The human capital effects of subsidized homeownership: Evidence from a natural experiment in Mumbai*

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Abstract

Governments in countries at all income levels subsidize homeownership and thereby transfer wealth to middle-class households. I use subsidized housing lotteries in Mumbai to identify the human capital effects of such a transfer and find that 3-5 years later, winners are more educated than non-winners, with effects concentrated among school-age youth. The intervention also increases rates of employment, particularly among older youth who have had the chance to complete their education. Effects are not likely to be driven by relocation, as winners live in neighborhoods with poorer school quality and lower rates of literacy and employment than non-winners.

JEL Codes: E24, I38, O18, H24, J62

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I. Introduction

What are the effects of subsidizing homeownership on household economic trajectories? Governments in many countries may do so using a variety of tools, including mortgage and home-price subsidies. These subsidies constitute wealth transfers to beneficiaries experienced in any combination of three payout structures: through a stream of in-kind benefits for those who choose to live in the subsidized home, through cash benefits among those who choose to rent it out, or lump-sum through resale. Aside from transferring wealth directly, these interventions also facilitate the purchase of an asset that has great potential for appreciation in value, particularly in growing cities, and forms the cornerstone of wealth accumulation for many families. In fact, home equity may be so fundamental to wealth that differential barriers to purchasing a home have been hypothesized to play a key role in intergenerational inequality across racial groups in the United States (Oliver and Shapiro 2013).

Understanding the effects of these subsidies on *human* capital is fundamental to learning about the mechanisms through which they affect economic fortunes more broadly. A substantial literature has attempted to do so in the United States, where homeowners can deduct much, if not all, of their mortgage interest from taxes (e.g. Richman, 1974; Essen et al., 1978; Green & White, 1997; Haurin et al., 2002; Dietz & Haurin, 2003; Cairney, 2005; Barker & Miller, 2009). Findings have been mixed, and all studies face the problem that those who select into homeownership may differ in many ways from those who do not.[1] Moreover, this question has yet to be investigated in other countries and contexts where subsidizing homeownership is common, particularly low- and middle-income countries.

The present study provides some of the first causally identifiable estimates of the effects of subsidizing homeownership on beneficiaries' educational attainment and employment. In particular, it estimates the effects of one policy configuration common in India, the subsidized sale of government-constructed homes to middle-class households. Governments in every major city in India, including Delhi, Mumbai, Bengaluru, Kolkata, Chennai, Hyderabad, Ahmedabad, and other smaller cities offer some form of this program, but they have yet to be systematically studied. Similar policies exist in Brazil, Uruguay, Nigeria, Kenya, Ethiopia, and elsewhere, too.[2] I take advantage of the fact that a program in Mumbai, a city of over 20 million residents, allocates the housing through a randomized lottery system.

From September 2017 to May 2018, I surveyed 834 total winners and non-winners of multiple lotteries that took place in 2012 and 2014 and found modest effects on educational attainment and employment outcomes. On average, individuals in winning households have 0.13 standard deviations, or over a half year, more education than those living in non-winning households.

[1] These papers invoke a selection-on-observables assumption or use longitudinal datasets to circumvent this problem, but findings are far from conclusive.

[2] Franklin 2019 studies causes and effects of relocation motivated by one such program in Ethiopia.

In other words, the intervention shifts individuals from roughly the 61st to 63rd percentile of average years of education in Mumbai. An exploratory analysis shows that this shift reflects an effect on individuals' likelihood of completing secondary and post-secondary education, with increases concentrated among school-age children (youth). Among household members who turned 16 after the lottery, for example, the intervention increases the likelihood of continuing schooling past grade ten by 14 percentage points. Among household members who turned 21 after the lottery, the intervention increases the likelihood of completing post-secondary education by 15 percentage points. Also, the intervention increases levels of employment among individuals by 4.2 percentage points; this effect size is 21.7 percentage points for youth who turned 21 after the intervention, or those who had the chance to complete their education. The overall employment effects actually represent a larger 7.5 percentage point increase in full-time labor offset by a *decrease* in rates of part-time employment.

There are many possible reasons for these effects, including shifts in budget constraints, attitudes about the future, and the perceived returns to education. Relocation perhaps does not explain effects, as winners live on average in neighborhoods with poorer school quality and lower rates of literacy and employment than non-winners. I do find evidence to suggest that the intervention leads to changes in attitudes and preferences. Winners report feeling happier about their financial situations, expect better lives for their children, are more likely to plan to stay in the city permanently, and have slightly more "individualistic" attitudes. Attitudes and beliefs potentially have important effects on households' investment decisions. Recent work (e.g. Mani et al. 2013; Haushofer and Fehr 2014) has found that the insecurity created by poverty can make it difficult to focus on long-term goals and lead to short-sighted behavior. Positive effects on the reported use of free or cheap healthcare services such as friends and family members' advice, in spite of no reported increase in the incidence of illness, support effects on attitudes as a possible mechanism for investment in longer term outcomes more generally.

Existing experimental work studies the effects of interventions that differ from the one studied here in important ways. One set of related studies is those of housing or rental subsidies that require relocation to receive the transfer, which means that the location of housing can drive effects.[3] Barnhardt et al. (2017) and van Dijk (2019) find, for example, that rental subsidies lead to broken social networks and differential effects on labor market outcomes depending on the location of the housing. Studies of United States' Moving to Opportunity (MTO) program (Katz et al. 2001; Ludwig et al. 2001; Ludwig et al. 2013; Chetty et al. 2016) similarly find some long-term positive effects of an intervention explicitly motivated by moving households to wealthier neighborhoods. In the intervention I study, winners may rent out their homes and are not required to relocate.

Studies of the effects of large cash prize (Imbens, Rubin, and Sacerdote 2001; Cesarini et al.

[3]Barnhardt et al. (2017, 7), for example, state that "failure to pay monthly rent resulted in the occupant losing the legal right to remain in the property."

2016; Cesarini et al. 2017) and land (Bleakley and Ferrie 2016) lotteries have found null or negative effects on human capital investment and employment, but these transfers have been in different institutional contexts and entail different vehicles. In low- and middle- income countries, other types of asset transfers such as urban land-titling (see e.g. Feder and Feeny 1991; Field 2005; Di Tella et al. 2007; Galiani and Schargrodsky 2010) and rural ultra-poor graduation programs (e.g. Banerjee et al. 2015) have received more attention, but both entail smaller wealth transfers than the program studied here. The long-term effects of small cash transfers on educational attainment have, so far, appeared to be null or modest (e.g. Araujo, Bosch, and Schady 2016; Haushofer and Shapiro 2018; see Bouguen et al. (2018) for a review) with a few exceptions (e.g. Mollina Millán et al. 2020). Yet these streams of income are uncertain; they may be reversed, cancelled, or changed in value by future administrations. The uncertainty itself could inhibit investment in human capital.

Overall, this paper is among the first to study the effects of a common policy that may facilitate asset accumulation and fundamentally change the trajectories of families. Subsidizing homeownership is an initiative pursued by governments in wealthy, low-, and middle-income countries alike, yet causal identification of the effects of these policies is difficult. Like the home mortgage interest deduction in the United States (see Glaeser and Shapiro 2003), these programs often benefit middle-class households rather than the poor; studying their effects is thus also essential to understanding the growth of inequality in certain contexts.

The paper proceeds as follows: Section II describes the intervention, and Section III describes the data collection process and sample. Section IV sets up the estimation strategy, and Section V presents the main results on education and employment. Section VI then discusses mechanisms, Section VII discusses program targeting, and Section VIII concludes.

II. The Intervention

The intervention studied here is motivated in part by the growing demand for urban housing; in India, about 404 million people are expected to migrate to cities by 2050 (UN World Urbanization Prospects 2015). As demand for living space increases, poorer households are forced to live on the least desirable and cheapest housing in a city.[4] As a result, governments have also attempted to increase the housing supply by encouraging private developers to build and by constructing housing themselves. In fact, state-level development boards have spearheaded programs that sell, rather than rent, subsidized units to eligible households in every major city in India. Moreover, in 2015, India's federal government claimed a housing shortfall of over 18 million units to motivate

[4]Several have studied interventions to solve problems faced by those who live in illegal settlements, such as lack of title (see e.g. Di Tella et al. 2007; Feder and Feeny 1991; Field 2005; Galiani and Schargrodsky 2010) or poor service delivery (see e.g. Burra 2005; Gulyani and Bassett 2007; Imparato and Ruster 2003). These interventions mostly help alleviate problems of informality faced by a city's poorest residents, but low housing supply may also cause members of higher socio-economic strata to live in housing that is low quality, far from the city enter, or shared with many.

a plan, Pradhan Mantri Awas Yojana (P-MAY, or "The Prime Minister's Housing Scheme), to build 20 million affordable homes by 2022. Grants to subsidize the construction and sale of low-income housing by local municipal boards remain a central component of this policy.

This study is based in Mumbai, Maharashtra, an area that attracts migrants from all over India.[5] I study the effects of an annual housing lottery run by the Mumbai Housing and Area Development Authority (MHADA), a subsidiary of the Maharashtra Housing and Area Development Authority that uses the same acronym. MHADA runs subsidized housing programs for economically weaker section (EWS) and low-income group (LIG)[6] urban residents who 1) do not own housing, and 2) who have lived in the state of Maharashtra for at least 15 continuous years within the 20 years prior to the sale. Winners have access to loans from a state-owned bank, and most take out 15-year mortgages. While the downpayment and mortgage leave this program out of the reach of many of the city's poorest residents, it gives eligible middle-class families without property the opportunity to purchase heavily subsidized apartments. I include lotteries that took place in 2012 and 2014. Information about the area, cost, and downpayment for the apartments in the included lotteries can be found in Table I.

The lottery homes were sold at a government "fair price" that government officials claim was 30-60% of market prices at the time of sale. Table I shows winners could eventually hope for large gains; 3-5 years after the lottery, the difference between the apartment purchase price and list price for older MHADA apartments of the same size in the same neighborhood appears to lie anywhere between Rs.661,700 (about $10,300 at 2017 conversion rates) to Rs.2,869,015 (about $45,000). The differences also provide some sense of the opportunity cost of each program for the government. Housing was constructed on land obtained for free from the city's dismantled textile industry - this land has been earmarked specifically for "social" projects and cannot be used for other purposes (Madan 2016). This means that the homes for sale do not lie on the city's outskirts, but are within the major metropolitan area and near major highways and transit lines. Each is within walking distance of the Mumbai suburban rail network, the main network that millions of city residents use to commute every day. Figure I shows the location of the 2012 and 2014 EWS and LIG MHADA apartment buildings and households in the sample at the time of application. Households were permitted to choose the building for which they submitted an application.[7]

Resale of the apartments is not permitted until 10 years after purchase. This rule generally seems to be enforced, both by MHADA officials and homeowners' associations active in each

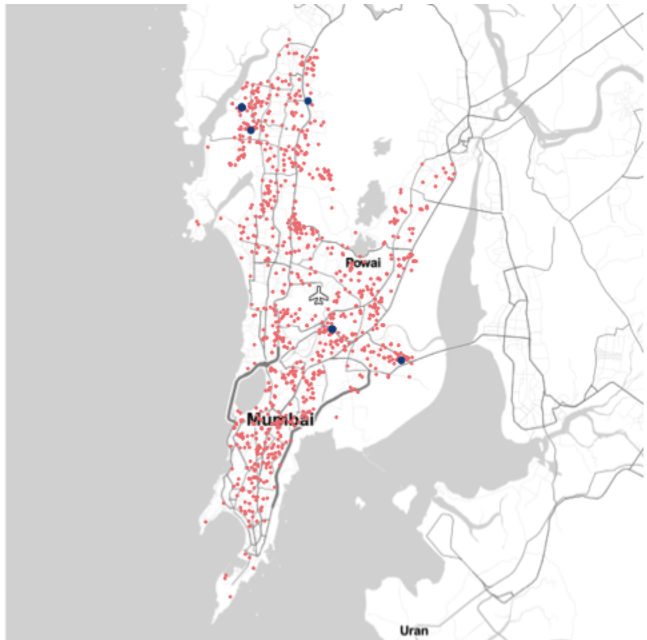
[5]Here, the population growth rate for Mumbai from 2010-2018 was approximately 13%. The private sector has been unable to meet the resulting growth in housing demand for one main reason: supply is constrained by a strict building height-to-land ratio. This rule originally stems from the facts that much of the city occupies land reclaimed from the Arabian Sea and that the airport lies near the center of the metropolitan area. Developers are thus incentivized to devote valuable central city square footage to higher end buildings, leading lower-income households to occupy slums, crowd into extremely small homes with friends and relatives, or live far from the city. One survey respondent, for example, claimed to have lived 2.5 hours by train from his place of work when he first moved to the city. According to the 2011 census, roughly 40% of the population of the Mumbai Metropolitan Area lives in slums (Government of

Table I: Lotteries included in the sample

Lottery #	N winners	Year	Group	Neighborhood	Area[1]	Allotment price[2]	Current price[3]	Downpayment[4]
274	14	2012	LIG	Charkop	402	2,725,211	5,000,000	15,050
275	14	2012	LIG	Charkop	462	3,130,985	6,000,000	15,050
276	14	2012	LIG	Charkop	403	2,731,441	5,000,000	15,050
283	270	2012	LIG	Malvani	306	1,936,700	2,800,000	15,050
284	130	2012	LIG	Vinobha Bhave Nagar	269	1,500,000	2,700,000	15,050
302	227	2014	EWS	Mankhurd	269	1,626,500	2,000,000	15,200
303	201	2014	LIG	Vinobha Bhave Nagar	269	2,038,300	2,700,000	25,200
305	61	2014	EWS	Magathane	269	1,464,500	5,000,000	15,200

[1] In square feet. Refers to "carpet area", or the actual apartment area and excludes common space.
[2] Price at which winners purchased the home in INR with the cost stated in the lottery year. In 2017, about 64 rupees made up 1 US dollar.
[3] Average sale list price of a MHADA flat of the same square footage in the same community. Data collected from magicbricks.com in 2017.
[4] In INR with the cost stated in the lottery year. Includes application fee of Rs.200.

Figure I: Location of the addresses of households in the sample (small pink dots) along with the location of apartment buildings (large blue dots) at the time of application



lottery building. Conversations with building residents reveal that one or two owners have successfully sold their homes before the 10 year period, but most interview respondents discussed considering sale only when permitted as they are likely to receive higher prices for legal sale. Additionally, apartment "society" (the local name for homeowners' associations) chairmen claimed to contact MHADA if they suspected an attempted sale due to a belief that apartment prices are sticky and early sales create a low "benchmark" for future sales in the same apartment complex.

Households can, however, put the apartments up for rent.[8] Fifty percent of households in the study have made this choice, and the median monthly rental income net of mortgage payments is Rs.2000, or roughly 30 USD. Finally, households do not pay taxes on their dwelling for five years after possession.

As mentioned earlier, beneficiaries were selected through a lottery process. In fact, the winning sample was stratified by caste and occupation group (Table C.I), as each lottery had quotas for these groups within which random selection occurred. The lottery caste/occupation group

India 2011).

[6]Members of the EWS earn up to 3200 USD/year. Members of the LIG earn up to 7400 USD/year.

[7]The centrality of program housing will likely vary across cities. Nevertheless, development authorities in other major cities, such as Delhi and Bangalore, offer housing areas that are similarly somewhat central such as Vasant Kunj (Delhi) and Koramangala (Bangalore).

[8]The ability of households to legally rent out lottery housing will likely vary across cities as well. Households will always, however, be able to rent homes out illegally. Development boards in other cities also may have other provisions to increase the flexibility of beneficiaries. The Bangalore Development Authority, for example, allows beneficiaries to swap allotted houses.

within which stratification occurred will be referred to as "blocks" from now on. Aside from evidence provided by the balance checks below, there are several reasons to believe that the the allotment process was fair, or truly randomized. First of all, in response to extreme scrutiny over the selection process and concerns about corruption, the lottery was conducted using a protected computerized process that was implemented in 2010.[9] Applicants also applied with their Permanent Account Numbers (PAN), which are linked to their bank accounts. Before conducting the lottery, MHADA officials used the PAN numbers to check both whether individuals had applied multiple times for the same lottery number and whether or not they met the criteria for eligibility. Note that applicants are able to apply for multiple lotteries within the same year, but not multiple times for the same lottery number.[10]

III. Data Collection

I estimate treatment effects on all outcomes based on in-person household surveys of a sample of both winning (treatment) and non-winning (control) households. There are about 1,000 applicants for each apartment; for this reason, I interviewed a random sample of non-winning applicants. I procured from MHADA phone numbers and addresses for both winners and a random sample of applicants drawn in the same stratified method used for the selection of winners. One non-winner was drawn for each winner within each block. Recall that a block consists of a caste/occupation group within each lottery. In this way, both the sample of winners and non-winners was a random draw from the sample of applicants.

In the case that households had applied for multiple lotteries included in the study (either within a year or across years), they would have a higher likelihood of appearing in either the sample of treatment or control households. The sampling procedure explicitly allowed for the possibility of the same household being drawn multiple times, and I had planned to include multiple rows for the household in question in this situation. For example, if a household won lottery A but also was drawn in the sample of non-winners for lottery B, its data would have been included as a set of outcomes under treatment for lottery A and under control for lottery B. Ultimately, no households were drawn more than once, likely reflecting the fact that being sampled from the pool of applicants is an extremely rare event.[11]

I accessed a total of 1,862 pre-treatment addresses, or those used at the time of application to the lottery. These addresses were first mapped by hand using Google Maps. Addresses that were incomplete (42), outside of Greater Mumbai (611), or could not be mapped (146) were removed

[9]In fact, a handful control group respondents complained about paying brokers who claimed to be able to help "fix" the lottery and were subsequently never heard from again.

[10]Prior to each lottery, MHADA releases a list of applicants deemed ineligible for the lottery because they have violated any of the income, homeownership, domicile, or single application requirements.

[11]As described by de Chaisemartin and Benhaghel (2015), this is a case of a randomized waiting list. I use what they describe as the consistent "Initial Offer" estimator. Not that given the size of this lottery, however, the bias of the "Ever Offer" estimator may be approaching zero.

from the sample. This left 531 and 532 control and treatment households, respectively. Table C.II demonstrates that even after this mapping procedure, I was left with roughly equal proportions of winners and applicants in each caste/occupation category, lottery income category, and apartment building. Given the assumption that the lottery was truly randomized and the fact that I used pre-treatment addresses for the mapping exercise, there is no reason to expect the mapping exercise to systematically favor treatment or control units.

I expect the mapping procedure to have favored wealthier applicants because 1) addresses that could not be mapped often referred to informal settlements, and 2) to create a sample that I could feasibly survey, I also dropped all who lived outside of Greater Mumbai, limiting my sample to urban applicants. Table C.III indeed shows that proportions of membership in certain categories in the mapped sample are significantly different from the original sample obtained from MHADA. Importantly, there are relatively fewer Scheduled Tribe members and more General Population (or Forward Castes) members in the mapped sample than in the full sample provided by MHADA. The mapped sample may thus have slightly higher socio-economic status than the full sample of applicants on average. While this issue may affect the external validity of the study, it should not impinge upon the internal validity or causal interpretation of results.

Once mapped, I can place households into state and municipal electoral wards to test for evidence of selection into the mapped treatment group by electoral ward. Selection by ward would indicate that individuals from certain locations or with certain political representatives are more likely than others to win the lottery. Here, I estimate regressions of the treatment indicator on the state and municipal ward membership indicators and calculate a heteroscedasticity-robust Wald statistic for the hypothesis that the coefficients on all of the indicators (other than block randomization dummies) are zero. The p-values for regressions on state and municipal ward membership are 0.35 and 0.46, respectively. These p-values leave me unable to reject a null hypothesis that members of any electoral constituency were equally likely to be in the mapped treatment group.

From the mapped sample, I randomly selected 500 households from each treatment condition to interview. From September 2017-May 2018, I worked with a Mumbai-based organization to contact the households and conduct interviews.[12] The process for contacting was as follows: The addresses and phone numbers provided by MHADA constituted the contact information for households at the time of application. Non-winners were attempted at these addresses. In cases where they had moved away, neighbors were asked for updated contact information, with which the enumerators once again attempted to contact non-winners. Winners resided at either the old addresses or new lottery buildings, as they were free to inhabit their new property or rent it

[12] The organization hires its enumerators from local neighborhoods, which is a practice that was important to the success of contacting my sample households. More information about the firm, Partners for Urban Knowledge Action Research (PUKAR), can be found here.

out. Lottery housing societies were thus first contacted to ascertain which of the winners were living at the apartments. Owner-occupiers were approached at the lottery apartments; landlords were approached at the addresses listed on the application using the procedure developed for non-winners.

In all cases, we attempted to speak to the individual who had filled out application for the lottery home. Applicants applied with their Permanent Account Numbers (PAN), which are linked to their bank accounts.[13] Given the sensitive nature of the information required for application, I assumed that the individual applying was most likely to be the head of the household. In the case a child had applied for the home (likely because the form could be completed online and youth may be better able to use computers and the internet than their parents), enumerators were instructed to speak to the household head. Interviews were conducted on Sundays and weekday evenings, or times when this individual was mostly likely to be home. In my sample, 78% of respondents had reportedly completed the applications themselves.

To recap, here is a timeline of the events relevant to the study:

May 25, 2012: Winners of 2012 lottery announced

May 2013: Winners of 2012 lottery begin taking possession

June 25, 2014: Winners of 2014 lottery announced

June 2015: Winners of 2014 lottery begin taking possession

September 15, 2017-May 15, 2018: Surveys

III.A. The sample

Table II: Reasons for attrition with p-values for difference in proportions tests.

	Control	Treatment	p
Surveyed	413	421	0.6
Address not found	9	7	0.8
Home demolished	1	0	1
Home locked	5	11	0.2
Respondent deceased	1	0	1
Refused	14	20	0.4
Unable to locate household that has moved	19	10	0.1
Incomplete survey	37	31	0.5
Total	**500**	**500**	-

[13] A PAN is issued by the Indian Income Tax Department to all eligible for an income tax. Its stated purpose is to minimize tax evasion. It has evolved to become a unique identifier for financial transactions and is mandatory for actions such as opening a bank account or receiving a taxed salary.

The data collection process yielded a sample of 834, with 413 (82.6%) of the surveyed households in the control condition and 421 (84.2%) households in the treatment condition. Full information on the number of households contacted in each stratum along with reasons for attrition can be found in Table II. I do not see strong evidence of differential rates of contact for control and treated units; the p-value for the difference in proportion contacted is 0.8. Balance tests for fixed or baseline characteristics among the contacted sample can be found in Table III. Importantly, both treatment groups have an equal proportion of those belonging to the *Maratha* caste group, a dominant group in Mumbai and Maharashtra more generally.[14] This is among the most politically powerful caste groups in Mumbai, and therefore particularly likely to call in a favor and "win" the lottery. Nevertheless, winners and non-winners appear to be similar based on a number of fixed observable covariates, limiting concerns of corruption in the lottery or differential selection into the treatment groups. In line with my pre-analysis plan, I also perform an omnibus test to judge whether observed covariate imbalance at the household level is larger than would normally be expected from chance alone. This test involves a regression of the treatment indicator on the covariates (Table C.IV) and calculation of a heteroscedasticity-robust Wald statistic for the hypothesis that all the coefficients on the covariates (other than block dummies) are zero. The p-value for this test is 0.39.[15]

Table IV provides a summary of the main outcome variables of interest among the surveyed control group. The sample is at about the 61st percentile for mean years of education in Mumbai based on the India Human Development Survey- II (IHDS-II), which was conducted in 2010 (Desai and Vanneman 2016). Most live in dwellings with permanent floors and roofs. In addition to these outcome statistics, about 31% of respondents claim that the household's main earner has formal employment with either the government or private sector. About 43% of respondents claim that the household's main earner has informal employment with the private sector.[16] None of the applicants, by rule, owns housing in the state of Maharashtra, and 57% claim to live in rental housing, while 77% report living in homes shared with extended families.[17] EWS and LIG group membership entailed annual income caps of Rs.192,000 and Rs.480,000, placing the highest earners in each category in the 47th and 94th percentile of annual income in Mumbai as collected by IHDS-II.[18] I thus describe the sample as decidedly middle-class and upwardly mobile. This description is corroborated by an interview conducted with the commissioner of the Mumbai Metropolitan Regional Development Authority, who saw the main beneficiaries of the housing

[14] *Kunbi Marathas* have been excluded from this group, as they are considered a "lower" caste group (*jati*) and do not intermarry with other *Marathas*. As there were too many *jatis* to generate a coherent balance test on *jati*, I tested balance on being a member of the dominant caste group. Balance tests on other *jatis* are available upon request.

[15] Other balance tests are available in Appendix C.

[16] A job is considered to be in the formal sector if individuals are given letters, contracts, or notification of pension schemes upon being hired.

[17] There may be overlap in these two categories.

[18] As in many cities with high levels of inequality, the income distribution in Mumbai is left skewed with a long right tail.

Table III: Balance tests on household and individual characteristics as measured through a survey.

Variable	Control	Treatment	sd	Pr(>|t|)
A: Household characteristics N=834				
OBC[1]	0.150	-0.021	0.035	0.543
SC/ST[2]	0.080	-0.018	0.026	0.499
Maratha[3]	0.295	0.018	0.045	0.690
Muslim	0.090	0.006	0.029	0.852
Kutcha[4]floor	0.031	0.028	0.019	0.136
Kutcha[4]roof	0.039	0.001	0.018	0.945
Originally from Mumbai	0.809	0.062	0.039	0.114
From the same ward as the apartment	0.097	0.023	0.030	0.454
B: Individual characteristics N=3,170				
Age	35.874	0.095	0.574	0.869
Female	0.485	0.000	0.011	0.998
OBC[1]	0.148	-0.022	0.023	0.340
SC/ST[2]	0.084	-0.029	0.021	0.165
Maratha[3]	0.292	0.024	0.032	0.457
Muslim	0.086	0.015	0.021	0.477
Kutcha[4]floor	0.028	0.030	0.023	0.188
Kutcha[4]roof	0.043	0.001	0.023	0.979
From Mumbai	0.812	0.051	0.026	0.052
From the same ward as the apartment	0.095	0.030	0.021	0.154

The "Control" column presents means for winning households. The "Treatment" column presents the difference between winning and non-winning households estimated through an OLS regression of each variable on indicators for winning the lottery. Each regression includes an interaction with the centered block-level indicator for randomization groups. All regressions include HC2 errors, with errors clustered at the household level for individual results.

[1] Other backward class caste group members

[2] Scheduled caste or scheduled tribe groups, also known as Dalits.

[3] A dominant group in Mumbai and Maharashtra more generally.

[4] "*Kutcha*" means "rough" or "impermanent." Variable measured at time of application through recall.

program to be working class households (Madan 2016). Citing experience from Latin American cities, Alan and Ward (1985, 5), find that public housing interventions generally do not benefit a city's poorest citizens, as they simply cannot afford the requisite rent or mortgage. Recall, however, that the sample mapped and surveyed is somewhat wealthier than the entire pool of applicants on average.

Table IV: Summary of the control group.

Statistic	Mean	St. Dev.	Min	Max
Housing quality				
Permanent floor	0.96	0.19	0	1
Permanent roof	0.79	0.41	0	1
Private water source	0.73	0.45	0	1
Private toilet	0.62	0.49	0	1
Assets				
Dining table	0.20	0.40	0	1
TV	0.91	0.29	0	1
Fridge	0.87	0.33	0	1
Gas	0.88	0.33	0	1
Computer	0.39	0.49	0	1
Internet	0.47	0.50	0	1
Smartphone	0.73	0.44	0	1
Car	0.06	0.23	0	1
2 wheeler	0.36	0.48	0	1
Bicycle	0.04	0.20	0	1
Household education and employment variables				
Public school (sons)	0.06	0.23	0.00	1.00
Public school (daughters)	0.05	0.22	0.00	1.00
After school tuition (sons)	0.20	0.39	0.00	1.00
After school tuition (daughters)	0.19	0.38	0.00	1.00
Main earner salaried	0.80	0.40	0	1
Main earner has govt. job	0.18	0.38	0	1
Individual education and employment variables				
Years of education	10.31	4.67	0	18
Working	0.46	0.48	0	1
Working full-time	0.47	0.49	0	1
Working part-time	0.09	0.30	0	1
Attitudes (main earner)				
Happy w/ financial situation	0.63	0.48	0	1
Children will have better lives than them	0.56	0.50	0	1
Would never leave Mumbai	0.77	0.42	0	1
Trust others	0.73	0.45	0	1
Believe effort leads to success	0.85	0.36	0	1
Claim to make own decisions	0.15	0.35	0	1

IV. Estimation

I estimate effects of winning the lottery within the contacted sample on reported local civic action, attitudes, knowledge of local politics, and motivations for vote choice. I follow my pre-analysis plan[19] and estimate the treatment effect on the pooled sample of lotteries, β, in the following equation where Y is the outcome (as measured through a survey), T is an indicator for treatment (winning the lottery), and $C_1...C_j$ is the group of fixed (or pre-treatment) covariates used for randomization checks, and ϵ is an error term. Given that randomization happened within blocks, I treat each of the blocks as a separate lottery and include a set of centered dummies, $B_1...B_l$ for each. Following Lin (2013), I allow for heterogeneous effects within the blocks by centering the block dummies and interacting them with the treatment indicator:

$$Y = \alpha + \beta T + \sum_{1}^{j} \gamma_j C_j + \sum_{1}^{l} \omega_l B_l + \sum_{1}^{l} \eta_l (T * B_i) + \epsilon \tag{1}$$

I label households as "treated" if they win the lottery in the specific year for which they appear in the sample. While this study potentially suffers from two-sided noncompliance (8% of treated units did not purchase homes), I simply conduct an intent-to-treat (ITT) analysis.[20] β can thus be interpreted as a weighted average of block-specific intent-to-treat effects. Following Imbens and Kolesar (2015), I compute standard errors using the HC2 estimator (MacKinnon and White 1985). Also, I make Benjamini-Hochberg corrections for the false discovery rate within "families" of outcomes. When an outcome is not binary or categorical, treatment effects are reported in standard deviations of the control group.

For education and employment results, I also analyze individual-level data that is based on a census of every household member to estimate individual-level treatment effects. This dataset drops all individuals born *after* the household-relevant lottery was conducted. These individuals are dropped to exclude post-treatment bias arising due to treatment effects on fertility.[21] Regressions here include block-centered dummies and covariates as well.

Again, note that this paper estimates average treatment effects across the different types of payout structures chosen. This is mainly because this choice reveals a type, and types remain unknown among the control group.[22] As a result, it is not possible to measure the effects conditional on this choice, let alone the effect of this choice itself, without additional modeling assumptions. Predictors of moving can be found in Table F.I. More generally, the study is not powered to detect heterogeneous effects at the household level.

[19] Deviations from the pre-analysis plan are explained in appendix A.

[20] This choice should typically bias treatment effects to zero.

[21] Note that winning the lottery has no effect on fertility. Results available upon request.

[22] Control group households do not seem to be good at describing their counterfactual behavior. In the survey, I asked them whether they would have chosen the in-kind transfer and moved into the homes had they won. About 95% said that they would, but only 50% of winning households chose the in-kind transfer.

V. Results

Figure II presents results for education and employment related variables measured at the individual and household levels. Household-level employment effects refer to the household's main earner. Household-level educational investment effects refer to whether an outcome holds for *any* of the sons or daughters. In an exploratory analysis, I find that positive effects on education and employment are particularly large among older youth.

V.A. Education

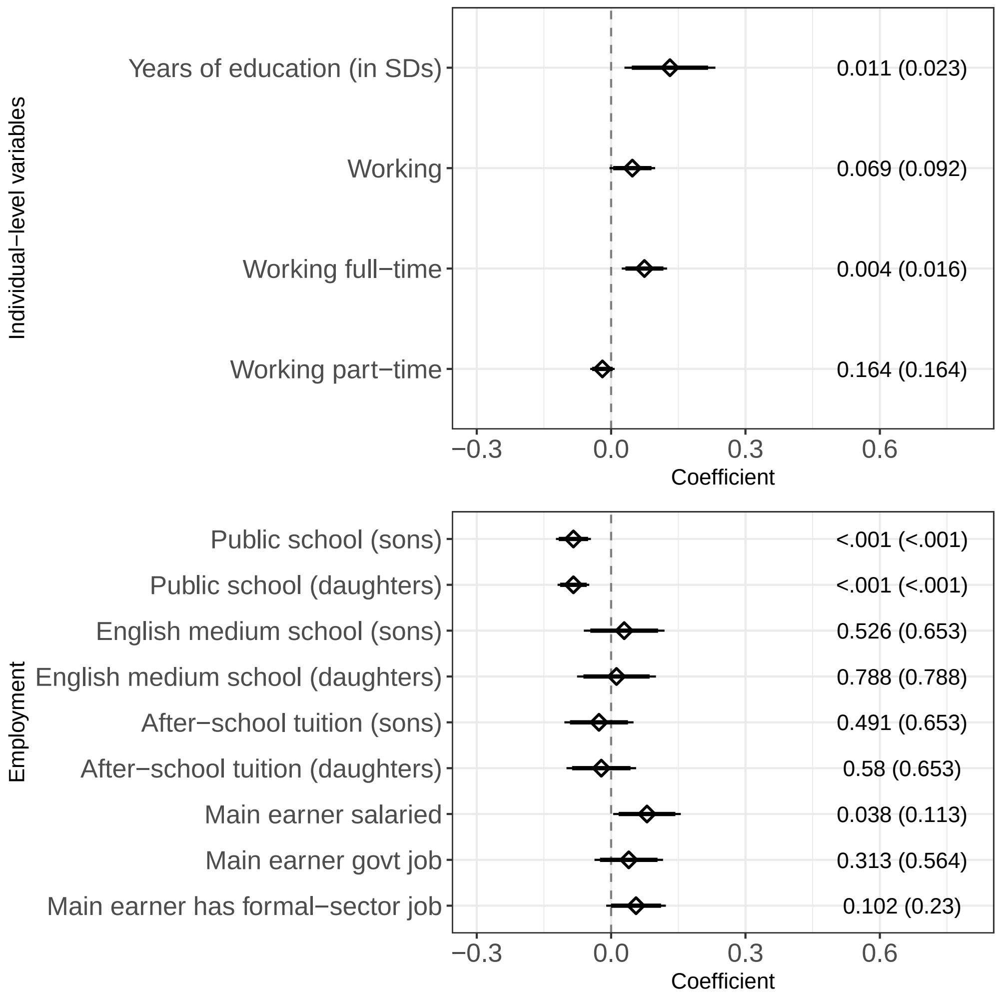
First, I estimate that the average number of years of education among winners is 0.13 standard deviations, or about 0.60 years, greater than among non-winners. On average, non-winners report having completed 10.31 years of education; based on data from IHDS-II (2016), the intervention shifts individuals from roughly the 61st to 63rd percentile for educational attainment in Mumbai. The intervention shifts individuals from roughly the 75th to 78th percentile of educational attainment in urban areas more generally. At the household level, I estimate that parents of winners are about 8.4 percentage points less likely to report sending their children to public school than parents of non-winners; in India, asking if children attend a public ("government") school is a more common way to draw the distinction between public and private schools than by asking if children attend private schools.[23] This is likely due to the extreme heterogeneity in the types of non-government providers of education in India; a private school can refer to a prestigious international school, or it could refer to a school run out of a private home (Harma 2011). In spite of this heterogeneity, public schools have the well-earned reputation of being significantly lower quality than their private counterparts in urban India (Kingdon 1996; De and Drèze 1999). These results are not accompanied by any measurable effects on a common practice in India, namely sending children to after school tuition. Note also that effects do not differ for sons and daughters, but this may be due to social desirability bias in responses.

At what margin does this shift occur? The distribution of the individual years of education for those living in winning and non-winning households shows a multimodal distribution of educational attainment, with means at 0, 10, 12, 15 years of education (Figure III). The modes at 0, 12, and 15 years represent barriers to beginning schooling, beginning post-secondary schooling, and beginning graduate schooling respectively.[24] The mode at 10 years possibly reflects the barriers to continuing education past 10th grade that are particularly high in India. Here, students sit for the All India Secondary School Examination (AISSE) at the end of grade 10. Only if they pass this exam can students advance past grade 10. Those who pass also receive an AISSE Secondary School Completion Certificate, which is in itself a certification that may be used for

[23]These results reflect differences in responses to the question "do any of your sons/daughters attend school type X?"

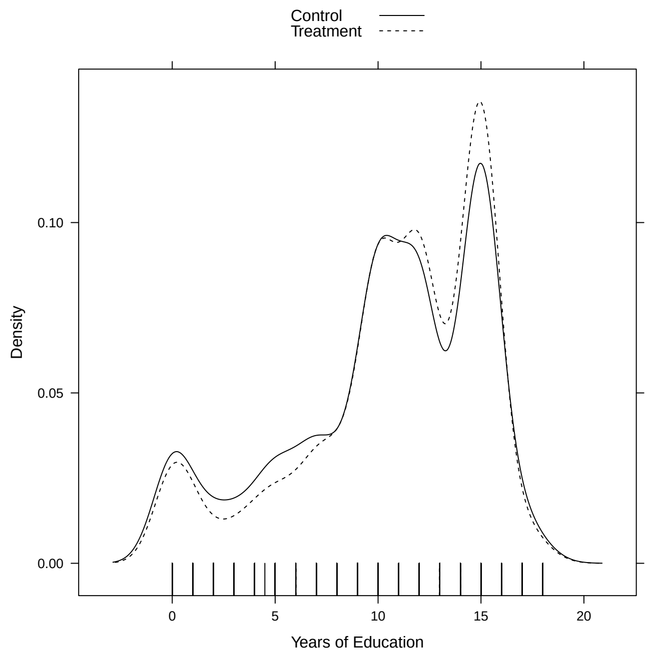
[24]In India, a bachelor's degree typically takes 3 years to complete.

Figure II: Treatment effects on household educational attainment and employment outcomes.



Employment here means having worked one hour or more in the past week. Full-time employment means working five days or more in the past week, while part-time employment refers to working fewer than five days in the past week. Household-level employment effects refer to the household's main earner. Whether or not an individual has a "formal sector" job is proxied for by whether s/he received a letter or a contract at the beginning of the job. Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the false discovery rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available intables E.I- E.III

Figure III: Distribution of individual years of education for the whole sample drawn using a Gaussian kernel.



certain jobs. Stopping one's education at grade 10 can be the result of a failure to pass the exam or the decision to discontinue schooling; continuation of school after grade 10 should increase rates of both secondary school completion *and* rates of post-secondary school education.

In an exploratory analysis that was not preregistered, I next consider whether winning the housing lottery increases the likelihood of overcoming each of these barriers (Table V). I estimate regressions of completing one's education past these barriers on the treatment indicator. Belonging to a household that has won the lottery indeed increases the likelihood of moving past grades 10 and 12 and completing post-secondary education. It does not seem to have an effect on actually beginning one's education. I also include an interaction with the treatment indicator and an indicator for whether each individual turned 6, 16, 18, and 21 in between being surveyed and the applicable lottery year. These years were chosen with the assumption that most individuals complete 6, 16, 18, and 21 years of age in their first, tenth, twelfth, and fifteenth years of education. In other words, I investigate whether the treatment effect is stronger for those who were at the conventional ages for completing one, ten, twelve, and fifteen years of education in between the lottery and being surveyed.

As the survey did not collect information on dates of birth or age at the time of the lottery but age at the time of the survey only, this coding was done using the following logic: For applicants to the 2012 and 2014 lotteries, surveys were conducted 5 years and some fraction of a year or 3 years and some fraction of a year after the lotteries, respectively. Suppose an individual was

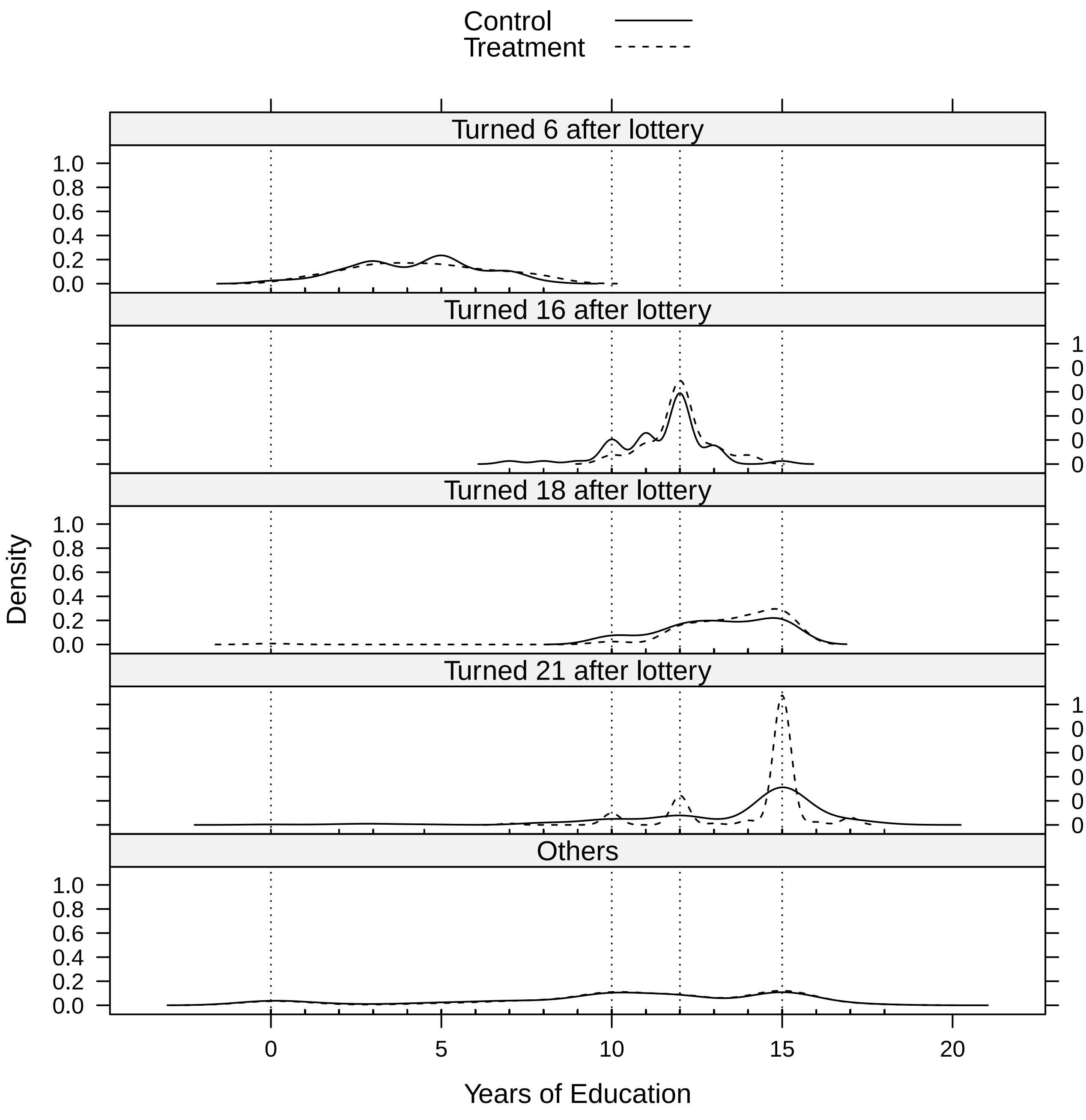
Table V: Regressions of individual completion of various years of education on the treatment indicator.

	Dependent variable:								
	Years of education	I(>0 years)		I(>10 years)		I(>12 years)		I($\geq$15 years)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T	0.618	0.009	0.010	0.071	0.056	0.056	0.039	0.041	0.036
	(0.177)	(0.009)	(0.009)	(0.018)	(0.019)	(0.019)	(0.021)	(0.017)	(0.017)
$Turned_6$[1]			0.057						
			(0.017)						
$Turned_{16}$					0.333				
					(0.042)				
$Turned_{18}$							0.387		
							(0.051)		
$Turned_{21}$									0.351
									(0.050)
TX$Turned_6$			−0.017						
			(0.018)						
TX$Turned_{12}$					0.093				
					(0.050)				
TX$Turned_{18}$							0.106		
							(0.067)		
TX$Turned_{21}$									0.114
									(0.068)
Constant	10.230	0.935	0.932	0.505	0.487	0.318	0.298	0.258	0.234
	(0.131)	(0.006)	(0.007)	(0.013)	(0.013)	(0.013)	(0.014)	(0.012)	(0.012)
Observations	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.051	0.047	0.049	0.053	0.088	0.058	0.109	0.058	0.107
Adjusted R^2	0.010	0.006	0.007	0.012	0.048	0.017	0.069	0.018	0.068

All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies.

[1] "$Turned_X$" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using age_T, or each individual's oldest possible age.

Figure IV: Distribution of individual years of education by cohort drawn using a Gaussian kernel. Vertical lines drawn to show 0, 10, 12, and 15 years.



age_s on the date of the survey in 2017, s, and participated in the 2012 lottery. On date s in 2012, she would be exactly $age_s - 5$. If her birthday had occurred between the lottery and the survey, she would have have been $age_s - 6$ at the time of the lottery. If her birthday had occurred before the lottery that year, she would be $age_s - 5$ at the time of the lottery. This same logic holds for participants of the 2014 lottery, except the lottery age could be either $age_s - 3$ or $age_s - 4$. In this way, one can code two possible ages age_l for individuals at the time of the lottery using age_s, which we will call $age_{\bar{l}}$ and $age_{\underline{l}}$ to correspond to the older and younger possible options. Individuals are further coded to have turned X years old ($Turned_X$) after the lottery if age_s is greater than or equal to X and age_l is less than X. Given the two possible values for age_l, there are also two values for $Turned_X$. For simplicity,tables in the text present results assuming all individuals were $age_{\bar{l}}$ at the time of the lottery. Results using $age_{\underline{l}}$ are similar and presented in appendix D.

I see some evidence to suggest that the housing lottery's effect on completing grades ten and college is stronger among those who turned 16 and 21 after winning, respectively (Table V) . Figure IV clearly shows that effects are concentrated among individuals who were of secondary and post-secondary school age after the lottery, rather than younger or older individuals. In particular, the figure displays a roughly 14 percentage point increase in the likelihood of completing grade 10 among members of winning households who turned 16 after the lottery and a 15 percentage point increase in the likelihood of completing post-secondary education among members of winning households who turned 21 after the lottery. The three panels for secondary and post-secondary school age children show a rightward shift in the distribution for educational attainment.

Imbalance in the age distribution for the relevant cohorts cannot account for these results. Table III shows that winners are slightly older than non-winners. As shown in Table C.V, this difference appears to be concentrated among older individuals, but is not statistically significant for any age group.

V.B. Employment

Table VI and Figure II show that these gains in educational attainment are accompanied by effects on individual employment. Employment here means having worked one hour or more in the past week.tables VII and VIII break effects down for part-time and full-time employment, or working fewer than 5 days and working 5 or more days in a week.[25] Individuals in winning households are 4.2 percentage points more likely to be employed than those living in non-winning households. Here, being employed is defined as working one hour or more in the past week. This effect can further be broken down into a 7.5 percentage point positive effect on full-time work offset by a negative effect on part-time labor. Effects on overall employment seem to be driven

[25]In India, most full-time employees work either 5 or 6 days a week.

Table VI: Regressions of individual employment on the treatment indicator.

	Dependent variable:					
	Employed					
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.042	0.038	0.051	0.045	0.035	0.058
	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)	(0.029)
$Turned_6$[1]	−0.016	−0.470				
	(0.012)	(0.014)				
$Turned_{16}$	0.001		−0.446			
	(0.025)		(0.027)			
$Turned_{18}$	0.138			−0.217		
	(0.035)			(0.052)		
$Turned_{21}$	0.644				0.160	
	(0.036)				(0.045)	
Older	0.566					0.406
	(0.013)					(0.024)
TX$Turned_6$		−0.023				
		(0.021)				
TX$Turned_{16}$			0.058			
			(0.041)			
TX$Turned_{18}$				0.065		
				(0.071)		
TX$Turned_{21}$					0.164	
					(0.068)	
TXOlder[2]						−0.021
						(0.035)
Constant	0.005	0.475	0.473	0.461	0.439	0.166
	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.020)
Observations	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.249	0.072	0.074	0.042	0.049	0.163
Adjusted R^2	0.216	0.031	0.034	0.0001	0.007	0.126

All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies.

[1] "$Turned_X$" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using $age_{\bar{I}}$, or each individual's oldest possible age.

[2] "Older" is an indicator for an individual being older than 21 at the time of the lottery.

by increases in full-time employment, as patterns for effects on full-time work mirror those for effects on overall employment, but with larger coefficient sizes. In contrast, the intervention may actually decrease levels of part-time employment. If the distinction between part-time and full-time labor is a rough proxy for wage and salaried labor, this breakdown in results complements positive estimates of household-level effects on the main earner being salaried or having a government job (Figure II). The "main" worker is defined as the family's highest earner.

I now conduct an exploratory analysis to determine whether the likelihood of employment occurs among the same groups that benefitted from gains in educational attainment. Model 1 in VI first shows that individuals become more likely to be employed as they become older; child labor is generally uncommon in this sample. It also shows that intervention increases the likelihood of employment by about 4 percentage points across all age groups. Models 2-6 further conduct an exploratory analysis to see whether effects are concentrated among certain cohorts. As shown in Model 6, among the age cohort that turned 21 or had the opportunity to pass through college since the lottery, the likelihood of being employed increases by 21.7 percentage points. This increase is in line with the finding that belonging to a winning family increases the likelihood of this age cohort completing college; children are more likely to complete their education and, in turn, more likely to find jobs.

VI. Discussion and Mechanisms

This section discusses possible mechanisms for the effects estimated above. There is little evidence to suggest that effects are driven by relocation among owner-occupiers. I instead propose that effects are driven by increases in permanent income that shift budget constraints and preferences in the medium term.

VI.A. Are effects driven by the owner-occupiers? Location-based outcomes

One explanation for these results could be that they are driven by owner-occupiers who relocate to a new neighborhood and experience better labor market and educational opportunities as a result.[26] I explore this possibility by examining effects on characteristics of neighborhoods based on census block and postal-code averages.[27] As shown in Figure V, the intervention actually leads winners to live, on average, in administrative wards with 0.34 standard deviation lower rates of literacy and 0.33 standard deviation lower rates of full-time employment than non-winners. The lottery also causes households to live in postal codes with a lower percentage of senior secondary

[26] Appendix Table F.I presents predictors of moving. Across all models, those who relocate are less likely to be SC/ST or *Marathas*, and more likely to have had impermanent floors at the time of lottery application, and more likely to be from the same ward as the lottery apartment.

[27] Ward-level data were taken from the 2011 Indian Census. Postal-code level data for 2017 were provided by the Department of School Education and Literacy, Ministry of Human Resource Development, Government of India. Find more information at http://schoolreportcards.in/SRC-New/.

Table VII: Regressions of individual part-time employment on the treatment indicator.

	Dependent variable:					
	Employed (part-time)					
	(1)	(2)	(3)	(4)	(5)	(6)
T	−0.021	−0.024	−0.025	−0.020	−0.021	−0.021
	(0.012)	(0.011)	(0.012)	(0.013)	(0.013)	(0.027)
$Turned_6$[1]	0.038	0.091				
	(0.036)	(0.045)				
$Turned_{16}$	0.027		0.093			
	(0.032)		(0.043)			
$Turned_{18}$	−0.039			0.063		
	(0.030)			(0.039)		
$Turned_{21}$	−0.091				−0.008	
	(0.029)				(0.028)	
Older[2]	−0.116					−0.098
	(0.022)					(0.022)
TX$Turned_6$		0.084				
		(0.071)				
TX$Turned_{16}$			0.017			
			(0.055)			
TX$Turned_{18}$				−0.036		
				(0.049)		
TX$Turned_{21}$					−0.010	
					(0.040)	
TXOlder						−0.0003
						(0.028)
Constant	0.172	0.082	0.082	0.083	0.087	0.155
	(0.022)	(0.009)	(0.009)	(0.009)	(0.009)	(0.020)
Observations	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.096	0.070	0.068	0.061	0.059	0.086
Adjusted R^2	0.055	0.028	0.026	0.019	0.018	0.046

Part-time employment is defined as working fewer than five days a week. All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies.

[1] "$Turned_X$" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using $age_{\bar{l}}$, or each individual's oldest possible age.

[2] "Older" is an indicator for an individual being older than 21 at the time of the lottery.

Table VIII: Regressions of individual full-time employment on the treatment indicator.

	Dependent variable:					
	Employed (full-time)					
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.075	0.071	0.082	0.077	0.069	0.082
	(0.018)	(0.019)	(0.019)	(0.020)	(0.019)	(0.035)
$Turned_6$[1]	−0.017	−0.400				
	(0.027)	(0.032)				
$Turned_{16}$	−0.008		−0.384			
	(0.030)		(0.037)			
$Turned_{18}$	0.122			−0.168		
	(0.037)			(0.053)		
$Turned_{21}$	0.588				0.181	
	(0.036)				(0.044)	
Older[2]	0.473					0.325
	(0.021)					(0.026)
TX$Turned_6$		−0.018				
		(0.051)				
TX$Turned_{16}$			0.051			
			(0.051)			
TX$Turned_{18}$				0.049		
				(0.074)		
TX$Turned_{21}$					0.148	
					(0.062)	
TXOlder						−0.009
						(0.038)
Constant	0.083	0.479	0.477	0.466	0.445	0.231
	(0.021)	(0.013)	(0.014)	(0.014)	(0.014)	(0.024)
Observations	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.211	0.082	0.084	0.059	0.071	0.138
Adjusted R^2	0.175	0.041	0.044	0.018	0.030	0.100

Full-time employment is defined as working five or more days a week. All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies.

[1] "$Turned_X$" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using $age_{\bar{I}}$, or each individual's oldest possible age.

[2] "Older" is an indicator for an individual being older than 21 at the time of the lottery.

schools (those that offer education through grade 12), schools that are 0.22 standard deviations less likely to be taught in English (a proxy for quality), and 0.38 standard deviations less likely to have offices for headmasters (a proxy for school size). Unlike MTO, the intervention provides households with the opportunity to move to generally poorer neighborhoods. Generally, then, relocation and exposure to better educational contexts or labor markets seem to be unlikely explanations for the positive education and employment results.

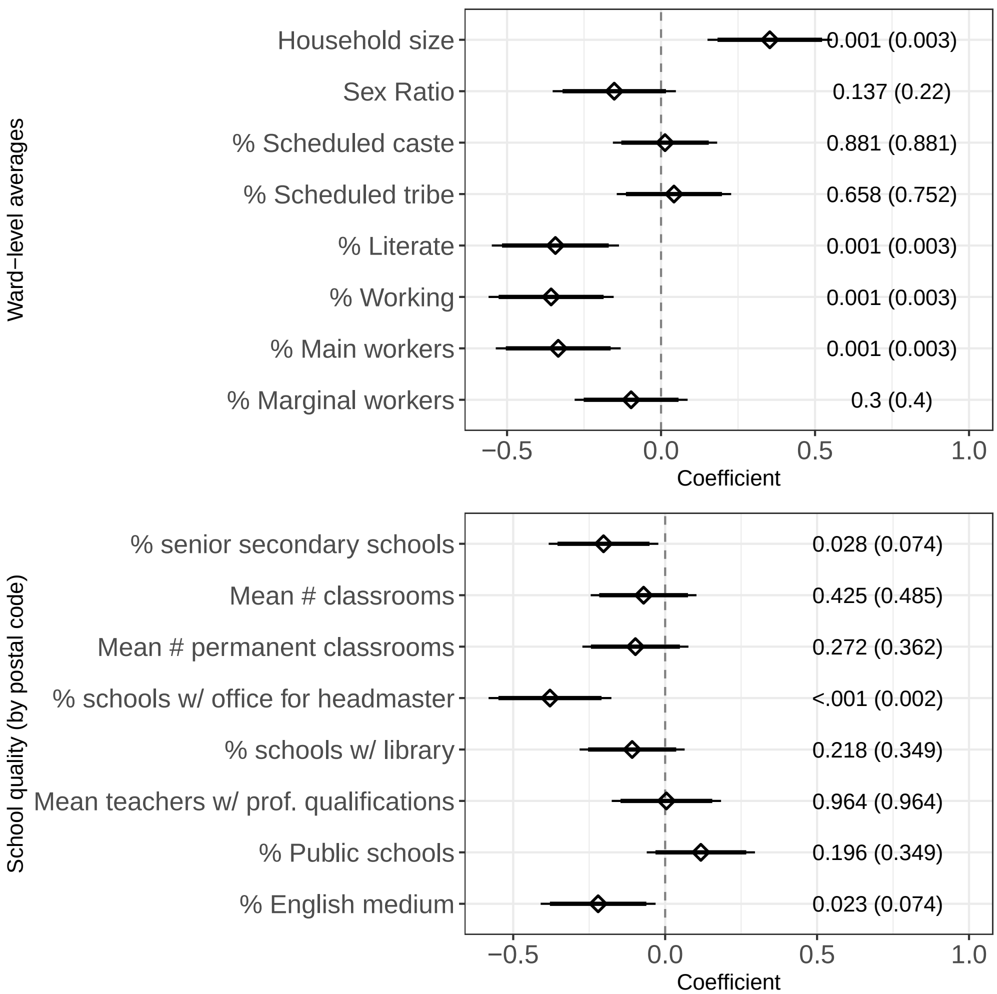
VI.B. Changes in the demand for education

There are, however, many reasons to expect that the intervention has an effect on the demand for education. One is that the wealth transfer may shift out household budget constraints. That this shift will lead to increased educational attainment seems particularly likely given the correlation between wealth or income and education in developing countries (Filmer and Pritchett 2001; Glewwe and Jacoby 2004), the effect of income transfers on educational attainment (Baird, McIntosh and Ozler 2011; Akresh, de Walque, and Kazianga 2013; Baird et al. 2014; Dahl and Lochner 2014; Benhassine et al. 2015; Aizer et al. 2016), and the idea of poverty traps in certain contexts more generally (Barham et al. 1995).

While the rule prohibiting sale prevents households from fully realizing the value of the subsidy during the time of the sale, the effect on permanent-income can still lead households to update their consumption habits in the nearer term (Friedman 1957). For landlords, this shift would be facilitated by the additional rental income; see appendix A for positive but imprecisely measured effects on reported monthly income. Also, households may be able to borrow against the equity accumulated in the home. This possibility is supported by positive effects on the likelihood of reporting that families would turn to commercial banks in the case of a financial emergency (Figure B.II). Winners report being 5 percentage points more likely to ask commercial banks for loans in cases of emergency, reflecting perhaps some ability to borrow against the home or better knowledge about financial institutions, but this effect is no longer statistically significant after accounting for multiple testing.

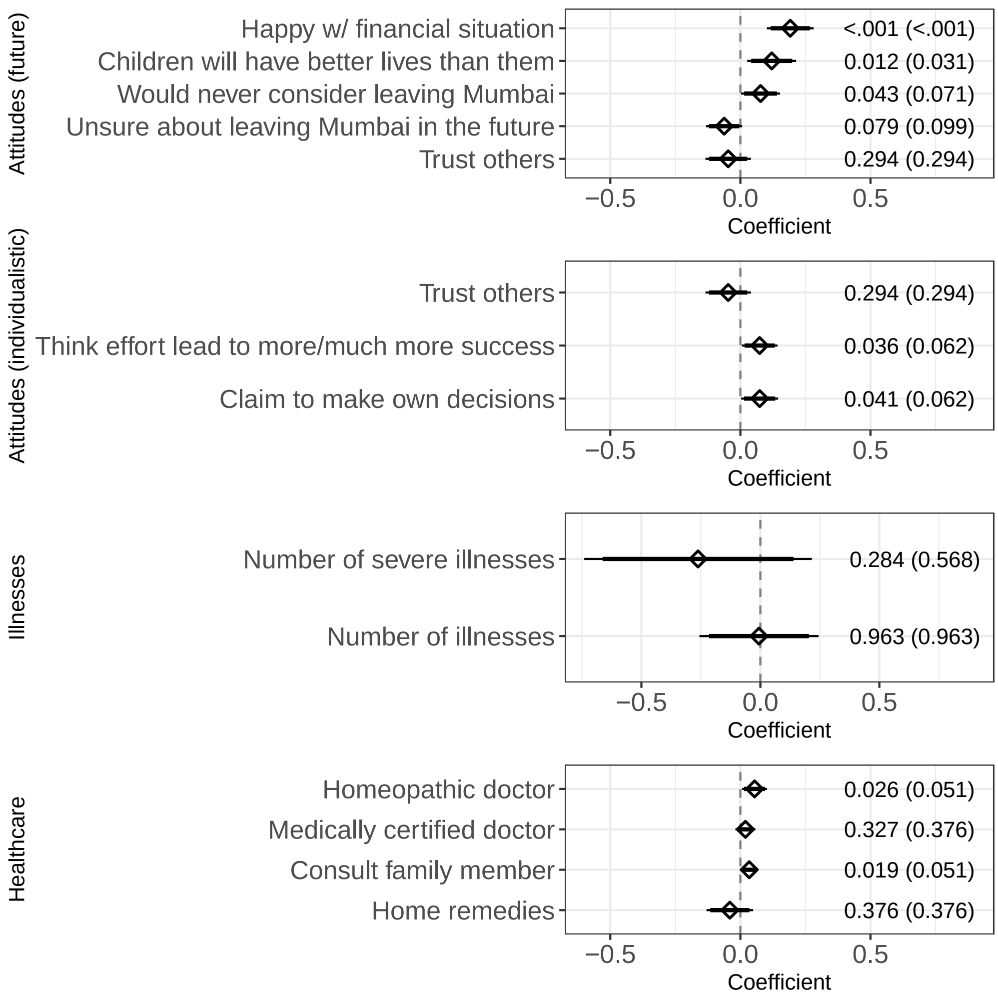
A large wealth transfer may increase the demand for education not only because of effects on income, but also because of changing underlying preferences. In particular, their attitudes about the future and time horizons may change. Not only are winners wealthier, but they can also expect the appreciation of home values and, therefore, household wealth. Winners seem to be well aware of this possibility; 91% of winning respondents are aware that the value of their properties had increased since purchase, 46% can place a value in INR on this increase, and 93.5% expect the value of the property to increase further in the future. Also, this increase in permanent income is relatively *certain*, unlike promises of pensions or cash payments, it cannot be revoked or changed by future administrations. Such a problem was encountered by beneficiaries of Mexico's conditional cash transfer program, *PROSPERA*, when many of its parameters were changed by

Figure V: Treatment effects on characteristics of wards and postal codes where households are living (in control group SDs).



Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the family-wise error rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available intables E.IV-E.VII. All ward and zip-code level variables are shown in standard deviations. Scheduled caste and scheduled tribe refer to the lowest caste group members in Indian society; members of these groups are considered to be extremely socially disadvantaged. Ward-level data were taken from the 2011 Indian Census. Postal-code level data for 2017 were provided by Department of School Education and Literacy, Ministry of Human Resource Development, Government of India. Find more information at http://schoolreportcards.in/SRC-New/.

Figure VI: Treatment effects on attitudinal and healthcare consumption outcomes.



To be "happy" with one's financial situation means to select the highest level of a 3-point scale. To believe children will have better lives than one means to say "yes" (as opposed to no) when asked "Do you expect your children to have better lives than you?" To never consider leaving Mumbai means selecting "would never leave" rather than "plan to leave in the future" or "might leave in the future" when asked if "Do you think you will leave Mumbai?" To trust others means to choose "yes" (on a three point scale) when asked "Do you think you can generally trust others?" For effort, effects are shown for whether individuals select "more" or "much more" (as opposed to "less" or "much less") when asked if they believe effort, or working hard, leads to success. For decision-making, effects are shown for whether individuals select "I make choices myself" rather than "traditional values," "neighborhood guidance", or "family guidance" when asked how they make important life decisions, with career, marriage, or education decisions given as an example. Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the false discovery rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available intables E.VIII-E.XI. Illness and healthcare outcomes refer to number of reported incidence of illnesses and binary measure of whether or not respondents refer using healthcare providers in the past month.

the new administration in 2019.

Figure VI shows effects on the household head's self-reported attitudes and beliefs about the future alongside individualistic attitudes. First, I estimate that winners are 19 percentage points more likely than non-winners to claim to be "happy" with the financial situation of the household. Winners also appear to believe they will pass on their good fortune to their children, as they are roughly 12 percentage points more likely than non-winners to say "yes" when asked if their children will have better lives than them. Finally, they are about 8 percentage points more likely than non-winners to respond that they "would never leave" when asked if would ever consider relocating from Mumbai, suggesting increased time horizons. These findings are complementary to research (e.g. Baird et al. 2013; Fernald et al. 2008; Haushofer and Fehr 2014; Haushofer and Shapiro 2016; Ozer et al. 2011; Ssewamala et al. 2009) that has found that income shocks can increase psychological well-being, happiness, and time horizons.

These results are supported by qualitative evidence from informal interviews with winners and non-winners. Non-winners did, in general, express a great deal of uncertainty about day-to-day life. "Anything can happen," said one interviewee. "Our area can flood, I could lose my job, or my mother could become ill. It may be easier to go back to our native place [village] where I have more family." In contrast, a winning interviewee said that the future of her family was "set." "We have a house in Mumbai now. There is no going back to the past life. My children can have better jobs and marriages than I could," she said.

These changes in attitudes may facilitate investment in children for a few reasons. Longer time horizons may lead to greater investment in items with longer-term payouts, such as education. Indeed, behavioral deficits, particularly present bias, have been found to explain suboptimal choices in education (Lavecchia et al. 2016). Optimism may reflect lower levels of economic or financial stress, which could also affect economic choice (Mani et al. 2013). Further evidence of this mechanism at work can be found in effects on household healthcare consumption (Figure VI). Control and treatment households experience no detectable difference in the incidence of illnesses or severe illnesses in the month prior to the survey. Nevertheless, treatment households are more likely to report having visited some type of healthcare provider in the past month, particularly family members and non-medically certified individuals such as homeopathic doctors that are common throughout India (Das and Hammer 2014). These healthcare providers are not costly, or in the case of family members, may even be free. Thus changes in this reported behavior may reflect changes in preferences rather than simply shifts in budget constraints. The overall point is that the intervention may shift both. The evidence connecting attitudes and economic choice remains weak, however, and is ripe for further investigation (Haushofer and Fehr 2014).

Finally, the intervention may increase the perceived returns to education (Jensen 2010). This could be be because as individuals become wealthier, they may derive greater utility from non-monetary gains to education that are higher on Maslow's 1943 hierarchy of needs, such as selfactualization. It could also be due to more individualistic or market-based values, which would increase the desire to invest in one's skills and future. When asked if they believe that effort leads to much more/more/less/much less success, winners are 7.3 percentage points more likely than non-winners to respond saying "more" or "much more." Also, when asked about how they make important life decisions, such as those about careers, marriages, or education, winners are 7.4 percentage points more likely to say "I make choices myself" rather than reporting taking guidance from traditional values, families, or neighborhoods. Following Di Tella et al. (2007), I attribute these effects to greater independence following the wealth shock.

The results in this study differ from those of other studies on the effects of wealth shocks on educational attainment. One reason for this could be differences in the margin at which effects are measured. Bleakley and Ferrie (2016), for example, find that winning a land lottery in Georgia, USA in 1832 did not increase the likelihood of *any* school attendance. I instead measure effects on years of education; indeed, Table V also shows that the Mumbai housing lottery has no effect on having more than 0 years of education, or beginning one's education. It is possible that among certain populations, barriers to beginning one's education are lower than barriers to continuing education after a certain point. Differences in the size and permanence of shocks may also account for divergences from studies of cash transfers (e.g. Araujo et al. 2016; Hausofer and Shapiro 2016) that find only null to moderate effects on educational attainment. The vehicle for the wealth transfer may also affect results; the land lottery studied by Bleakley and Ferrie (2016) may increase the need for household labor on the farm, thereby increasing the opportunity cost of sending one to school. Most importantly, the context and target population probably matter a great deal. Cesarini et al. (2016) find few human capital returns to a wealth shock in Sweden, but they argue that this is likely due in part to Sweden's strong social safety net, something which doesn't exist in urban Mumbai. Also, the returns to schooling vary greatly across time and space; this is demonstrated clearly by the large literature attempting to estimate these returns in different contexts (Psacharpoulos 1994; Psacharpoulous and Patrinos 2004).

VI.C. Effects on employment

I also observe an increase in full-time employment among precisely the same group of individuals exhibiting gains in educational attainment, namely older youth. If the gains in education are causing the effects on employment, then it would appear that increases in *post-secondary* education are affecting employment outcomes. These findings are somewhat surprising given the stylized fact that youth unemployment in India is highest among post-graduates.[28]

But the relationship between educational attainment and employment is one that will vary greatly across context and has yet to be fully explored in urban India, let alone Mumbai. Im-

[28]https://www.businessinsider.in/indias-unemployment-rate-stands-at-13-2-among-graduates-and-post-graduates-cmie/articleshow/68517075.cms

portantly, this study was conducted from mid-2017 to early 2018, a period which saw a spike in unemployment rates among urban youth, particularly in the informal sector.[29] This spike has been attributed by many to the effects of a new national goods and services tax and a surprise "demonetization" initiative, which effectively cancelled a large portion of the national currency literally overnight. If the low returns to post-graduate education are due to the large size of India's informal labor market, returns may have been higher during this period that was particularly difficult for small and informal businesses.

Again, these results diverge from those of other studies on the effects of wealth shocks on employment and labor supply. A rental housing program studied by van Dijk (2019) finds that beneficiaries had worse labor outcomes than non-beneficiaries, an outcome attributed to distance from markets among those who relocate. The intervention here, however, does not force relocation. More significantly, these results diverge from those of studies finding that unearned income decreases labor supply in the United States (Imbens, Rubin, and Sacerdote 2001) and Sweden (Cesarini 2017). As with the education results, the context studied here differs substantially from that of these studies. It is possible that due to competition in the labor market, there are skills-based constraints here to being hired; higher levels of educational attainment among winners may reflect a rational response to these constraints and subsequently be responsible for higher levels of employment.

VII. A transfer to the middle class

Overall, this study finds modest educational and employment effects of a substantial housing subsidy for middle class households. How might we think about the size of these effects relative to other interventions? One benchmark is provided by Baird, McIntosh, and Özler (2019), who find that a conditional cash transfer program that providing $10 a month for two years to adolescent women increased school attainment by over than 0.6 years (over a base of 7 years) and the likelihood of completing primary school by 8 percentage points (over a base of 37%). These effect sizes are comparable to the effects on education reported in the present study at a lower cost.

One reason for modest effects of a large intervention may be issues with targeting. The program studied here is, essentially, a large transfer to fairly well-off families. It is possible that this study's sampling strategy of interviewing only applicants with coherent addresses simply drops poor winners, but it appears that the program reaches wealthier citizens *by design*. Its income thresholds and requirement that winners pay a 15 year mortgage certainly keep it out of the reach of a city's poorest residents. As a result, it is unlikely that these households are credit constrained.

These parameters are not unique to the program in Mumbai, as most similar programs in

[29] https://www.bbc.com/news/world-asia-india-47068223

other cities entail mortgages and similar income categories. A mortgage subsidy similarly favors the wealthy (Glaeser and Shapiro 2003).[30] While it is beyond the scope of this paper to fully understand why these programs are targeting in this way, it seems possible that not only are effects small among this population, but that large transfers to the middle class in the form of housing subsidies actually exacerbate inequalities within a city. It is essential for future studies to measure both effects of similar programs on the poorer groups of applicants dropped from this study and the effects of different policy configurations that may more effectively target the poor.

VIII. Conclusion

In this paper, I propose that the main function of a subsidized housing program in Mumbai, India is the transfer of wealth to eligible middle-class households. Through a survey of winners and non-winners of multiple housing lotteries that occurred in 2012 and 2014, I find this wealth transfer increases educational attainment and employment rates, particularly among youth. Winners also possess both more optimistic and individualistic attitudes, which could be partially responsible for human capital investment and also suggest the possibility of longer-term effects. These effects occur even though winners tend to live in areas with lower levels of employment and worse schools, and are accompanied by changes in winners' attitudes about the future.

This is a short-term study. I find effects only on older youth, presumably because others are too young to display effects on educational attainment and employment outcomes. It is also too soon to measure effects on the children of youth themselves. As a result, a long-run study of this program will be essential to understanding the full potential of this program to change family trajectories.

The program evaluated is part of a larger set of policy instruments that subsidize the price of homes. Because homes are large assets, can appreciate substantially in value in rapidly growing urban areas, and tend to be purchased by all types of families everywhere, understanding the effects of subsidizing homeownership is important to identifying important sources of human capital accumulation. These effects on human capital accumulation have implications not only for families, but also for countries and time-periods witnessing large initiatives to promote homeownership. Given the fact that households must be able to purchase the unsubsidized portion of the apartment, however, the intervention may tend to benefit middle- or middle-class households over their poorer counterparts. This feature of the program along with its positive effects may exacerbate inequalities in a setting.

[30] Moreover, cities such as Mumbai actually have separate relocation and rehabilitation programs for poor households living in slums. These programs often require relocation; as discussed by Barnhardt *et al.* 2017 and van Dijk 2019, mandatory relocation may have adverse effects on social networks and labor market access that could offset other benefits.

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