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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. Using a survey to explore mechanisms, lottery winners report better environmental conditions and shorter commute times. Their children also attend better schools and live in neighborhoods with less crime.

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1 Introduction

Children growing up in high-poverty areas fare worse than those who live in lower-poverty neighborhoods on a wide range of economic, health, and educational outcomes, with quasi-experimental and experimental evidence indicating this relationship is causal (Aaronson, 1998; Currie and Yelowitz, 2000; Chetty et al., 2016; Nakamura et al., Forthcoming; Chetty and Hendren, 2018; Chyn, 2018; Deutscher, 2020; Laliberté, 2021).[1] Motivated by such findings, policymakers often provide housing assistance to low-income households.[2] In developing countries, the potential for housing assistance to transform the economic opportunity of disadvantaged children appears especially pronounced given that many of these children grow up in high-poverty densely populated neighborhoods characterized by high crime, poor-quality housing, and a lack of adequate living space (Marx et al., 2013).

Remarkably, evidence from developing countries often reveals that public housing *negatively* affects recipients: Research from Argentina (Alzúa et al., 2016), Brazil (Chagas and Rocha, 2019), Chile (Rojas-Ampuero and Carrera, 2021), Ethiopia (Franklin, 2019), and India (Barnhardt, Field, and Pande, 2017) find that public housing receipt *decreases* the earnings, employment, and well-being of beneficiaries. A leading hypothesis for these adverse effects is that while public housing improves dwelling quality, housing projects are often located on city peripheries and so families lose in terms of proximity to jobs, social networks, and public goods such as schools (Lall et al., 2008; Barnhardt et al., 2017).

This paper leverages public housing lotteries conducted in Colombia to estimate the impact of high-quality public housing on child outcomes. It does so in the context of Colombia's "Free Housing" program, which provides housing to over 100,000 highly-disadvantaged families. The program is unique in two dimensions:

[1] See Chyn and Katz (2021) for an up-to-date review of the neighborhood effects literature.

[2] From 2007-2012, Latin America alone has seen multi-billion housing assistance programs started in Argentina, Brazil, Colombia, and Mexico. As one example, Brazil's *Minha Casa Minha Vida* program started in 2009 and cost 89.5 billion USD from 2009-2014. See Buckley et al. (2016) for a list of multibillion-dollar housing programs launched in developing countries over the past two decades.

(i) the public housing units were built in desirable areas near city centers, and (ii), the housing unit was given to recipients for free, although the unit could not be rented or sold for ten years. As the housing was oversubscribed, thirty percent of units were randomly assigned via lottery. We leverage these lotteries to show the causal impact of public housing receipt on children's schooling 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 ICFES – Colombia's high school exit exam which is used for university admissions – by seven percentage points and after accounting for selection into test-taking boosts ICFES exam scores by 0.1-0.2 standard deviations.[3] Public housing receipt also increases college-going by 1.4 percentage points or ten percent compared to the control mean. Given that our education data cover up to 2019, these effects are for children who resided in public housing for an average of 4.2 years.

There are many potential mechanisms driving the large treatment effects that we discover. Compared to lottery losers, lottery winners received a free housing unit in a public housing project, which impacted their child through: increased family wealth, assigned property rights, improved physical housing quality, changed neighborhoods, and new local public schools. The public housing also impacted their parents, with Camacho, Caputo, and Sanchez (2021)'s analysis of the same program finding that winning the lottery raised labor force participation, employment, and earnings. While we cannot assess the impact of each potential channel separately, we investigate the importance of changes to neighborhood and school quality in driving our results. For school quality, we use *pre-lottery*[4] data to estimate school value-added. We find that lottery winners attended better schools in terms of value-added

[3]To account for selection, we follow Angrist et al. (2006) and censor observed scores at or above a particular percentile and assign the censoring point to all those with scores below this percentile along with those who did not take the test. See Section 4.2 for more details.

[4]We use pre-lottery data to ensure our value-added estimates are not contaminated by the newly-built housing projects. In particular, we might expect that the increased wealth and stability the public housing affords recipients may 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.

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. We also document that lottery winners moved to better neighborhoods, as measured by environmental conditions, crime, and proximity to public services.

Our findings stand in stark contrast to the literature on housing assistance in developing countries, which we review in Appendix B. For instance, Barnhardt et al. (2017) use a housing lottery in India and find that after 14 years public housing receipt generated no improvement in family income or human capital and negatively affected social connectedness. Likewise, Franklin (2019) exploits a lottery in Ethiopia and estimates that moving into the public housing does not impact earnings and reduces social interactions. Finally, Picarelli (2019) investigates a program that allocated publicly-built homes to eligible households for free in South Africa and finds declines in labor earnings among recipient households. As in our context, the counterfactual housing experienced by lottery losers in these papers can be characterized as poor-quality housing located in high-poverty, high-crime neighborhoods with limited property rights.

The Free Housing program shares many characteristics with the aforementioned studies that likely advance recipients' economic outcomes: increased family wealth, upgraded physical housing quality (Galiani et al., 2017), newfound property rights (Field, 2007), and improved housing stability (Collinson et al., 2021). A large difference between the Free Housing program and those prior programs, however, is the public housing quality in terms of location. In particular, public housing in developing countries is typically located far from city centers to save costs. For instance, the public housing 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. In contrast, Colombia's Free Housing program was purposely built in desirable areas near amenities. We confirm

the proximity of public housing to amenities using a survey, with lottery winners reporting a 30-40 percent (or 5-10 minutes) reduction in commute times to nearby amenities such as public transit, grocery stores, and parks (Camacho et al., 2021). Public housing take-up was also near-universal, indicating that program participants perceive public housing as being a substantial upgrade compared to their counterfactual housing.

This paper contributes to several strands of literature. First, our findings indicate that the provision of high-quality public housing located in desirable areas advances the economic opportunity of disadvantaged children in the developing world. Our results stand in stark contrast to prior research on public housing in developing countries, highlighting the importance of location for public housing to improve recipients' outcomes. This mirrors findings in developing countries where housing assistance (e.g., renovating existing housing) generates large improvements in outcomes when location is kept *fixed* (Cattaneo et al., 2009; Galiani et al., 2017; Franklin, 2020; Kumar, 2021). Our results also align with those in Rojas-Ampuero and Carrera (2021) who find that individuals in public housing projects that had a new subway built nearby saw improved labor market earnings.

Second, this paper connects with a growing literature that examines the impact of public housing on child outcomes. In the United States, much of the literature has focused on the impact of housing vouchers on child outcomes, finding mixed results.[5] On 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, Currie and Yelowitz (2000) find that children in public housing projects are less likely to have been held back in school. Similarly, Schwartz et al. (2020) determine that housing vouchers in New York City raise students' test score performance. Chyn (2018) finds that children affected by

[5]See Collinson et al. (2015) for a review.

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. 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. Similar to our setting, children affected by housing vouchers were affected in multiple dimensions, often including increased wealth as well as improvements in housing, neighborhood, and school quality.

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). Understanding this source of heterogeneity is crucial to developing high-quality public housing. For example, changes to school quality among recipients could explain why some experiments in the United States have found null effects for adult economic outcomes (Katz et al., 2001; Sanbonmatsu et al., 2006; Kling et al., 2007; Ludwig et al., 2013) but positive effects among children (Chetty et al., 2016; Chyn, 2018). Our findings highlight that generous public housing programs can generate large gains in the educational outcomes of recipients, with a substantial driver of those improvements coming through attending better schools, mirroring findings in Laliberté (2021). The location of public housing – namely in desirable areas near public amenities such as high-quality schools – therefore appears to be a key feature for public housing programs to increase the economic opportunity of the disadvantaged.

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 the mechanisms underlying these results being discussed in Section 5. Section 6 concludes.

2 Background

On April 23, 2012 President Manuel Santos introduced Law 1537, establishing the *Programa Vivienda Gratuita* or "Free Housing" program which provided a *free* residence for the disadvantaged.[6] 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 federal government allocated 4 trillion pesos (roughly 2.2 billion USD using 2012 exchange rates). Given the amount of money allocated and the number of housing units required, a limit for construction costs of 40 million pesos (roughly $22,000 USD in 2012) per unit was set.[7] The government then opened up a call for mayors and governors to identify properties for the new housing units, 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 subsidized housing was located in peripheral land that lacked public services or in regions with high flood risks.[8]

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.

[6] 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.

[7] 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.

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

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 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.[9] Figure 1(b) then 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 so 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 same political party of 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.

[9] A further 70 developments that contained 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 and so we focus on the 225 developments constructed before 2015.

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, Figure A.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 units from the Free Housing program represented.

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, *La Hoya*, 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* 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 1 provides empirical support that the public housing units improved access to amenities. To do so, it uses a survey[10] that was conducted among lottery winners and losers and presents the (self-reported) travel time to various amenities. After

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

the lottery, lottery winners report 30-40 percent reductions (or 5-10 minutes) in commute times to the nearest public transport station, grocery store, park, 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.[11] 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.[12] Using the project-specific list, 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*[13] to contact each household on the list to apply. The *Cajas de compensación familar* attempted to notify each potential beneficiary of

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

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

[13] *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.

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 then determining their eligibility for the program.

Given the use of federally-determined beneficiary lists, the selection process was 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 who received houses being subsequently found ineligible and evicted.[14]

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.

[14]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).

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 Director 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 is able to inspect the unit and then signs 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 public housing 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 public housing 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. These combinations roughly correspond to housing project-by-eligibility-group-by-priority-tier fixed effects.[15] Here-

[15]Lottery fixed effects do not exactly correspond to project-by-eligibility-group-by-priority-tier fixed effects since a few housing projects have multiple lotteries for a given eligibility group which

after, we call these project-by-lottery-identifier groupings 'lottery fixed effects.'

Our analysis incorporates the fact that some 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).[16] Formally, we estimate the impact of receiving public housing on child outcomes using the following regression:

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

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 4) and so the effect of winning the lottery can roughly be interpreted as the impact of receiving public housing.

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.

occur because assigned units can sometime become available after the initial lottery (e.g., when a recipient is evicted or does not accept the housing unit).

[16] 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.

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 the first lottery application.[17] The 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. 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 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,[18] however, we can link these individuals to

[17] Combined with the restriction that children must reach the age of 18 by 2019 makes is so that children in our data are aged 12 to 15 at the time of the first lottery application.

[18] The full name and date of birth of the household head are also used to help link children to household heads (in addition to national ID).

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).[19] 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 III: 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). We 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 III specifically targeted 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 3 – discussed in the Results section below – where we compare lottery winners and losers) and control for several pre-lottery covariates in our empirical models.

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.[20] In

[19]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.

[20]While Colombia has a vibrant private school sector with a market share of twenty percent, the vast majority of children eligible for public housing attend public schools. For example, 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). Furthermore, the ICFES data we describe next contain both public and private school students and so we can create an alternative graduation measure of

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.[21]

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 who must pass the exam in order to graduate. 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 education institution in the country.[22] 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

whether a child took and passed the ICFES – a requirement for high school graduation in both public and private schools. Results using this alternative measure are near-identical to our main high school graduation results, indicating that the presence of private schools does not bias our results.

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

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

by the *Centro Nacional de Consultoria* between August 6 and September 6, 2020. The survey 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 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, although are somewhat more disadvantaged as one would expect.

The next three columns of Table 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).[23] Column (5) then 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.

[23]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.

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 3 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 3 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 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.[24] 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 4 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

[24]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.

sample of individuals that 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 4.2 years. The compliance rate is high, especially considering that the few lottery losers who eventually received public housing only obtained it several years later and so the average lottery loser only experienced public housing for 0.01 years. Given the high rate of compliance, 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 4.2 years on educational outcomes.

4.2 Results on Children's Educational Outcomes

Table 5 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 by age 18 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 similar to our results for high school graduation (to be expected given that the exit exam is required for graduation). 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 5 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.

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 6 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 SD. If we instead censor at the tenth percentile (Column (3)), our estimate drops somewhat to 0.09 SD.

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 SD. If we instead censor at the tenth percentile, our point estimate falls to 0.22 SD. 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.3 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.[25] Overall, our selection-corrected estimates indicate that winning the lottery generated large improvements in ICFES scores of around 0.1-0.2 SD.

5 Mechanisms

To explore the mechanisms underlying our results, we perform two 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.

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 among lottery winners and losers, rather than conflating school quality with other potential influences caused by the nearby

[25]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 SD for the median student (results not shown).

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[26] in 2006-2008. (Table A.1 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 will be 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,[27] and μ_s is a school's value-added or the contribution 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.[28]

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

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

[27]We include all the detailed sociodemographic controls we use in Table 3 along with school-grade means of those controls.

[28]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 estimation and plug-in).

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. (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 in the control vector X_{ics} in equation 2. Doing so ensures that our school quality measures are picking up the influence of the schools rather than the effects of the neighborhoods themselves. Results are shown in Figure A.4 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.5 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 lottery winners attend.

5.2 Neighborhood Quality

Using the household survey and household locations in 2019, we next investigate how the neighborhoods where treatment and control families live differ along multiple dimensions, including (perceived) environmental conditions and (police-reported) crime. (In addition, Section 2 introduced Table 1 which showed that the lottery winners reported substantial reductions in commute times to various public services and amenities.)

We start by analyzing participants' perceptions across different neighborhood attributes. To do so, we use their 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 also construct a simple index that takes the equal weighted average of the z-score of all these questions on neighborhood attributes. We label this as "low-quality" neighborhood index; a higher value of this index reflects more frequent neighborhood problems. Column (1) in Table 7 shows that lottery winners are significantly less likely to report problems in their neighborhoods compared to lottery losers (a 0.09 SD decline in the 'low-quality' index), which is driven 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%).

Next, 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*.[29] 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 5 to 6 years post-lottery.

[29] *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.

Table 8 compares the local crime of neighborhoods for lottery winners and losers. In the same way that we created the 'low-quality' neighborhood index, we construct a crime index that integrates information on incidences of assaults, robberies, and homicides. Column (1) shows that lottery winners live in neighborhoods with significantly less crime as shown by a 0.05 SD decline in the crime index. Columns (2)-(4) then report results for each type of crime, suggesting that improvements in neighborhood safety are driven by a broad-based decline for each crime considered, although the reduction in robberies (7% decrease relative to the control mean) and assaults (4%) are particularly notable.

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 link 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 schools, hospitals, parks, supermarkets, police stations, and public transport. In addition, the public housing projects are located in areas with high-quality local public schools, with the children of lottery winners attending substantially better schools in terms of value-added compared to lottery losers. Indeed, differences in school quality can explain roughly one-third of public housing's impact on high school graduation. Compared to lottery losers, those that won the lottery also perceive their neighborhoods to be less polluted and more amenable and live in safer

communities.

The generosity of the Free Housing program in terms of location likely drives the effects that we find. In contrast, much of the literature – especially in developing countries – evaluates public housing projects located in areas that are less than ideal (e.g., on the city's periphery) which greatly reduce their desirability and ability to generate economic opportunity. Such differences in location can reconcile the large positive effects that we uncover relative to the null or negative impacts found in prior work. Policymakers must therefore take care that any public housing they provide is suitably located if they wish to transform the economic opportunities of disadvantaged children.

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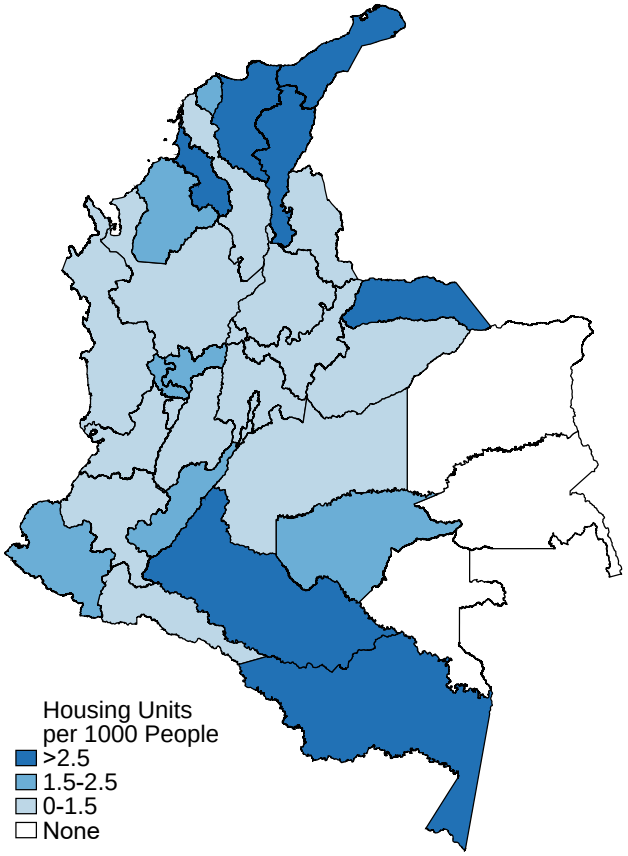
FIGURE 1: Location of Housing Projects

(A) Location of Housing Projects

(B) Project Density by Department



Notes: Map of Colombia (Map data: Google, 2021). Figure 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 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 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: School Value-Added for Lottery Winners Relative to Lottery Losers

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. Post-Lottery Distance in Minutes to Selected Locations

	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 2. Summary Statistics

	All Eligibility Groups			'Extreme Poor' Only		
	All Applicants	Direct Assignment	Lottery Participants	All Eligible	All Applicants	Lottery Participants
	(1)	(2)	(3)	(4)	(5)	(6)
Household Head Characteristics						
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						
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						
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						
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 3 as they include all applicants, while Table 3 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 3. 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					
Age at First Lottery	13.84	13.86	13.83	0.03	0.01
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					
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					
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					
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					
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	-

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 4. 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	0.011	0.150	0.013
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 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 5. 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 6. OLS and Tobit Selection-Corrected Estimates of the Effects of Public Housing 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 5. 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 7. Households' Perceptions on Neighborhood Attributes

	Low-quality neighborhood 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 their neighborhood attributes among lottery winners and losers. 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 "low-quality" neighborhood 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. 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 sq, 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 8. Post-Lottery Crime at the Neighborhood Level

	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	100,520	100,520	100,520	100,520

Notes: This table compares crime in the neighborhoods where lottery winners reside relative to those where the control group lives. 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*, 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. 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

A Appendix Tables and Figures

FIGURE A.1: Example of Applicant Housing Relative to Public Housing

(A) Example of Applicant Housing in Lorica

(B) Government Housing Project in Lorica

Notes: Figure 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 for this example come from the city of Lorica which is located in the department of Córdoba on the Caribbean coast. Figure A.1(a) shows the residence of an applicant for the public housing project *Urbanización La Victoria en Lorica*. Figure A.1(b) then shows housing units in the *Urbanización La Victoria en Lorica* housing project where the applicant moved to after winning the lottery.

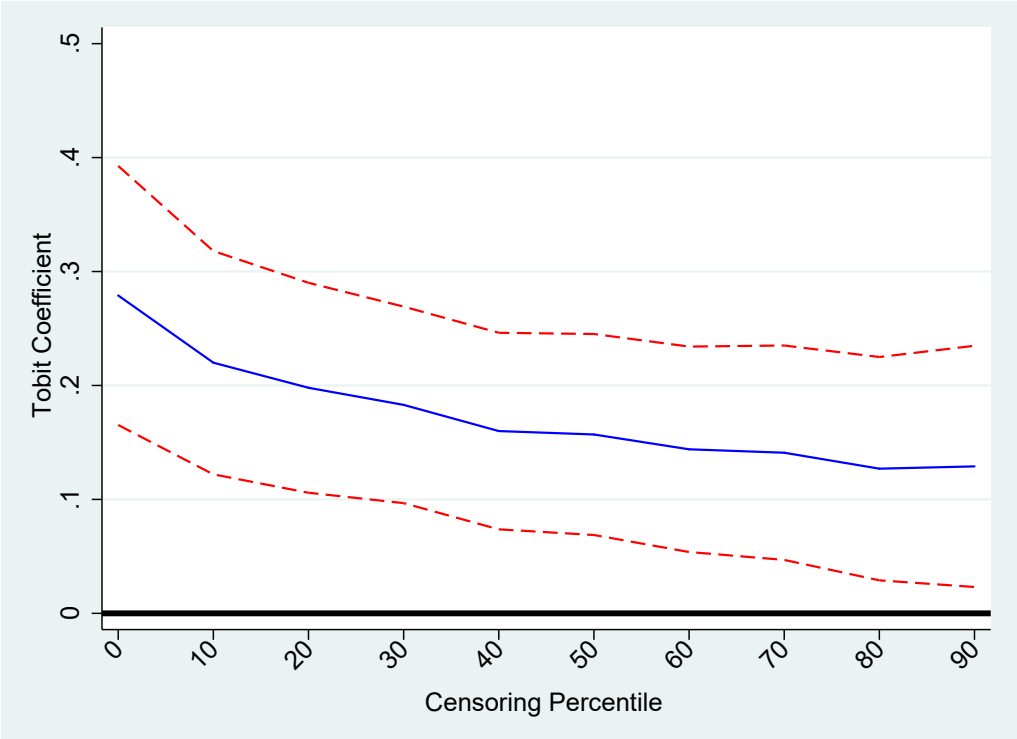
FIGURE A.2: Location of Housing Projects Compared to Location of Main Analysis Sample

(A) Location of Housing Projects

(B) Location of Main Analysis Sample

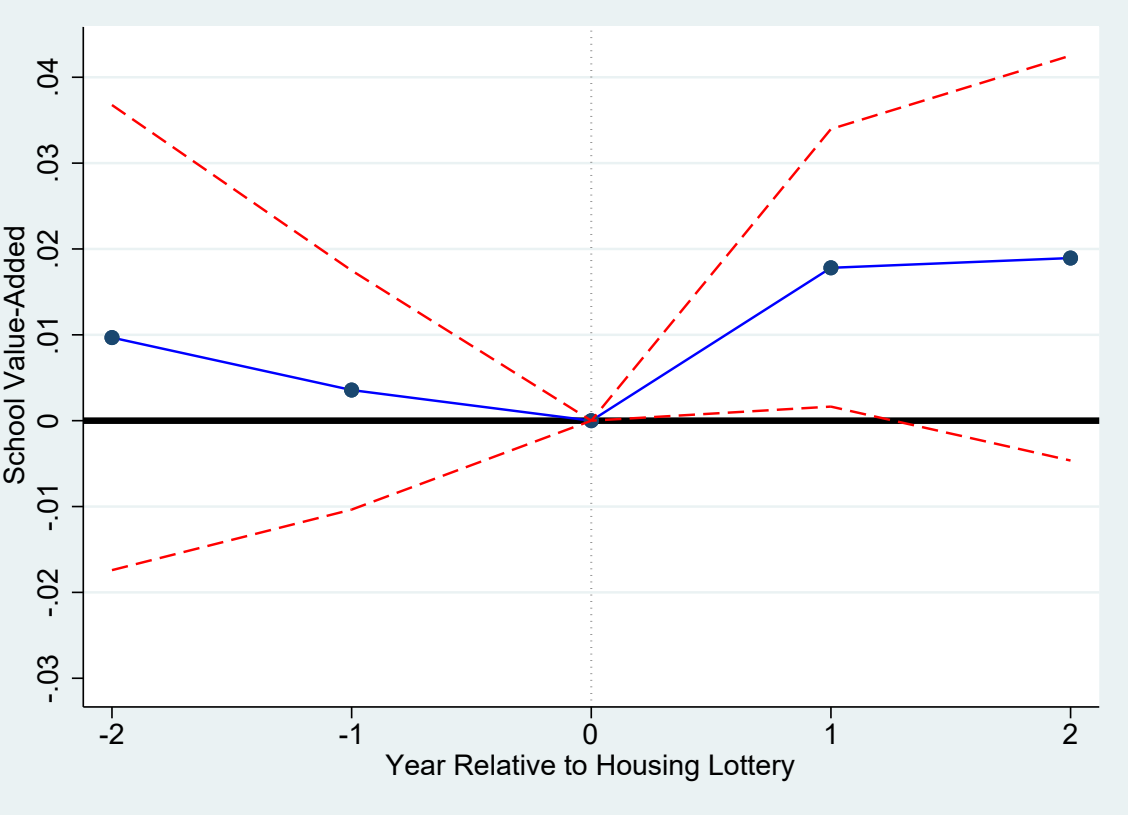
Notes: Figure A.2(a) is identical to Figure 1(a) and displays the location of the 225 projects in our data with the size of each pin corresponding to the relative size of the project in terms of number of housing units, with a minimum size imposed for projects with few observations to ensure that they 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: 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.

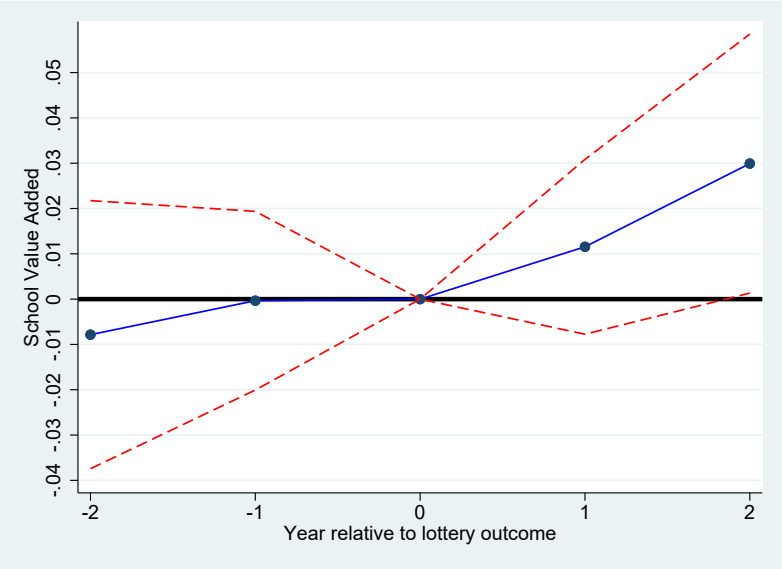
FIGURE A.4: School Value-Added for Lottery Winners Relative to Lottery Losers (Neighborhood Fixed Effects)



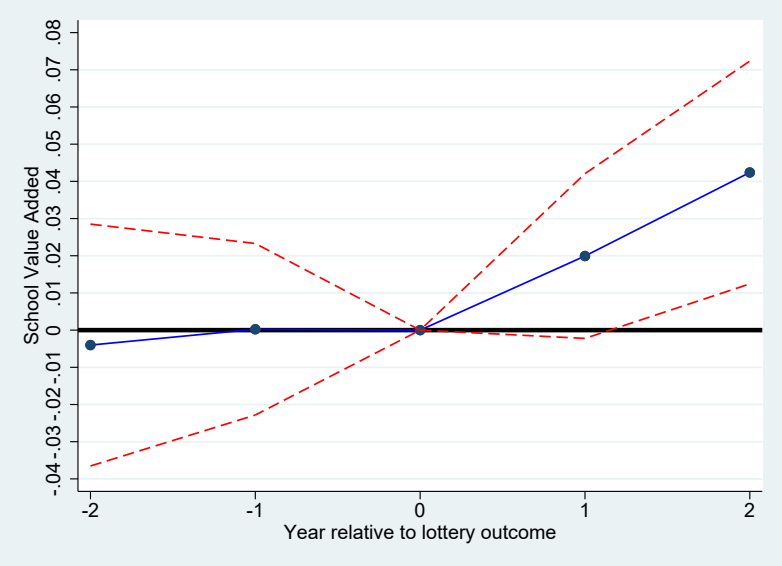
Notes: This figure replicates Figure 3 but includes neighborhood fixed effects (measured pre-lottery using the SISBEN III) as controls in the control vector X_{ics} when calculating school value-added in equation (2). This ensures that our school value-added results are driven by differences in school 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.5: 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

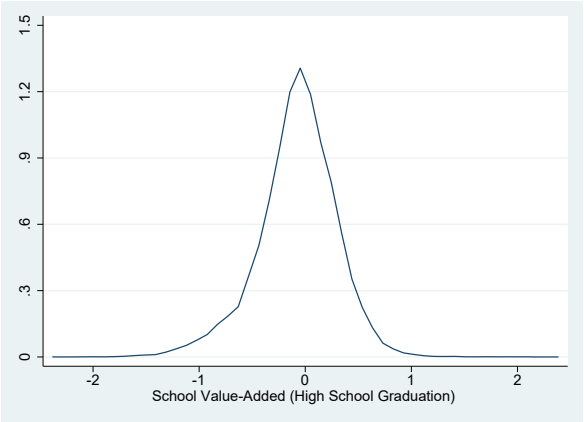


Notes: These figures replicate Figure 3 but use ICFES scores (rather than high school graduation) as the dependent variable when calculating school value-added in equation (2). Figure A.5(a) just use the raw (standardized) ICFES scores, while Figure A.5(b) employs a selection-corrected (standardized) 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.

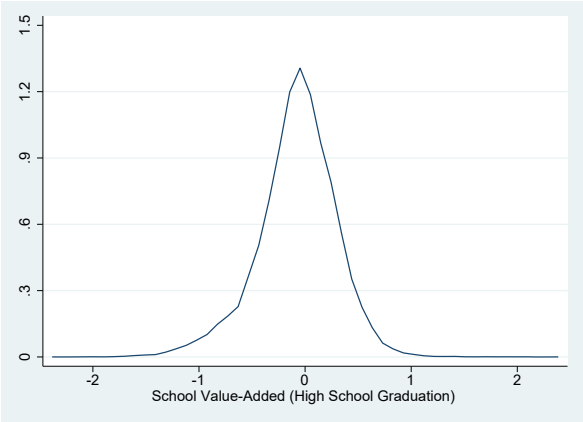
FIGURE A.6: Distribution of School Value-Added

Overall School Value-Added Distributions

(A) High School Graduation Value-Added Distribution

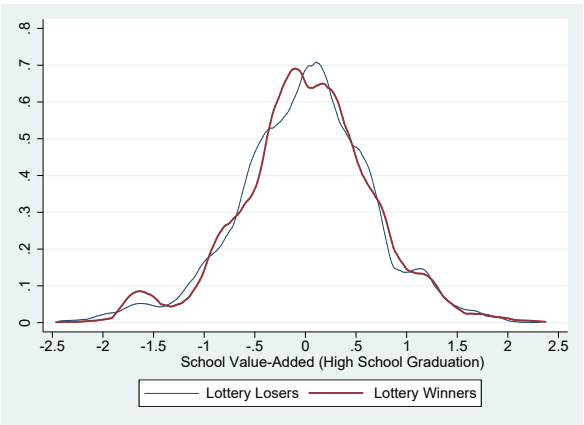


(B) ICFES Score Value-Added Distribution

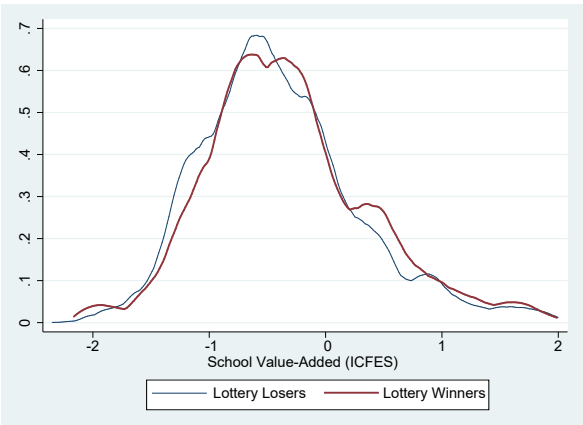


Distribution of School Value-Added for Lottery Winners and Losers

(C) High School Graduation Value-Added Distribution by Lottery Status



(D) ICFES Score Value-Added Distribution by Lottery Status



Notes: These figures show the distribution of (standardized) school value-added, with value-added calculated using equation (2) and either high school graduation or ICFES (not selection-corrected) scores being used as the dependent variable. Figures A.6(a) and A.6(b) show the estimated distribution of value-added when the dependent variable in equation (2) is high school graduation and ICFES scores, respectively. Figures A.6(c) and A.6(d) then display the school value-added distribution for the schools attended by lottery winners and lottery losers when the dependent variable in equation (2) is high school graduation and ICFES scores, respectively.

TABLE A.1. 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.

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 chose 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 (2021)	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%.