Transferring wealth: the human capital effects of subsidizing homeownership*

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Abstract

Governments often subsidize homeownership, thereby transferring wealth to households. When studying subsidized housing lotteries in Mumbai, I find that beneficiaries reinvest this wealth in human capital. About 3-5 years after the lotteries, winners are more educated and more likely to be employed than non-winners, with large effects concentrated among school-age youth. Effects are unlikely to be driven solely by relocation to areas with better opportunities, as at the time of the survey, winners live in neighborhoods with worse schools, lower rates of literacy, and lower employment rates than non-winners. The results highlight how these illiquid government wealth transfers can shape beneficiaries' long-term economic trajectories in a short time.

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

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1 Introduction

How does subsidizing homeownership affect household economic trajectories? Governments in many countries use a variety of tools to make homeownership more affordable for citizens, including mortgage and home-price subsidies. These subsidies are wealth transfers to beneficiaries experienced in any combination of three payout structures: 1) through a stream of in-kind benefits for those who choose to live in the subsidized home; 2) through cash benefits among those who choose to rent it out; 3) or lump-sum through resale of the home. Aside from transferring wealth directly, these interventions also help families purchase an asset that forms the cornerstone of wealth accumulation for many. 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 (Oliver and Shapiro 2013).

Do beneficiaries further invest their newfound wealth in their children's education? Investment in this important determinant of human capital allows families to pass these fortunes onto the next generation. Bleakley and Ferrie (2016), for example, find that winners of a plot of land in Antebellum Georgia did not invest more in their children, and beneficiaries' descendants therefore did not have measurably different economic outcomes than those of non-beneficiaries. A substantial literature has attempted to estimate human capital effects of home subsidies 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 many studies face the problem that those who select into homeownership may differ in many ways from those who do not.[1] Experimental studies from low- and middle-income countries focus on housing or rental subsidies that require relocation to receive the transfer, which means that the location of housing can drive effects (Brandhardt et al. 2017; van Dijk 2019; Franklin 2019; Picarelli 2019).[2] Barnhardt et al. (2017), van Dijk (2019), and Picarelli (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.

Many home subsidy programs, however, do not require relocation. I measure the

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

[2]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."

effects of one policy configuration common in India, the subsidized sale of government-constructed homes to middle-class households. These programs are found in every major city in India, including Delhi, Mumbai, Bengaluru, Kolkata, Chennai, Hyderabad, Ahmedabad, and are frequently offered in some form across smaller cities as well. I study a program in Mumbai, a city of over 20 million residents, which allocates the subsidized housing through a randomized lottery system.

Sale prices of similar homes suggest that a lower bound on the subsidy that beneficiaries might ultimately realize ranges between 10,000-45,000 USD, an order of magnitude greater than the largest transfers provided in studies of unconditional cash transfers (e.g. Haushofer and Shapiro 2016). The subsidy is somewhat illiquid, however, because beneficiaries cannot resell the homes for ten years. They are, however, legally permitted to put them up for rent, and those who do earn, on average, about 50 USD per month when mortgage is subtracted from the rental income. Because the sale price of the homes covers construction and marketing costs, these programs incur few direct costs on implementing governments. While the opportunity cost of building homes on urban land is high, land-use laws might prohibit the use of the land for more lucrative purposes in any case.

From September 2017 to May 2018, I surveyed 834 total households of winners and non-winners of multiple lotteries that took place in 2012 and 2014. On average, individuals in winning households have over a half year more education than those living in non-winning households, over a base of 10 years. The treatment effect reflects an increase in winners' likelihood of completing secondary and post-secondary education.

The full sample effect reflects much larger effects among school-age children, or youth. Among household members who turned 16 after the lottery, the intervention increases the likelihood of beneficiaries 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.

The intervention further increases levels of employment among individuals by 4.2 percentage points. Subgroups among which I observe large education gains also have better employment outcomes, suggesting that education gains drive employment gains. The effect size is 19.9 percentage points for youth who turned 21 after the intervention, or those who are old enough to have had completed their education in between the lottery and being surveyed. The overall employment effects represent a larger 7.5 percentage point increase in full-time labor offset by a *decrease* in rates of part-time employment.

I explore four possible mechanisms for these effects, including relocation, shifts in budget constraints, changes in attitudes about the future, and changes in the perceived returns to education. Relocation to areas with better education and employment opportunities 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. It is more likely that the intervention alters behavior through changes to household budget constraints. I also 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.

Existing studies of cash transfer programs provide benchmarks for the effect sizes. Araujo, Bosch, and Shady (2016) conduct a 10-year follow up of a cash transfer program in Ecuador (*Bono de Desarollo Humano*, or BDH) providing households with children between 7-50 USD a month. When restricting the analysis to children living in households that began receiving transfers when they were young adults, they find that the receipt of transfers increased secondary school completion rates by 1-2 percentage points, over a base of 75%. There are no measurable effects on employment. Even while the MHADA lottery provides a much larger wealth transfer in the long-run, the intervention generates a similar monthly income transfer for about half of beneficiaries. Nevertheless, effect sizes are much larger than for BDH, as the treatment increases secondary school completion by about 5-6 percentage points over a control group mean of roughly 32% in the full sample, and 14.5 percentage points over a control group mean of 38.7% in the appropriate age cohort.

Effect sizes on education are slightly larger and more comparable in the case of conditional cash transfers (CCTs). Baez and Camacho (2011) and Alam (2011)examine the long-run effects of CCTs in Colombia and Pakistan that respectively provide between 7-20 USD a month to households with children enrolled in school. They all find that the interventions increase secondary school completion by 4-6 percentage points. Neither reports effects on post-schooling employment outcomes. Parker and Vogl (2018) find much larger effects in a long-run study of Mexico's *Progresa* CCT; a program providing 9.50-60 USD a month increased completion rates by 10-15 percentage points among those exposed to the program when young. While men in the control group are anyway employed at high rates, the intervention increases employment among women by 7-11 percentage points. In 3-5 years, then, the MHADA lottery increases high school completion and employment at rates similar to a program that a) explicitly incentivizes schooling and b) to which children were exposed for most of their schooling, rather than near the end of primary school or at the beginning of secondary school. It further increases college completion, an effect unseen (but measured) in the case of *Progresa*.

The results on education and employment, moreover, do not provide a full picture of the welfare gains the MHADA program generates. McIntosh and Zeitlin (2018) suggest that general transfers may have effects on the same outcomes as those targeted by more restrictive and tailored programs, such as CCTs, but at lower rates because households are spending in other areas as well. The relative flexibility in the use of MHADA benefits suggests that the program improves other aspects of household welfare as well, beyond the human capital gains measured here.

Comparisons to studies of other types of wealth transfers may be more appropriate. 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; the MHADA program has no effect on 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. 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, which does not exist in urban Mumbai. The results also 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. 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. This study thus introduces a new context and transfer type to the relatively sparse literature on the effects of wealth and asset transfers.

This paper is among the first to study the effects of a common policy delivering large transfers that may fundamentally change the economic 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 essential to understanding the growth of inequality.

2 The program

Across India, 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 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. The private sector has been unable to meet the resulting growth in housing demand because 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.

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) 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. Members of the EWS earn up to 3,200 USD/year. Members of the LIG earn up to 7,400 USD/year. Winners have access to loans from a state-owned bank, and most take out 15-year mortgages. Interests rates range between 10 and 15%. 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 1.

Housing was constructed on land obtained 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). 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 1 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.[3]

The lottery homes were sold at a "fair price" that government officials claim covers the costs of construction and marketing. Table 1 shows that these prices were small fractions of the market values of the homes; 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 USD at 2017 conversion rates) to Rs. 2,869,015 (about 45,000 USD). These prices do not account for untaxed informal payments made above the list price, and are thus a lower bound on the potential value of the lottery homes.

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

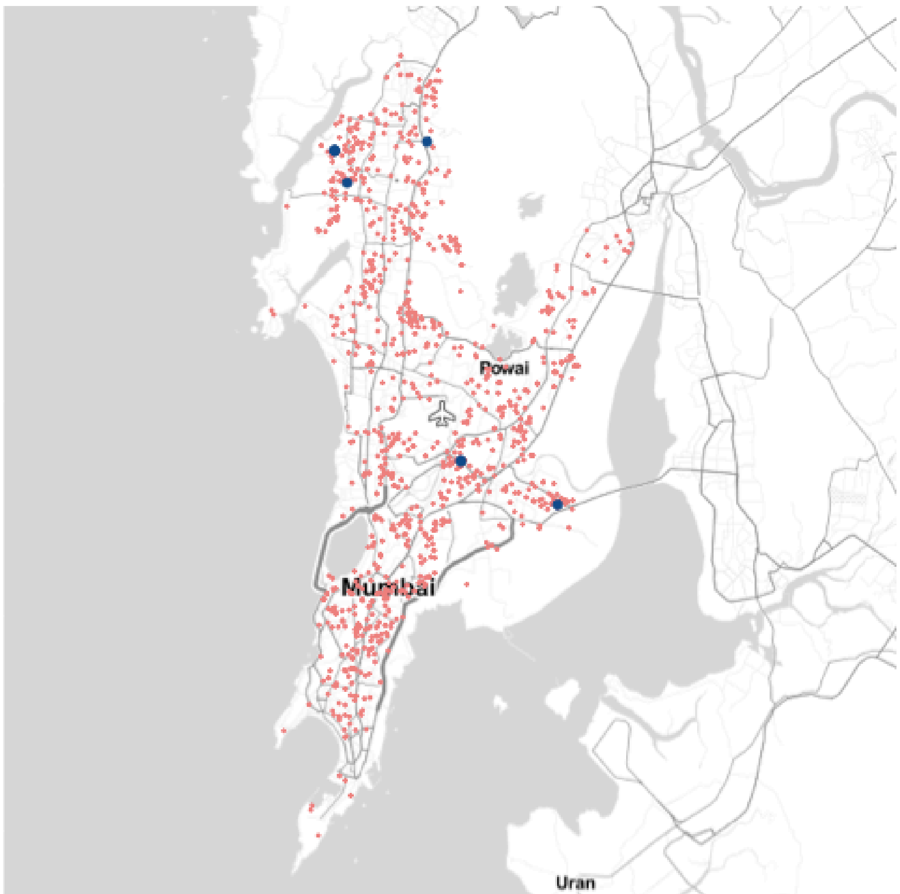
[3]The centrality of program housing will likely vary across cities. Nevertheless, development authorities in other major cities, such as Delhi and Bangalore, offer housing areas that are similarly somewhat central such as Vasant Kunj (Delhi) and Koramangala (Bangalore).

Table 1: Lotteries included in the sample

Lottery ID	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 USD.
[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 1: 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



active in each 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, other apartment members have reportedly contacted MHADA if they suspected an attempted sale because they believed 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.[4] Half of households in the study have made this choice, and the median monthly rental income *net of mortgage payments* is Rs. 3000, or roughly 50 USD. The legality and prevalence of rental make this program fundamentally different from those in which rental is not permitted (Picarelli 2019) or extremely uncommon (Franklin 2019). Finally, households do not pay taxes on their dwelling for five years after possession.

In the long term, these transfers are large. In a study of the long-run effects of cash transfers, Haushofer and Shapiro (2018) give households about 1,000 USD in seven monthly installments as part of their "large transfer" treatment arm. The wealth gains from the MHADA lottery are at least 10 to 45 times the magnitude of these transfers. The key difference is that the rule preventing resale leaves these gains to be illiquid in the short term. Landlords, or those renting out the homes, do of course see an increase of about 50 USD in monthly income.

Because the apartments are sold at cost, the large transfer incurs minimal direct costs for implementing governments across 15 years, or the time period across which loans are typically repaid. The main cost for governments is the opportunity cost of selling or using the valuable urban land for another purpose. Given the restrictions on land usage, these opportunity costs may not actually exist for decision-makers in government because they simply cannot use the land for another purpose in a given time period.

3 Research design and data collection

Beneficiaries were selected through a lottery process. Aside from evidence provided by the balance checks below, there are several reasons to believe that the the allotment

[4]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.

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.[5] Applicants also applied with their Permanent Account Numbers (PAN), which are linked to their bank accounts.[6] 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.[7]

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.[8] All winners from the relevant year were included in the sampling frame. The winning sample was stratified by caste and occupation group (Table A.3), as each lottery had quotas for these groups within which random selection occurred. Note that applicants are able to apply for multiple lotteries within the same year, but only once per lottery.

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 stratum. Recall that a stratum consists of a caste/occupation group within each lottery. In this way, both the sample of winners and non-winners were random draws 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

[5] 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.

[6] 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.

[7] 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.

[8] This paper uses the same research design and data as Kumar 2019.

than once, likely reflecting the fact that being sampled from the pool of applicants is an extremely rare event.[9]

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 from the sample. This left 531 and 532 control and treatment households, respectively. Table A.4 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 A.5 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.[10] 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. [11]

From the mapped sample, I randomly selected 500 households from each treatment

[9] 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.

[10] A scheduled tribe member is part of an officially designated group of socially and economically disadvantaged people in India.

[11] 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 stratum 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.

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 were initially approached at either the old addresses or new lottery buildings, based on whether or not lottery apartment "society" chairmen reported that they were renting out their units.[13] 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. 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

[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.

[13] A society here can be thought of as a homeowners' association.

3.1 The sample

Table 2: 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**	-

The data collection process yielded a sample of 834, with 413 (82.6% contact rate) of the surveyed households in the control condition and 421 (84.2% contact rate) 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 2. 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 3. 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. 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 its members are therefore particularly likely to call in a favor and "win" the lottery. 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 A.6) and calculation of a heteroscedasticity-robust Wald statistic for the hypothesis that all the coefficients on the covariates (other than stratum dummies) are zero. The p-value for this test is 0.39.[15]

[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.

Table 3: Balance tests on household and individual characteristics as measured through a survey.

Variable	Control[1]	Treatment[2]	sd[3]	Pr(>|t|)
A: Household characteristics N=834				
OBC[4]	0.150	-0.021	0.035	0.543
SC/ST[5]	0.080	-0.018	0.026	0.499
Maratha[6]	0.290	0.018	0.045	0.690
Muslim	0.090	0.090	0.029	0.852
Kutcha floor[7]	0.031	0.028	0.019	0.136
Kutcha roof[7]	0.039	0.001	0.018	0.945
Originally from Mumbai	0.810	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	36.000	0.095	0.574	0.869
Female	0.500	0.000	0.011	0.998
OBC	0.150	-0.022	0.023	0.340
SC/ST	0.110	-0.029	0.021	0.165
Maratha	0.270	0.024	0.032	0.457
Muslim	0.089	0.015	0.021	0.477
Kutcha floor	0.013	0.030	0.023	0.188
Kutcha roof	0.026	0.001	0.023	0.979
Originally from Mumbai	0.770	0.051	0.026	0.052
From the same ward as the apartment	0.095	0.030	0.021	0.154

[1] Intercept in an OLS regression of a each variable on an indicator for winning the lottery. Each regression includes an interaction with the centered stratum-level indicator for randomization groups.
[2] Coefficient on variable in an OLS regression of a each variable on an indicator for winning the lottery.
[3] All regressions include HC2 errors, with errors clustered at the household level for individual results.
[4] Other backward class caste group members.
[5] Scheduled Caste or Scheduled Tribe groups, also known as Dalits, members of the lowest ranks of the caste system.
[6] A dominant group in Mumbai and Maharashtra more generally.
[7] "*Kutcha*" means "rough" or "impermanent." Variable measured at time of application through recall.

EWS and LIG group membership is defined by 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.[16] Furthermore, with about 10 years of education on average, 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. About 30% of control group 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.[17] 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.[18] I thus describe the sample as middle-class. 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 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, who 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.

4 Estimation

I follow my pre-analysis plan[19] and estimate the treatment effect β, on i households or individuals across the pooled sample of lotteries. In the following equation, Y_i is the outcome (as measured through a survey), T_i is an indicator for treatment (winning the lottery), and ϵ_i is an error term.[20] Given that randomization happened within strata, I include a set of centered dummies, $S_1 ... S_l$ for each. Following Lin (2013), I allow for heterogeneous effects within the strata by centering the stratum dummies and interacting

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

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

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

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

[20] In line with the pre-analysis plan, results with covariate adjustment including a group of fixed (or pre-treatment) covariates used for randomization inference are provided in appendix.

them with the treatment indicator:

$$Y_i = \alpha + \beta T_i + \sum_{1}^{l} \omega_l S_l + \sum_{1}^{l} \eta_l (T \times S_l) + \epsilon_i \tag{1}$$

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 non-compliance (8% of treated units did not purchase homes), I simply conduct an intent-to-treat (ITT) analysis.[21] β can thus be interpreted as a weighted average of stratum-specific intent-to-treat effects. Given that randomization occurred at the household level within strata, I following Imbens and Kolesar (2015), I compute standard errors using a heteroskedasticity-robust estimator (HC2) for standard errors (MacKinnon and White 1985). Also, I make Benjamini-Hochberg corrections for the false discovery rate within "families" of outcomes.

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 child-bearing.[22] Regressions here include stratum-centered dummies, covariates, and errors clustered at the household level.

Again, note that this paper estimates average treatment effects across the different types of payout structures chosen. The control group's counterfactual choice remain unknown.[23] 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 A.12. The study is not powered to detect heterogeneous effects at the household level.

5 Results

Table 4, Panels A-B, present results for education and employment related variables measured at the individual and household levels. Household-level employment effects

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

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

[23]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.

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; families with no children take on a value of "0". I find that positive effects on education and employment are particularly large among older youth. I also find evidence to suggest that gains in employment arise from a higher likelihood of having full-time or even salaried work.

5.1 Education

First, I estimate that the average number of years of education among winners is about 0.60 years greater than the mean of 10 years for non-winners. At what margin does this effect 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 2). 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 national or state board exams (depending on their school's affiliation) at the end of grade 10. Only if they pass this exam can students advance past grade 10. Those who pass also receive a Secondary School Certificate, which is in itself a certification that is often required for 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.

Winning the housing lottery increases the likelihood of overcoming each of these barriers (Table 5).[25] Belonging to a household that has won the lottery increases the likelihood of moving past grades 10 and 12 and completing post-secondary education by 7.1 percentage points (14%), 5.6 percentage points (17.6%), and 4.1 percentage points (15.9%), respectively. It does not have an effect on actually beginning one's education.

Effects are larger among youth subgroups. Figure 3 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. The three panels for secondary and post-secondary school age children show a rightward shift in the distribution for educational attainment. In a regression analysis, I include an interaction with the treatment

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

[25] This analysis was not preregistered and can be considered exploratory.

Table 4: Treatment effects for main outcomes and outcomes related to mechanisms.

	Variable[1]	Control[2]	Treatment[3]	sd[4]	Adjusted p[5]
Main outcomes	**A: Individual-level education and employment[8]**				
	Years of education	10.000	0.610	0.230	0.018
	Working	0.460	0.044	0.026	0.120
	Working full-time	0.480	0.077	0.026	0.012
	Working part-time	0.092	-0.021	0.014	0.120
	B: HH-level education and employment[9]				
	Public school (sons)	0.095	-0.086	0.020	0.000
	Public schools (daughters)	0.088	-0.089	0.018	0.000
	English medium school (sons)	0.280	0.022	0.046	0.700
	English medium school (daughters)	0.270	0.009	0.045	0.840
	After-school tuition (sons)	0.220	-0.037	0.039	0.520
	After-school tuition (daughters)	0.220	-0.031	0.040	0.560
	Main earner salaried	0.780	0.079	0.039	0.130
	Main earner govt. job	0.180	0.038	0.039	0.520
	Main earner formal sector job	0.096	0.053	0.034	0.260
Outcomes related to proposed mechanisms	**C: Ward level neighborhood characteristics (control group SDs)[6,9]**				
	HH size	22.000	0.350	0.100	0.002
	Sex ratio	22.000	-0.150	0.100	0.220
	%Scheduled caste	2.200	0.013	0.086	0.880
	%Scheduled tribe	3.500	0.042	0.095	0.750
	%Literate	30.000	-0.340	0.100	0.002
	%Working	21.000	-0.360	0.100	0.002
	%Main workers	19.000	-0.330	0.100	0.002
	%Marginal workers	6.400	-0.097	0.094	0.400
	D: Postal code level school characteristics (control group SDs)[7,9]				
	%Senior secondary schools	1.600	-0.200	0.092	0.075
	%public schools	2.300	0.120	0.091	0.350
	Mean # classrooms	3.800	-0.071	0.089	0.490
	Mean # permanent classrooms	3.800	-0.071	0.089	0.490
	% schools w/ office for headmaster	36.000	-0.380	0.100	0.000
	% schools with library	55.000	-0.110	0.088	0.350
	Mean # teachers w/ prof qualifications	3.300	0.004	0.092	0.960
	%English medium	3.100	-0.220	0.096	0.075
	E: Future-looking attitudes[9]				
	Happy w/ financial situation	0.600	0.200	0.046	0.000
	Children will have better lives than them	0.560	0.120	0.048	0.022
	Would never leave Mumbai	0.770	0.087	0.039	0.032
	Unsure about leaving Mumbai	0.180	-0.073	0.036	0.042
	F: Individualistic attitudes[9]				
	Trusts others	0.740	-0.054	0.045	0.230
	Thinks effort leads to greater success	0.810	0.072	0.035	0.096
	Claims to make own decisions	0.130	0.067	0.036	0.096
	G: Healthcare[9]				
	N Illnesses in the last month	0.730	0.006	0.250	0.980
	Homeopathic doctor	0.036	0.052	0.024	0.064
	Medically certified doctor	0.950	0.015	0.020	0.570
	Homeopathic doctor	0.004	0.037	0.014	0.044

[1] All variable definitions available in appendix.
[2] Intercept in an OLS regression of outcome on treatment indicator and an interaction with the treatment indicator and centered stratum-level indicator for randomization groups on each outcome variable.
[3] Coefficient on treatment indicator in an OLS regression of out come on treatment indicator and an interaction with the treatment indicator and centered stratum-level indicator for randomization groups on each outcome variable.
[4] HC2 errors, with errors clustered at the household level for individual results.
[5] Benjamini-Hochberg adjusted p-values.
[6] Data from 2011 Indian Cenus.
[7] Postal-code level data for 2017 were provided by Department of School Education and Literacy, Ministry of Human Resource Development, Government of India.
[8] N=3,170
[9] N=834

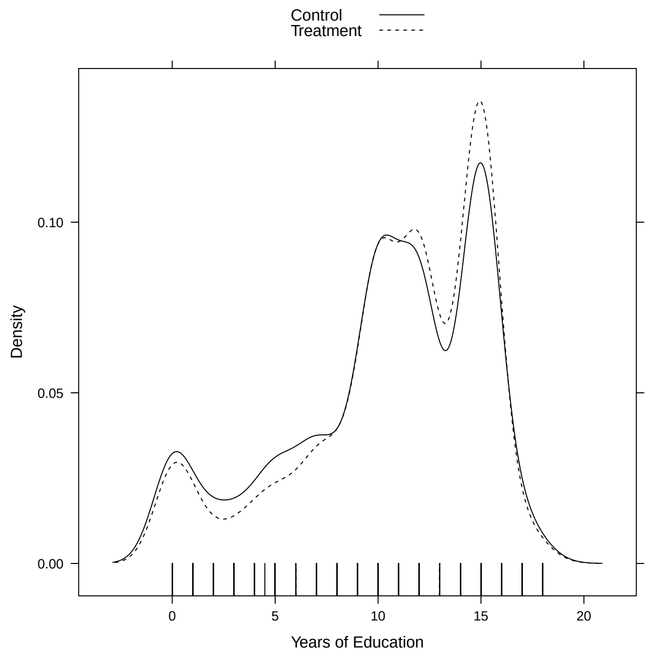
Table 5: Regressions of individual completion of various years of education on the treatment indicator.

	Dependent variable:								
	Years of education	I(>0 years)		I(>10 years)		I(>12 years)		I(≥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)
T×$Turned_6$			−0.017						
			(0.018)						
T×$Turned_{16}$					0.093				
					(0.050)				
T×$Turned_{18}$							0.106		
							(0.067)		
T×$Turned_{21}$									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 stratum 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.

Figure 2: Distribution of individual years of education for the whole sample drawn using a Gaussian kernel.

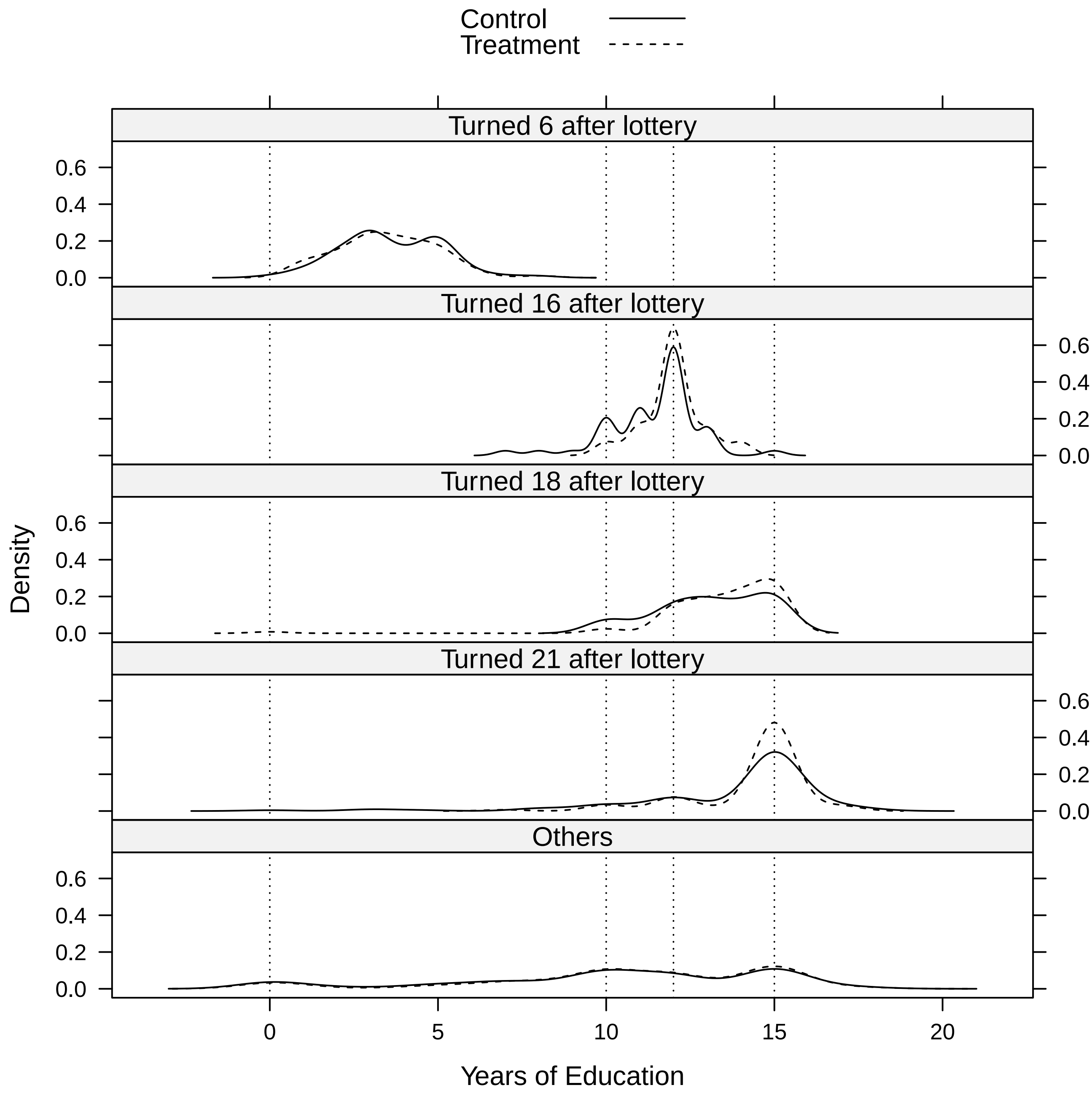


indicator and an indicator for whether each individual turned 6, 16, 18, and 21 in between being surveyed and the applicable lottery year (Table 5). These years were chosen with the assumption that most individuals complete 6, 16, 18, and 21 years of age in their first, tenth, twelfth, and fifteenth years of education. [26]

The program's effect on completing grades ten and college is stronger among those who turned 16 and 21 after winning, respectively (Table 5). I estimate a roughly 15 percentage point (45%) increase in the likelihood of completing grade 10 among members

[26] 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 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.

Figure 3: Distribution of individual years of education by cohort drawn using a Gaussian kernel. Vertical lines drawn to show 0, 10, 12, and 15 years.



of winning households who turned 16 after the lottery. I also estimate a 15 percentage point (43%) increase in the likelihood of completing post-secondary education among members of winning households who turned 21 after the lottery. Imbalance in the age distribution for the relevant cohorts cannot account for these results. Table 3 shows that winners are slightly older than non-winners. As shown in Table A.7, this difference appears to be concentrated among older individuals, but is not statistically significant for any age group.

At the household level, I estimate that parents of winners are about 8.4 percentage points (74%) 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.[27] 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 are free and tend to be of 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 sending children to after-school tuition, a common practice in India. Note also that effects do not differ for sons and daughters, but this may be due to social desirability bias in responses.

5.2 Employment

Table 4 shows that gains in educational attainment are accompanied by effects on individual employment. Individuals in winning households are 4.8 percentage points (11.5%) more likely to be employed than those living in non-winning households. Employment here means having worked one hour or more in the past week. This effect can further be broken down into a 7.5 percentage point (15.6%) positive effect on full-time work offset by a negative (but statistically insignificant) effect on part-time labor.[28] As increases in employment are concentrated among the same cohort experiencing gains in education, the fact that they are better educated may help them secure full-time jobs for which there is likely greater competition or higher skills requirements than part-

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

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

Table 6: 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[2]	0.566					0.406
	(0.013)					(0.024)
T×$Turned_6$		−0.023				
		(0.021)				
T×$Turned_{16}$			0.058			
			(0.041)			
T×$Turned_{18}$				0.065		
				(0.071)		
T×$Turned_{21}$					0.164	
					(0.068)	
T×Older						−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 stratum 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.

time labor. 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 **??**). The "main" worker is defined as the family's highest earner.

As with the gains to education, these effects on employment are particularly large among older youth. Model 1 in 6 first shows that individuals become more likely to be employed as they become older; child labor is generally uncommon in this sample. Models 2-6 further conduct an exploratory analysis to see whether effects are concentrated among among the same groups that benefitted from gains in educational attainment. 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 19.9 percentage points, or about 45%. The likelihood of full-time employment among this subgroup increases by 21.7 percentage points, or 48.8% (Table 7). 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.

6 Mechanisms behind effects on educational attainment

This section discusses possible mechanisms for the effects on educational attainment. There is little evidence to suggest that effects are driven by relocation to areas with better opportunities. I instead propose that effects on education are driven by increases in permanent income that change short-term budget constraints, intertemporal budget constraints, and preferences.

6.1 Opportunities for education and employment

The results could be driven by owner-occupiers who relocate to a new neighborhood and experience better labor market and educational opportunities as a result.[29] I explore this possibility by estimating effects on household municipal ward and postal-code characteristics.[30] As shown in Table 4, Panels C-D , the intervention actually leads winners

[29] Appendix Table A.12 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.

[30] 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,

Table 7: 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)
T×$Turned_6$		−0.018				
		(0.051)				
T×$Turned_{16}$			0.051			
			(0.051)			
T×$Turned_{18}$				0.049		
				(0.074)		
T×$Turned_{21}$					0.148	
					(0.062)	
T×Older						−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 stratum 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.

to live, on average, in municipal 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 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. Relocation and exposure to better educational contexts or labor markets seem to be unlikely explanations for the positive education and employment results.

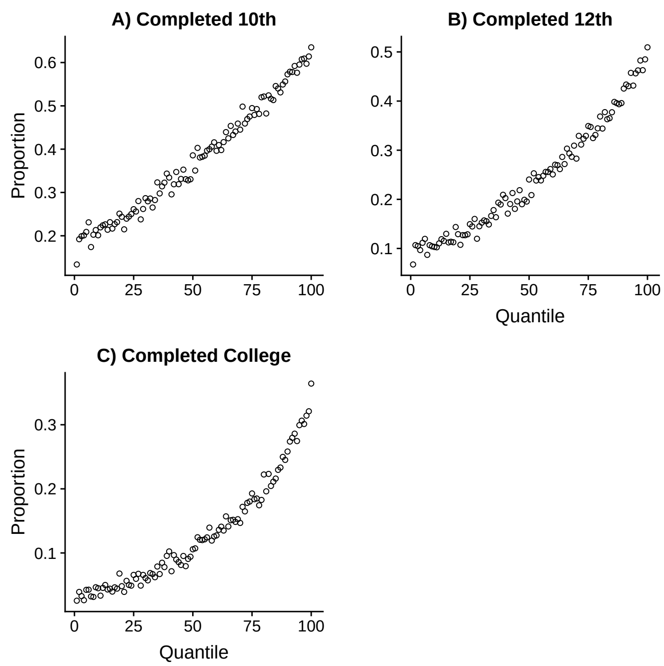
6.2 Budget constraints

The intervention might increase educational attainment by shifting out short term and inter-temporal budget constraints. The decrease in sending children to public (as opposed to private) school, for example, is evidence that households are spending more on education (Table 4, Panel B). That shifts in budget constraints 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). Figure 4 further shows a very close relationship between consumption and educational attainment in urban India.

The intervention is an illiquid increase in households' permanent income because of the rule prohibiting sale during the time of the study. Nevertheless, short-term budget constraints may shift outwards for two reasons. For landlords, this shift would be facilitated by the additional rental income; see appendix for positive but imprecisely measured effects on reported monthly income. Also, the wealth transfer's effect on long-term, permanent, income may increase short-term budgets because 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

Government of India. Find more information at http://schoolreportcards.in/SRC-New/. Education-related variables were selected to reflect those values that could be verified by surveyors working for the Government of India, rather than school self-reported values such as pass rates.

Figure 4: Mean rates of completing various years of education at different quantiles of education in India.



Data source: Indian National Sample Survey, 68th round. Urban households only. N=176,236, divided into 100 bins.

banks in the case of a financial emergency (Table A.2). 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 accumulate equity or better knowledge about financial institutions, but this effect is no longer statistically significant after accounting for multiple testing.

6.3 Attitudes and preferences

A large wealth transfer may increase educational attainment not only through effects on budget constraints, but also by changing underlying preferences. First, beneficiaries' time horizons might increase. 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.

Table 4, Panel E shows effects on the household head's self-reported attitudes and beliefs about the future. 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 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 (Table 4, Panel G). 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. Existing 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 self-actualization. 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.

7 Mechanisms behind effects on employment

I 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.[31]

But the relationship between educational attainment and employment is one that

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

will vary greatly across context and has yet to be fully explored in urban India, let alone Mumbai. Importantly, 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.[32] 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 fact that most jobs are in an informal labor market that may not require college degrees, returns to a college degree may have been higher during this period that was relatively favorable to formal businesses. This conjecture is supported by the results on full-time and salaried work.

8 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. Furthermore, several important parameters, such as the cost, subsidy size, and characteristics of the beneficiary population will vary across instances of the intervention. This variation highlights the importance of other studies determining whether the findings hold for programs implemented in different places or at different times.

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

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

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.

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