# The human capital effects of subsidized government-constructed homes in urban India*

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**Abstract**

How do widespread initiatives to subsidize government-constructed homes affect household economic trajectories? Existing research focuses on programs requiring relocation, leading the location or property rights associated with the new housing to drive effects. I measure the effects of a subsidized housing lottery in Mumbai, India in which winners can either live in or rent out the homes. After 3-5 years, effects on winners' housing quality and asset ownership are small or modest. Yet they have higher education and employment rates than non-winners, with effects concentrated among youth. Effects occur even though winners live in neighborhoods with worse schools and lower employment rates than non-winners at the time of measurement. I propose that the main mechanisms for the education effects are outward shifts in short-term budget constraints, decreased present bias, and changes in the perceived returns to education. A common policy delivering large but illiquid transfers can change important outcomes in a short time. As households must be able to purchase the unsubsidized portion of the apartment, however, the intervention tends to reach not low-income households but the middle-class, which potentially leads the program to deepen inequalities.

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

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# Introduction

Governments use a variety of tools to make homeownership more affordable for citizens, including mortgage and home-price subsidies. One particularly common policy is the subsidized sale of government constructed homes to households. Such policies exist in cities across countries including, but not limited to, India, Brazil, Uruguay, Nigeria, Kenya, Ethiopia, and South Africa. They are particularly common in India; they can be found in every major city, including Delhi, Mumbai, Bengaluru, Kolkata, Chennai, Hyderabad, Ahmedabad, and are frequently offered in some form across smaller cities as well. What are their effects on household economic trajectories?

Existing studies on the effects of such housing programs tend to focus on housing or rental subsidies that require relocation, which means that the location or property rights associated with the new housing can drive effects. Barnhardt et al., (2017), for example, find that beneficiaries of a program in Ahmedabad suffer from broken social networks arising from the distance of the housing from their neighborhoods of origin. Picarelli (2019) and van Dijk (2019) find that for programs in South Africa and the Netherlands, respectively, the distance of the housing from labor markets negatively affected household economic outcomes. As many of these programs are targeted at households living in informal settlements, other have also found that relocation to housing for which beneficiaries have secure property rights may also drive effects. Franklin (2020), for example, finds that a program in Cape Town aiming to relocate households living in informal settlements increased women's employment by alleviating the burdens that informal or poor quality housing places on households. These findings are complementary to Field's (2007) earlier work finding that property rights can improve household economic outcomes, particularly employment, by removing the burden of protecting informal rights.

This paper, in contrast, presents the effects of a program in Mumbai that does not require beneficiaries to relocate. Households are permitted to rent out the homes, and can resell them after 10 years. A lower bound on the subsidy that beneficiaries might ultimately realize ranges between 10,000-55,000 USD, depending on the apartment location. Rental income for households that do not relocate is, on average, 50 USD per month net of mortgage. Because households are able to choose whether or not to relocate, the program design prevents negative characteristics of the housing location from undermining the economic gains of the subsidy. The substantial mortgage further puts this program out of the reach of most households living in informal settlements or very poor quality housing, thereby allowing households to make the decision to move on the basis of factors other than housing quality or informality. The program finally allocates the housing through a randomized lottery system and thus allows the causal identification of its effects among applicants.

I estimate the reduced-form effects of this program on households winning homes in 2012 and 2014 using an original survey of 834 households. Even though control group households tend to live in housing with permanent (as opposed to makeshift) floors and roofing, I estimate positive effects on housing quality. I estimate no effects on durable asset ownership, suggesting that the intervention does not increase investment in material consumption.

The real gains are to education. On average, the intervention increases individual years of education by about half a year over a control group mean of 10 years. The treatment effect reflects an increase in winners' likelihood of completing secondary and post-secondary education. These full sample effects are concentrated 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 15 percentage points (pp). Among household members who turned 21 after the lottery, the intervention also increases the likelihood of completing post-secondary education by 15 pp.

The intervention further increases levels of employment among individuals by 4.4 pp over a mean employment rate of 46% in the control group. The overall employment effects represent a 7.7 pp increase in full-time labor and no measurable effect on part-time labor. The subgroups among which I observe large education gains also have better employment outcomes, suggesting that the education gains drove employment gains. The effect size is 19.5 pp 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.

These effects on human capital suggest that the program will have an effect on long-term household economic outcomes. Investment in education in particular might allow families to increase the size of these fortunes and pass them onto the next generation (Becker, 1964). In addition to being an important outcome itself, educational attainment is also a proxy for social and economic status in developing countries wherein informal labor markets and joint household production functions can make it difficult to measure individual income (Asher et al., 2020).

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 comparing households that were just eligible and ineligible for receiving transfers throughout a child's secondary schooling, they find that the receipt of transfers increased secondary school completion rates by 1-2 pp, over a base of 75%. There are no measurable effects on employment. Even while the housing lottery provides a much larger wealth transfer in the long-run, the intervention generates a present-term monthly cash transfer of a similar magnitude and across a similar time period. Nevertheless, effect sizes are much larger than for BDH. Parker and Vogl (2018) find more comparable effects in a long-run study of Mexico's *Progresa* conditional cash transfer; a program providing between 9-60 USD a month increased completion rates by 10-15 pp among those exposed to the program when young. While men in the control group were already employed at high rates, the intervention increased employment among women by 7-11 pp. In 3-5 years, then, the housing lottery increased high school completion and employment at rates similar to a program that a) explicitly incentivized 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 increased college completion, an effect unseen (but measured) in the case of *Progresa*.

Why does this intervention generate such large effects relative to the stream of benefits it provides in the short term? It is possible that the intervention affects investment in education by facilitating moves to areas with better educational and employment opportunities. Chetty et al.'s (2016) study of the United States' Moving to Opportunity (MTO) program, for example, finds many positive effects on younger children of an intervention explicitly motivated by moving households to wealthier neighborhoods. Yet this mechanism seems unlikely to explain the results of the present study, as winners on average live in neighborhoods with poorer school quality and lower rates of literacy and employment than non-winners. Predictors of moving further provide evidence to suggest that households mitigate the effects of housing location by strategically choosing whether or not to relocate to the new housing.

As poor or informal housing quality at the time of application is a predictor of relocation, it is also possible that the effects are driven by the small segment of the population that does relocate. The mechanism here would be similar to the one put forth by Franklin (2020), where an increase in housing quality and/or tenure security allows households to shift time use from protecting property rights to working outside the home. Yet treatment effects are negative among those living in informal housing at baseline, consistent with the finding that those living in poorer quality housing at baseline are more likely to relocate to the worse neighborhoods than those without. In other words, informality drives households to relocate to the new housing at the expense of educational and employment opportunities.

I provide evidence for other possible mechanisms for these effects related to the size of the long-term wealth transfer and the vehicle through which it is delivered. These include shifts in short-term budget constraints generated by rental income and an ability to borrow on accumulated equity, decreased present bias due to these shifts in budget constraints and an increase in permanent income, and changes in the perceived returns to education. I see 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. This finding provides evidence for effects on present bias as a mechanism for investment in longer-term outcomes.

This paper makes three contributions to an emerging literature on the effects of housing policies in low- and middle-income countries. First, it is among the first studies of a housing program that does *not* require relocation. In this type of policy, beneficiaries receive a flow of in-kind transfers in the form of housing benefits or rental income. They can choose to experience the wealth transfers 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; or 3) lump-sum through the eventual resale of the home.

Second, the study joins an emerging literature on the psychology and economics of poverty (Haushofer and Fehr, 2014) demonstrating that income and wealth transfers can increase investment in human capital and employment by easing cognitive and behavioral constraints. This argument goes against conventional wisdom and evidence from other contexts suggesting that unearned income reduces the incentive to work (Imbens and Rubin, 2001).

Third, the study introduces a new context and transfer type to the relatively sparse literature on household behavioral responses to asset transfers. In contrast to existing research, the present study shows the potential for illiquid transfers to change household behavior in just a few years. The vehicle for the transfer may partly account for differences in findings. 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. Unlike housing, however, receiving rural land may increase the need for household labor on the farm, thereby increasing the opportunity cost of sending one to school. Similarly, in studies based in the Philippines and Bangladesh, Edmonds and Theoharides (2020) and Sulaiman (2015), respectively, find that productive asset grants can actually increase child labor to manage the asset in the short term. The context and target population are also important. Cesarini et al. (2016) find few human capital, health, or developmental 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 India and other low- and middle-income countries where the subsidized housing programs are particularly common.

It is important to study the effects of programs to subsidize homeownership as they are pursued by governments in wealthy, low-, and middle-income countries alike. Transfers made through housing may be particularly appealing to policymakers because the sale price of the homes covers construction and marketing costs, and these programs incur few direct costs on implementing governments. Land-use laws further limit the theoretically high opportunity cost of building subsidized homes on urban land.

But precisely because households must contribute to receive the transfer, these programs often benefit middle-class households rather than the poor. This is a general feature of transfers made through home-subsidy programs, like the home mortgage interest deduction in the United States (Glaeser and Shapiro, 2003). Studying the effects of this class of transfers is thus essential to understanding the growth of inequality.

## The program

Across India, state-level housing development boards have spearheaded programs that sell, rather than rent, subsidized units to eligible households in every major city. In 2015, India's federal government further announced a plan, Pradhan Mantri Awas Yojana ("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.

I study the effects of one such program implemented by the Mumbai Housing and Area Development Authority (MHADA). 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. Beneficiaries have access to loans from a state-owned bank, and most take out 15-year mortgages at 10-15% annual percentage rates. 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. MHADA constructs housing on land obtained from the city's dismantled textile industry. 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 are permitted to choose the building for which they submitted an application.

In 2012 and 2014, the EWS group could purchase a 269 square foot apartment for about Rs. 1,500,000 (about 23,500 USD at the time), while the LIG group could purchase a 403 square foot apartment for about Rs. 2,700,000 (about 42,000 USD). All applications required a refundable fee of Rs. 200 (about 3 USD). Table 1 shows that these prices are 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 lies between Rs. 661,700 (about 10,300 USD at 2017 conversion rates) to Rs. 3,535,500 (about 55,000 USD).[1]

[1] These prices do not account for untaxed informal payments made above the list price, and are thus a

Resale of the apartments is not permitted until 10 years after purchase, a rule enforced both by MHADA officials and homeowners' associations active in each lottery building. Households can, however, put the apartments up for rent. 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. Households do not pay taxes on their dwelling for five years after possession.

Beneficiaries are selected through a lottery process, allowing causal identification of the program's effects. In response to extreme public scrutiny over the selection process and concerns about corruption, the lottery is conducted using a protected computerized process that was implemented in 2010. Applicants also apply with their Permanent Account Numbers (PAN), which are linked to their bank accounts and allowed the verification of income thresholds.[2] The winning sample is stratified by caste and occupation group (Table SI.2), as each lottery has quotas for these groups within which random selection occurs.

# 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. All winners from the EWS and LIG lotteries occurring in 2012 or 2014 were included in the sampling frame. As there were roughly 1,000 applicants for each apartment, I surveyed a random sample of non-winning applicants. MHADA provided phone numbers and addresses for both winners and a random sample of non-winning applicants drawn in the same stratified method used for the selection of winners.

Applicants could apply for multiple lotteries at a time. Households that had applied for multiple lotteries included in the study (either within a year or across years) would have

lower bound on the potential value of the lottery homes.

[2]A PAN is equivalent to a taxpayer identification number.

a higher likelihood of appearing in either the treatment or control sample. The sampling procedure explicitly allowed for the possibility of the same household being drawn multiple times. 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 household was drawn more than once.

I accessed a total of 1,862 addresses used at the time of application to the lottery. I first mapped them using Google Maps. I dropped addresses that were incomplete (42), outside of Greater Mumbai (611), or could not be mapped (146). This left 531 and 532 control and treatment households, respectively. As I dropped households using baseline addresses, I would expect this procedure to be independent of treatment assignment. Indeed, in the sample remaining after mapping, I see similar proportions of winners and applicants in each caste/occupation category, lottery income category, and apartment building (Table SI.3). The mapping procedure did favor wealthier applicants by dropping informal settlements and all who lived outside of Greater Mumbai, limiting my sample to urban applicants. Table SI.4 shows that there are relatively fewer Scheduled Tribe members and more General Population (i.e. Forward Castes) members in the mapped sample than in the full sample provided by MHADA.[3]

From the mapped sample, I randomly selected 500 households from each treatment condition to survey. From September 2017-May 2018, I worked with a Mumbai-based organization to contact the households and conduct surveys. 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. Among winners, owner-occupiers were approached at

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

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. In the case a child had applied for the home, enumerators were instructed to speak to the household's main decision-maker. Ultimately, 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

## The sample

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. The p-value for the difference in proportion contacted is 0.8. Full information on the number of households contacted in each stratum along with reasons for attrition can be found in Table SI.5.

Balance tests for fixed or baseline characteristics among the contacted sample can be found in Table 2. Winners and non-winners are similar across a number of fixed observable covariates, limiting concerns of corruption in the lottery or differential attrition across the treatment groups. Both treatment groups have an equal proportion of those belonging to the *Maratha* caste group, a dominant group in Mumbai and Maharashtra more generally.

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. There is also balance on the date on which interviews were conducted, meaning that treatment group interviews were not conducted systematically earlier or later than control group interviews. Additional balance tests are available in Appendix E.

I describe the sample as middle-class. 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 reported in the India Human Development Survey- II (Desai and Vanneman, 2016).[4] Furthermore, with about 10 years of education on average, the sample is at about the 61st percentile for years of education in Mumbai. Most live in dwellings with permanent floors (94%) and roofs (78%). Yet there is room for improvement; only 60% have their own toilets, and 75% have their own private taps. Shared taps and toilets are common features in the Mumbai *chawls*, or cheap apartments built for laborers, where many control group members live.

## Estimation

I follow my pre-analysis plan and estimate the treatment effect $\beta$, on $i$ households or individuals across the pooled sample of lotteries (Equation 1). $Y_i$ is the outcome, $T_i$ is an indicator for treatment (winning the lottery), and $\epsilon_i$ is an error term.[5] 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 interacting the centered stratum dummies with the treatment indicator:

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

[5] Covariate adjusted results using fixed characteristics yield similar standard errors (Table SI.11).

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

I label households as "treated" if they win the lottery in the specific year for which they appear in the sample. While this study suffers from noncompliance (8% of treated units did not purchase homes), I simply conduct an intent-to-treat (ITT) analysis. $\beta$ can thus be interpreted as a weighted average of stratum-specific intent-to-treat effects. Given that randomization occurred at the household level, I compute standard errors using a heteroskedasticity-robust estimator (HC2) for standard errors (MacKinnon and White, 1985). I make Benjamini-Hochberg (1995) corrections for the false discovery rate within "families" of outcomes.

For education and employment results, I use data from a household roster to estimate individual-level treatment effects. This dataset drops all individuals born *after* the household-relevant lottery was conducted to mitigate post-treatment bias arising due to treatment effects on child-bearing.[6] Regressions here include stratum-centered dummies and errors clustered at the household level.

I estimate average treatment effects pooled across owner-occupiers and landlords because the control group members' counterfactual choices remain unknown. Predictors of moving can be found in Table 7. The study is not powered to detect heterogeneous effects at the household level.

# Results

Table 3 presents treatment effects for the main outcomes of interest along with those related to potential mechanisms. Panel A first presents results for housing quality at the time of the survey. Most control group members have permanent or load-bearing roofs (78%), private

[6] Winning the lottery has no measurable effect on the birth of new children.

taps (75%), and private toilets (60%). Nevertheless, I observe positive treatment effects for these variables, likely driven by those who relocate from poor quality housing. I observe no measurable treatment effects for durable asset ownership (Panel B) aside from a negative effect on asset ownership.

Panels C-D in Table 3 present 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; families with no children take on a value of "0". As described below, I find that positive effects on education and employment are particularly large among older youth.

## Education

First, I estimate that the mean years of education among winners is 0.61 years greater than the mean of 10 years for non-winners. At what margin do these gains 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 modes 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.[7] The mode at 10 years 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 receive a Secondary School Certificate, which is in itself a certification required for 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

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

of post-secondary school education.

Winning the housing lottery increases the likelihood of overcoming each of these barriers.[8] 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 pp (14%), 5.6 pp (17.6%), and 4.1 pp (15.9%), respectively. It does not have an effect on actually beginning one's education.

Effect sizes are actually larger among youth subgroups. I 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 (Table 4). 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.[9]

The program's effect on completing grades ten and college is larger among those who turned 16 and 21 after winning, respectively (Table 4). I estimate a roughly 15 pp (18%) increase in the likelihood of completing grade 10 among members of winning households who turned 16 after the lottery. I estimate a 15 pp (26%) increase in the likelihood of completing 15 years or more (post-secondary education) among members of winning households who turned 21 after the lottery. I observe no treatment effects on educational attainment among those who were older than 22, or school age, at the time of the lottery.

The intervention also affects school choice. At the household level, I estimate that parents of winners are about 8.6 pp (90.5%) and 8.9 (100%) less likely to report sending their sons and daughters, respectively, to public school than parents of non-winners. Here, asking if children attend a public ("government") school is a more common way to draw the distinction

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

[9] I measure age at the time of the survey, so age at the time of the lottery ($age_l$) could take on two values, $age_{\bar{l}}$ and $age_{\underline{l}}$, depending on the timing of the respondents' birthdays. For simplicity, tables in the text present results assuming all individuals were $age_{\bar{l}}$ at the time of the lottery. Individuals are coded to have turned X years old ($Turned_X$) after the lottery if $age_s$ is greater than or equal to X and $age_{\bar{l}}$ is less than X. Results using $age_{\underline{l}}$ are similar and presented in appendix F.

between public and private schools than by asking if children attend private schools. 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). Generally, 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 that effects do not differ for sons and daughters, but this may be due to social desirability bias in responses.

Imbalances across treatment and control groups cannot account for these results. First, imbalance in the age distribution for relevant cohorts does not appear to be driving results. Table 2 shows that winners are slightly older than non-winners. This difference appears to be concentrated among older individuals, but is not statistically significant for any age group (Table SI.7). Additionally, selection into the sample, namely a more educated cohort of older respondents, cannot account for results. Yet there are no measurable treatment effects on educational attainment among the older age cohort (Table 4) Furthermore, if this were indeed the case, I would expect older treatment group respondents to be less likely to have lower levels of education and more likely to have higher levels of education. Yet Table SI.8 shows that there are no treatment effects on primary education (4 or fewer years) or tertiary education (more than 12 years) among those who were older than 22 at the time of the lottery.

## Employment

Table 3 shows that gains in educational attainment are accompanied by effects on individual employment. Individuals in winning households are 4.4 pp (9.6%) 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 7.7 pp (16%)

positive effect on full-time work offset by a negative (but imprecise) effect on part-time labor. Here, full-time work is defined as working either 5 or 6 days a week. If the distinction between part-time and full-time labor is a rough proxy for wage and salaried labor, this set of results complements positive estimates of household-level effects on the main earner being salaried or having a government job (Table 3). 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. Table SI.8 shows that there are no detectable treatment effects among those older than 22 at the time of the lottery. Model 1 in Table 5 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 explore whether effects are concentrated among among the same groups that benefitted from gains in educational attainment. 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 19.5 pp, or about 32.5% (Model 6). The likelihood of full-time employment among this subgroup increases by 21.9 pp, or 34.8%. 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. 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-time labor.

# Mechanisms

I now consider multiple possible mechanisms for the effects on education presented above. I consider relocation, shocks to property rights, shifts in budget constrains, shifts in present bias, changes in the perceived returns to education, and misreporting.

## Relocation

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. Indeed, Chetty et al.'s 2016 study on the United States' MTO program finds that moving to a higher opportunity neighborhood significantly increases college attendance and earnings among children who were below 13 when they moved, suggesting that neighborhoods can play an important role in human capital accumulation. I explore this possibility by estimating effects on household municipal ward and postal-code characteristics. The intervention leads winners to live, on average, in municipal wards with lower rates of literacy and lower rates of full-time employment than non-winners (Table 3, Panels E-F). It also causes households to live in postal codes with a lower percentage of senior secondary schools (those that offer education through grade 12), schools less likely to be taught in English (a proxy for quality), and 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 thus seem to be unlikely explanations for the positive education and employment results.

Why, then, do we see results that differ so much from other studies (e.g. Barnhard et al., 2017; van Dijk,2019) that also entail apartments won in worse neighborhoods? One possible reason is that households in this intervention can choose whether or not to relocate; if the costs are too high, then they will not. A set of regressions to uncover the predictors of moving (Table 6) show that complementary to expectations generated by Barnhard et al. (2017), relocation is costly for the poor. Scheduled castes and tribes, or those typically at the bottom of the socio-economic ladder, are less likely to relocate than others. At the same time, those with makeshift roofs (a proxy for informal housing) are more likely to move; intuitively, the benefits of relocating to permanent housing is greater for this group.

In these regressions, I also include a variable, Change in Employment Rate, that measures the difference in the employment rate between a household's baseline neighborhood and the neighborhood in which their lottery apartment is located. The standardized version of the variable is included in the regressions. In line with the suggestion that the lottery apartments are in worse neighborhoods than baseline apartments on average, the mean of this variable is -0.018. Across multiple model specifications, a one standard deviation increase in the employment rate of the apartment neighborhood relative to the baseline neighborhood is associated with 17-20pp increase in the likelihood that a household will relocate. In other words, households are strategically deciding whether or not to relocate depending on their predictions about how relocating would affect their economic well-being. Note, however, that the main results are unlikely to be driven by a subsample of the households moving from worse baseline neighborhoods to better apartments neighborhoods, as there is no positive relationship between the indicator variable I(Change in Employment Rate >0), and the likelihood of relocating (Table 7).

## Property rights

Another possible mechanism is Field's (2007) argument that an increase in tenure security in the new housing allows households to shift time use from protecting property rights to working outside the home. Where older children are protecting property rights or working while someone else does the same, an increase in tenure security could allow these children to attend school. In other words, the results of my study could be driven by the 15% of the sample (Table 3, Panel A) enjoying improved housing (proxied for by the variable "makeshift roof). Tables 8-9 in suggest that this is not the case. Table 8 shows that the treatment effect on individual years of education is actually negative among those living in households with a makeshift roof at baseline, and households with a makeshift roof at baseline are less likely to have salaried main earners. These conditional average treatment effects are in line with the finding that those with makeshift roofs are more likely to relocate to the worse neighborhoods than those without. Note further that there is no effect on employment among those who were past school age at the time of the lottery (Table SI.8); if the tenure security mechanism were driving results, I would be likely to see effects on employment among older household members as well. Overall, the analysis suggests that the results on education and employment are not driven by those living in informal or impermanent housing. It also suggests that beneficiary households face important tradeoffs between household quality and employment and education prospects.

## Budget constraints

The intervention might increase educational attainment by shifting out short-term 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 3, Panel C). Haushofer and Shapiro (2018) find unconditional income transfers increase educational spending and improve educational outcomes, and there exists a close relationship between consumption and educational attainment in urban India (Figure SI.3).

Even though the wealth transfer is mostly illiquid, short-term budget constraints may shift outwards for a few reasons. Landlords receive rental income. See appendix D for positive but imprecisely measured effects on reported monthly income. Households may also be able to borrow against the equity accumulated in the home. Winners report being 5 pp more likely to ask commercial banks for loans in cases of emergency, possibly reflecting some ability to borrow against the accumulated equity or better knowledge about financial institutions, but this effect is no longer statistically significant after accounting for multiple testing.

Yet as discussed in the introduction, the effect sizes are much larger than those of cash transfers of similar sizes, suggesting that other mechanisms may also be important.

## Present bias

Both the shift in short-term budget constraints and the illiquid subsidy's impact on permanent income might decrease present bias. Past research has found that income or wealth shocks can decrease stress and therefore increase time horizons (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). Decreased present bias may lead to greater investment in items with longer-term payouts, such as education. Behavioral deficits, particularly present bias, have been found to explain suboptimal choices in education (Lavecchia et al., 2016).

Table 3, Panel H shows that the intervention increased winners' optimism about their financial futures. Optimism may reflect lower levels of economic or financial stress, which could also affect decision-making (Mani et al., 2013). Winners are 20 pp more likely than non-winners to claim to be "happy" with the financial situation of the household. Winners appear to believe they will pass on their good fortune to their children, as they are roughly 12 pp more likely than non-winners to say "yes" when asked if their children will have better lives than them. Finally, they are about 8.7 pp more likely than non-winners to respond that they "would never leave" and roughly 7.3 pp less likely to say they are "unsure" when asked if would ever consider relocating from Mumbai, indicating the intervention increased time horizons and decreased uncertainty about the future.

Evidence of decreased present-bias can also be found in effects on household healthcare consumption (Table 3, Panel H). 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 common throughout India (Das and Hammer, 2014). These healthcare providers are cheap, or in the case of family members, may even be free. Thus changes in this reported behavior may reflect changes in present bias rather than simply shifts in budget constraints.

## Returns to education

Finally, the intervention may increase the returns to education. This could be be because as individuals become wealthier, they may derive greater utility from non-monetary gains to education 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 7.2 pp more likely than non-winners to respond saying "more" or "much more." When asked about how they make important life decisions, such as those about careers, marriages, or education, winners are 6.7 pp more likely to say "I make choices myself" rather than reporting taking guidance from traditional values, families, or neighborhoods.

## Misreporting

Finally, as results are based on survey measures, it is possible that they are driven by misreporting. Winning the apartment may put household respondents in a different social class compared to non-owners, with different (perceived) reference levels of education, employment and income. This is unlikely, however, as the apartments are on average in neighborhoods with lower educational resources than those in which control group members live. Furthermore, this mechanism would increase reported education across the board, rather than just in school-age youth. Recall, however, that I do not see educational effects on older cohort members Table SI.8. It is also possible that respondents misreported to show that they were using the lottery win productively. The survey team took care to ensure that it was absolutely not represented with the lottery. Respondents were shown information about the researchers, the NGO conducting surveys, and an official letter from my institution.

### Mechanisms behind effects on employment

More information is needed to fully understand the effects on employment, which run counter to expectations that unearned income would reduce labor supply (e.g. Imbens et al., 2001; Cesarini et al., 2017). It is possible that the effects are driven not by labor supply, but one's success in finding a job. Youth unemployment is a persistent problem in urban India, with official estimates for 2017-18 hovering around 19% and 27% for men and women, respectively (Sabnavis, 2019). 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. It is possible that these results are specific to the time period of the study. 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 (Kaul, 2019). Some attribute this spike to a new national goods and services tax and a surprise “demonetization” initiative, which effectively cancelled a large portion of the national currency literally overnight. Returns to a college degree may have been higher during this period that was relatively favorable to formal businesses that did not rely as heavily on cash. This conjecture is supported by the results on full-time and salaried work.

## Conclusion

I propose that the main function of a subsidized housing program *not requiring relocation* 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 that winning an apartment increases educational attainment and employment rates, particularly among youth. 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. Overall, the study indicates that urban housing subsidies, particularly those that allow households to choose whether or not to relocate, can play an important role in human capital accumulation and intergenerational mobility.

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, highlighting the importance of future studies of other programs.

The results presented here further 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 conditional programs, 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.

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 lower 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.

# Tables and Figures

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

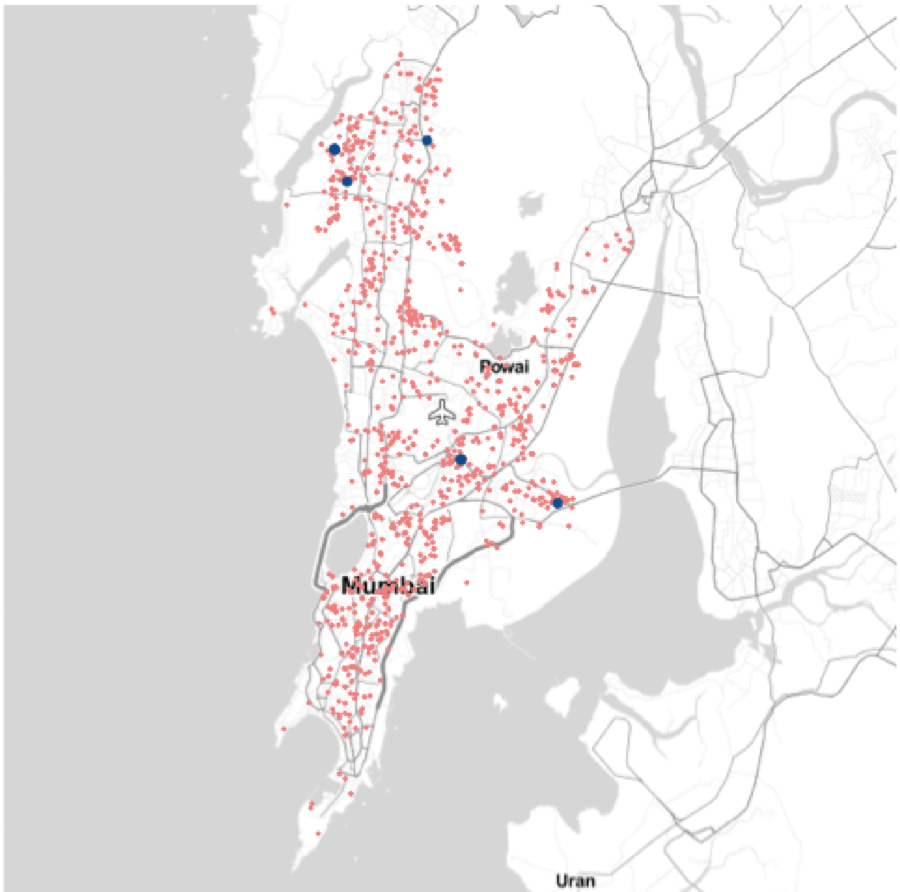

Table 1: Lotteries included in the sample

| 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.

Table 2: Balance tests on household and individual characteristics as measured through a survey.

| Variable | Control[1] | Treatment[2] | s.e.[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 |
| Makeshift floor | 0.031 | 0.028 | 0.019 | 0.136 |
| Makeshift roof | 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 |
| Date of interview[7] | 126.000 | 5.300 | 8.000 | 0.510 |
| **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[4] | 0.150 | -0.022 | 0.023 | 0.340 |
| SC/ST[5] | 0.110 | -0.029 | 0.021 | 0.165 |
| *Maratha*[6] | 0.270 | 0.024 | 0.032 | 0.457 |
| Muslim | 0.089 | 0.015 | 0.021 | 0.477 |
| Makeshift floor | 0.013 | 0.030 | 0.023 | 0.188 |
| Makeshift 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 variable on treatment indicator. Each regression includes an interaction with the centered stratum-level indicator for randomization groups. [2] Coefficient on variable in an OLS regression of each variable on treatment indicator. [3] HC2 errors, with errors clustered at the household level for individual results. [4] Other backward class caste group members.

[5] Scheduled Caste/Scheduled Tribe, a historically disadvantaged social group.

[6] A dominant group in Mumbai and Maharashtra more generally. [7] Refers to the day of the interview where the first interview was conducted on day 1.

Table 3: Treatment effects for outcomes and variables related to proposed mechanisms. N=834 unless otherwise noted.

| Variable[1] | Control[2] | Treatment effect[3] | s.e.[4] | Adjusted p[5] |
|---|---|---|---|---|
| **A: Housing quality** | | | | |
| Makeshift floor | 0.019 | 0.011 | 0.016 | 0.490 |
| Makeshift roof | 0.220 | -0.150 | 0.034 | 0.00 |
| Private tap | 0.750 | 0.130 | 0.039 | 0.001 |
| Private toilet | 0.600 | 0.250 | 0.043 | 0.000 |
| **B: Asset ownership** | | | | |
| Stand-alone closet | 0.710 | -0.098 | 0.049 | 0.210 |
| Dining table | 0.210 | -0.021 | 0.039 | 0.790 |
| Working TV | 0.910 | 0.034 | 0.026 | 0.480 |
| Working Fridge | 0.880 | 0.047 | 0.031 | 0.450 |
| Gas for cooking | 0.890 | 0.037 | 0.029 | 0.480 |
| Electricity for cooking | 0.880 | 0.008 | 0.033 | 0.940 |
| Computer | 0.380 | 0.024 | 0.049 | 0.790 |
| Internet | 0.510 | -0.110 | 0.050 | 0.200 |
| Sewing Machine | 0.130 | 0.022 | 0.035 | 0.790 |
| Mobile phone | 0.700 | -0.028 | 0.047 | 0.790 |
| Smart phone | 0.750 | 0.037 | 0.042 | 0.750 |
| Car | 0.064 | 0.001 | 0.025 | 0.980 |
| Two-wheeler | 0.360 | 0.001 | 0.048 | 0.980 |
| Bicycle | 0.078 | -0.079 | 0.018 | 0.000 |
| **C: HH-level education and employment** | | | | |
| Public school (sons) | 0.095 | -0.086 | 0.020 | 0.000 |
| Public school (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 |
| **D: Individual-level education and employment[6]** | | | | |
| 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 |
| **E: Ward level neighborhood characteristics[7]** | | | | |
| HH size | 4.500 | 0.074 | 0.021 | 0.000 |
| Sex ratio | 0.850 | -0.006 | 0.004 | 0.170 |
| %Scheduled caste | 0.064 | 0.001 | 0.003 | 0.780 |
| %Scheduled tribe | 0.010 | 0.000 | 0.000 | 0.780 |
| %Literate | 0.810 | -0.010 | 0.003 | 0.002 |
| %Working | 0.400 | -0.007 | 0.002 | 0.002 |
| %Main workers | 0.380 | -0.007 | 0.002 | 0.002 |
| %Marginal workers | 0.023 | 0.000 | 0.000 | 0.430 |
| **F: Postal code-level school characteristics[8]** | | | | |
| %Senior secondary schools | 0.120 | -0.016 | 0.007 | 0.064 |
| %Public schools | 0.330 | 0.015 | 0.013 | 0.390 |
| Mean # classrooms | 8.300 | -0.130 | 0.190 | 0.560 |
| Mean # permanent classrooms | 8.000 | -0.200 | 0.190 | 0.400 |
| % schools w/ office for headmaster | 0.970 | -0.011 | 0.003 | 0.000 |
| % schools with library | 0.980 | -0.002 | 0.002 | 0.390 |
| Mean # teachers w/ prof qualifications | 14.000 | 0.051 | 0.400 | 0.900 |
| %English medium | 0.430 | -0.030 | 0.013 | 0.064 |
| **G: Sources for loans** | | | | |
| Savings | 0.600 | 0.033 | 0.049 | 0.650 |
| Family, friends and neighbors | 0.550 | 0.030 | 0.050 | 0.650 |
| Informal lender | 0.012 | 0.005 | 0.012 | 0.650 |
| Commercial bank | 0.049 | 0.058 | 0.028 | 0.200 |
| Don't know | 0.036 | -0.021 | 0.016 | 0.510 |
| **H: Future-looking attitudes** | | | | |
| 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 |
| **I: Individualistic attitudes** | | | | |
| 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 |
| **J: Healthcare** | | | | |
| 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 |
| Family member's advice | 0.004 | 0.037 | 0.014 | 0.044 |

[1] Variable definitions for survey-based outcomes available in Table SI.1. [2] Estimate for $\alpha$ in Equation 1. [3] Estimate for $\beta$ in Equation 1. [4] HC2 errors, with errors clustered at the household level for individual results. [5] Benjamini-Hochberg adjusted p-values.
[6] N=3,170 [7] Data from 2011 Indian Census. Measured for where households live at the time of survey. [8] Postal-code level data for 2017 from the Ministry of Human Resource Development, Government of India. Measured for where households live at the time of survey.

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

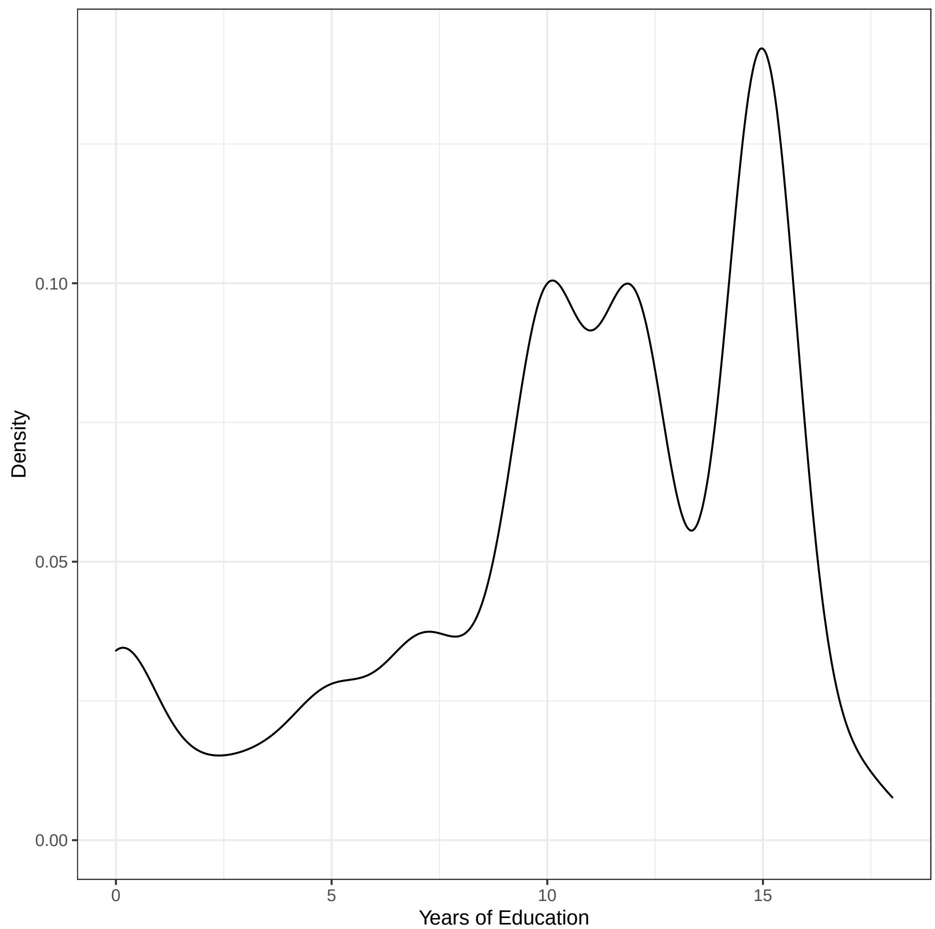

Table 4: Regressions of individual completion of various years of education on the treatment indicator.

| | *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) | (10) |
| T | 0.400 | 0.720 | 0.008 | 0.010 | 0.071 | 0.056 | 0.056 | 0.039 | 0.041 | 0.036 |
| | (0.150) | (0.390) | (0.009) | (0.009) | (0.018) | (0.019) | (0.019) | (0.021) | (0.017) | (0.017) |
| $Turned_6$[1] | −3.500 | | | 0.057 | | | | | | |
| | (0.240) | | | (0.017) | | | | | | |
| $Turned_{16}$ | 4.100 | | | | | 0.330 | | | | |
| | (0.240) | | | | | (0.042) | | | | |
| $Turned_{18}$ | 3.400 | | | | | | | 0.390 | | |
| | (0.260) | | | | | | | (0.051) | | |
| $Turned_{21}$ | 6.600 | | | | | | | | | 0.350 |
| | (0.270) | | | | | | | | | (0.050) |
| Older[2] | 4.100 | 1.700 | | | | | | | | |
| | (0.230) | (0.320) | | | | | | | | |
| T×Older | | −0.160 | | | | | | | | |
| | | (0.450) | | | | | | | | |
| T×$Turned_6$ | | | | −0.016 | | | | | | |
| | | | | (0.018) | | | | | | |
| T×$Turned_{16}$ | | | | | | 0.093 | | | | |
| | | | | | | (0.050) | | | | |
| T×$Turned_{18}$ | | | | | | | | 0.110 | | |
| | | | | | | | | (0.067) | | |
| T×$Turned_{21}$ | | | | | | | | | | 0.110 |
| | | | | | | | | | | (0.068) |
| Constant | 6.700 | 9.100 | 0.940 | 0.930 | 0.510 | 0.490 | 0.320 | 0.300 | 0.260 | 0.230 |
| | (0.210) | (0.280) | (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 | 3,170 |
| $R^2$ | 0.290 | 0.076 | 0.047 | 0.049 | 0.053 | 0.088 | 0.058 | 0.110 | 0.058 | 0.110 |
| Adjusted $R^2$ | 0.260 | 0.035 | 0.005 | 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. [2] "Older" is an indicator for an individual being older than 21 at the time of the lottery.

Table 5: Regressions of individual employment on the treatment indicator.

| | *Dependent variable:* | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Employed | | | | | | Employed (full-time) | | | | Employed (part-time) | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
| T | 0.042 | 0.038 | 0.051 | 0.045 | 0.035 | 0.058 | 0.082 | 0.077 | 0.069 | 0.082 | −0.025 | −0.020 | −0.021 | −0.021 |
| | (0.014) | (0.015) | (0.016) | (0.016) | (0.016) | (0.029) | (0.019) | (0.020) | (0.019) | (0.035) | (0.012) | (0.013) | (0.013) | (0.027) |
| $Turned_6$[1] | −0.016 | −0.470 | | | | | | | | | | | | |
| | (0.012) | (0.014) | | | | | | | | | | | | |
| $Turned_{16}$ | 0.001 | | −0.450 | | | | −0.380 | | | | 0.093 | | | |
| | (0.025) | | (0.027) | | | | (0.037) | | | | (0.043) | | | |
| $Turned_{18}$ | 0.140 | | | −0.220 | | | | −0.170 | | | | 0.063 | | |
| | (0.035) | | | (0.052) | | | | (0.053) | | | | (0.039) | | |
| $Turned_{21}$ | 0.640 | | | | 0.160 | | | | 0.180 | | | | −0.008 | |
| | (0.036) | | | | (0.045) | | | | (0.044) | | | | (0.028) | |
| Older[2] | 0.570 | | | | | 0.410 | | | | 0.330 | | | | −0.098 |
| | (0.013) | | | | | (0.024) | | | | (0.026) | | | | (0.022) |
| T×$Turned_6$ | | −0.023 | | | | | | | | | | | | |
| | | (0.021) | | | | | | | | | | | | |
| T×$Turned_{16}$ | | | 0.058 | | | | 0.051 | | | | 0.017 | | | |
| | | | (0.041) | | | | (0.051) | | | | (0.055) | | | |
| T×$Turned_{18}$ | | | | 0.065 | | | | 0.049 | | | | −0.036 | | |
| | | | | (0.071) | | | | (0.074) | | | | (0.049) | | |
| T×$Turned_{21}$ | | | | | 0.160 | | | | 0.150 | | | | −0.010 | |
| | | | | | (0.068) | | | | (0.062) | | | | (0.040) | |
| T×Older | | | | | | −0.021 | | | | −0.009 | | | | −0.0003 |
| | | | | | | (0.035) | | | | (0.038) | | | | (0.028) |
| Constant | 0.005 | 0.470 | 0.470 | 0.460 | 0.440 | 0.170 | 0.480 | 0.470 | 0.450 | 0.230 | 0.082 | 0.083 | 0.087 | 0.150 |
| | (0.012) | (0.011) | (0.011) | (0.011) | (0.011) | (0.020) | (0.014) | (0.014) | (0.014) | (0.024) | (0.009) | (0.009) | (0.009) | (0.020) |
| Observations | 3,170 | 3,170 | 3,170 | 3,170 | 3,170 | 3,170 | 3,170 | 3,170 | 3,170 | 3,170 | 3,170 | 3,170 | 3,170 | 3,170 |
| $R^2$ | 0.250 | 0.072 | 0.074 | 0.042 | 0.049 | 0.160 | 0.084 | 0.059 | 0.071 | 0.140 | 0.068 | 0.061 | 0.059 | 0.086 |
| Adjusted $R^2$ | 0.220 | 0.031 | 0.034 | 0.0001 | 0.007 | 0.130 | 0.044 | 0.018 | 0.030 | 0.100 | 0.026 | 0.019 | 0.018 | 0.046 |

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.

Table 6: OLS estimates of predictors of moving among winning applicants.

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Moving | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| OBC | −0.130 | −0.110 | −0.150 | −0.110 | −0.140 | −0.110 |
| | (0.073) | (0.081) | (0.073) | (0.081) | (0.072) | (0.081) |
| SCST | −0.200 | −0.180 | −0.200 | −0.180 | −0.200 | −0.180 |
| | (0.081) | (0.096) | (0.080) | (0.096) | (0.080) | (0.096) |
| Maratha | −0.140 | −0.140 | −0.140 | −0.140 | −0.140 | −0.140 |
| | (0.059) | (0.066) | (0.059) | (0.066) | (0.059) | (0.066) |
| Muslim | 0.004 | 0.013 | −0.007 | 0.013 | −0.006 | 0.013 |
| | (0.085) | (0.092) | (0.085) | (0.092) | (0.085) | (0.092) |
| Makeshift floor | 0.400 | 0.380 | 0.360 | 0.380 | 0.400 | 0.380 |
| | (0.150) | (0.170) | (0.160) | (0.170) | (0.150) | (0.170) |
| From Mumbai | −0.069 | −0.086 | −0.071 | −0.086 | −0.075 | −0.086 |
| | (0.060) | (0.069) | (0.060) | (0.069) | (0.060) | (0.069) |
| From same ward as apt | 0.210 | 0.180 | 0.200 | 0.180 | 0.220 | 0.180 |
| | (0.079) | (0.089) | (0.080) | (0.089) | (0.078) | (0.089) |
| Change in Literacy Rate | −0.054 | −0.052 | −0.066 | −0.052 | −0.070 | −0.052 |
| | (0.038) | (0.056) | (0.051) | (0.056) | (0.036) | (0.056) |
| Change in Employment Rate[1] | 0.130 | 0.160 | 0.160 | 0.160 | 0.140 | 0.160 |
| | (0.037) | (0.044) | (0.040) | (0.044) | (0.038) | (0.044) |
| LIG | −0.071 | 0.086 | | | | |
| | (0.060) | (0.450) | | | | |
| Scheme 275 | | | −0.025 | 0.920 | | |
| | | | (0.270) | (0.690) | | |
| Scheme 276 | | | −0.110 | 0.410 | | |
| | | | (0.260) | (0.600) | | |
| Scheme 283 | | | −0.160 | 0.190 | | |
| | | | (0.190) | (0.590) | | |
| Scheme 284 | | | 0.053 | 1.000 | | |
| | | | (0.200) | (0.690) | | |
| Scheme 302 | | | 0.052 | 0.450 | | |
| | | | (0.210) | (0.540) | | |
| Scheme 303 | | | 0.012 | 0.250 | | |
| | | | (0.190) | (0.600) | | |
| Scheme 305 | | | 0.030 | 0.160 | | |
| | | | (0.200) | (0.570) | | |
| 2014 lottery | | | | | 0.087 | −0.840 |
| | | | | | (0.051) | (0.570) |
| Constant | 0.720 | 0.650 | 0.720 | 0.320 | 0.640 | 1.200 |
| | (0.080) | (0.310) | (0.190) | (0.510) | (0.066) | (0.320) |
| Block dummies? | No | Yes | No | Yes | No | Yes |
| Observations | 421 | 421 | 421 | 421 | 421 | 421 |
| $R^2$ | 0.120 | 0.250 | 0.140 | 0.250 | 0.120 | 0.250 |
| Adjusted $R^2$ | 0.095 | 0.080 | 0.100 | 0.080 | 0.098 | 0.080 |

All regressions include HC2 errors. Indicators for LIG, Year, and Scheme are run in different models due to collinearity.

[1] Reflects difference in rate between apartment location and baseline location.

Table 7: OLS estimates of predictors of moving among winning applicants (Change in Employment Rate replaced with I(Change in Employment Rate>0) .

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Moving | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| OBC | −0.140 | −0.095 | −0.140 | −0.095 | −0.140 | −0.095 |
| | (0.074) | (0.082) | (0.075) | (0.082) | (0.074) | (0.082) |
| SCST | −0.210 | −0.200 | −0.210 | −0.200 | −0.210 | −0.200 |
| | (0.082) | (0.098) | (0.082) | (0.098) | (0.082) | (0.098) |
| Maratha | −0.130 | −0.140 | −0.130 | −0.140 | −0.130 | −0.140 |
| | (0.060) | (0.067) | (0.060) | (0.067) | (0.060) | (0.067) |
| Muslim | −0.031 | −0.013 | −0.040 | −0.013 | −0.036 | −0.013 |
| | (0.086) | (0.093) | (0.086) | (0.093) | (0.086) | (0.093) |
| Makeshift floor | 0.370 | 0.330 | 0.340 | 0.330 | 0.370 | 0.330 |
| | (0.160) | (0.170) | (0.160) | (0.170) | (0.160) | (0.170) |
| From Mumbai | −0.078 | −0.097 | −0.076 | −0.097 | −0.081 | −0.097 |
| | (0.061) | (0.071) | (0.061) | (0.071) | (0.061) | (0.071) |
| From same ward as apt | 0.290 | 0.250 | 0.290 | 0.250 | 0.300 | 0.250 |
| | (0.081) | (0.095) | (0.084) | (0.095) | (0.080) | (0.095) |
| Change in Literacy Rate | 0.031 | 0.073 | 0.049 | 0.073 | 0.024 | 0.073 |
| | (0.032) | (0.049) | (0.045) | (0.049) | (0.028) | (0.049) |
| I(Change in Employment Rate >0)[1] | 0.033 | 0.028 | 0.063 | 0.028 | 0.041 | 0.028 |
| | (0.073) | (0.090) | (0.079) | (0.090) | (0.073) | (0.090) |
| LIG | −0.041 | 0.140 | | | | |
| | (0.061) | (0.460) | | | | |
| Scheme 275 | | | −0.014 | 1.000 | | |
| | | | (0.270) | (0.700) | | |
| Scheme 276 | | | −0.120 | 0.430 | | |
| | | | (0.260) | (0.610) | | |
| Scheme 283 | | | −0.058 | 0.370 | | |
| | | | (0.200) | (0.610) | | |
| Scheme 284 | | | 0.110 | 1.000 | | |
| | | | (0.200) | (0.700) | | |
| Scheme 302 | | | 0.091 | 0.590 | | |
| | | | (0.220) | (0.550) | | |
| Scheme 303 | | | 0.025 | 0.400 | | |
| | | | (0.190) | (0.610) | | |
| Scheme 305 | | | 0.027 | 0.260 | | |
| | | | (0.200) | (0.580) | | |
| 2014 lottery | | | | | 0.035 | −0.790 |
| | | | | | (0.050) | (0.580) |
| Constant | 0.640 | 0.550 | 0.580 | 0.120 | 0.590 | 1.100 |
| | (0.085) | (0.320) | (0.200) | (0.520) | (0.071) | (0.330) |
| Block dummies? | No | Yes | No | Yes | No | Yes |
| Observations | 421 | 421 | 421 | 421 | 421 | 421 |
| $R^2$ | 0.090 | 0.220 | 0.100 | 0.220 | 0.090 | 0.220 |
| Adjusted $R^2$ | 0.068 | 0.045 | 0.066 | 0.045 | 0.068 | 0.045 |
| Adjusted $R^2$ | 0.095 | 0.080 | 0.100 | 0.080 | 0.098 | 0.080 |

All regressions include HC2 errors. Indicators for LIG, Year, and Scheme are run in different models due to collinearity. [1] Reflects difference in rate between apartment location and baseline location.

Table 8: Treatment effects on individual-level outcomes conditional on roof-type at time of application (measured through recall).

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Years of education | Employed | Employed-full time | Employed-part time |
| | (1) | (2) | (3) | (4) |
| T | 0.720 | 0.047 | 0.079 | −0.020 |
| | (0.180) | (0.016) | (0.020) | (0.013) |
| Makeshift roof | −0.370 | 0.002 | 0.057 | 0.016 |
| | (0.790) | (0.044) | (0.061) | (0.050) |
| T×Makeshift roof | −2.500 | −0.073 | −0.066 | −0.031 |
| | (1.200) | (0.099) | (0.110) | (0.060) |
| Constant | 10.000 | 0.450 | 0.460 | 0.086 |
| | (0.130) | (0.012) | (0.014) | (0.009) |
| Observations | 3,170 | 3,170 | 3,170 | 3,170 |
| $R^2$ | 0.054 | 0.035 | 0.055 | 0.059 |
| Adjusted $R^2$ | 0.012 | −0.008 | 0.013 | 0.018 |

All regressions include an interaction with the centered stratum-level indicator for randomization groups and standard errors clustered at the household level.

Table 9: Treatment effects on household-level outcomes conditional on roof-type at time of application (measured through recall).

| | *Dependent variable:* | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Pub sch (m) | Pub sch (f) | Eng med sch (m) | Eng med sch (f) | Tuition (m) | Tuiton (f) | Salaried | Govt job | Formal sector job |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| T | −0.081 | −0.083 | 0.023 | 0.005 | −0.042 | −0.029 | 0.100 | 0.043 | 0.052 |
| | (0.020) | (0.018) | (0.047) | (0.046) | (0.040) | (0.040) | (0.039) | (0.040) | (0.034) |
| Makeshift roof | 0.110 | 0.120 | 0.079 | −0.220 | −0.044 | −0.030 | 0.210 | 0.047 | −0.062 |
| | (0.054) | (0.049) | (0.120) | (0.120) | (0.110) | (0.110) | (0.100) | (0.110) | (0.091) |
| T×Makeshift roof | −0.120 | −0.140 | −0.002 | 0.091 | 0.120 | −0.065 | −0.600 | −0.140 | 0.039 |
| | (0.092) | (0.083) | (0.210) | (0.210) | (0.170) | (0.170) | (0.170) | (0.170) | (0.150) |
| Constant | 0.091 | 0.083 | 0.280 | 0.280 | 0.220 | 0.220 | 0.770 | 0.180 | 0.098 |
| | (0.013) | (0.012) | (0.031) | (0.030) | (0.026) | (0.027) | (0.026) | (0.026) | (0.023) |
| Observations | 823 | 822 | 823 | 822 | 834 | 834 | 834 | 834 | 834 |
| $R^2$ | 0.210 | 0.240 | 0.170 | 0.180 | 0.180 | 0.170 | 0.150 | 0.210 | 0.140 |
| Adjusted $R^2$ | 0.054 | 0.096 | 0.011 | 0.019 | 0.025 | 0.006 | −0.008 | 0.055 | −0.025 |

All regressions include an interaction with the centered stratum-level indicator for randomization groups and HC2 standard errors.

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