_0, X]$$
$$E\,[y_W - y_S | W_1 > W_0, S_1 = S_0, X]$$

do not depend on the distance deviation between the realized and expected distance to the unit. As we might find this constant effects assumption to be unrealistic in our setting, it is relaxed in the selection model.

With two endogenous variables, we need two further conditions for identification:

1. No household switches between **W** and **O** in response to winning the lottery

2. No household who otherwise would have chose **W, 0** switch to **S** in response to the lottery (Heckman and Pinto, 2018)

Finally, as is standard in IV models, we further need need conditions on independence, instrument exclusion, and monotonicity. Let $y_{z,k}$ denote the realizations of Y when $\mathbf{Z} = z$ and $D = k$ and let $k_z$ denote $k = \mathbb{1}[D = k], \mathbf{Z} = z$. Then these conditions can be written succinctly as:

$$\textbf{Independence:} \left((y_{z,S}, y_{z,W}, y_{z,O}, W_z, O_z)_{z\in\{0,1\}}\right) \perp\!\!\!\perp Z|X$$

$$\textbf{Exclusion:} Pr\left(y_{0,k} = y_{1,k}\right) = 1 \; \forall \; k \in \{S, W, O\}$$

$$\textbf{Monotonicity:} Pr\left(W_1 \geq W_0\right) = Pr\left(O_1 \geq O_0\right) = 1$$

Results from the interacted 2SLS model are presented in Table 19.

Our results imply that the observed effects are not explained entirely by changes in parental wealth. In fact, we see that impacts secondary completion, and post-secondary attendance are concentrated amongst households that own and occupy the unit, while treatment effects are indistinguishable from zero for households that rented out or sold their unit. These results imply that condominium neighborhoods and home quality play an important role in explaining our results. We are working to characterize specific characteristics of the neighborhood that correlate with treatment effects.

## 5.2 Selection Model

**Work In Progress – Incomplete**

We extend the model developed in Kline and Walters (2016) and Mountjoy (2019) to allow for multiple endogenous treatments and multiple fall-back states. We incorporate household preferences and potential outcomes over these treatment and fall-back states. Like the interacted 2SLS approach, we use the lottery instrument interacted with household and condominium site covariates to identify causal effects for each treatment. The model allows for different strata-specific treatment effects, which was the primary limitation of the 2SLS approach.

## 5.3 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 noncondominium housing to zero. Here, $\Psi_k$ is the mean treatment-level utility for treatment $k$ while $\nu_k$ are unobserved idiosyncratic components that vary across households. Households maximize state-specific utility:

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

where $D_{h(i)}(X, z) = k$ represents the observed outcome. Following Kling et al. (2007), 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 $\gamma$ terms govern selection on unobservables. They are assumed to enter into the potential outcome framework linearly and to be additively separable from observables. To see this, suppose $\gamma_{bm,bm} > 0$ and $\gamma_{s,bm} = -\gamma_{bm,bm}$, then selection into purchasing a unit and moving reflects Roy-style selection where children in households switching form $s$ to $bm$ experience larger gains in outcomes.

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.

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 will then use the parameters from our probit model to build our control function estimates which are 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 additive separability of potential outcomes in observables and unobservables, as is common in this literature (Heckman et al., 2006). This rules out selection coefficients ($\gamma$) depending on $X$ which is testable by comparing selection coefficients across different subsets of $X$. Further, we require that (1) the instrument shifts choice probabilities for all covariate groups defined by $X$; (2)

choice probabilities differ across covariate groups conditional on winning the lottery; (3) the instrument must shift the conditional mean values of $\nu_{h(i)k}$ in a non-proportional manner for all $k \in \{O, W\}$.

With model estimates in hand, we can simulate policy changes that change complier shares. Consider, for instance, a policy change that allows the implied subsidy associated with the condominium unit to increase for households who actually move in. Or similarly, a policy that makes these units more attractive via complementary infrastructure improvements or bundled job search/education programs. We can predict the effects of these policy changes by modeling them as changes in the mean utility of renting out or selling ($\Psi_W$) or owning and occupying ($\Psi_O$).

# 6 Discussion

# 7 Conclusion

**Table 4:** Summary Statistics and Balance

| | DV Mean (DV SD) [Observations] (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) |
| | [2326] | | [480] | [622] |
| HHH Years in Addis | 38.713 | -0.340 | 37.509 | 39.215 |
| | (11.547) | (1.031) | (12.217) | (11.001) |
| | [2315] | | [477] | [620] |
| HHH Married | 0.686 | -0.025 | 0.686 | 0.683 |
| | (0.464) | (0.040) | (0.465) | (0.466) |
| | [2326] | | [480] | [622] |
| HHH Years Ed | 10.094 | 0.928** | 10.901 | 10.232 |
| | (4.244) | (0.389) | (4.569) | (4.153) |
| | [2326] | | [480] | [622] |
| Orthodox | 0.679 | 0.032 | 0.695 | 0.686 |
| | (0.467) | (0.036) | (0.461) | (0.464) |
| | [2326] | | [480] | [622] |
| Amharic | 0.700 | 0.020 | 0.672 | 0.732 |
| | (0.458) | (0.037) | (0.470) | (0.443) |
| | [2326] | | [480] | [622] |
| BL: HHH Wage Emp | 0.488 | 0.056 | 0.575 | 0.469 |
| | (0.500) | (0.036) | (0.495) | (0.499) |
| | [2269] | | [476] | [610] |
| BL: HHH No Income | 0.251 | 0.031 | 0.221 | 0.287 |
| | (0.434) | (0.033) | (0.415) | (0.453) |
| | [2269] | | [476] | [610] |
| HHH Father Wage Emp | 0.393 | 0.027 | 0.315 | 0.435 |
| | (0.488) | (0.046) | (0.465) | (0.496) |
| | [1866] | | [352] | [503] |
| HHH Father Casual/Self Emp | 0.571 | -0.025 | 0.671 | 0.512 |
| | (0.495) | (0.048) | (0.471) | (0.500) |
| | [1866] | | [352] | [503] |
| HHH Mother Wage Emp | 0.103 | -0.017 | 0.071 | 0.109 |
| | (0.304) | (0.024) | (0.257) | (0.311) |
| | [1902] | | [361] | [510] |
| HHH Mother Casual/Self Emp | 0.211 | -0.024 | 0.170 | 0.232 |
| | (0.408) | (0.035) | (0.376) | (0.423) |
| | [1902] | | [361] | [510] |
| BL: HH Size | 3.619 | -0.042 | 3.786 | 3.519 |
| | (2.216) | (0.199) | (2.130) | (2.070) |
| | [2326] | | [480] | [622] |
| N Winners | 2326 | 1176 | 480 | 622 |
| Samp Weights | X | X | X | X |
| Controls | | X | | |
| Joint F-Stat | | 1.585 | | |

**Table 5:** Children's Educational Attainment

| | (1)<br>1(Enrolled) | (2)<br>1(Enrolled) | (3)<br>Primary | (4)<br>Secondary | (5)<br>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** | -0.036 | -0.122*** |
| | (0.015) | (0.024) | (0.017) | (0.028) | (0.036) |
| Constant | 0.632*** | 0.421*** | 0.850*** | 0.629*** | 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 |

**Table 6:** Children's Education - Exposure Design

| | (1)<br>1(Enrolled) | (2)<br>1(Enrolled) | (3)<br>Primary | (4)<br>Secondary | (5)<br>Post-Sec Att |
|---|---|---|---|---|---|
| Exposure (Years) | 0.007* | 0.017*** | -0.007 | -0.016 | -0.005 |
| | (0.004) | (0.005) | (0.005) | (0.010) | (0.011) |
| 1(Won Lottery) | -0.009 | -0.020 | 0.052* | 0.119*** | 0.094* |
| | (0.029) | (0.035) | (0.028) | (0.039) | (0.053) |
| 1(Male) | -0.010 | -0.018 | 0.041** | -0.029 | -0.114*** |
| | (0.016) | (0.025) | (0.018) | (0.029) | (0.038) |
| Constant | 0.618*** | 0.399*** | 0.860*** | 0.646*** | 0.343*** |
| | (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 |

**Table 7:** 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 |

**Table 8:** 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 |

**Table 9:** 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 |

**Table 10:** Cognitive Tests - Index

| | (1) | (2) | (3) |
|---|---|---|---|
| | Math Index | Literacy Index | Testing Index |
| 1(Won Lottery) | 0.372 | 0.306 | 0.532 |
| | (0.318) | (0.376) | (0.487) |
| 1(Male) | 0.486* | 0.387 | 0.543 |
| | (0.270) | (0.354) | (0.432) |
| Constant | -0.106 | -0.680 | -0.517 |
| | (0.385) | (0.511) | (0.583) |
| N | 127 | 127 | 126 |
| Waitlist Mean | -0.261 | -0.303 | -0.424 |
| Birth Cohort FEs | X | X | X |
| Sample | 6-12 | 6-12 | 6-12 |

**Table 11:** Children's Income

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Any Income | Formal Emp | Self Emp | Days Worked | Asinh(Prim Inc) | 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 |

**Table 12:** Interacted 2SLS:

| | (1) 1(Enrolled) | (2) 1(Enrolled) | (3) Primary | (4) Secondary | (5) Post-Sec Att |
|---|---|---|---|---|---|
| **O** | 0.073** | 0.109* | -0.118 | 0.075 | 0.090 |
| | (0.034) | (0.057) | (0.112) | (0.060) | (0.086) |
| **W** | 0.012 | 0.038 | 0.100* | 0.092* | 0.062 |
| | (0.028) | (0.047) | (0.056) | (0.055) | (0.071) |
| 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 | 113.425 | 92.41200000000001 | 46.458 | 115.14 | 115.14 |
| **W** First-stage F | 257.647 | 144.064 | 99.01300000000001 | 117.37 | 117.37 |
| Overid P-value | . | . | . | . | . |

IVs: lotto_winner same_subcity

**Table 13:** Interacted 2SLS

| | (1) 1(Enrolled) | (2) 1(Enrolled) | (3) Primary | (4) Secondary | (5) Post-Sec Att |
|---|---|---|---|---|---|
| **O** | 0.114** | 0.177** | -0.012 | 0.181** | 0.198 |
| | (0.050) | (0.079) | (0.073) | (0.088) | (0.127) |
| **W** | -0.011 | -0.002 | 0.039 | 0.018 | -0.012 |
| | (0.034) | (0.059) | (0.046) | (0.071) | (0.091) |
| 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 | 70.52500000000001 | 60.482 | 56.687 | 43.582 | 43.582 |
| **W** First-stage F | 158.596 | 94.298 | 102.489 | 54.748 | 54.748 |
| Overid P-value | . | . | . | . | . |

IVs: lotto_winner c.dist_cond_wmax

**Table 14:** Interacted 2SLS

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | 1(Enrolled) | 1(Enrolled) | Primary | Secondary | Post-Sec Att |
| **O** | 0.090* | 0.147* | -0.007 | 0.167** | 0.161 |
| | (0.047) | (0.077) | (0.074) | (0.085) | (0.124) |
| **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 | 78.3 | 63.672 | 58.43 | 44.938 | 44.938 |
| **W** First-stage F | 175.022 | 97.532 | 100.202 | 54.919 | 54.919 |

IVs: $1(LottoWinner)$; $1(LottoWinner) \times$ (Distance to Unit $-$ $\mathbb{E}$[Distance to Unit])

**Table 15:** Interacted 2SLS

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | 1(Enrolled) | 1(Enrolled) | Primary | Secondary | Post-Sec Att |
| **O** | 0.087* | 0.155* | -0.004 | 0.187* | 0.159 |
| | (0.052) | (0.084) | (0.094) | (0.098) | (0.135) |
| **W** | 0.004 | 0.011 | 0.035 | 0.014 | 0.015 |
| | (0.036) | (0.062) | (0.059) | (0.084) | (0.100) |
| 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 | 60.547 | 47.218 | 40.898 | 30.878 | 30.878 |
| **W** First-stage F | 132.707 | 73.28700000000001 | 72.748 | 38.045 | 38.045 |
| Overid P-value | . | . | . | . | . |

IVs: lotto_winner Z2

**Table 16:** Interacted 2SLS

| | (1) 1(Enrolled) | (2) 1(Enrolled) | (3) Primary | (4) Secondary | (5) Post-Sec Att |
|---|---|---|---|---|---|
| **O** | 0.086 | 0.153* | -0.002 | 0.186* | 0.159 |
| | (0.053) | (0.084) | (0.095) | (0.098) | (0.134) |
| **W** | 0.004 | 0.011 | 0.033 | 0.015 | 0.015 |
| | (0.036) | (0.062) | (0.059) | (0.084) | (0.100) |
| 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 | 59.037 | 46.997 | 40.117 | 31.302 | 31.302 |
| **W** First-stage F | 130.667 | 72.979 | 71.691 | 38.384 | 38.384 |
| Overid P-value | . | . | . | . | . |

IVs: lotto_winner Z6

**Table 17:** Interacted 2SLS

| | (1) 1(Enrolled) | (2) 1(Enrolled) | (3) Primary | (4) Secondary | (5) Post-Sec Att |
|---|---|---|---|---|---|
| **O** | 0.092** | 0.144* | -0.010 | 0.152* | 0.162 |
| | (0.046) | (0.077) | (0.064) | (0.083) | (0.127) |
| **W** | 0.001 | 0.017 | 0.038 | 0.039 | 0.012 |
| | (0.032) | (0.057) | (0.042) | (0.067) | (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 | 39.363 | 32.035 | 29.415 | 23.315 | 23.315 |
| **W** First-stage F | 93.93000000000001 | 53.146 | 55.763 | 31.97 | 31.97 |
| Overid P-value | .837 | .8230000000000001 | .861 | .55 | .965 |

IVs: lotto_winner Z1 Z6

**Table 18:** Interacted 2SLS

| | (1)<br>1(Enrolled) | (2)<br>1(Enrolled) | (3)<br>Primary | (4)<br>Secondary | (5)<br>Post-Sec Att |
|---|---|---|---|---|---|
| **O** | 0.084 | 0.157* | 0.002 | 0.200* | 0.157 |
| | (0.059) | (0.093) | (0.111) | (0.113) | (0.149) |
| **W** | 0.004 | 0.013 | 0.034 | 0.017 | 0.015 |
| | (0.035) | (0.060) | (0.056) | (0.082) | (0.097) |
| 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 | 38.681 | 29.896 | 25.123 | 18.753 | 18.753 |
| **W** First-stage F | 157.569 | 86.61 | 84.61 | 44.496 | 44.496 |
| Overid P-value | . | . | . | . | . |

IVs: Z1 Z6

**Table 19:** Interacted 2SLS

| | (1)<br>1(Enrolled) | (2)<br>1(Enrolled) | (3)<br>Primary | (4)<br>Secondary | (5)<br>Post-Sec Att |
|---|---|---|---|---|---|
| **O** | 0.088* | 0.149* | -0.008 | 0.168** | 0.165 |
| | (0.047) | (0.077) | (0.073) | (0.085) | (0.124) |
| **W** | 0.004 | 0.014 | 0.037 | 0.027 | 0.011 |
| | (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 | 39.72 | 32.141 | 30.403 | 23.293 | 23.293 |
| **W** First-stage F | 93.236 | 48.77 | 52.174 | 27.873 | 27.873 |
| Overid P-value | .079 | .16 | .8280000000000001 | .542 | .16 |

IVs: lotto_winner c.dist_cond_wmax Z1

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# A Tables and Figures

**Figure A.1:** Condominium Openings

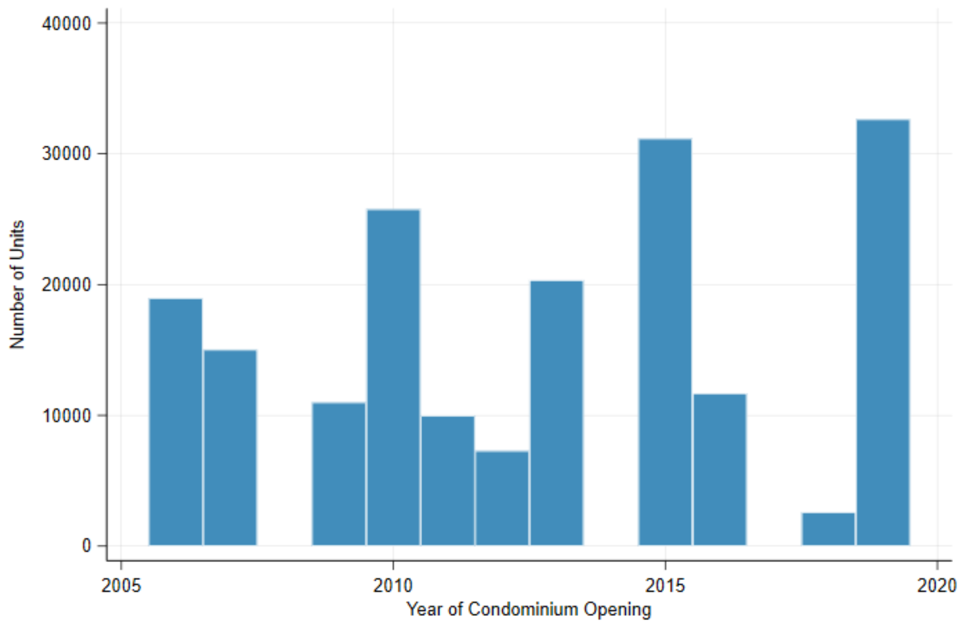
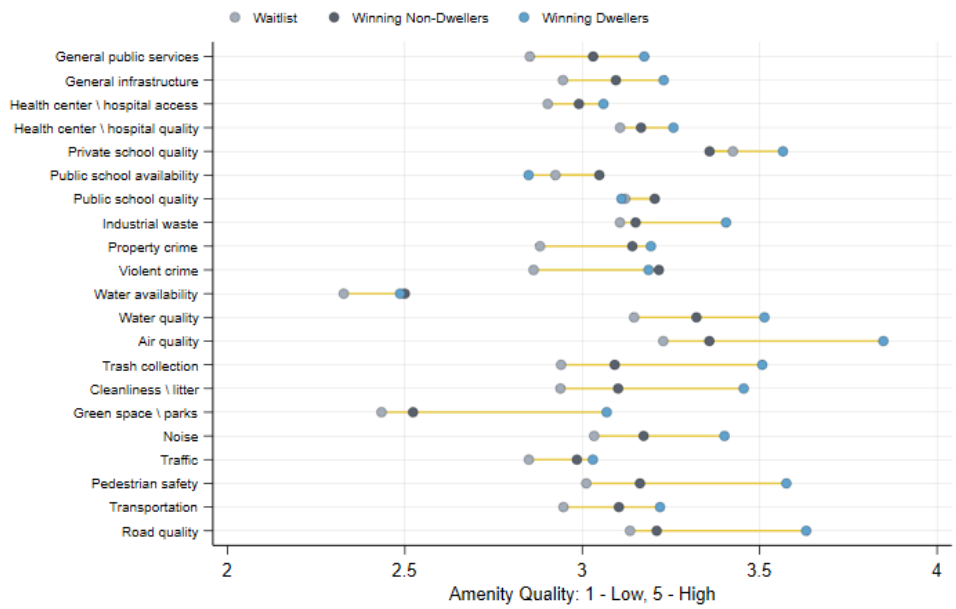



**Table A.1:** Condominium Openings

| | 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




# B Site Re-Sampling

# C Attrition

# D Cognitive Tests and Aspirations

### Stroop Test

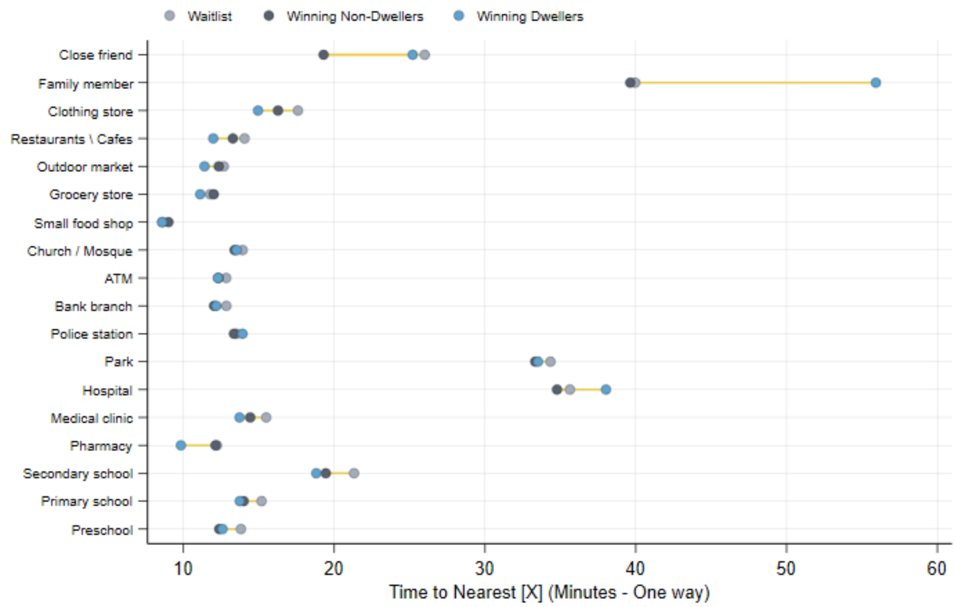
### Raven's Matrices

### Numeracy

### Literacy

### Aspirations

**Figure A.3:** Neighborhood Distance




# E Strengths and Difficulties

**Figure A.4:** Educational Attainment

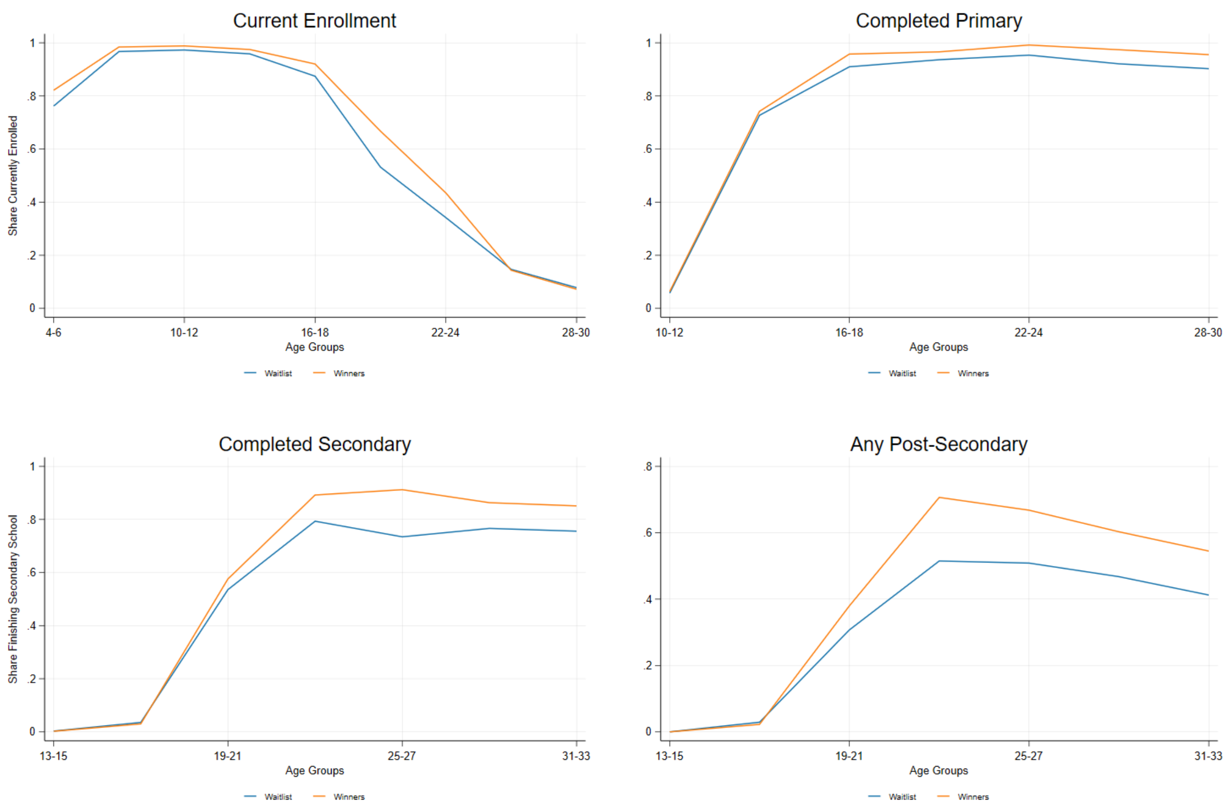


**Figure A.5:** Years of Childhood Exposure

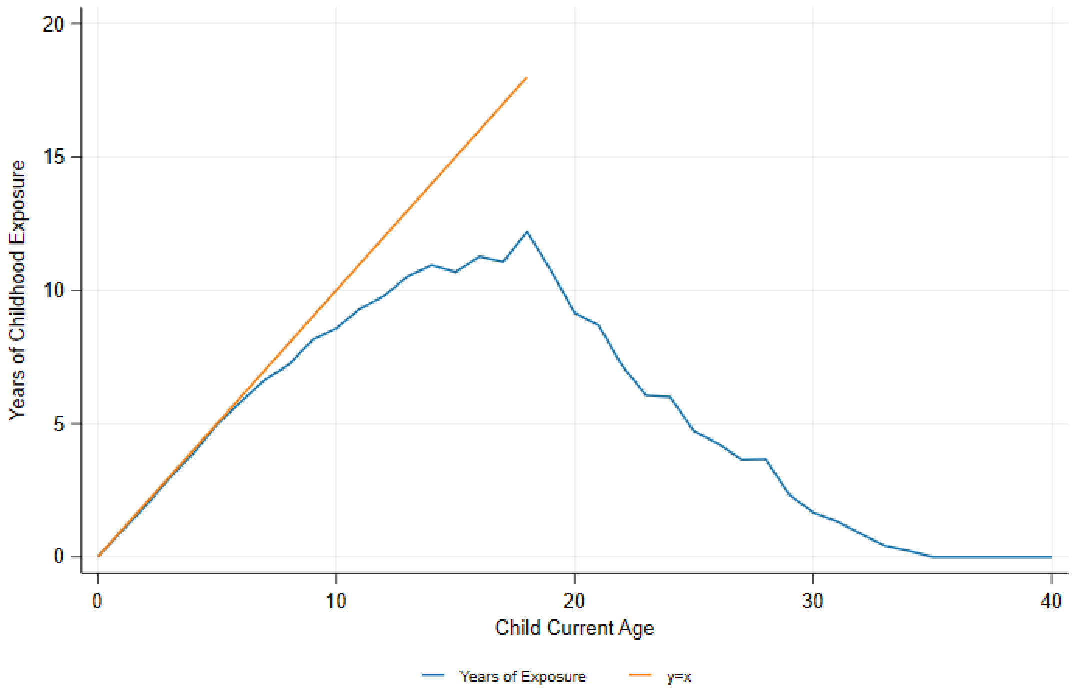



**Table A.2:** Children's Enrollment - Exposure Design

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | 1(Enrolled) | 1(Enrolled) | 1(Enrolled) | 1(Enrolled) |
| Exposure [3 Yr] | 0.024** | 0.052*** | 0.034* | 0.048* |
| | (0.011) | (0.016) | (0.019) | (0.028) |
| 1(Won Lottery) | -0.019 | -0.034 | | |
| | (0.033) | (0.040) | | |
| 1(Male) | -0.008 | -0.016 | -0.026 | -0.047 |
| | (0.016) | (0.025) | (0.021) | (0.036) |
| Constant | 0.657*** | 0.449*** | 0.696*** | 0.486*** |
| | (0.025) | (0.034) | (0.030) | (0.041) |
| N | 4558 | 2615 | 3892 | 1858 |
| No Exposure Mean | 0.677 | 0.493 | 0.683 | 0.449 |
| Birth Cohort FEs | X | X | X | X |
| HH FEs | | | X | X |
| Sample | 5-30 | 14-30 | 5-30 | 14-30 |

**Table A.3:** 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 |

**Table A.4:** 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 |

**Table A.5:** 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.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:** Strength and Difficulty Scores

| | SDQ Scores (2-18) | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) Male | (3) Female | (4) | (5) Male | (6) Female | (7) |
| 1(Won Lottery) | 0.028 | 0.147 | -0.093 | 0.076 | 0.192 | -0.136 | |
| | (0.088) | (0.103) | (0.100) | (0.140) | (0.170) | (0.204) | |
| Exposure (Years) | | | | -0.007 | -0.007 | 0.006 | 0.003 |
| | | | | (0.021) | (0.024) | (0.030) | (0.033) |
| 1(Male) | 0.035 | | | 0.035 | | | -0.107* |
| | (0.056) | | | (0.056) | | | (0.056) |
| Constant | -0.291** | -0.304* | -0.313* | -0.288** | -0.302* | -0.317* | 0.035 |
| | (0.122) | (0.155) | (0.163) | (0.123) | (0.156) | (0.166) | (0.138) |
| N | 2443 | 1241 | 1199 | 2443 | 1241 | 1199 | 1492 |
| Waitlist Mean | -0.022 | -0.065 | 0.027 | -0.022 | -0.065 | 0.027 | 28.937 |
| Birth Cohort FEs | X | X | X | X | X | X | X |
| HH FEs | | | | | | | 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)<br>Imp Floor | (2)<br>Iron Roof | (3)<br>Imp Walls | (4)<br>Qual Index 1 | (5)<br>Area PP | (6)<br>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)<br>Ln(Rent) Sim | (2)<br>Ln(Rent) Est | (3)<br>Ln(Rent) | (4)<br>Rent Val All | (5)<br>Ln(Sale) Sim | (6)<br>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)<br>Any Work | (2)<br>Formal Emp | (3)<br>Self Emp | (4)<br>Casual Emp | (5)<br>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) Asinh HHH Tot Inc | (2) Asinh HHH + Spouse Tot Inc | (3) Asinh HH Tot Inc Pc |
|---|---|---|---|
| 1(Won Condo) | 0.595*** | 0.789*** | 0.766*** |
| | (0.226) | (0.212) | (0.217) |
| Constant | 4.193*** | 5.367*** | 2.944*** |
| | (0.899) | (0.792) | (0.907) |
| N | 2265 | 2135 | 2269 |
| Waitlist Mean | 6.824 | 7.129 | 6.239 |
| 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) | 0.012* | 0.018* | -0.005 | -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 | 0.059 | -0.055 | -0.058 |
| | (0.021) | (0.036) | (0.029) | (0.039) | (0.063) | (0.051) |
| Constant | 0.701*** | 0.485*** | 0.913*** | 0.734*** | 0.494*** | 0.505*** |
| | (0.028) | (0.039) | (0.023) | (0.050) | (0.055) | (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 |