The Effects of Free Homes on Children: Evidence from a National Experiment in Colombia*

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Abstract

We analyze the effect of Colombia's ambitious "Free Housing" program on children's educational outcomes. The program was generous, giving free housing to beneficiaries in desirable areas. We evaluate the program by leveraging housing lotteries and linking applicants to their children. We find that public housing increases high school graduation by seven percentage points – a seventeen percent increase relative to the control mean – and boosts exit exam scores and college-going. Exploring mechanisms, lottery winners attend better schools, their families become wealthier, and they live in higher income neighborhoods with less crime. *JEL* codes: O10, O18, O22, I25, I38.

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1 Introduction

Poor children are more likely to become poor adults, especially in low- and middle-income countries. Breaking the intergenerational persistence of poverty is particularly challenging in these settings given that children are exposed to multiple sources of disadvantage and have few human capital investment opportunities (Currie and Vogl, 2013). As such, policies that ameliorate a single source of disadvantage may be insufficient to address children's needs. Breaking the cycle of poverty likely requires more comprehensive approaches; however, such "big push" policies tend to be rare in deprived contexts (Banerjee et al., 2015; Balboni et al., 2022).

This paper investigates the extent to which a big-push-style housing program can break the intergenerational poverty trap. It does so by investigating the impact on recipients' children of a remarkably generous assistance program that combined a large in-kind wealth transfer with family relocation to better neighborhoods. The program we analyze is Colombia's *Programa de Vivienda Gratuita* or "Free Housing Program" which granted housing units to over 100,000 highly-disadvantaged families. The housing unit was given to recipients for free, with the only stipulation being that the unit could not be sold or rented for ten years. Program recipients thus received a large wealth transfer as the market value of a housing unit was approximately 30,000 USD, representing roughly ten years of wages for the average recipient. In addition, recipients relocated to better neighborhoods as the public housing units were purposely built in desirable areas near city centers, economic opportunities, and public services. As the housing was oversubscribed, thirty percent of units were randomly assigned via lottery. We leverage these lotteries to show the causal impact of winning a highly desirable public housing unit on children's human capital outcomes.

We find that public housing receipt increases high school graduation rates by seven percentage points, a seventeen percent increase relative to the control mean of forty-two percent. Receiving public housing also raises the probability of taking the high school exit exam, which is used for university admissions, by seven percentage points and, after accounting for selection into test-taking (Angrist et al., 2006), boosts high school exit exam scores by 0.1-0.2 standard deviations. Public housing receipt also increases college-going by 1.4 percentage points or ten percent compared to the control mean. These effects are for children who resided in public housing for an average of 4.1 years.

In the program we study public housing receipt represents a treatment *bundle*, which

impacts children through multiple dimensions: increased family wealth, assigned property rights, improved physical housing quality, changed neighborhoods, and new local public schools. Each of these program components is a potential mediator through which the "Free Housing" program affects children's education. We turn to our unique administrative and survey data to shed light on how the program impacted three likely mediators: Schools, neighborhoods, and family income and wealth.

For schools, we measure school quality via value-added (Deming, 2014) constructed using pre-lottery data.[1] We find that lottery winners attended better schools in terms of value-added after the lottery (but not before), with the magnitude of the value-added difference indicating that roughly one-third of public housing's impact on high school graduation can be attributed to the improved schools attended by lottery winners relative to losers.

In term of neighborhoods, we document that lottery winners move to neighborhoods that are more central, have fewer problems, feature lower crime, and are richer. In particular, we use a survey to confirm the proximity of public housing to amenities, with lottery winners reporting a 30-40 percent (or 5-10 minutes) reduction in commute times to nearby amenities such as public transit, schools, grocery stores, and parks (Camacho et al., 2021). Survey responses also indicate lottery winners had fewer problems with their neighborhood (e.g., noise, trash, etc). Using police and Census data, we also find that lottery winners' neighborhoods have less crime and are wealthier. To assess the importance of neighborhoods, we search for heterogeneity based on the change in neighborhood poverty lottery participants would experience if they win the lottery. We find homogenous effects across lottery-induced changes to neighborhood poverty, indicating that neighborhood quality (as measured by poverty) is unlikely to be a key mediator in our context.

For family wealth and income, we link lottery winners and losers to administrative data collected roughly five years after the lottery which contain detailed information on family wealth, labor supply, income, access to services, and expenditures. We find that five years post-lottery, the households of lottery winners own more durable goods (e.g., fridge, washing machine, etc.), have better access to services (e.g., natural gas, internet,

[1]We use pre-lottery data to ensure that our value-added estimates are not contaminated by the housing projects themselves. In particular, we expect that the increased wealth and stability the public housing affords recipients would allow them to perform better in school than expected (conditional on covariates), causing the value-added of recipients' schools to be upward biased if contemporaneous data were used.

etc.), are more likely to be employed, earn higher income, and spend more on educational goods and food.[2] A mediation analysis indicates that increased family wealth along with improved school quality are the two key mediators driving the large educational gains that we see.

This paper is related to several bodies of research. First, to the literature on "big push" programs aimed at breaking poverty traps. The broad idea of these types of intereventions – which go back to Hirschman (1958) and Murphy et al. (1989) – is that, given the presence of nonlinearities in the dynamics of poverty, the size of the transfer is critical for pushing people out of poverty (e.g., see Balboni et al. (2022) and references therein). Some examples of big push interventions include asset transfer programs such as the "Graduation Program" (Banerjee et al., 2015, 2021) and the "Targeting the Ultra-poor Program" (Bandiera et al., 2017; Balboni et al., 2022), unconditional cash transfers (Benhassine et al., 2015; Bleakley and Ferrie, 2016), and conditional cash transfers such as Mexico's PROGRESA (Bobonis and Finan, 2009) and Colombia's *Familias en Acción* (Attanasio et al., 2021).[3] Our contribution to this literature is twofold. First, we show that housing can be a linchpin for big-push-type interventions. Second, we demonstrate that such big-push policies can have large intergenerational effects.[4]

Second, our paper is linked to the literature on public housing programs in developing countries.[5] Existing studies have found null or *negative* impacts of public housing interventions on household outcomes.[6] For instance, Barnhardt et al. (2017) investigate a public housing lottery in India for slum residents and find that after 14 years winning the lottery had no impact on family income and human capital and a negative effect on social connectedness. Likewise, Franklin (2019) exploits a lottery in Ethiopia and estimates that moving into public housing does not impact earnings and reduces social interactions. Picarelli (2019) investigates a program that allocated publicly-built homes

[2]See Camacho et al. (2021) for a detailed analysis of the economic impacts of the program.

[3]See Fiszbein and Schady (2009); Parker and Todd (2017); Millán et al. (2019) for comprehensive reviews of cash transfer programs.

[4]Few studies have examined the intergenerational impacts of generous interventions in low- to middle-income countries. An exception is Agte et al. (2022) who show that a microfinance program offering flexible repayment schemes to poor Indian borrowers increased human capital investment which dramatically raised the probability that their children attended college.

[5]Here, we provide a brief summary of the most relevant papers. Appendix B provides a more in-depth summary.

[6]An exception is Kumar (2021) who finds positive effects of a subsidized housing program in India on family income and children's education. Positive effects occured despite program recipients living in lower-quality neighborhoods with worse schools. The author therefore argues that the program acted as a wealth transfer, since recipients did not reside in the subsidized house but instead subletted it.

to eligible households for free in South Africa and finds declines in labor earnings among recipient households. Similarly, Belchior et al. (2023) explore a large housing subsidy in Brazil and find negative impacts on formal employment. We contribute to this literature by providing experimental evidence that housing programs can significantly boost children's outcomes. In doing so, our results shed light on housing program characteristics that can be critical for their success. In particular, three key differences between the Colombian Free Housing Program and other housing interventions in the developing world are: (i) the location of public housing in desirable areas,[7] (ii) the housing unit was high-quality in terms of construction,[8] and (iii) the unit was given to recipients for free.[9]

Finally, our paper connects with the literature that examines housing programs in the United States (see Collinson et al. (2015); Chyn and Katz (2021) for recent reviews), which has found mixed effects on children's outcomes. On the one hand, Jacob et al. (2015) take advantage of a randomized housing voucher lottery in Chicago and find little impact of housing assistance on a wide variety of child outcomes. Similarly, Jacob (2004) does not detect any effect of housing assistance in the form of vouchers for students affected by high-rise public housing demolitions in Chicago. On the other hand, Schwartz et al. (2020) find that housing vouchers in New York City raise students' test score performance. Similarly, Pollakowski et al. (2022) determine that an additional year in public housing increases earnings at age 26 by 6 percent. Chyn (2018) finds that children affected by public housing demolitions who were given vouchers to move to less disadvantaged neighborhoods were more likely to complete high school, be employed, obtain higher earnings, and commit fewer violent crimes. Likewise, Currie and Yelowitz (2000) find that children in public housing projects are less likely to have been held back in school. Chetty et al. (2016) come to similar conclusions in their analysis of the Moving to Opportunity experiment, finding that young children (below age 13) who moved to better neighborhoods had higher levels of college attendance and earnings, although earlier analyses of the same program found limited effects (Ludwig et al., 2013). A common

[7]For instance, the public housing project in Barnhardt et al. (2017) was located 7.5 miles from the city center which the authors hypothesize made it undesirable. In line with this hypothesis, one-third of lottery winners did not take-up the public housing offer and a further one-third exited public housing over the next decade.

[8]This aspect of the intervention is similar to other successful place-based interventions, such as replacing dirt floors with cement (Cattaneo et al., 2009) or upgrading the physical quality of housing infrastructure for slum residents (Galiani et al., 2017).

[9]In contrast, most public housing programs provide a subsidy, either to buy or rent. In addition, recipients also received property rights, which an influential literature documents has positive effects in developing countries (Field, 2007; Di Tella et al., 2007; Galiani and Schargrodsky, 2011).

characteristic of these experimental evaluations in the U.S. is that while neighborhood quality significantly improved with the intervention, school quality exhibited limited variation. In addition, the wealth transfer was less consequential as it usually came in the form of a voucher that capped rent at thirty percent of income. In contrast, in our context children of lottery winners attend significantly better schools and the wealth transfer was *tremendously* generous. Therefore, we complement this literature by documenting the important role that schools and transfers play in breaking poverty traps.

The rest of the paper is organized as follows: The next section describes the Free Housing program. Section 3 then sets out our empirical methodology and introduces the data. These are followed by our results in Section 4, with Section 5 discussing the mechanisms underlying these results. Section 6 concludes.

2 Background

On April 23, 2012 President Juan Manuel Santos introduced Law 1537, establishing the *Programa Vivienda Gratuita* or "Free Housing" program which provided a *free* residence for the disadvantaged.[10] The law was in line with the government of Colombia's long-standing support for home ownership and received broad political support with Congress quickly passing the legislation. The program was ambitious in scope, aiming to build and deliver 100,000 homes to the disadvantaged for *free* within two years.

To build the necessary housing units, the national government allocated COP 4 trillion (roughly USD 2.2 billion using 2012 exchange rates). Given the amount of money allocated and the number of housing units required, a limit for construction costs of COP 40 million (roughly USD 22,000 in 2012) per unit was set.[11] The government then opened up a call for mayors and governors to identify properties for the new housing units (which would be transfered without charge to the program), setting an application deadline of July 3, 2012. The properties had to meet certain criteria set out by the government, such as: nearby availability of public services, have the necessary zoning and construction permits, be on 'urban' land, and not be located in areas at risk of natural disasters. These criteria were set to avoid endemic problems in Colombia's previous public housing programs whereby

[10] See Gilbert (2014) for a detailed description of public housing programs in Colombia and the political context of the program's introduction. We rely on Departamento Nacional de Planeación (2014) for the technical details of the program.

[11] Even though construction costs are higher in bigger cities, this limit did not vary across the country. Given this, smaller municipalities generally constructed larger housing units in terms of square footage.

subsidized housing was located in peripheral land that lacked public services or in regions with high flood risks.[12]

A total of 650 properties were put forward for consideration of which 298 were deemed suitable for the development of a housing project. Private builders then submitted bids with a point system determining winners, with bids being evaluated on: services provided, development layout, and the size and quality of the homes. Over one hundred companies obtained contracts, although over half of the housing units were built by ten companies which included the three largest construction companies in the country.

Project Locations and Quality: Figure A.1(a) displays the locations of the development projects across the country built by the end of 2014, with the size of the pin corresponding to the number of housing units in the project. In the end, 225 developments were built across 191 municipalities between 2012-14, which created a total of 66,242 housing units.[13] Figure A.1(b) presents the number of housing units per 1000 people for each Colombian *departamento* which are administrative divisions roughly equivalent to U.S. states. The figure shows that the number of housing projects are relatively equitably distributed across *departamentos* on a per capita basis, aside from some *departamentos* in the east of the country which are covered by the Amazon and have minimal population. The notable exception to this is the Caribbean coast where nearly twice as many housing units per capita were built, possibly as this region was affiliated with the Minister of Housing at the time, Vargas Lleras.

The housing units usually involved two-bedroom apartments in cities or single-story row houses in towns. The size of these developments varied widely: On average, housing projects consisted of 330 units but some projects only had a few dozen units while others were full-sized neighborhoods or apartment complexes with over 4,000 units. The housing developments were also prioritized for social infrastructure through an agreement with various ministries. For example, the Ministry of Technology provided internet connection points, the Department of Sport built sport fields, the Ministry of the Interior installed security cameras, and the Ministry of Culture provided 8 books for each housing unit. The only stipulation for recipients was that they could not sell or rent the house for a period of ten years after receiving the deed. Our best estimate of the average market

[12]For example, one-fifth of Colombia's subsidized housing in 2011 was found to be on land highly-susceptible to flooding (Gilbert, 2014).

[13]A further 70 developments containing roughly 30,000 housing units were completed in 2015. Given the sample restrictions we make (see Section 3.2), we only include pre-2015 developments in our sample.

value of a housing unit upon receipt is USD 30,000.[14]

Overall, these housing projects represented a substantial improvement in terms of both physical structure and location compared to recipients' prior residences. In terms of physical quality, the housing projects were well-built, largely due to quality controls put in place by the government, including that the units had to pass inspections before builders were paid for their work. The homes were between 425-500 square feet and were required to have 2 bedrooms, a bathroom, a kitchen, space for a dining room, as well as sewer and electrical connections. As an example, Panel A of Figure 1 displays the pre-lottery housing for an applicant compared to the government provided housing units that the applicant eventually received. The photos make clear the poor housing conditions that the household faced before the lottery and the substantial improvement the housing unit from the Free Housing program represented. Panel B of Figure 1 also shows some examples of large public housing projects from the program in two major cities: Pasto and Bogota.

Location was another aspect in which the public housing represented a large upgrade for recipients. As the properties had to meet several criteria in terms of proximity to public services, most of the projects were located in desirable areas with many amenities. A government report detailed that 75% of the projects are located near main avenues, 76% are located near a park, and 80% are near a school (Departamento Nacional de Planeación, 2014). For example, a major free housing project in Bogotá, *Plaza de la Hoja*, is located directly next to a station on the *TransMilenio* – the city's key public transportation system – and is only 20 minutes away from Bolívar Square in central Bogotá via public transit. In contrast, the majority of recipients previously lived in "*comunas*," which are located in the hilly suburban and peripheral areas of major Colombian cities. Houses in these *comunas* typically lack property rights, are poorly-built, and the neighborhoods themselves feature high crime rates and are located far from city centers (i.e., roughly equivalent to the notorious *favelas* in Brazil).

Table A.1 provides empirical support that the public housing units improved access to amenities. To do so, it uses a survey[15] that was conducted among lottery winners and losers and presents the (self-reported) travel time to various amenities. After the

[14]Our estimate comes from current market prices (as of early 2024) for these public housing units which average COP 125 million. Deflating to 2015, this would equate to COP 76.5 million which converts to USD 30,000 in 2015. We observe market prices because in 2021 the Colombian government changed the law and allowed units to be sold after residing in the unit for five years.

[15]We discuss this survey further in Section 3.2.

lottery, lottery winners report 30-40 percent reductions (or 5-10 minutes) in commute times to the nearest public transport station, grocery store, park, school, and hospital. Reductions in travel time were also seen for various other public amenities, indicating that the location of public housing projects were substantially better than recipients' counterfactual housing. The only commute time that increased for lottery winners was visiting family members or relatives which is in line with moving to a new neighborhood, potentially away from relatives who remained in your old neighborhood.

Program Eligibility: Three groups of individuals were eligible for the program: (i) victims of natural disasters, (ii) internally displaced persons (usually due to armed conflict), and (iii) the 'extreme poor.' These groups were then further subdivided into up to eight priority tiers based on need. The three eligibility groups were not mutually exclusive as individuals could belong to the 'extreme poor' and either be victims of natural disaster or internally displaced.[16] Effectively, however, the groups were mutually exclusive as individuals would be assigned to the group where their priority tier would be the highest.

Identification of beneficiaries and their priority tier was conducted across several government agencies which identified 250,000 potential beneficiaries. The Ministry of Housing then constructed project-specific lists of beneficiaries as only current residents of the municipality were eligible for a given project.[17] Using the project-specific lists, the Ministry of Housing opened a call for applications from potential beneficiaries when each project neared completion and entrusted the country's *Cajas de compensación familar*[18] to contact each household on the list to apply. The *Cajas de compensación familar* attempted to notify each potential beneficiary of their eligibility via a phone call (although the success rate of reaching individuals via phone is unclear), alongside a public information campaign about the program through radio, television, newspaper, billboards, and informational campaigns in their local communities. Applications for each project could also be made by households not on the potential beneficiary list, with auditors determining their eligibility for the program.

Given the use of federally-determined beneficiary lists, the selection process was

[16] The victims of natural disaster and internally displaced groups were, however, mutually exclusive.

[17] Individuals would also be ineligible if they had previously been granted a housing subsidy or if they owned property.

[18] *Las Cajas de compensación familar* are non-profit entities in Colombia that are overseen by the State. Each *departamento* has one of these entities whose main duty is to administer the 'family subsidy,' a social benefit to middle- and low-income beneficiaries that is funded by a 4 percent payroll tax. Effectively, these entities serve a similar function to that of the U.S. Social Security Administration.

mostly free of political interference, making many local politicians hoping to use the program to curry favor with voters unhappy (Gilbert, 2014). That said, fraud in the program inevitably occurred with some beneficiaries subsequently being found ineligible and evicted after receiving houses.[19]

Assignment of Beneficiaries to Houses: As each project was nearing completion, the project's housing units were assigned to one of the three beneficiary groups. Housing units were assigned across the specific groups following the broad assignment rules embedded in the authorizing law, with the exact distribution of units being jointly determined by the Ministry of Housing and the mayor of the municipality. In general, the decision-makers tried to match the distribution of units to the distribution of beneficiaries in that municipality, although favored internally displaced persons due to the government's focus on reparations for victims of the long-standing conflict.

Once the supply of units for each beneficiary group was set, the assignment of units among each group was conducted according to priority tier until all units had been assigned. If there were more applicants than units within a priority tier, a lottery would be held to determine the recipients. Approximately 70 percent of recipients were directly assigned to housing, while 30 percent were assigned via lottery.

We clarify the assignment mechanism with an illustrative example of a housing project with 100 units designated for the 'extreme poor.' Suppose that 200 'extreme poor' apply for housing, with the applicants evenly divided among five priority tiers. Then, all eighty individuals belonging to the first two priority tiers receive housing, while the eighty individuals in the last two priority tiers do not. Among the third priority tier, however, there are forty applicants for the twenty remaining housing units. Housing for these individuals would then be assigned via lottery.

The lotteries were run by the Department of Social Prosperity. To ensure fairness, the draws were publicized via radio and local press with potential beneficiaries invited to attend the draw. The draw was then conducted at a suitable site (e.g., a soccer stadium), with chairs and water provided for attendees. By law, the draw had to be attended by several public officials (or their designees): (i) the Governor of the *departamento*, (ii) the Mayor of the municipality, (iii) the Director of Social Prosperity, (iv) the Executive Direc-

[19]For example, 13 of the 91 beneficiaries of the first public housing project to open in La Pradera, Valle were later found to be ineligible and were evicted. This was, however, a relatively rare phenomena with only 170 public housing units (as of November 2019) being revoked from beneficiaries for being ineligible or breaking the program's rules (e.g., subletting their unit).

tor of the National Housing Fund, and (v) the Municipal Representative (the Colombian equivalent of an ombudsman).

After the lottery to determine recipients, another draw was conducted to assign recipients to housing units. To do so, the project's housing units were placed in a physical urn and recipients were invited up one at a time to draw their housing unit. If a recipient did not physically attend the lottery, one of the public officials drew their housing unit at the end of the draw for them. Once assigned to a unit, the recipient was able to inspect the unit and then would sign the deed in the presence of a notary. The average time between unit assignment and delivery of the house was four months.

Figure 2 visualizes the distribution of applicants and beneficiaries across the three eligibility groups. First, we note that there are relatively few applicants who were victims of natural disasters and almost none of these individuals participated in a lottery. As lottery participants form our analysis sample, individuals who were victims of natural disasters will not contribute meaningfully to our empirical analysis. Turning to the 'extreme poor,' we see that there are roughly 60,000 applicants from this group. Among these applicants, 14 percent were directly assigned to public housing, 28 percent did not receive public housing as they had insufficient priority, and 57 percent participated in a lottery. Among lottery participants, a quarter won the lottery and so received public housing.

The largest applicant group was the internally displaced consisting nearly 73,000 households. Conditional on applying, these individuals were far more likely to receive housing: 41 percent directly received public housing while only 24 percent were rejected due to insufficient priority. The remaining 35 percent participated in a lottery, with 44 percent of these lottery participants winning. The improved odds of receiving public housing among the internally displaced compared to the 'extreme poor' was in line with the government favoring this group as a form of reparation for victims of conflict.

3 Empirical Strategy and Data

We describe our empirical strategy which leverages the public housing lotteries to estimate the intent-to-treat impact of receiving a public housing unit by comparing outcomes of winners and losers. The data sources used for this project are also detailed.

3.1 Empirical Strategy

As public housing for a subset of applicants was assigned by lottery, we can intuitively compare outcomes between those who won the lottery and those who did not to provide an unbiased estimate of being offered a public housing unit on education. As we have many lotteries in our data, we include lottery fixed effects to ensure that only winners and losers within the same lottery are compared. Fortunately, each lottery at a housing project was given a unique identifier and so project-by-lottery-identifier groupings uniquely identify lotteries in our data. The project-specific lottery identifiers roughly correspond to eligibility-group-by-priority-tier groupings.[20] Hereafter, we call these project-by-lottery-identifier groupings 'lottery fixed effects.'

Our analysis incorporates the fact that a few municipalities had several projects, implying that applicants could apply multiple times for public housing and, since each project's lottery is independent, the probability that an applicant wins will rise with the number of applications. Fortunately, our data contain the date of application and so we only use the lottery outcome from each applicant's first application (Ketel et al., 2016).[21] Applicants who lose the first lottery but subsequently apply and gain access to a housing project will therefore be considered lottery losers in this setup. Importantly, in our context lottery losers who eventually gain access to public housing represent a small proportion of applicants and even those who eventually obtain a housing unit receive it at a much later date than lottery winners. Therefore, by the end of our data (in 2019) lottery losers effectively had no exposure to public housing (see Table 2 where the mean years of public housing among lottery losers is 0.1.)

Formally, we estimate the impact of receiving a public housing unit on child outcomes using the following regression:

$$y_i = \alpha + \beta D_i + \delta X_i + LC_i + \epsilon_i \,, \tag{1}$$

[20] Lottery identifiers do not exactly correspond to eligibility-group-by-priority-tier groupings since a few housing projects have multiple lotteries for a given eligibility group. Multiple lotteries occur when housing units become available after the initial lottery (e.g., because a recipient is evicted, leaves, or does not accept the housing unit).

[21] Alternatively, one could define lottery risk sets as the group of non-degenerate lotteries to which an applicant applied (Abdulkadiroğlu et al., 2011). Unfortunately, while our data include date of application and date of housing receipt, they do not contain date of lottery. Therefore, there are a few cases where we are unsure if the applicant has applied to multiple lotteries simultaneously or applied to the subsequent lottery after losing the first making it difficult to define the risk sets.

where y_i is the outcome of child i, D_i is a dummy variable equal to one if the child's family won the *first* lottery they applied for, and X_i is a vector of controls which include an applicant's age at first lottery along with pre-lottery characteristics (e.g., gender, family wealth, etc.). We also include lottery fixed effects for the *first* lottery that child i's family applied for, LC_i, which ensures that the probability of receiving housing is identical among individuals (conditional on the lottery fixed effects). Our parameter of interest is β, which is the impact of winning the lottery on child outcome y. Compliance with the first admission lottery is very high in our data (see Table 2) and so the effect of winning the lottery can roughly be interpreted as the impact of receiving a public housing unit.

3.2 Data

We now describe the various data sets that we have assembled. To start, we highlight the cohorts that will be the focus of our study.

Sample Restrictions: Our goal is to evaluate the impact of the Free Housing program on the educational attainment of children. In particular, our key outcome of interest is high school graduation. To do so, we must restrict our data to individuals who were children at the time of the housing lottery and are old enough to have graduated high school by the end of our data in 2019.

We therefore make two sample restrictions. First, we restrict our data to children who are at least 18 by the end of 2019 to ensure that the child had the opportunity to finish high school. In Colombia, high school ends after eleventh grade when students are usually 17. Restricting our data to those 18 or older in 2019 therefore ensures that these children have reached the age to graduate, allowing for one year of grade repetition. Second, we restrict our sample to children aged 15 or below at the time of their first lottery application. This restriction is made so that the child has not already dropped out of school at the time of the lottery since the legal dropout age in Colombia is 16. Combined with the restriction that children must reach the age of 18 by 2019 makes is so that (almost) all children in our data are aged 13 to 15 at the time of their first lottery application.[22] These restrictions also ensure that children have been in public housing a sufficient time period for effects to appear.

Public Housing Program Data: We start with data on the universe of public housing applications. These applications are made by the household head and contain information

[22]There are a few (≈100) 12-year-old children in our data from the earliest lotteries.

on the household's eligibility for the program, the eligibility group that they belonged to, their priority tier, how public housing assignment was determined (i.e., by lottery or directly admitted), the lottery identifier (if applicable), the lottery outcome (if applicable), and the date of housing receipt (for lottery winners and those directly admitted). Given that our empirical strategy only uses information from lottery participants, we focus on the 60,042 households whose public housing receipt was determined by lottery.

The application data only contain information on the household head. Using the national ID of the household head, however, we can link these individuals to their children (and spouses) using the SISBEN III (described below). We match 94 percent of household heads in the application data to the SISBEN III (and thus to any children in their household).[23] Among the 60,042 households whose public housing receipt was determined by lottery, a total of 15,026 children belonging to 13,415 households meet our sample criteria defined above.

SISBEN: The SISBEN or the "Census of the Poor" is a census of Colombia's low-income population which aims to capture the wealth of individuals for means tested social programs, such as free health care and conditional cash transfers. The data are collected door-to-door by surveyors and include rich demographic and socioeconomic information of all household members including sex, age, date of birth, education, marital status, occupation, income, household size, dwelling characteristics, and indicators of household wealth (e.g., has a fridge).

Our main results use the third wave of the SISBEN or "SISBEN III" which was conducted in 2009-10, a few years before the first housing lottery. The SISBEN III covers roughly 28.5 million people, corresponding to about 62 percent of the population. Since the SISBEN specifically targets poor households, however, the coverage rate for the disadvantaged individuals eligible for public housing is near-universal. The SISBEN III data allow us to examine baseline characteristics of the lottery participants (see Table 1 – discussed in the Results section below – where we compare the pre-lottery characteristics of lottery winners and losers) and control for several pre-lottery covariates in our empirical models.

We also use the fourth wave of the SISBEN or "SISBEN IV" which was conducted in

[23]Matching of individuals in the housing application data to the SISBEN III was done by the *Departamento Nacional de Planeación* who reported a match rate of 94 percent. The matched data was then provided to the researchers. The researchers therefore do not have access to the underlying raw housing application data, although the high match rate alleviates concerns that a differential match rate between lottery winners and losers could substantively bias results.

2019-20 to compare the post-lottery employment, income, spending, wealth, and housing characteristics of lottery winners and losers. Unfortunately, the data collection for the SISBEN IV was interrupted by COVID so our match rate is imperfect.[24] Still, we are able to match 70.5% of household heads in the application data to the SISBEN IV.

Universe of Students in Colombia's Public Schools: The second administrative data source we use is the core database of the Ministry of Education, which provides information on school progression for all students in public schools. (While the Ministry of Education data do not include private schools, the ICFES data that follow do.) In particular, the data allow us to observe the first year that a child entered the school system (e.g., first grade) up to high school graduation (or dropout) for everyone who was ever enrolled in the public school system. The data indicate whether a student has received a high school diploma as well as the specific school that a child attends each year (although it does not contain information on test scores). We use data up to 2019, the last year available.[25]

End-of-High School Exam (ICFES): The ICFES is the national high school exit exam administered by the *Instituto Colombiano para el Fomento de la Educación Superior* (ICFES). The exam is mandatory for all high school seniors (including private schools) who must pass the exam in order to graduate.[26] The exam scores are also used for admission purposes for those who apply to college. The ICFES includes separate tests on math, Spanish, social studies, sciences, and an elective subject. We aggregate the subject-specific scores into a continuous variable that captures the average score across all individual subjects and standardize these scores to have mean zero and standard deviation one each year. The data are available up to 2019. We use both test-taking and ICFES scores as outcomes in our analysis.

Universe of Students in Tertiary Education (SNIES): The third administrative dataset is the National Information System on all students enrolled in any tertiary ed-

[24]The SISBEN IV data collection is now complete, but our ability to match the housing application data to the SISBEN ended after September 2021 when the data collection was still ongoing.

[25]Colombia's academic year mirrors the calendar year.

[26]Therefore, ICFES-passing can be used as an alternative measure of high school graduation that includes children in private schools. Our results using this alternative measure (not reported) are near-identical to our main high school graduation results. This is because few students in our population attend private schools as over ninety-three percent of children from families belonging to the 'extreme poor' eligibility group attend public schools (Ministry of Education, 2016: https://www.mineducacion.gov.co/1759/articles-356787_recurso_1.pdf).

ucation institution in the country.[27] This resource provides information on student progression across public and private universities, community colleges, or any other tertiary education agency. The data are available up to 2019. We match the lottery sample to the SNIES to measure college-going.

Household Survey: We also have access to a household survey designed specifically to investigate the impact of the public housing program among a representative sample of lottery participants (Camacho et al., 2021). The survey was conducted by the *Centro Nacional de Consultoria* between August 6 and September 6, 2020. The survey equal-weighted lottery winners and losers and was administered via telephone and interviewed individuals who participated in the housing lottery from 40 projects. The response rate to the survey was 89 percent, giving us a total of 2,563 surveys, including 1,264 lottery winners and 1,299 lottery losers. We note that the survey includes all lottery participants which differs from our main analysis sample which focuses on children of a certain age who participated in the lottery. Regardless, the survey provides us a unique opportunity to investigate the impact of winning the lottery on households' access to amenities and the quality of the neighborhood they reside in (see Section 5.2).

Descriptive Statistics: Column (1) of Table A.2 shows summary statistics (measured pre-lottery) for all individuals who applied to public housing. It is clear that applicants to public housing are relatively disadvantaged, with fewer than half having a fridge, ten percent having a washing machine, and three percent owning a vehicle. In comparison, roughly eighty percent of Colombians have a fridge, sixty percent have a washing machine, and twenty-five percent have a vehicle. Columns (2) and (3) then display summary statistics for applicants who were directly assigned a public housing unit and those who participated in a lottery, respectively. These two groups appear relatively similar to applicants at large.[28]

The next three columns of Table A.2 focus specifically on individuals who are part of the 'extreme poor' eligibility group. Doing so allows us to investigate selection into applying for public housing as we observe all 'extreme poor' individuals in Colombia in the SISBEN III whose summary statistics we report in column (4).[29] Column (5) then

[27] For more information on the SNIES data: https://snies.mineducacion.gov.co/portal/.

[28] *A priori*, one would expect directly assigned to be more disadvantaged. While this is true for a given project, it is not true in aggregate as larger cities tend to have more applicants being directly assigned and applicants from larger cities owned more durable goods and had better pre-lottery access to services.

[29] We note that being 'extreme poor' does not imply that you are eligible for public housing. In particular, you must also reside in a municipality with a housing project.

focuses on 'extreme poor' applicants; public housing applicants are more disadvantaged than the 'extreme poor' population as a whole since they are less educated, less likely to have assets such as fridges or washing machines, and are less likely to be employed.

Our analysis sample then restricts our data to children who are: (i) younger than 16 when applying for public housing, and (ii) 18 or older in 2019. This sample consists of 15,026 children, of whom 3,917 won the lottery and 11,109 lost the lottery. For the most part these children are from different families, although our data does include roughly 1,600 siblings. Figure A.2(b) shows the locations of our analysis sample, with the size of the pin indicating the proportion of our sample that applied to a given project. Compared to the spatial distribution of public housing units (see Figure A.2(a)), our analysis sample is somewhat overrepresented in cities along the Caribbean coast.

Column (1) of Table 1 shows summary statistics for our sample of children (measured pre-lottery). The average age of a child at their first lottery is 13.8 years and about half come from families where the parents are married. Households tend to have an average of 5.8 members. While the program was not targeted to rural households, we do see that about twenty percent of the sample resided in rural areas prior to the lottery. Ninety-five percent of the sample has access to electricity and 80 percent have access to water and sewage in their home.

4 Results

We first discuss the validity of our empirical design based on lotteries and then present the first-stage and reduced-form results. Given the high levels of compliance to the lottery (especially in terms of years in public housing), our results are reported as intent-to-treat estimates. Throughout, standard errors are adjusted for two-way clustering at the municipality and family levels to account for the fact that children face common municipality-level shocks and our data sometimes feature multiple entries per family (Cameron et al., 2011).

4.1 Validity

The validity of the empirical design laid out in Section 3 relies on the fact that the lotteries were indeed random (conditional on lottery fixed effects). Given the publicity surrounding these lotteries and the fact they were well-attended by both public officials and potential recipients (see Section 2), we suspect there is limited scope for cheating.

Regardless, we verify that these lotteries appear to be random by checking for covariate balance.

Table 1 checks for covariate balance among the lottery winners and losers, with all covariates measured before the lottery. Columns (2) and (3) show treatment and control means of pre-lottery child demographics, household head characteristics, dwelling attributes, and measures of household wealth. Differences between the treatment and control means are shown in column (4), with the p-value from a formal test of equality between the lottery winners and losers that accounts for lottery fixed effects being reported in column (5). Reassuringly, the table shows that there are few statistically significant differences between lottery winners and losers. Only two characteristics are statistically different across lottery winners and losers: (i) "child's age at first lottery" (a difference of 0.03 years or 11 days), and (ii) "house has water/sewage" (a difference of 4 percentage points). Considering that we are testing balance for twenty characteristics, it is expected that by chance some of these covariates will not be statistically balanced[30] and a joint hypothesis test cannot reject the null hypothesis that all coefficients in Table 1 are equal to zero. In addition, controlling for these (and other) characteristics in our empirical models has little impact on our coefficients of interest.

First-Stage: While we expect almost all lottery winners to accept the free public housing given the generosity of the program, lottery losers may still receive public housing since they can apply to another housing project in the same municipality. Table 2 shows the 'first-stage' results of winning the lottery on receiving public housing both in terms of ever receiving public housing and the number of years the child was in public housing (up to 2019). We report results both for the full sample of individuals who participated in the lottery and our main analysis sample of children.

Focusing on our main analysis sample in columns (3) and (4), we find that winning the lottery raises the probability of receiving public housing by 80 percent and increases the number of years the child resides in public housing by 4.1 years. We note that the compliance rate of 80 percent gives a somewhat skewed picture of the first-stage, since the non-compliance among lottery losers is driven by those receiving public housing many years later. In particular, compliance among lottery winners was near-universal (95%) and while 15% of lottery losers did eventually gain access to public housing, this occurred several years later such that the average lottery loser only experienced public housing for

[30]E.g., given that we are testing twenty covariates, the probability that two or more covariates will be statistically significant at the five percent level is 26.4 percent.

0.1 years. Given the high rate of compliance in terms of years in public housing, we report intent-to-treat (or 'reduced-form') estimates hereafter. We interpret these intent-to-treat estimates as the impact of living in public housing for 4.1 years on educational outcomes.

4.2 Results on Children's Educational Outcomes

Table 3 reports our main estimates of public housing's impact on children's educational outcomes, with column (1) reporting results of equation (1) without any controls (aside from lottery fixed effects) while column (2) includes detailed controls (measured pre-lottery). As expected, the inclusion of controls has little effect on our results and so we treat results from column (2) as our preferred estimates. We find that the children of lottery winners have substantially improved educational outcomes compared to lottery losers. The point estimates reveal that winning the lottery increases high school graduation rates by seven percentage points, a staggering *seventeen* percent increase relative to the control mean of forty-two percent. Similarly, we find that winning the lottery increases high school exit exam (ICFES) taking by seven percentage points, which are near-identical to our results for high school graduation (to be expected given that the exit exam is required for graduation). (The ICFES results include students attending both public and private schools and so provide an alternative high school graduation measure that is robust to attrition from public-private school sorting.) We also find that public housing receipt increases years of education by 0.51 years (or 6% relative to the control mean) and the probability of enrollment at a tertiary education institution by 1.4 percentage points (or 10% compared to the control mean).

The second panel of Table 3 reports the impact of public housing on exam scores from the ICFES. In terms of performance on the exam, we find that lottery winners score 0.03 standard deviations higher than lottery losers. We also investigate the math and reading subcomponents of the ICFES and find that winning the lottery increases math and reading scores by 0.01 and 0.04 standard deviations, respectively. While these test score improvements are not statistically significant, we note that public housing receipt also increases ICFES-taking and so these estimates are likely contaminated by selection bias. In particular, we expect that winning the housing lottery encourages academically weaker students to remain in school and take the ICFES which would bias our test score estimates downward. We correct for this selection bias below.

We find limited heterogeneity in our results by baseline characteristics. Specifically, Figure A.3 shows the impact of winning the lottery on high school graduation when

the sample is split by several baseline characteristics: gender, age at lottery, household head's education, and mother's marital status. No significant differences across these demographic characteristics emerge. We note that we have limited variation in children's age at lottery given that our sample restrictions imply that the effective range for child's age at lottery is 13-15.

Selection Bias Correction: Our ICFES test score results likely feature sample selection bias as only students who did not drop out of high school took the exam and we have demonstrated that public housing receipt lowers high school dropout by *seventeen* percent. To address this selection issue, we follow Angrist et al. (2006). This strategy codes the latent scores of those who did not take the ICFES as falling below a particular percentile, then censors the ICFES distribution at or above this value, and finally uses Tobit to correct for censoring.

Table 4 reports our results. As a benchmark, we report the selection contaminated estimates in column (1). Column (2) then censors the ICFES distribution at the first percentile among test-takers *but* does not adjust for censoring in the estimation (i.e., we simply assign the first percentile of the ICFES score to those who obtain a lower score or who did not take the exam). Doing so, we estimate that winning the lottery raises ICFES scores by 0.13 standard deviations. If we instead censor at the tenth percentile (Column (3)), our estimate drops somewhat to 0.09 standard deviations.

Once we account for censoring in the estimation using Tobit, the impact of winning the lottery on ICFES scores grows. Censoring at the first percentile – reported in Column (4) – leads to a point estimate of 0.28 standard deviations. If we instead censor at the tenth percentile, our point estimate falls to 0.22 standard deviations. A natural test for the empirical strategy is to compare Tobit estimates across different censoring points; these estimates should be similar if the selection model is correctly specified at each of these censoring points. Figure A.4 compares the Tobit estimates across all possible censoring points, finding that point estimates are very stable when the distribution is censored with a cutoff that removes the lower 10-90 percent of scores.[31] Overall, our selection-corrected

[31]The lack of the stability in the tails was also found by Angrist et al. (2006), perhaps because Tobit assumes the latent ICFES score distribution is normally distributed, which may be an especially poor approximation in the tails. Following Angrist et al. (2006), we have also relaxed the normality assumption by constructing quantile-specific nonparametric bounds which only assume that winning the lottery is never harmful, a reasonable assumption in this setting given that public housing could always be turned down. These bounds indicate that public housing receipt raises test scores by 0.03-0.11 standard deviations for the median student (results not shown).

estimates indicate that winning the lottery generated large improvements in ICFES scores of around 0.1-0.2 standard deviations.

5 Mechanisms

To explore the mechanisms underlying our results, we perform three separate descriptive analyses. First, we investigate whether school quality matters using school value-added methods. Second, we compare neighborhood quality measures among lottery winners and losers to gauge the potential for neighborhood effects to be driving our results. Third, we explore family income, labor force participation, assets, and expenditures among lottery winners and losers to gauge the potential for wealth effects to be generating our results. We end the section by conducting a mediation analysis.

5.1 School Quality

Lottery winners moved to new neighborhoods, often necessitating them to change schools. These lottery-induced school changes among beneficiaries are one possible mechanism driving the large educational gains we find. To investigate this, we use value-added methods to measure the quality of schools attended by lottery winners and losers before and after the lottery. We measure school quality by constructing school value-added using pre-period data. Using pre-period data ensures that we cleanly capture differences in school quality, rather than conflating school quality with other potential influences caused by the nearby public housing (which may occur if we used contemporaneous data since the public housing could impact school value-added).

To estimate school value-added, we use data from cohorts entering lower-secondary schools[32] in 2006-2008. (Table A.3 reports summary statistics for these data.) Crucially, our choice to only use cohorts entering in 2006-2008 guarantees that no children who are part of the Free Housing program are in this sample. We formally model high school graduation as follows:

$$y_{ics} = \alpha + \beta X_{ics} + \mu_s + \epsilon_{ics}\,, \tag{2}$$

where y_{ics} is an indicator that student i in cohort c entering school s received a high school diploma, X_{ics} is a vector of controls,[33] and μ_s is a school's value-added or the contribution

[32]Education in Colombia is divided into three phases: elementary (grades 1-5), lower secondary (grades 6-9), and upper secondary (grades 10-11).

[33]We include all the detailed sociodemographic controls we use in Table 1 along with school-grade

of school s to student i's probability of graduation. We estimate equation (2) using data from the Ministry of Education linked to the SISBEN III for all students entering lower-secondary schools in 2006-2008, covering 1,634,937 students attending 10,658 schools. Following the literature, we estimate μ_s using empirical Bayes to minimize mean squared error.[34]

The estimated school value-added, $\hat{\mu}_s$, is the component of the average graduation rate for each school that is not explained by the individual characteristics of its students. Crucial to the estimation of school value-added is that the control vector, X_{ics}, is sufficiently rich so that the estimated value-added captures school-specific characteristics that raise graduation rather than the characteristics of the students themselves. In developed countries, lagged test scores are often used as the key control variable in value-added models (Chetty et al., 2014). Given the lack of test score data, we follow other researchers in the South American context (e.g., see Neilson (2021)) and instead rely on controls based on finely-grained data covering household characteristics (e.g., household earnings and wealth, parental education, number of siblings, etc.). Given the detail in these socioeconomic controls – which far surpasses those available in most education datasets – we believe that our school value-added estimates should feature limited bias and so this descriptive exercise can provide a clear picture of the quality of schools attended by lottery winners and losers.

Figure 3 displays the coefficients of the difference in mean school value-added for the schools attended by lottery winners and losers for each year relative to the lottery date as in an event-study design. (The difference in school value-added in the year of the lottery is normalized to zero.) Lottery winners and losers attend similar quality schools up to the year of the lottery. After the lottery, however, lottery winners start attending schools with higher value-added relative to lottery losers. The magnitude of the post-lottery jump in school value-added for lottery winners (relative to losers) is approximately 0.02. Taken at face value, this implies that roughly one-third of the improvement to high school graduation experienced by lottery winners relative to losers can be attributed to the better schools that they attend.

Robustness: We repeat the above exercise but also include neighborhood fixed effects

means of those controls.

[34]Formally, $\mu_s = y_s \frac{\sigma_s^2}{\sigma_s^2 + \sigma_\epsilon^2 / \sum_c n_{sc}}$, where $y_s \equiv \sum_c n_{sc} y_{sc} / \sum_c n_{sc}$ is the fixed effect of school s in equation (2), n_{sc} is the number of students in cohort c at school s, and σ_s^2 and σ_ϵ^2 are the variances of μ_s and ϵ_{ics} (which we estimate via maximum likelihood and plug-in).

– where neighborhoods are defined by census-segment[35] – in the control vector X_{ics} in equation (2). Doing so allows us to separate out the influence of schools relative to neighborhoods as in Laliberté (2021) by ensuring that our school quality measures are not picking up the influence of the neighborhoods. Results are shown in Figure A.5 and are nearly identical to our baseline model without neighborhood fixed effects. Alternatively, we could calculate value-added in terms of the ICFES scores (rather than high school graduation). We therefore repeat the exercise but replace the dependent variable in equation (2) with students' ICFES scores. Results are reported in Figure A.6 using both the raw ICFES scores and selection-corrected ICFES scores. Once again, roughly one-third of the improvement in ICFES scores is attributed to the better schools that lottery winners attend.

5.2 Neighborhood Quality

Using the household survey and household locations in 2019/2020 from the SISBEN IV, we next investigate how the neighborhoods where treatment and control families live differ along multiple dimensions, including (perceived) neighborhood problems, crime (police-reported), and poverty. (In addition, Section 2 introduced Table A.1 showing that lottery winners reported substantial reductions in commute times to various amenities.) We note that neighborhood quality is not strongly correlated with school value-added in our setting: The correlation between school value-added and our measures of neighborhood quality are only 9-12 percent. This indicates that school and neighborhood quality measures capture different attributes of children's close environments.

First, we use the household survey to contrast perceptions on neighborhood problems between lottery winners and losers. To do so, we use participants' survey responses on how often they notice problems in their communities such as bad street odors, extreme noises, trash on streets, etc. Given the number of questions asked, we construct a simple index that takes the equal weighted average of the z-score of all these questions on neighborhood problems. We label this the "neighborhood problem" index; a higher value of this index reflects more frequent neighborhood problems. Column (1) in Table 5 shows that lottery winners are 0.09 standard deviations less likely to report problems in their neighborhoods compared to lottery losers. Table A.4 reports results for each neighborhood problem separately, showing that reductions in neighborhood problems are driven

[35] A census-segment in Colombia roughly equates to a ZIP code in the United States.

by lower incidences of bad street odors (a reduction of 16% with respect to the control mean), air pollution (20%), water pollution (23%), and the presence of insects and rodents (35%).

Second, we investigate whether the neighborhoods where lottery winners reside are safer to those where the control group lives. For this exercise, we need to identify the location of lottery winners and losers in 2019. To do so, we use the SISBEN IV which was run in 2019-20 and records individuals' exact home address. Household locations are then geo-located to their police *cuadrante*.[36] We then use data from the National Police Department on major crimes – assaults, robberies, and homicides – reported in years 2018, 2019, and 2020 at the *cuadrante* level to construct a measure of crime for the neighborhoods of lottery winners and losers roughly 5 years post-lottery. We combine information on incidences of assaults, robberies, and homicide to construct a "neighborhood crime" index in the same way that we created the "neighborhood problem" index. Column (2) of Table 5 shows that lottery winners live in neighborhoods where crime is 0.05 standard deviations lower relative to lottery losers. (Table A.5 reports results separately by each major crime type.)

Third, we compare the neighborhoods where lottery winners and losers reside based on household wealth and income. To do so, we define neighborhoods using Colombian geographic subdivisions which are roughly equivalent to U.S. ZIP codes.[37] To make columns comparable, we then calculate a "neighborhood poverty" index in a similar manner to our other two neighborhood indices based on a score that measures indicators of household wealth on the SISBEN and income (appropriately signed so positive indicates poorer) of all households in a neighborhood. Column (3) of Table 5 indicates that lottery winners live in neighborhoods that are 0.04 standard deviations richer than lottery losers.

Finally, we explore whether there exist heterogeneous effects based on the change in neighborhood quality a participant would experience if they win the lottery. We use neighborhood poverty to define neighborhood quality given that it is commonly-used for this purpose (Chetty et al., 2016).[38] For each lottery participant, we calculate the *change* in neighborhood poverty that the participant would experience if they win the lottery

[36] *Cuadrantes* are small and well-defined geographical areas within Colombian cities used for police street patrols. These geographic areas are assigned six police officers (divided into three shifts, so two officers per shift) to patrol them. For example, Bogotá has 1,048 *cuadrantes* and so each *cuadrante* contains roughly 6,800 people.

[37] Specifically, we use census-segment subdivisions which contain an average of 12,000 persons which is similar to U.S. ZIP codes which on average contain 10,000 individuals.

[38] We get similar heterogeneity results if we define neighborhood quality using crime rates instead.

(and accept the public housing) versus remaining at their current address. The average lottery participant who wins public housing would move from a neighborhood with a poverty rate of 20.1 percent to one with a poverty rate of 5.7 percent.[39]

Figure A.7 visualizes the heterogeneity by dividing participants into quartiles based on the change in neighborhood poverty that they would experience if they win the lottery (with quartile 1 representing the least change in neighborhood poverty). No clear relationship between participants' change in neighborhood poverty and the effect of winning the lottery on high school graduation are apparent in the figure. Therefore, differences in neighborhood quality are unlikely to be driving the treatment effects that we observe.

5.3 Family Wealth and Expenditures

We next explore the impact of winning the lottery on family wealth and expenditures. To do so, we link our sample of 15,026 children to the SISBEN IV which was conducted in 2019-2020. Unfortunately, the collection of the SISBEN IV was interrupted in March 2020 due to COVID and so we are only able to match 10,084/15,026 (67%) of our main sample to the SISBEN IV. Match rates, however, are similar among children who won and lost the lottery.[40]

The SISBEN IV contains detailed data on household wealth and dwelling amenities. To reduce dimensionality, we create two indices. The first index uses the durable goods owned by the child's household[41] to construct a 'household wealth' index. The second index is a 'household amenities' index that captures the services that are available to the child's household.[42] Both indices are constructed by standardizing each component and then taking the arithmetic average. We then use the household's percentile rank in this index as our outcome.

Log household income is also used to quantify household wealth. The household income measure we use from the SISBEN IV is self-reported. While these data may suffer

[39] We define a neighborhood's poverty rate as the proportion of households in the neighborhood who are 'extreme poor.'

[40] Specifically, we match 2,706/3,917 (69%) of the lottery winners and 7,378/11,109 (66%) of the lottery losers.

[41] We include the following durable goods: (i) fridge, (ii) washing machine, (iii) motorcycle, (iv) vehicle, and (v) any other assets or properties.

[42] We construct the index using the following nine characteristics and services of their dwelling: (i) walls made of 'durable' material, (ii) floors made of 'durable' material, (iii) the number of people per room, (iv) electricity access, (v) running water access, (vi) sewer and sanitation access, (vii) trash collection services, (viii) natural gas access, and (ix) internet.

from self-reporting bias, they crucially include income from both formal and informal employment. As informal employment is prevalent among our sample (roughly 50%), we believe that this income measure is more accurate than administrative tax data which only includes formal earnings. We also investigate (log) monthly family expenditures on two highly-relevant categories for educational attainment: education and food (Figlio and Winicki, 2005). All these variables are then used as dependent variables in equation (1).

Table 6 reports the intent-to-treat estimates of the effect of winning the public lottery on the wealth and expenditures of the child's family roughly five years post-lottery. It is evident that winning the lottery leads to large improvements in family wealth and income: lottery winners earn over 30% more household income than lottery losers and their asset and amenities indices are 10-13 percentiles higher. In terms of expenditure, lottery winners also spend 10-20 percent more on food and education (although these estimates are only marginally significant).

5.4 Mediation Analysis

The preceding subsections investigate how winning the lottery affects school quality, neighborhood quality, and household wealth. We find that public housing receipt improves all of these outcomes, naturally leading to a question on the relative importance of each mediator in driving our finding that public housing substantially improves educational outcomes. Given the strong assumptions required (see below), we view this mediation analysis as descriptive in nature.

Our mediation analysis follows the methodology proposed by Heckman et al. (2013). We start by defining the following mechanism specification:

$$M_i^j = \alpha + \gamma^j D_i + \delta X_i + LC_i + \epsilon_i \,, \tag{3}$$

where M_i denotes the intermediate mediator outcome j for child i and all other variables are defined in equation (1). (As a reminder, D_i indicates winning the lottery, X_i are controls, and LC_i are lottery fixed effects.)

We then regress:

$$Y_i = \alpha + \sum_j \theta^j M_i^j + \kappa^{Res} D_i + \delta X_i + LC_i + \epsilon_i \,, \tag{4}$$

where Y_i is high school graduation. The explanatory power for each mediator variable j is

then given by $\frac{\gamma^j \theta^j}{\beta}$ where β represents the total effect of winning the lottery on high school graduation from equation (1) (found to be 6.7-percentage points). The coefficient κ^{Res} then captures the component of the treatment effect that is not explained by improvements in intermediate mediator outcomes and can be expressed as $\kappa^{Res} = \beta - \sum_j \gamma^j \theta^j$. The validity of our mediation analysis hinges on the strong assumption that any unmeasured inputs are uncorrelated with our measured mediators (i.e., our estimates of θ^j are unbiased).[43]

Table A.6 reports the results of this exercise. Looking at the first row, we see that the proportion of the treatment effect that is unexplained by our mediators, κ^{Res}, is statistically insignificant although still represents 30% of the total treatment effect. We next investigate the percent of the total treatment effect explained by nine mediator variables. It is evident that the impact of public housing on family wealth (measured by our wealth and amenity indices) is a significant mediator, accounting for one-third of the total treatment effects. The remaining one-third of the total treatment effect is attributed to school quality. We find that neither household income nor expenditures are significant mediators. Neighborhood quality is also not a pivotal mediator, which aligns with both: (i) the lack of heterogeneity among lottery participants based on neighborhood change if they win the lottery, and (ii) our school value-added analysis whereby the inclusion of neighborhood fixed effects did not substantively alter our results (see Figure A.5).

6 Conclusion

This study investigates the effects of Colombia's "Free Housing" program on children's educational attainment and achievement. To do so, we leverage public housing lotteries and connect applicants to their children. These children are then linked to administrative datasets on public school enrollment, end-of-high school exams, and tertiary education. We find that receiving free public housing increases high school graduation rates by *seventeen* percent and enrollment in tertiary education by *ten* percent. Large improvements in years of education and exit exam scores are also seen.

The program that we study is highly-generous, providing housing units for free and in desirable areas of the city, close to a wide range of services such as high-quality schools,

[43] As discussed in Heckman and Pinto (2015), randomized assignment (as in our design) allows one to identify the causal effect of treatment on measured inputs and so the γ^js estimated in equation (3) are unbiased.

hospitals, parks, supermarkets, police stations, and public transport. Given this, approximately five years post-lottery the winners have higher income and wealth. Furthermore, the children of lottery winners attend better schools in terms of value-added and live in richer and safer neighborhoods. A mediation analysis uncovers that the vast majority of the program's intergenerational effects come through increased family wealth and better schools.

Evidence on the efficacy of housing assistance has been mixed, with its impact likely to vary with housing, neighborhood, and school characteristics (van Dijk, 2019). Our findings highlight that generous public housing programs in low- to middle-income countries can generate large gains in the educational outcomes of recipients, with a substantial driver of those improvements coming through increased family wealth and improved schools. The financial generosity of a public housing program and its location, particularly in desirable areas near high-quality schools and economic opportunities, appear to be the critical ingredients for public housing to break the intergenerational poverty trap.

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FIGURE 1: Housing Examples

Panel A: Example of Applicant Housing Relative to Public Housing

(A) Example of Applicant Housing in Lorica



(B) Government Housing Project in Lorica



Panel B: Example of Large Public Housing Projects

(C) Public Housing Project in Bogota



(D) Public Housing Project in Pasto



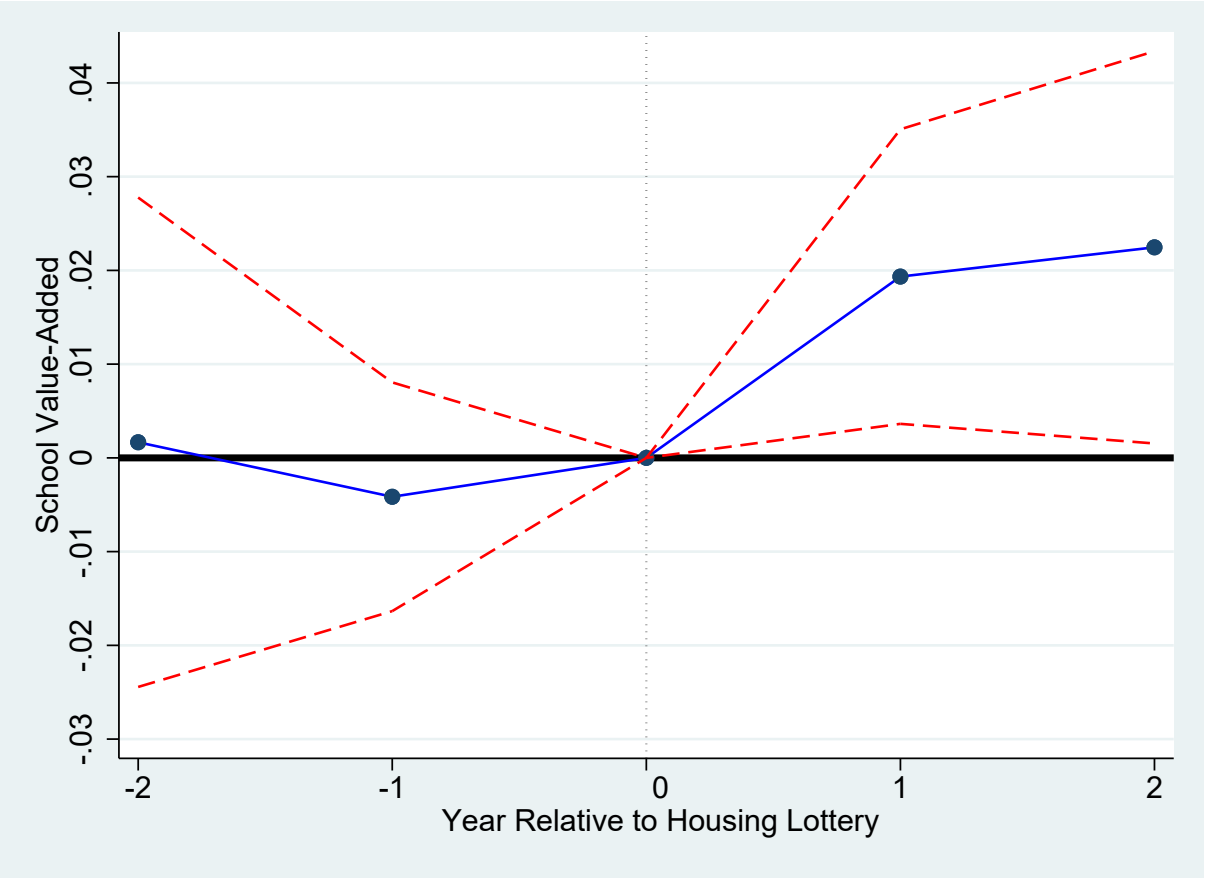
Notes: Panel A shows an example of pre-lottery housing for an applicant compared to the government provided housing units that the applicant eventually received from the "Free Housing" program. The photos come from the city of Lorica which is located in the department of Córdoba on the Caribbean coast. Figure 1(a) shows the residence of an applicant for the public housing project *Urbanización La Victoria en Lorica*. Figure 1(b) then shows housing units in the *Urbanización La Victoria en Lorica* housing project where the applicant moved to after winning the lottery. Panel B highlights two large public housing projects that were part of Colombia's "Free Housing" program. The photo in Figure 1(c) comes from Bogota, the capital of Colombia. The photo in Figure 1(d) then shows the public housing project in Pasto which is the capital of the department of Nariño in the west of the country.

FIGURE 2: Applicants by Eligibility Group and their Outcome



Notes: This figure shows the number of applicants by eligibility group where each applicant represents a household. For each eligibility group, the colors within the bar denote the number of applicants who were directly assigned to public housing, won the housing lottery, lost the housing lottery, and had 'Insufficient Priority.' 'Insufficient Priority' represents those who did not receive public housing and did not participate in the lottery as all housing units had been assigned before their priority tier was reached. We note that some individuals who applied were rejected before being assigned an eligibility group as their paperwork could not be verified; these individuals are not included in the figure. The exact numbers for each group are as follows: The extreme poor: 59,613 applicants, 25,300 lottery losers, 8,820 lottery winners, and 8,510 directly assigned. Internally displaced: 72,779 applicants, 14,186 lottery losers, 11,331 lottery winners, and 29,741 directly assigned. Victims of natural disasters: 13,296 applicants, 221 lottery losers, 184 lottery winners, and 7,303 directly assigned.

FIGURE 3: Impact of Winning Housing Lottery on School Quality



Notes: This figure shows the value-added of schools attended by lottery winners compared to losers relative to the lottery date of their first application (at year '0'). School value-added is calculated using cohorts from a pre-period to ensure that the public housing itself does not impact our school quality measure. Specifically, we use sixth grade entering cohorts from 2006-08 to construct school value-added. We then calculate and graph the difference in mean value-added for the schools attended by lottery winners compared to losers for each year relative to the lottery date. We normalize the difference in value-added between lottery winners and losers to be zero in the year of the lottery (i.e., year '0'). The dashed lines represent 95 percent confidence intervals with standard errors clustered at the municipality and family level.

TABLE 1. Covariate Balance

	Overall Mean (1)	Treated (Won Lottery) (2)	Control (Lost Lottery) (3)	Difference (Treated-Control) (4)	Test of Equality (p-value) (5)
Child Demographics *(Pre-Lottery)*					
Age at First Lottery	13.84	13.86	13.83	0.03	0.02
Head's Age at Birth	27.78	27.91	27.74	0.17	0.20
Female	0.49	0.50	0.48	0.02	0.13
Lived in Urban Area	0.78	0.75	0.79	-0.04	0.92
Household Head Characteristics *(Pre-Lottery)*					
Household Size	5.81	5.80	5.82	-0.02	0.24
Married	0.53	0.51	0.53	-0.02	0.55
Employed	0.51	0.50	0.51	-0.01	0.83
High School Graduate	0.26	0.27	0.26	0.01	0.14
Some Tertiary Education	0.14	0.13	0.14	-0.01	0.14
Dwelling Attributes *(Pre-Lottery)*					
Number of Rooms	2.77	2.79	2.77	0.02	0.22
Number of Bathrooms	0.89	0.88	0.89	-0.01	0.20
Has Shower	0.53	0.51	0.53	-0.02	0.32
Access to Services *(Pre-Lottery)*					
Electricity	0.95	0.94	0.95	-0.01	0.81
Water/Sewage	0.80	0.77	0.81	-0.04	0.03
Cable TV	0.18	0.21	0.16	0.05	0.35
Trash Collection	0.76	0.71	0.78	-0.07	0.99
Household Wealth *(Pre-Lottery)*					
Has Vehicle	0.03	0.03	0.03	0.00	0.56
Has Fridge	0.43	0.43	0.43	0.00	0.61
Has Washing Machine	0.11	0.11	0.11	0.00	0.93
Has TV	0.73	0.71	0.73	-0.02	0.39
# of Children	15,026	3,917	11,109	15,026	15,026

Notes: This table reports means for lottery winners ('treated') and losers ('control') along with treated-control differences in pre-lottery characteristics for the children of applicants who applied for public housing in Colombia between 2012 and 2014 and whose housing assignment was determined via lottery. The pre-lottery characteristics come from the SISBEN III and were collected in 2009-10. The sample is restricted to children who were 15 or younger at the time of their first lottery application and were 18 or older in 2019. The sample includes one observation per child, with children being assigned to treatment according to their first application. Column (5) reports the p-value of a hypothesis test on whether the difference between the treatment and control group is zero. The hypothesis test is implemented by regressing the covariate on a public housing offer for a child's first lottery application, controlling for lottery fixed effects. Standard errors are two-way clustered at the municipality and family level.

TABLE 2. Impact of Winning Housing Lottery on Public Housing Receipt (First-Stage)

	Full sample		**Main Analysis sample**	
	Ever Winning Housing Unit	Years in Public Housing (to 2019)	Ever Winning Housing Unit	Years in Public Housing (to 2019)
	(1)	(2)	(3)	(4)
Panel A. Without individual controls				
Won lottery	0.810***	4.212***	0.794***	4.209***
	(0.023)	(0.123)	(0.024)	(0.108)
Panel B. With individual controls				
Won Lottery	0.817***	4.222***	0.796***	4.209***
	(0.021)	(0.123)	(0.024)	(0.108)
Mean (control group)	0.098	0.110	0.150	0.129
Observations	60,042	60,042	15,026	15,026
% Winning Lottery	0.28	0.28	0.26	0.26

Notes: This table reports the effect of winning the public housing lottery on receiving public housing and so represents the 'first-stage' of our empirical strategy. We report the 'first stage' results both in terms of ever receiving public housing and the number of years of public housing the child experienced from the date of their first lottery application up until the end of 2019. All regressions include lottery fixed effects to ensure that only individuals in the same lottery are being compared. Panel A reports results when no controls are included (aside from lottery fixed effects), while Panel B contain controls for a child's gender, age at first lottery, whether a family lived in urban/rural area, household size, along with characteristics of the household head including age at birth, marital status, employment status and education (all measured pre-lottery in 2009-10). We show results for the 'full sample' which includes all households whose public housing receipt was subject to a lottery and the 'main analysis sample' which consists of children who were 15 or younger at the time of their first lottery application and were 18 or older in 2019. Of the 15,026 children in the main sample, 3,917 won the lottery and 11,109 lost the lottery. Standard errors are two-way clustered at the municipal and family level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

TABLE 3. Impact of Winning Housing Lottery on Educational Outcomes

Impact of Winning First Housing Lottery on:	No Controls (1)	Demographic Controls (2)	Control Mean (3)	# of Observations (4)
Panel A. Schooling Outcomes				
Years of Education	0.567***	0.511***	9.00	15,026
	(0.081)	(0.075)		
High School Graduation	0.077***	0.067***	0.42	15,026
	(0.018)	(0.017)		
Took ICFES	0.077***	0.065***	0.47	15,026
	(0.015)	(0.014)		
Enrolled in Tertiary Education	0.018**	0.014*	0.14	15,026
	(0.008)	(0.007)		
Panel B. High School Exit Exam (ICFES) Scores: No Selection Bias Correction				
ICFES Score	0.030	0.025	-0.36	7,447
	(0.025)	(0.028)		
ICFES Score (Math)	0.007	0.004	-0.42	7,447
	(0.027)	(0.027)		
ICFES Score (Reading)	0.045*	0.040	-0.41	7,447
	(0.029)	(0.029)		

Notes: This table reports intent-to-treat estimates of the effect of winning the public lottery on schooling outcomes as described by equation (1). All regressions include lottery fixed effects to ensure that only individuals in the same lottery are being compared. Column (1) reports results when no controls are included (aside from lottery fixed effects), while column (2) contain controls for a child's gender, age at first lottery, whether a family lived in urban/rural area, household size, along with characteristics of the household head including age at birth, marital status, employment status and education (all measured pre-lottery in 2009-10). The sample includes children who were 15 or younger at the time of their first lottery application and were 18 or older in 2019. Of the 15,026 children in the main sample, 3,917 won the lottery and 11,109 lost the lottery. The sample is smaller for the 'ICFES' outcomes as many children did not take the ICFES as they dropped out of school. Standard errors are two-way clustered at the municipal and family level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

TABLE 4. OLS and Tobit Selection-Corrected Estimates of the Impact of Winning Housing Lottery on ICFES Test Scores

	OLS	OLS censored at 1%	OLS censored at 10%	Tobit censored at 1%	Tobit censored at 10%
	(1)	(2)	(3)	(4)	(5)
A. ICFES Score					
"Won" in 1st lottery	0.025	0.128***	0.092***	0.279***	0.220***
	(0.028)	(0.031)	(0.024)	(0.058)	(0.050)
Control mean	-0.36	-1.16	-0.88	-1.16	-0.88
B. ICFES Math Score					
"Won" in 1st lottery	0.004	0.149***	0.090***	0.326***	0.226***
	(0.027)	(0.034)	(0.024)	(0.066)	(0.053)
Control mean	-0.42	-1.42	-0.96	-1.42	-0.96
C. ICFES Reading Score					
"Won" in 1st lottery	0.040	0.165***	0.108***	0.345***	0.265***
	(0.029)	(0.033)	(0.024)	(0.064)	(0.049)
Control mean	-0.41	-1.39	-0.96	-1.39	-0.96
N	7,447	15,026	15,026	15,026	15,026

Notes: This table reports selection-corrected estimates of the impact of winning the lottery on ICFES test scores following the methodology of Angrist et al. (2006). Column (1) simply reports the selection contaminated estimates; these estimates are identical to those reported in column (2) of Table 3. Column (2) then censors the ICFES distribution at the first percentile among test-takers *but* does not adjust for censoring in the estimation, with column (3) doing an identical exercise at the tenth percentile. Columns (4) and (5) then report the estimates when Tobit is used to correct for censoring. Lottery fixed effects and controls for a child's gender, age at first lottery, whether a family lived in urban/rural area, household size, along with characteristics of the household head including age at birth, marital status, employment status and education (all measured pre-lottery in 2009-10) are included. Standard errors are two-way clustered at the municipal and family level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

TABLE 5. Impact of Winning Housing Lottery on Neighborhood Quality

	Neighborhood Problem Index (1)	Neighborhood Crime Index (2)	Neighborhood Poverty Index (3)
Won Lottery	-0.088***	-0.049**	-0.037***
	(0.029)	(0.021)	(0.003)
Control Mean	0.03	0.03	0.01
# Observations	2,563	10,084	10,084

Notes: This table compares the neighborhoods where lottery winners reside relative to lottery losers on the basis of perceived neighborhood problems (Column (1)), crime (Column (2)), and poverty (Column (3)). Observations for column (1) are number of survey respondents., while observations in columns (2) and (3) represent individuals from our main analysis sample which we can match to the SISBEN IV. Data for column (1) come from household survey responses from 1,264 lottery winners and 1,299 lottery losers. The survey which was conducted via telephone between August 6 and September 6, 2020 and had a response rate of 89 percent. For column (2), we use the SISBEN IV which was run in 2019-20 and geolocate individuals exact home address to their police *cuadrante*. We then use data from the National Police Department on major crimes reported in years 2018-2020 at the *cuadrante* level to construct a measure of crime for the neighborhoods of lottery winners and losers in 2019 (5-6 years post-lottery). In column (3), we geolocate individuals exact home address on the SISBEN IV to census-segments (roughly equivalent to U.S. ZIP codes) and use SISBEN IV data to calculate mean household wealth and income in that neighborhood. To allow for easy comparisons across columns, we construct indices for each neighborhood characteristic by taking equal weighted average of the z-score of all components that enter into the construction of the index. For the neighborhood problem index, these components are: noise, odors, crowding, trash, air pollution, water pollution, presence of insects and rodents, and presence of other invasive animals. The impact of winning the lottery on each of these subcomponents is reported in Table A.4. For the neighborhood crime index, these components are: assaults, robberies, and homicides. The impact on each of these crime categories is reported in Table A.5. For the neighborhood crime index, these components are: the 'SISBEN score' which measures indicators of household wealth and household income (appropriately signed so positive indicates poorer). All regression include lottery fixed effects to ensure that only individuals in the same lottery are being compared. For column (1), we also control for: household's head gender, age and age squared, education, marital status, poverty score, and household size (all measured at baseline), and year fixed effects. For columns (2) and (3), we also control for the year an individual was interviewed in the SISBEN IV. For column (1), standard errors are clustered at the municipality level, while standard errors are clustered at the *cuadrante* or neighborhood and interview year level for columns (2) and (3). ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

TABLE 6. Impact of Winning Housing Lottery on Family Wealth and Expenditures

	Family Wealth Indices		Family Income		Family Expenditures	
	Assets (percentile)	Amenities (percentile)	Household Head Employed	Log Income	Log Education	Log Food
	(1)	(2)	(3)	(4)	(5)	(6)
Control Mean	33.55	47.96	0.43	6.44	1.46	11.70
Panel A. Without Demographic Controls						
Won lottery	9.37***	13.16***	0.019*	0.341**	0.220*	0.114*
	(1.31)	(1.11)	(0.012)	(0.163)	(0.127)	(0.059)
Panel B. With Demographic Controls						
Won lottery	9.08***	13.06***	0.015	0.330*	0.178	0.093*
	(1.32)	(1.11)	(0.013)	(0.168)	(0.119)	(0.058)
Observations	10,084	10,084	10,084	10,084	10,084	10,084

Notes: This table reports intent-to-treat estimates of the effect of winning the public housing lottery on outcomes related to household wealth and expenditures on the SISBEN IV which was conducted roughly 8 years after the housing lotteries (in 2019-2020). The match rate for our lottery sample to the SISBEN IV is 10,084/15,026 (67%). Column (1) reports results for a household "asset index" which is composed of the follow assets: (i) fridge, (ii) washing machine, (iii) motorcycle, (iv) vehicle, and (v) any other assets or properties. Similarly, the dependent variable in Column (2) is an amenities index which measures the following amenities that a household has access to: (i) walls made of 'durable' material, (ii) floors made of 'durable' material, (iii) the number of people per room, (iv) electricity access, (v) running water access, (vi) sewer and sanitation access, (vii) trash collection services, (viii) natural gas access, and (ix) internet. Both these indices are then constructed by standardizing each component and then taking the arithmetic average and calculating the household's percentile rank which we use as our outcome. Column (3) details whether the household head is employed in either the formal or informal sector at the time of their SISBEN IV interview. Columns (4), (5), and (6) then use as the dependent variable the household's reported log monthly income, log monthly education expenditures, and log monthly food expenditures from the SISBEN IV, respectively. All regressions include lottery fixed effects to ensure that only individuals in the same lottery are being compared. Panel A reports results when no controls are included (aside from lottery fixed effects), while column (2) contain controls for a child's gender, age at first lottery, whether a family lived in urban/rural area, household size, along with characteristics of the household head including age at birth, marital status, employment status and education (all measured pre-lottery in 2009-10). The sample includes children who were 15 or younger at the time of their first lottery application and were 18 or older in 2019. Of the 10,084 children in this sample, 2,706 won the lottery and 7,378 lost the lottery. Standard errors are two-way clustered at the municipal and family level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

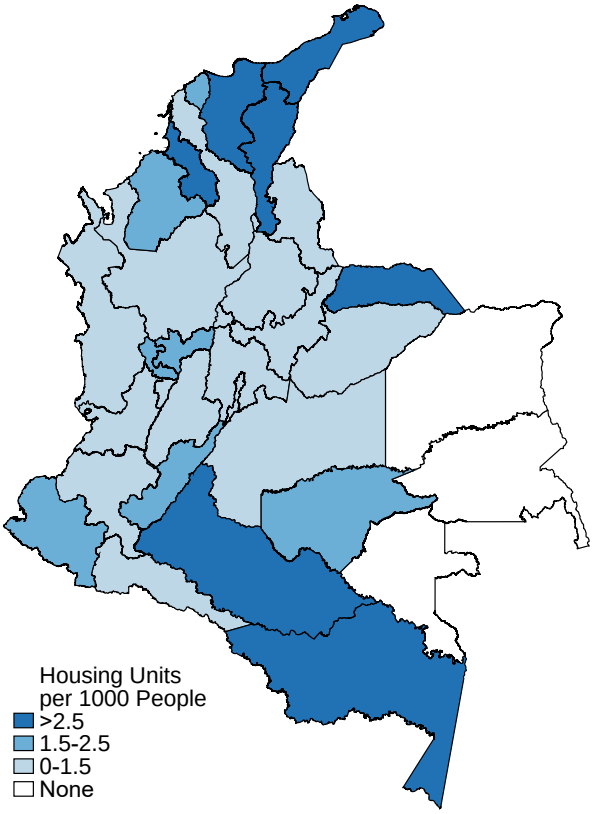
A Online Appendix Figures and Tables

FIGURE A.1: Location of Housing Projects

(A) Location of Housing Projects



(B) Project Density by Department



Notes: Map of Colombia (Map data: Google, 2021). Figure A.1(a) displays the location of the 225 projects in our data with the size of each pin corresponding to the relative size of the project in terms of the number of housing units. A minimum size is imposed for projects with few units to ensure that they are visible. Figure A.1(b) shows the density of housing units across the 32 departments of Colombia and the capital district of Bogotá. We exclude the department of San Andrés, Providencia and Santa Catalina for visual clarity, although no projects were built there. Note that departments in the East lie in the Amazon and are sparsely populated.

FIGURE A.2: Location of Housing Projects Compared to Location of Main Analysis Sample

(A) Location of Housing Projects

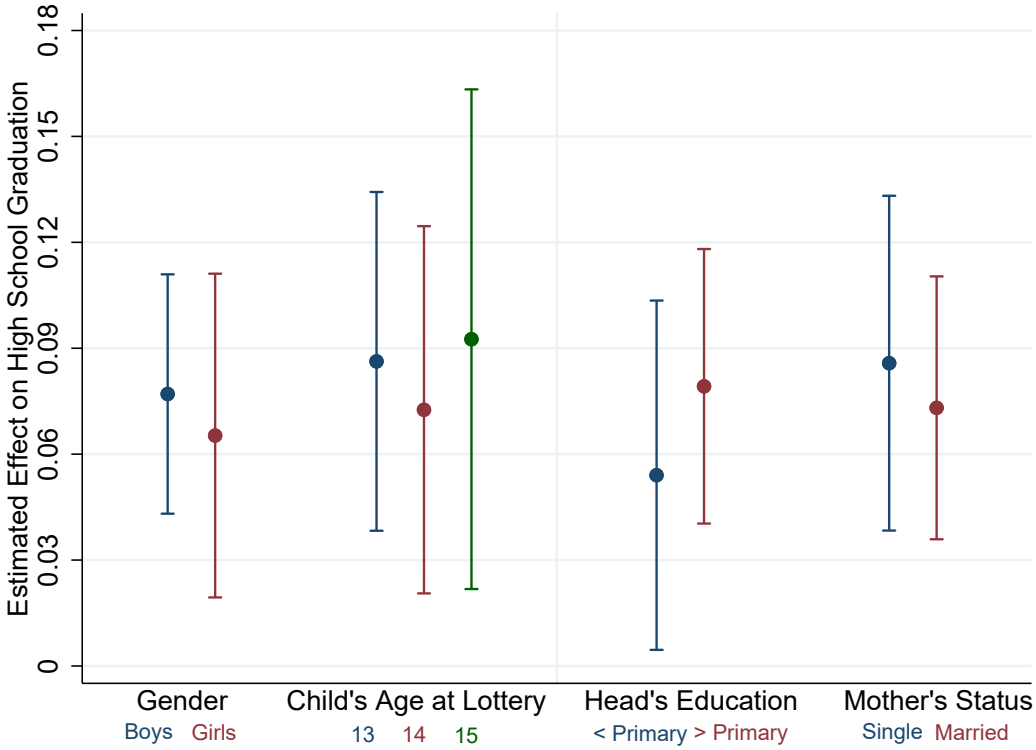


(B) Location of Main Analysis Sample



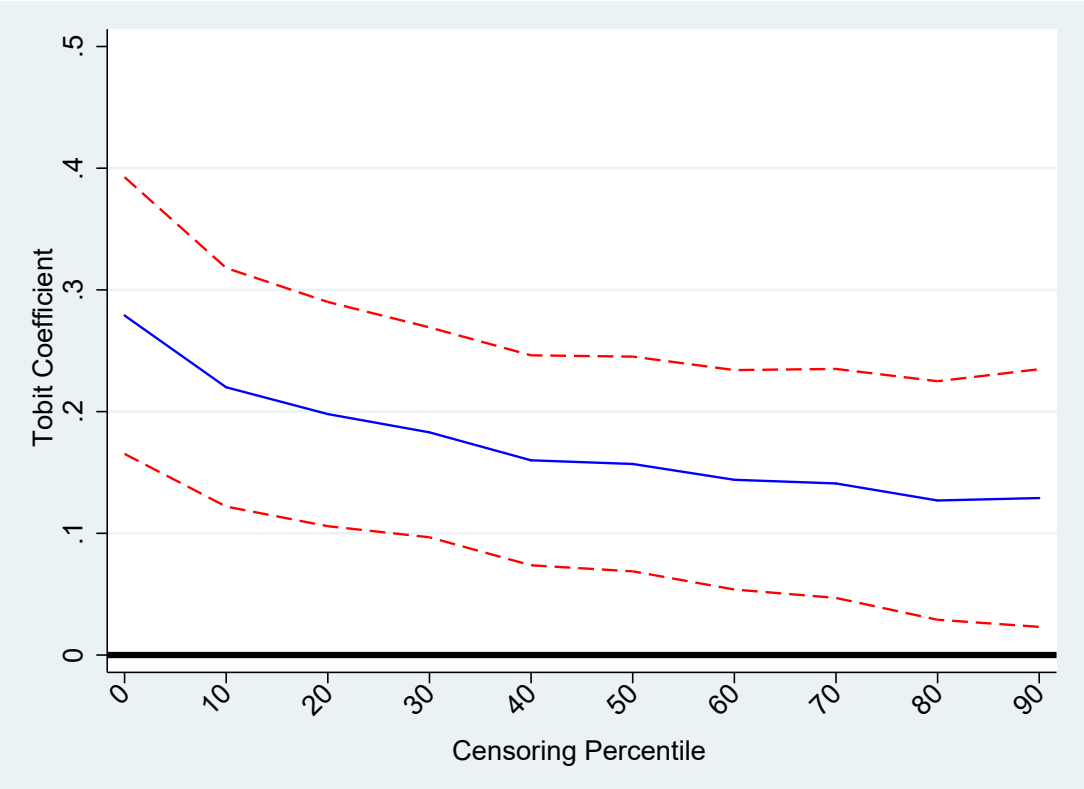
Notes: Figure A.2(a) is identical to Figure A.1(a) and displays the location of the 225 projects in our data with the size of each pin corresponding to the relative size of the project in terms of number of housing units, with a minimum size imposed for projects with few observations to ensure that they are visible. Figure A.2(b) then shows the location of our main analysis sample of 15,026 children in our data with the size of each pin corresponding to the relative number of children who applied to a given project. (Once again, a minimum size is imposed to ensure projects with few children are visible.) To make the two figures comparable, the pins in Figure A.2(b) are scaled up by a factor of five relative to Figure A.2(a) so that the pin sizes in each figure correspond to the relative proportion of the respective samples.

FIGURE A.3: Heterogeneity by Baseline Demographics



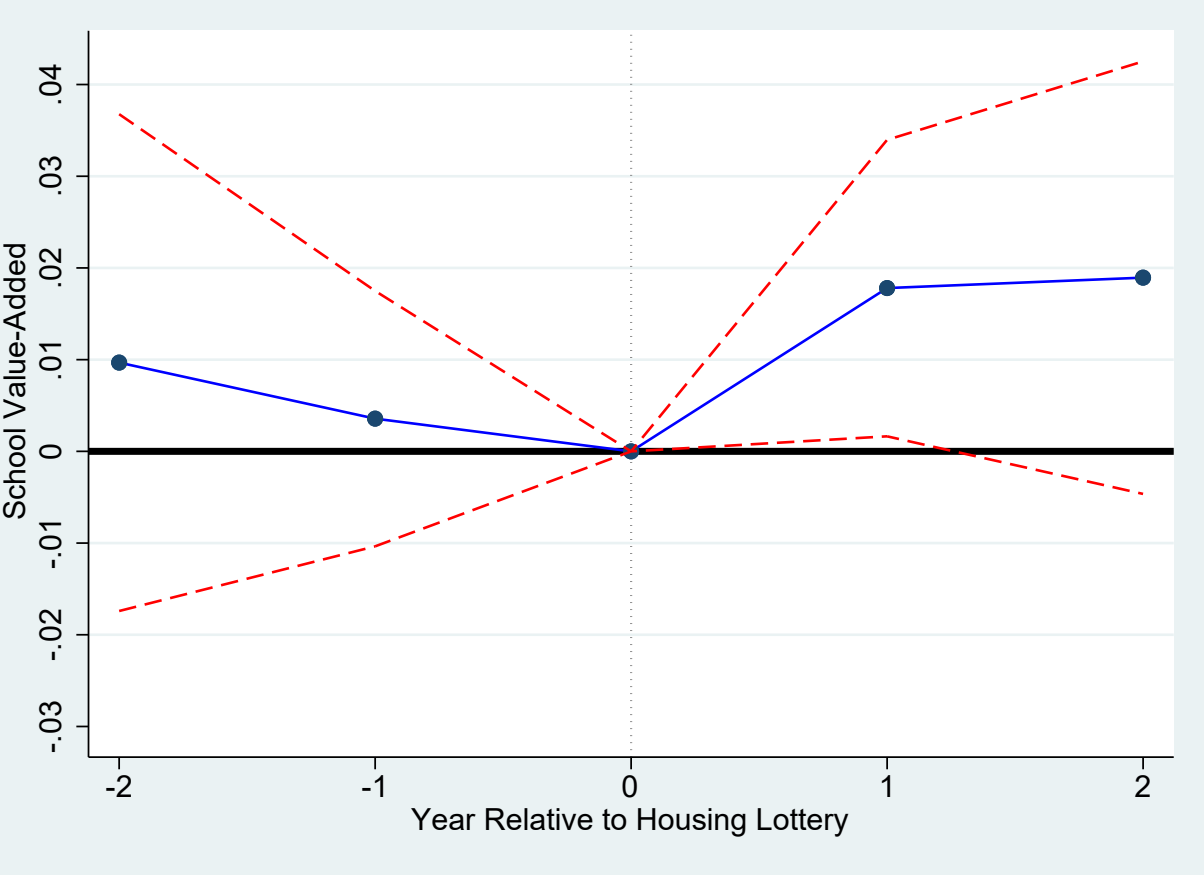
Notes: The figure plots heterogeneity of the impact of public housing on high school graduation by several baseline demographic characteristics. To do so, we split our sample by the given demographic and estimate our main regression given by equation (1). The characteristics are: gender, child's age at lottery, household head's education, and mother's marital status. All demographics are measured pre-lottery in 2009-10. Note that for child's age at lottery we have include the few (≈100) 12-year-old at lottery children in our data in the 13-year-old category. Lottery fixed effects and controls for a child's gender, age at first lottery, whether a family lived in urban/rural area, household size, along with characteristics of the household head including age at birth, marital status, employment status and education (all measured pre-lottery in 2009-10) are included. Whiskers indicate 95% confidence intervals with standard errors being two-way clustered at the municipal and family level.

FIGURE A.4: Tobit Coefficients by Censoring Percentile in Score Distribution



Notes: The figure plots selection-corrected estimates for various censoring points of the effects of winning the public housing lottery on the ICFES test score. To correct for selection into ICFES-taking, we code the latent scores of those who did not take the ICFES as falling below a particular percentile and then censor the ICFES distribution at or above this value and use Tobit to correct for censoring. The figure then shows our selection-corrected estimates for the various censoring points that we used (indicated on the x-axis). Lottery fixed effects and controls for a child's gender, age at first lottery, whether a family lived in urban/rural area, household size, along with characteristics of the household head including age at birth, marital status, employment status and education (all measured pre-lottery in 2009-10) are included. The dashed lines indicate 95% confidence intervals. Standard errors are two-way clustered at the municipal and family level.

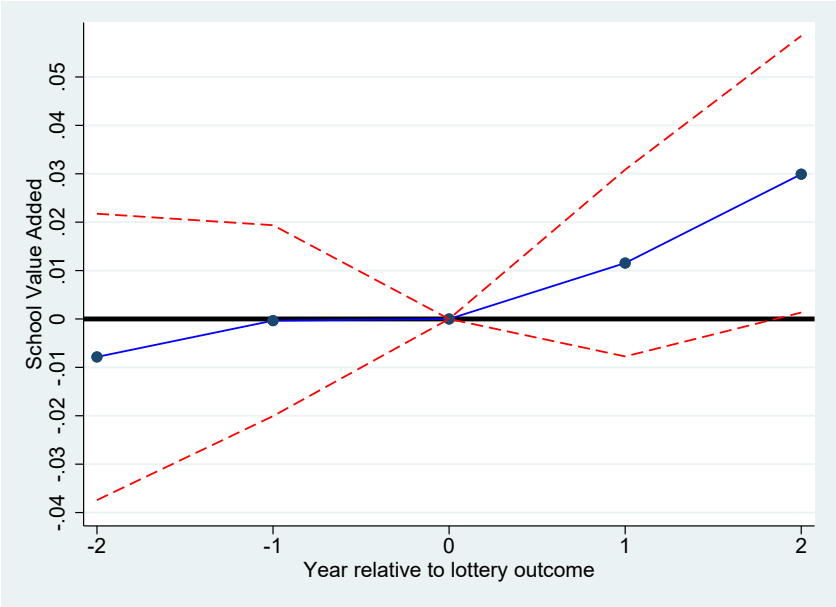
FIGURE A.5: School Value-Added for Lottery Winners Relative to Lottery Losers (Including Neighborhood Fixed Effects in Value-Added Estimation)



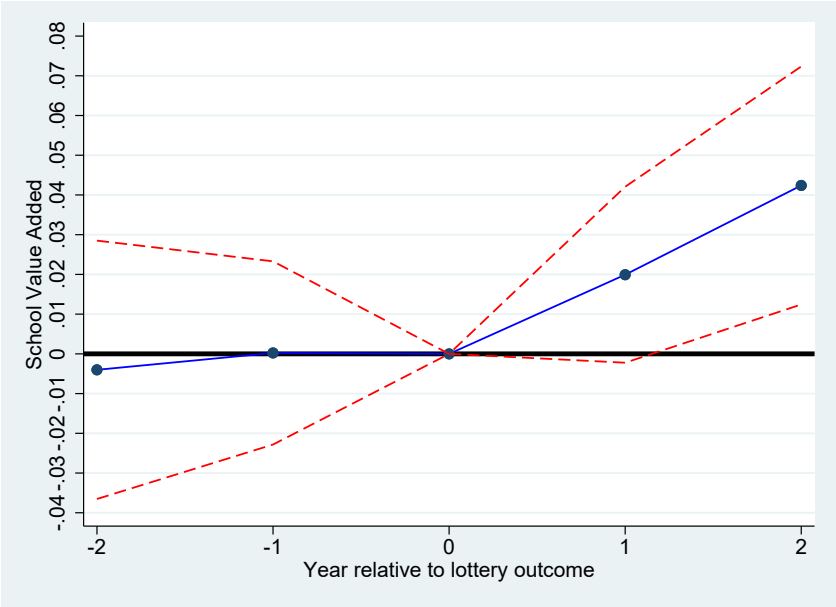
Notes: This figure replicates Figure 3 but includes neighborhood fixed effects (measured pre-lottery using the SISBEN III) as controls in the control vector X_{ics} when calculating school value-added in equation (2). This ensures that our school value-added results are driven by differences in school quality rather than differences in neighborhood quality. The figure then shows the value-added of schools attended by lottery winners compared to losers relative to the lottery date of their first application (at year '0'). School value-added is calculated using cohorts from a pre-period to ensure that the public housing itself does not impact our school quality measure. Specifically, we use sixth grade entering cohorts from 2006-08 to construct school value-added. We then calculate and graph the difference in mean value-added for the schools attended by lottery winners compared to losers for each year relative to the lottery date. We normalize the difference in value-added between lottery winners and losers to be zero in the year of the lottery (i.e., year '0'). The dashed lines represent 95 percent confidence intervals with standard errors clustered at the municipality and family level.

FIGURE A.6: School Value-Added for Lottery Winners Relative to Lottery Losers: Value-Added Measured Using ICFES Scores

(A) Raw ICFES Scores

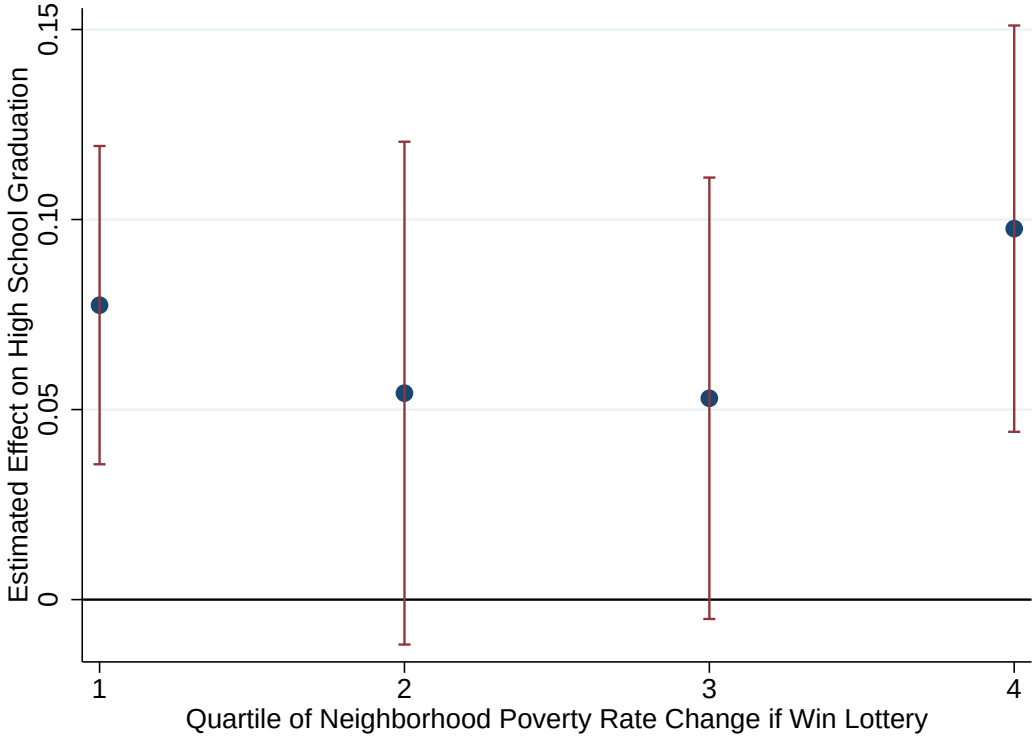


(B) Selection-Corrected ICFES Scores



Notes: These figures replicate Figure 3 but use ICFES scores (rather than high school graduation) as the dependent variable when calculating school value-added in equation (2). Figure A.6(a) just use the raw (standardized) ICFES scores, while Figure A.6(b) employs a selection-corrected (standardized) ICFES score using the method described in Section 4.2 where we censor observed scores at or above the tenth percentile and assign the tenth percentile score to all those with scores below the tenth percentile along with those who did not take the test. The figures then shows the value-added of schools attended by lottery winners compared to losers relative to the lottery date of their first application (at year '0'). School value-added is calculated using cohorts from a pre-period to ensure that the public housing itself does not impact our school quality measure. Specifically, we use sixth grade entering cohorts from 2006-08 to construct school value-added. We then calculate and graph the difference in mean value-added for the schools attended by lottery winners compared to losers for each year relative to the lottery date. We normalize the difference in value-added between lottery winners and losers to be zero in the year of the lottery (i.e., year '0'). The dashed lines represent 95 percent confidence intervals with standard errors clustered at the municipality and family level.

FIGURE A.7: Heterogeneity by Change in Neighborhood Quality if Win Lottery



Notes: The figure explores whether there exist heterogeneous effects based on the change in neighborhood quality an participant would experience if they win. To do so, it divides lottery participants into quartiles based on the change in neighborhood poverty that the participant would experience if they win the lottery (and accept the public housing) versus remaining at their current address. The regression given by equation (1) is then run for each quartile and these point estimates are placed on the figure. Lottery fixed effects and controls for a child's gender, age at first lottery, whether a family lived in urban/rural area, household size, along with characteristics of the household head including age at birth, marital status, employment status and education (all measured pre-lottery in 2009-10) are included. Whiskers indicate 95% confidence intervals with standard errors being two-way clustered at the municipal and family level.

TABLE A.1. Impact of Winning Housing Lottery on Distance to Local Amenities

	Public Transport Station (1)	Preschool (2)	School (3)	College or University (4)	Grocery Store (5)	Park (6)
Won Lottery	-10.066***	-2.824**	-2.105*	-4.438*	-10.407**	-6.279***
	(1.880)	(1.172)	(1.083)	(2.282)	(4.958)	(1.200)
Control Mean (Minutes)	22.41	21.35	21.46	38.21	27.89	19.54
# Observations	2,563	2,563	2,563	2,563	2,563	2,563

Continued...

	Hospital or Clinic (7)	Pharmacy (8)	Police Station (9)	Bank or ATM (10)	Church (11)	Family member or relative (12)
Won Lottery	-6.408***	-6.831***	-7.035***	-6.246***	-2.689**	9.115**
	(1.592)	(1.293)	(1.392)	(1.842)	(1.136)	(3.689)
Control Mean (Minutes)	31.74	19.84	24.20	33.39	20.84	30.60
# Observations	2,563	2,563	2,563	2,563	2,563	2,563

Notes: This table comes from Camacho et al. (2021) and details self-reported travel times in minutes to various amenities for lottery winners compared to losers. The control mean reports average travel times among lottery losers. The survey was conducted via telephone between August 6 and September 6, 2020. The response rate to the survey was 89 percent and it collected information from 1,264 lottery winners and 1,299 lottery losers. We note that the survey includes all lottery participants which differs from our main analysis sample which focuses on the children of lottery participants. Standard errors are clustered at the municipality and family level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

TABLE A.2. Summary Statistics

	All Eligibility Groups			'Extreme Poor' Only		
	All Applicants	Direct Assignment	Lottery Participants	All Poor	All Applicants	Lottery Participants
	(1)	(2)	(3)	(4)	(5)	(6)
Household Head Characteristics *(Pre-Lottery)*						
Household Size	4.87	4.74	4.90	4.89	4.98	4.98
Married	0.46	0.46	0.46	0.53	0.45	0.45
Employed	0.48	0.48	0.52	0.64	0.50	0.52
High School Graduate	0.34	0.35	0.34	0.42	0.38	0.34
Housing Characteristics *(Pre-Lottery)*						
Number of Rooms	2.30	2.23	2.25	2.54	2.31	2.28
Number of Bathrooms	0.89	0.87	0.89	1.01	0.90	0.91
Has Kitchen	0.82	0.82	0.80	0.82	0.80	0.79
Access to Services *(Pre-Lottery)*						
Electricity	0.96	0.96	0.95	0.98	0.97	0.97
Water/Sewage	0.77	0.75	0.77	0.93	0.81	0.83
Trash Collection	0.75	0.75	0.74	0.95	0.75	0.77
Cable TV	0.16	0.17	0.16	0.34	0.15	0.15
Household Wealth *(Pre-Lottery)*						
Has Vehicle	0.03	0.03	0.03	0.05	0.03	0.03
Has Fridge	0.44	0.43	0.40	0.60	0.42	0.40
Has Washing Machine	0.10	0.13	0.10	0.20	0.11	0.10
Has TV	0.72	0.71	0.70	0.86	0.74	0.73
# of Households	145,688	45,554	60,042	1,513,339	59,613	34,120

Notes: Columns (1)-(3) of this table report summary statistics for lottery applicants. The summary characteristics come from the SISBEN III and were collected in 2009-10. Column (1) reports the summary statistics for all applicants, which consists of all applicants whose paperwork was not rejected and so were assigned a priory tier. Column (2) then restricts the sample to applicants who were directly assigned to public housing as they had sufficient priority, while column (3) restricts the sample to applicants who participated in the lottery (these do not align with the sample in Table 1 as they include all applicants, while Table 1 focuses on our analysis sample of children). Columns (4)-(6) limit the sample to applicants who were part of the 'extreme poor' eligibility group as we can identify these individuals in the SISBEN III and so can compare all eligible individuals to applicants. Column (4) reports summary statistics for individuals who would be considered 'extreme poor' in 2009-10 according to the SISBEN III. Note that not all of these individuals would be eligible for public housing, however, as they also must live in a municipality with a public housing project. Columns (5) and (6) then display summary statistics for all applicants and lottery participants among the 'extreme poor' eligibility group, respectively.

TABLE A.3. Summary Statistics on Public Schools Used to Estimate Value-Added

	Mean	S.D.
All Lower-Secondary Schools (2006-08)		
School Value-Added on HS graduation	-0.033	0.154
School Value-Added on ICFES Score	-0.066	0.252
School Size	153.4	166.4
# of Students	1,634,937	
# of Public Schools	10,658	
Lower-Secondary Schools Attended by Lottery Sample		
School Value-Added on HS graduation	0.002	0.100
School Value-Added on ICFES Score	-0.106	0.227
School Size	323.5	211.1
# of Students	1,173,334	
# of Public Schools	3,627	

Notes: This table displays summary statistics for students and schools used to estimate value-added. 'All Lower-Secondary Schools' include all lower-secondary public schools in the country that operated during the 2006-08 period. Only students who are matched to the SISBEN III are included in the data. Value-added is then estimated among the 'all school' sample. 'Lower-Secondary Schools Attended by Lottery Sample' then restricts the all schools sample to only those in which at least one child in our analysis sample of 15,026 children attended during the 2010-2016 period (i.e., both before and after the lottery occurred). Note that the schools attended by children in our lottery sample tend to be larger which is driven by the fact that the public housing was built in more urban areas.

TABLE A.4. Impact of Winning Housing Lottery on Households' Perceptions of Neighborhood Problems

	Neighborhood problem index	Noise (vehicles, machinery)	Bad street odors	Crowded public spaces (sidewalks, streets)
	(1)	(2)	(3)	(4)
Won lottery	-0.088***	-0.008	-0.041*	0.032*
	(0.029)	(0.023)	(0.021)	(0.017)
Control Mean	0.03	0.20	0.25	0.16
# Observations	2,563	2,563	2,563	2,563

Continued...

	Trash on streets	Air pollution	Water pollution	Presence of insects, rodents	Presence of other invasive animals
	(5)	(6)	(7)	(8)	(9)
Won lottery	-0.008	-0.049**	-0.045**	-0.120***	-0.030*
	(0.027)	(0.019)	(0.019)	(0.014)	(0.017)
Control Mean	0.25	0.25	0.20	0.34	0.22
# Observations	2,563	2,563	2,563	2,563	2,563

Notes: This table compares household responses about neighborhood problems among lottery winners and losers for each neighborhood problem asked in our survey. The data come from the household survey which was conducted via telephone between August 6 and September 6, 2020. The response rate to the survey was 89 percent and it collected information from 1,264 lottery winners and 1,299 lottery losers. Given the number of questions asked, column (1) creates a simple "neighborhood problem" index that takes the equal weighted average of the z-score of the eight questions in columns (2)-(9); a higher value of this index reflects more frequent neighborhood problems. Column (1) is identical to column (1) of Table 5. We note that the survey includes all lottery participants which differs from our main analysis sample which focuses on the children of lottery participants. All regressions include control for: household's head gender, age and age squared, education, marital status, poverty score, and household size (all measured at baseline), year fixed effects; and lottery fixed effects to ensure that only individuals in the same lottery are being compared. Standard errors are clustered at the municipality level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

TABLE A.5. Impact of Winning Housing Lottery on Neighborhood Crime by Crime Type

	Crime Index	Assaults	Robberies	Homicides
	(1)	(2)	(3)	(4)
Won Lottery	-0.049**	-1.184*	-2.620***	-0.103
	(0.021)	(0.691)	(0.894)	(0.071)
Control Mean	0.03	29.77	38.09	2.87
# Observations	10,084	10,084	10,084	10,084

Notes: This table compares crime in the neighborhoods where lottery winners reside relative to those where lottery losers live for three types of crime: assaults, robberies, and homicides. To do so, we use the SISBEN IV which was run in 2019-20 and records individuals' exact home address. The home address is then geo-located to their police *cuadrante* which are small and well-defined geographical areas within Colombian cities used for police street patrols. We then use data from the National Police Department on major crimes – assaults, robberies, and homicides – reported in years 2018-2020 at the *cuadrante* level to construct a measure of crime for the neighborhoods of lottery winners and losers in 2019 (5-6 years post-lottery). Column (1) combines the various crimes into a crime index by taking the equal weighted average of the z-score of the three crime types in columns (2)-(4); a higher value of this index reflects more frequent crime. Column (1) is identical to column (2) of Table 5. Lottery fixed effects are included to ensure that only individuals in the same lottery are being compared. We also control for the year an individual was interviewed in the SISBEN IV. Standard errors are clustered at the *cuadrante* and interview year level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively. Models

TABLE A.6. Mediation Analysis

Outcome Variable: High School Graduation	First-Stage (γ^j) (1)	Second-Stage (θ^j) (in percentage points) (2)	% Explained by Mechanism (3)
Won lottery (κ^{Res})	0.067***	2.00	30.3%
	(0.017)	(1.99)	
Family Wealth			
Household Asset Index	9.08***	0.121***	16.7%
	(1.32)	(0.029)	
Household Amenities Index	13.06***	0.099**	19.6%
	(1.11)	(0.040)	
Family Income and Expenditures			
Household Head Employed	0.015	0.179	0.0%
	(0.013)	(2.339)	
Log Household Income	0.330*	0.033	0.2%
	(0.168)	(0.245)	
Expenditure on Education	0.178	0.526***	1.4%
	(0.119)	(0.189)	
Expenditure on Food	0.093*	-0.305	-0.4%
	(0.058)	(0.243)	
Neighborhood Quality			
Neighborhood Poverty Index	-0.037***	0.750	-0.4%
	(0.003)	(0.551)	
Neighborhood Crime Index	-0.049***	-0.712	0.5%
	(0.021)	(2.779)	
School Quality			
School Value-Added	0.021***	100.75***	32.1%
	(0.008)	(16.74)	

Notes: The first row of Column (1) reports intent-to-treat estimates of the effect of winning the public lottery on high school graduation as described by equation (1), which is identical to the results reported in the 'High School Graduation' in Column (2) of Table 3. This point estimate of 0.067 is then attributed to the nine mechanisms that we explore in the below rows according to the methodology described in Section 5.4. The first row of column (2) reports the total treatment effect that cannot be explained by the nine mechanisms. For the nine mechanisms, Column (1) reports the parameter γ^j from regression (3) which represents the impact of winning the lottery on the mediator. Column (1) estimates are identical to those from Tables A.5 and 6 (and the Figure 3 pre-post difference for school VA). Column (2) then reports the parameter θ^j (multiplied by 100 for expositional clarity) from regression (4). The total treatment effect explained by each of our nine mechanisms is then simply $\gamma^j\theta^j$ which is just the product of columns (1) and (2) (divided by 100). Column (3) then reports the explanatory power for each mediator which is the production of columns (1) and (2) divided by the intent-to-treat estimate of 0.067 (i.e., $\frac{\gamma^j\theta^j}{\beta}$. Controls are included throughout and include lottery fixed effects, gender, age at first lottery, whether a family lived in urban/rural area, household size, along with characteristics of the household head including age at birth, marital status, employment status and education (all measured pre-lottery in 2009-10). The sample includes children who were 15 or younger at the time of their first lottery application and were 18 or older in 2019. Standard errors are two-way clustered at the municipal and family level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

B Literature on Impact of Public Housing in Developing Countries

Paper	Program, Data, and Eligibility	Housing Characteristics	Research Design and Findings
Alzúa, Amendolaggine, Cruces, and Greppi (2016)	Investigate a public housing program in Rosario, Argentina. The study looks at 9,536 applicants for 405 public housing units that were assigned via lottery. Data come from the lottery applications made combined with administrative records of registered employment. To be eligible for the housing, applicants had to live in Rosario and have a formal income of at least US$540.	Housing was built in the northwestern outskirts of the city of Rosario, roughly 30 minutes from the city center. Units were two-bedroom rowhouses of 645 square feet on properties of 1600-2700 square feet. The housing was heavily subsidized with recipients paying 20 percent of income in rent. Once the total rent paid equaled construction costs (usually would take 20-30 years), the rent payment would be halted.	Impacts of the program identified by comparing lottery winners to losers. Find that the public housing receipt decreased employment by 7 percentage points. In addition, recipients perceived access to local job opportunities was significantly reduced.
Barnhardt, Field, and Pande (2017)	Investigate program run in Ahmedabad, India where poor women from the city slum who were part of the Self Employed Women's Association became eligible for a public housing lottery. The data consist of 497 women, of which 110 were selected via lottery to receive the public housing.	Public housing consisted of single-story rowhouses of approximately 200 square feet. The housing was located on the city's periphery, 7.5 miles from the city center. The housing was heavily subsidized, with an initial move-in cost of less than $US20 and monthly rent of US$2.	Impacts of program identified by comparing lottery winners to losers. 14 years after the lottery, lottery winners and losers were indistinguishable in terms of current income, labor force participation, household health, and child outcomes, while lottery winners were *worse* off in terms of social networks. The program had significant exit: 34 percent of winners refused public housing and a further 32 percent that moved in relocated to the slum within ten years.
Franklin (2019)	Investigates a large-scale government housing program in Addis Ababa, Ethiopia. Households were eligible for the housing if they lived in Addis Ababa for at least 6 months and did not own a property. The data cover a random sample of 1,600 households who participated in a lottery that determined assignment to the public housing (out of a total 34,000 apartments assigned via lottery).	Housing was built on the outskirts of the city, being at least 15 km from the city center. The housing was studio to three-bedroom apartments of 350 to 1050 square feet. The housing was sold to applicants with a 20% down payment that averaged $10,000. The applicants covered the remaining housing value using a mortgage to be paid over 15 years. The author calculates that the housing subsidy was 40% percent compared to market rates.	Impacts of program identified by comparing lottery winners to losers. He finds that winning the lottery does not affect labor supply or earnings. Lottery winners report reduced social lives, although also reduced conflict with neighbors and an increased willingness to contribute to public goods. Take-up was limited: 46% of lottery winners moved into the government housing, with the remainder subletting their units.

Picarelli (2019)	Investigate a housing relocation program in South Africa covering 2.8 million households. The study uses data from 1,946 households using the National Income Dynamics Study. Households earning less than 3500 (roughly US$500 in 2010) were eligible for the program.	Housing was built using a fixed grant for private sector operators hired by local authorities to build housing. The housing had minimum quality requirements, such as a minimum size of 430 square feet. To save costs, the housing was built in greenfield developments on the outskirts of cities located between 10 to 45km from employment centers. The housing was given to recipients for free (technically were supposed to pay R2479, although was not enforced).	Impacts of the program identified using a RD design that compares households earning less than R3500 and so were eligible for the housing to those earning over R3500 and were ineligible. The author finds that two to four years after receiving housing, labor supply of recipient households decline by between half to one standard deviation. Evidence is also limited that the public housing recipients experienced improvements in housing or neighborhood quality.
Chagas and Rocha (2019)	Investigates the Minha Casa Minha Vida program in Brazil, one of the largest housing programs in the world covering 5 million households and costing US$3.6 billion a year. The study focuses on Segment I, which consisted of 1.76 million units. Eligibility was restricted to families with incomes below US$400 per month. The author uses data on 361,805 applicants from Rio de Janeiro and 12,084 applicants from São José do Rio Preto selected via lottery.	The public housing in both cities was built in the outskirts of each city and so were located far from the city center (usually 20-30km from the city center). The housing units were 440-485 square feet and had access to basic sanitation, drinking water and electricity. Beneficiaries receive a heavily subsidized loan to cover the cost of purchasing their housing unit; the housing subsidy was roughly 90% of the houses' value for up to 120 months.	Impacts of the program identified by comparing lottery winners to losers. Find that the public housing receipt decreased employment by 3.3% in São José do Rio Preto and 5.9% in Rio de Janeiro. Public housing receipt also increased the likelihood of participating in Brazil's income transfer program, indicative of winners being worse of economically.
Franklin (2020)	Investigates South Africa's housing program, which has provided over 3 million housing units since 1994. The study focuses on Cape Town, using longitudinal household data from 1,350 households covered by the Cape Area Panel Study. Individuals were eligible for public housing if they had a dependent (such as a spouse), earned less than R3500 per month, did not own a property, and were a South African citizen. Eligibility requirements, however, were often unenforced.	Uniquely the public housing in Cape Town was built adjacent to the slums that beneficiaries previously lived, and so households that took up public housing only moved a small distance from their prior housing. The public housing units were single-story, standalone houses on distinct plots, usually with one or two bedrooms, one bathroom and a communal kitchen and living area, connected to electricity and running water in the home. Beneficiaries were given the housing unit for free with no mortgage or restrictions on its use.	To identify the impact of the public housing, the author uses distance between households' original place of living and the location of newly-built housing projects as an instrument to deal with non-random selection into public housing as the allocation procedure selected recipients based on proximity to housing developments. He then compares households near completed projects to those near planned but incomplete projects to deal with non-random location of the public housing projects. Public housing receipt is found to increase total household earnings by 19 percent.

Kumar (2021)	Study the effects of a subsidized housing program that offers loans from state-owned banks to low- and mid-low-income urban residents in Maharashtra, India to acquire apartments. The study samples individuals who won the loan lottery and a random subsample of those who did not. The final sample covers 834 households using in-person household surveys, of which 421 received the loan and 413 did not.	The main function of the intervention is to transfer a large subsidy to households and the flexibility with which they can consume the benefit. The subsidy represented 30% of the commercial value and households did not have to pay property taxes for the first 5 years. They can also choose the unit within the building. Resale of the apartments is permitted only after 10 years but households could rent out the units, with half of the sample deciding to do so.	Identifies the impact of the program by comparing lottery winners to losers. He finds that winning households have higher incomes, are more likely to be employed full time (16%), and children are 17.6% more likely to finish high school and 15.9% more likely to complete post-secondary education. As for mechanisms, lottery winners on average live in lower-quality neighborhoods with worse schools, suggesting effects are not driven by neighborhood or school effects. The author hypothesizes that the treatment effects are instead driven by increases in income due to recipients' ability to sublet their housing unit.
Rojas-Ampuero and Carrera (2023)	Study the intergenerational effects of a slum clearance program implemented between 1979-85 in Santiago, Chile whereby individuals in slums were relocated to public housing. Data come from digitized slum Censuses conducted before the slum clearance combined with post-clearance homeowner data. These data are then matched to administrative data, giving a sample of 55,343 children from 17,651 unique families. All individuals in a targeted slum were affected by the slum clearance.	The location of the public housing projects varied: about two-thirds of recipients were relocated to projects on the periphery of the city while the remaining one-third received public housing at their initial location. The public housing units were either in apartment block or small "starting-kit" houses with a living room, a bathroom and a kitchen where bedrooms could be added on top of the unit. Basic services such as water, electricity, and sewage were provided. Recipients received a 75% government subsidy to pay for their unit and then were granted property rights.	The authors compare children who were displaced and sent to projects on the city's periphery to those who were provided housing at the same location as their old slum. The authors find that children who went to projects on the city's periphery have 10 percent lower earnings and are 12% less likely to graduate from high school compared to the non-displaced. Destination projects explained 70% of the total effect of displacement on labor earnings and 35% of the total effect on schooling. Authors also find that access to a newly-built subway reduces the earnings effect of displacement by 25%.
Belchior, Gonzaga, and Ulyssea (2023)	Investigate the *Minha Casa, Minha Vida* program in Brazil which provided heavily subsidized houses. Data on program applicants the program lotteries come from the city of Rio de Janeiro. Focus on 2,580 housing units that were assigned in 2015. Program eligibility was restricted to those with a monthly income less than R$ 1,600 (US$ 454) which is roughly the median for Rio de Janeiro.	Focus on six public housing projects across three neighborhoods in Rio de Janeiro. These projects were very far from the city center (about 50 km or 2.5 hours via public transit). Recipients received a subsidy worth roughly 90 to 95 percent of the house value to buy the house. Average house value was roughly R$ 63,000 (US$ 19,000). The remainder of the house value was then paid via monthly installments over ten years (average monthly payment of R$ 50 or US$ 15). Houses could not be sold.	The authors exploit the double-randomization in the program where applicants were first randomly assigned via lottery to receive a house and then randomly assigned via lottery to one of six housing projects in three different neighborhoods. Housing take-up was relatively low, at 50 percent. The authors find that receiving a house reduces the probability of being formally employed by 1.7 percentage points. The authors find that the neighborhood of the assigned house matters, with labor market access being the key neighborhood characteristic.