# Transferring wealth: the welfare effects of an affordable housing program in Mumbai*

Tanu Kumar†

May 1, 2019

**Abstract**

This paper uses survey data from a housing lottery in Mumbai to understand the medium-term household-level effects of the subsidized the sale of housing to lower-middle class urban families. The program consists of of wealth transfer manifested, for some, through a stream of housing benefits. Beneficiaries can also rent out or eventually sell the housing to enjoy in-kind or lump sum benefits. Overall, the program leads individuals to express greater optimism about the future and increases educational attainment, particularly at the secondary and post-secondary level. This is among the first studies of the effects of the subsidized sale of an asset in a developing country.

*This project has been supported by the J-PAL Governance Initiative, the Weiss Family Program Fund for Development Economics at Harvard University, the Institute of International Studies at the University of California, Berkeley, and the American Political Science Association Centennial Center. Research has been approved by the Committee for Protection of Human Subjects at the University of California, Berkeley, protocol 2017-04-9808. A pre-analysis plan has been registered with EGAP. I am extremely grateful for Partners for Urban Knowledge Action Research and particularly Nilesh Kudupkar for assistance with data collection. Thank you to Pradeep Chhibber, Joel Middleton, Edward Miguel, and Alison Post for their mentorship and advice throughout this project. Michael Koelle and participants at DevPec 2019 also provided valuable comments. Most importantly, I thank the hundreds of survey and interview respondents who gave their time to this study. Portions of this paper, where noted, have appeared in another paper that uses the same research design but focuses on different outcomes.

†Travers Department of Political Science, UC Berkeley. `tkumar[at]berkeley[dot]edu`

# 1 Introduction

What are the welfare effects of asset transfers? This paper focuses on the effects of subsidizing homeownership, an initiative implemented globally by governments in many forms, including mortgage subsidies and home-price subsidies. In particular, it estimates the household-level effects of one policy configuration particularly common in India, the subsidized sale of homes to lower-middle class households.

In India, this intervention is motivated in part by the growing urban population; here, about 404 million people are expected to migrate to cities by 2050 (UN World Urbanization Prospects 2014). As demand for living space increases, poorer households are forced to live on the least desirable and cheapest housing in a city, namely in illegal settlements or a city's outskirts to which public services may not yet extend. There exists a range of interventions to deal with the problems posed by these living situations, such as land titling (see e.g. Di Tella et al. 2007; Feder and Feeny 1991; Field 2005; Galiani and Schargrodsky 2010) and the extension of services (see e.g. Burra 2005; Gulyani and Bassett 2007; Imparato and Ruster 2003). These interventions mostly help alleviate problems of informality faced by a city's poorest residents, but low housing supply affects members of higher socio-economic strata, too. As a result, governments have also attempted to increase formal housing supply by encouraging private developers to build and by constructing housing themselves. [1]

Such programs have been spearheaded in all major Indian cities by state-level development boards created by India's Second Five Year Development Plan (1951-1956) that provided central government funding to states to develop low-income housing (Pornchokchai 2008). This same development plan advocated cooperative citizen ownership in all sectors of the economy; as a result, the housing boards developed apartments that would be sold, rather than rented, to individuals and buildings that would be collectively maintained by all owners (Ganpati 2010; Shinde 2019; Sukumar 2001). This policy of construction for ownership continued even as the

[1] Perhaps formal housing programs are appealing to governments for political reasons as well. Alan and Ward (1985, p. 5-6) claim that public housing serves three main functions in society: it provides visual evidence that the government is providing for the poor, construction creates jobs, and it provides homes for government supporters and officials.

central government's development plans moved towards policies favoring the facilitation of private construction after the economic liberalization of the 1990s. Moreover, in 2015, India's federal government claimed a housing shortfall of over 18 million to motivate a plan, Pradhan Mantri Awas Yojana (P-MAY), 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 model of state subsidized affordable homeownership is common; similar policies exist in Brazil, Uruguay, Nigeria, Kenya, and elsewhere.

The constitutes a wealth transfer to households. Homes can be effective vehicles for wealth accumulation; the extent to which this is true depends on whether and how much a home's price appreciates over time (Stegman et al. 2007). The fact that homes in the program studied are sold well below market rate guarantees immediate price appreciation. Households experience this wealth transfer in any combination of three payout structures: through a stream of in-kind benefits enjoyed when living in the home, through cash benefits when renting the home out, or lump-sum through sale.

In this paper, I study the effects of a subsidized home sale program that allocates homes through a lottery in Mumbai, India, a metropolitan area of over 20 million. In 2017, I surveyed winners and non-winners of multiple lotteries that took place in 2012 and 2014 to understand the effect of winning on an array of outcomes. I first find that the wealth transfer is visible through improved housing quality among winners. Winners also report feeling happier about their financial situations and expect better lives for their children. This finding is striking given the decreased levels of cash savings and the fact that a rule in program prohibits beneficiaries from selling the apartments for ten years after possession; as a result, beneficiaries have not realized the wealth transfer that the subsidized asset sale entails. The expectations for the lives of children may be justified, as the average number of years of education among winning households is over a half year greater than that of non-winning households. Individual-level data shows that household members who turned 16 in between the lottery and being surveyed were 13 percentage points more likely to continue to advanced secondary school and thereby potentially complete grade school and even college. I do not, however, see any evidence to suggest that winners live in wealthier neighborhoods, more literate neighborhoods, or those with better schools, making it unlikely that the effects are driven be relocation among those choosing the in-kind transfer. These findings are consistent with research showing that wealth predicts educational attainment.

This paper is among the first to study the effects of subsidized home sales and subsidized asset transfers more generally. Urban land-titling and rural ultra-poor graduation programs (e.g. Banerjee et al. 2015) have received more attention, but both are targeted at a lower income class than the program studied here. Subsidized asset sale programs may facilitate asset accumulation and fundamentally change the trajectories of lower-middle class households that can pay for the asset in question, just not at the prices in a supply-constrained market.

## 2 The intervention

This study is based in Mumbai, Maharashtra, an area that attracts migrants from all over India.[2] The population growth rate from 2010-2018 was approximately 13%. The private sector has been unable to meet the resulting growth in housing demand for one main reason: supply is constrained by a strict building height-to-land ratio. This rule originally stems from the facts that much of the city occupies land reclaimed from the Arabian Sea and that the airport lies near the center of the metropolitan area. Developers are thus incentivized to devote valuable central city square footage to higher end buildings, leading lower income households to occupy slums, crowd into extremely small homes with friends and relatives, or live far from the city. One survey respondent, for example, claimed to have lived 2.5 hours by train from his place of work when he first moved to the city. According to the 2011 census, roughly 40% of the population of the Mumbai Metropolitan Area lives in slums (Ministry of Housing and Urban Poverty Alleviation 2015).

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 econom-

[2]Portions of the description of the intervention, data collection, and description of the sample overlap with AUTHOR 2019, which uses the same design to investigate different outcomes.

ically weaker section (EWS) and low-income group (LIG)[3] urban residents who 1) do not own housing, and 2) who have lived in the state of Maharashtra for at least 15 continuous years within the 20 years prior to the sale. Winners have access to loans from a state owned bank and most take out 25 year mortgages. While the downpayment and mortgage leave this program out of the reach of many of the city's poorest residents, it gives eligible lower middle-class families without property the opportunity to purchase heavily subsidized apartments. I study 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.

Table 1: Lottery apartments included in the sample

| Scheme # | Lottery Year | Group | Neighborhood | Area[1] | Cost[2] | Downpayment[3] |
|---|---|---|---|---|---|---|
| 274 | 2012 | LIG | Charkop | 402 | 2,725,211 | 15,050 |
| 275 | 2012 | LIG | Charkop | 462 | 3,130,985 | 15,050 |
| 276 | 2012 | LIG | Charkop | 403 | 2,731,441 | 15,050 |
| 283 | 2012 | LIG | Malvani | 306 | 1,936,700 | 15,050 |
| 283 | 2012 | LIG | Vinobha Bhave Nagar | 269 | 1,500,000 | 15,050 |
| 302 | 2014 | EWS | Mankhurd | 269 | 1,626,500 | 15,200 |
| 303 | 2014 | LIG | Vinobha Bhave Nagar | 269 | 2,038,300 | 25,200 |
| 305 | 2014 | EWS | Magathane | 269 | 1,464,500 | 15,200 |

[1] In square feet. Refers to "carpet area", or the actual apartment area and excludes common space.
[2] In INR with the cost stated in the lottery year.
[3] In INR with the cost stated in the lottery year. Includes application fee of Rs.200.

The lottery homes were sold at a government "fair price" that was 30-60% of market prices. The subsidy estimates are based on neighborhood prices per square foot, but they do not account for the fact that government housing has a lower resale value than privately constructed housing likely because of the mild social stigma and particular aesthetic associated with government housing. Housing was constructed on land obtained for free from the city's dismantled textile industry - this land was earmarked specifically for "social" projects and cannot be used for other purposes (Madan 2016). Importantly, this means that the homes for sale do not lie on the city's outskirts, but are fairly central and squarely on major highways and transit lines. Each is within walking distance of the Mumbai suburban rail network, the main network that millions of city

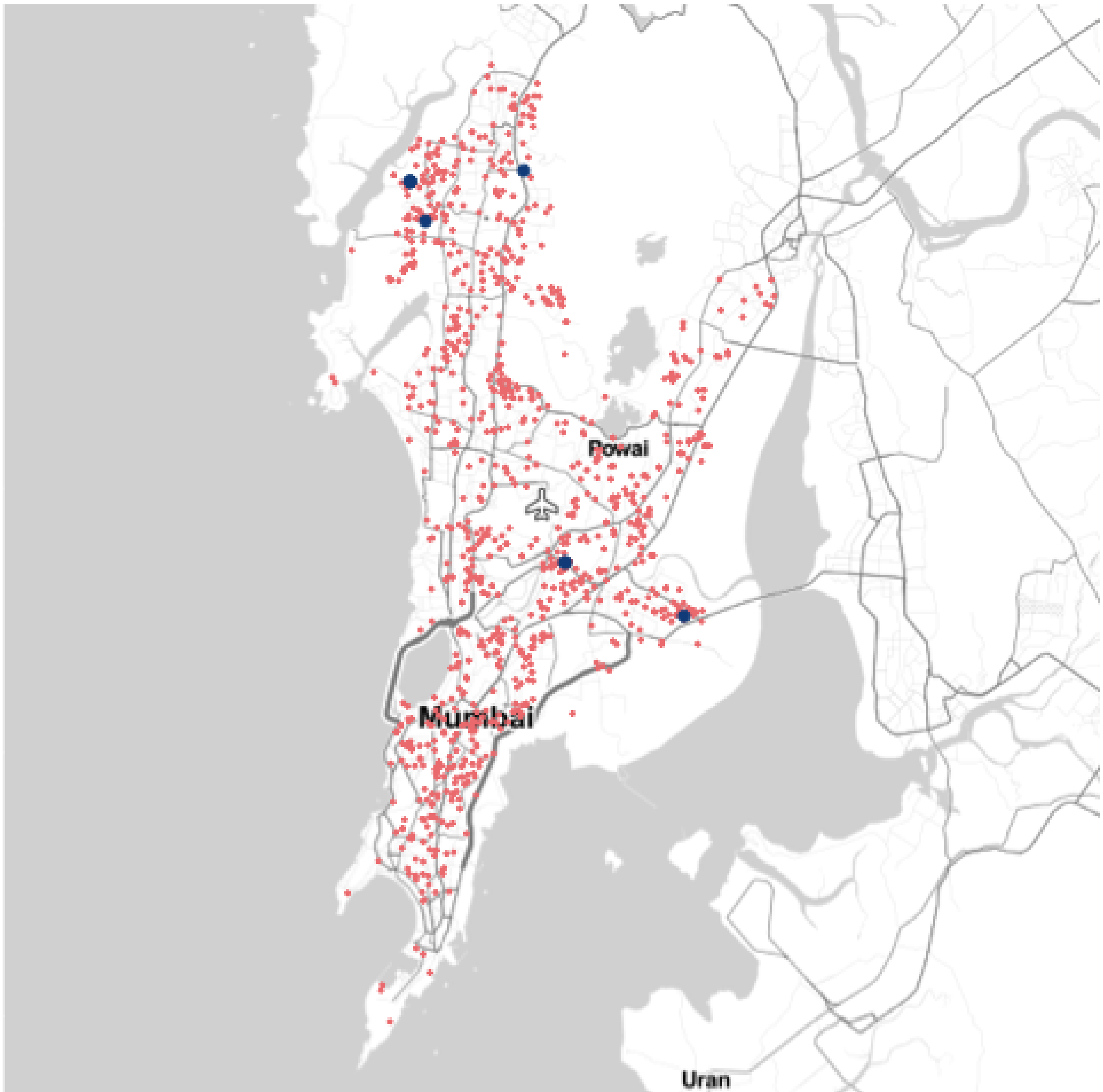
[3] Members of the EWS earn up to 3200 USD/year. Members of the LIG earn up to 7400 USD/year.

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. At the time of application, households were permitted to choose the building for which they submitted an application. The ability of households to choose their preferred building along with the proximity of the buildings to transit options suggests that these buildings, unlike those studied by Barnhardt *et al.* (2017), were not necessarily isolated or extremely disconnected from winners' neighborhoods at the time of application.

Resale of the apartments is not permitted until 10 years after purchase. This rule generally seems to be enforced, both by MHADA officials and homeowners' associations active in each lottery building. Conversations with building residents reveal that one or two owners have successfully sold their homes before the 10 year period, but most interview respondents discussed considering sale only when permitted as they are likely to receive higher prices for legal sale. Additionally, apartment "society" (the local name for homeowners' associations) chairmen claimed to contact MHADA if they suspected an attempted sale due to a belief that early sales create an artificially low "benchmark" for future sales in the same apartment complex. Households can, however, put the apartments up for rent. Fifty percent of households in my sample have made this choice. Finally, households do not pay taxes on their dwelling for five years after possession.

As mentioned above, beneficiaries are selected through a lottery process. In fact, the winning sample was stratified by caste and occupation groups (Table A1), as each apartment building had quotas for these groups within which randomization occurred. The building/caste-occupation group within which stratification occurred will be referred to as "blocks" from now on. Aside from evidence provided by the balance checks below, there are several reasons to believe that the this process was fair, or truly randomized. First of all, the lottery was conducted using a protected computerized process that was implemented in 2010. Interestingly, 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. Applicants also applied with their Permanent Account Numbers (PAN), which are linked to their bank accounts. Before conducting the lottery, MHADA officials used the PAN numbers to check both whether individuals had applied multiple

Figure 1: Location of the addresses of households in the sample (pink) along with the location of apartment buildings (blue) at the time of application




times for the same lottery round and whether or not they met the criteria for eligibility. 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.

## 3 Conceptual framework

This intervention, like other asset transfers, can be thought of as a wealth transfer. Here, the transfer consists of the subsidy; beneficiaries receive its value over time either through in-kind payments of reduced-price housing benefits, cash payments should they choose to rent out the home, or lump sum transfer through sale (although beneficiaries are technically not permitted to take advantage of this last option during the time of the study.[4] Households can make this choice to maximize utility based a wide variety of parameters, including their discount rates, that will vary with the asset and program in question. This paper is agnostic about the the payout structure households choose and instead seeks to estimate average effects across all beneficiaries.

Increases in wealth, however, are notoriously hard to measure. I first look for evidence of the wealth transfer by estimating effects on common components of asset-based measures of wealth (Filmer and Pritchett 2001). Of course, the asset in question is the vehicle for the wealth transfer and will affect the way in which households' newly acquired wealth manifests itself. Those choosing in-kind payments will likely display wealth gains of a similar type. In the case of the housing program, for example, those choosing in-kind transfers are likely to enjoy access to housing with better quality walls, flooring, and service delivery, all of which are components of most asset based indices of wealth in developing countries (Davila et al. 2014). In contrast, those choosing cash or lump sum transfers will display greater variation in the observable implications of the wealth transfer. As a result, I estimate average treatment effects on components of wealth associated with the asset being transferred in this case, namely features of housing quality. I also estimate average effects on ownership of other durable assets with the expectation that these effects may be driven by those choosing cash transfers.

Next I estimate potential adverse effects of the in-kind transfer, particularly on travel times to work and to see friends and family. Studies of home rental programs that effectively only entail an in-kind transfer have found that the resulting relocation can lead to broken social networks

[4]Actually, this is true of many types of welfare programs, from those that provide subsidized rations to those that deliver sewing machines. At the opening of the Bollywood film *Sui Dhaga*, the protagonist is seen working for a small shop that rents out sewing machines procured through an Indian scheme designed to provide enterprising Indians with sewing machines.

(Barnhardt et al. 2017) and negative labor market outcomes (van Dijk 2019). Rental programs do not provide households with streams of benefits in perpetuity but rather only while they make use of the asset. Such programs also do not permit households to choose a lump sum transfer. Rental programs thus involve greater constraints on the way in which transfers are used, which is potentially a reason for the adverse effects such as of these programs. For this reason, I expect to find relatively small effects here, as the subsidized sale programs allow greater flexibility in the payout structure of the program.

I also estimate the effects of this wealth transfer on household attitudes. Several (e.g. Haushofer and Fehr 2014; Mani et al. 2013) have found that the insecurity created by poverty can make it difficult to focus on long-term goals and lead to short-sighted behavior. In many places, homeownership also represents the attainment of a certain level of socio-economic status. This perceived status attainment could be due to the home's wealth returns or other cultural beliefs, often generated by government messaging to promote homeownership (Vale 2007). Indeed, in July 2018, *The Hindustan Times* ran a story documenting the pride and satisfaction reported by members of 13 households in Mumbai that had fulfilled their dreams of homeownership.[5] Thus the specific vehicle of this wealth transfer, a home, has the potential to yield improvements in psychological well-being among beneficiaries as well.

Finally, to better understand the extent to which parents pass on these wealth gains to children, I estimate effects on choices about school quality and both household and individual-level educational attainment. Wealth is, after all, commonly cited as a predictor of educational attainment in developing countries (Filmer and Pritchett 2001). Multiple mechanisms are potentially at work here. As households grow wealthier, they may be willing to invest more, in terms of actual schooling fees and the opportunity cost of forgone income, into education. Moreover, as they become wealthier, they may derive greater utility from gains from education that are higher on Maslow's 1943 hierarchy of needs, such as self-actualization. As discussed above, the intervention may also intervention improve psychological well-being, which in itself may decrease discount rates and increase investments into one's future.

[5]https://www.hindustantimes.com/real-estate/i-bought-a-home-13-tales-from-proud-new-homeowners-in-mumbai/story-SHgB8vdfpjcbkFRHyhP68L.html

For household level effects, I follow my preanalysis plan and estimate the treatment effect, $\beta$, in the following equation where $Y$ is the outcome (as measured through a survey) , $T$ is an indicator for treatment (winning the lottery), $C_1 ... C_j$ is the group of fixed (or pre-treatment) covariates used for randomization checks, and $B_1 ... B_l$ is a set of dummies for the blocks within which randomization occurred:

$$Y = \alpha + \beta T + \sum_{1}^{j} \gamma_j C_j + \sum_{1}^{l} \eta_i (T * (B_i - \bar{B}_i)) \tag{1}$$

It is likely that certain households apply for the lottery year after year, thereby increasing their probability of winning *any* lottery. I thus only label households as "treated" if they win the lottery in the specific year for which they appear in the sample. Following the pre-analysis plan and Lin (2013), I include an interaction between the treatment indicator and the mean-centered block indicators to account for varying probabilities of treatment assignment within each block. The full regression output for all results can be found in the appendix. Following Imbens and Kolesar (2015), I compute standard errors using the HC2 estimator (MacKinnon and White 1985). As described in the pre-analysis plan, I make Benjamini-Hochberg corrections for the false discovery rate within "families" of outcomes. While this study potentially suffers from two-sided noncompliance (8% of treated units did not purchase homes), I simply conduct an intent-to-treat (ITT) analysis. This choice should bias treatment effects to zero.

Again, note that this paper estimates average treatment effects across the different types of payout structures chosen. This is mainly because this choice reveals a type, and types remain unknown among the control group.[6] 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.

[6]Control group households are not 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.

## 4 Data collection

I estimate treatment effects on all outcomes based on in-person household surveys of both winning (treatment) and non-winning (control) households. I procured from MHADA phone numbers and addresses for winners and a random sample of applicants that were drawn in the same stratified sampling method used for the selection of winners. There were an equal number of treated and control units in each block, and I accessed total 1,848 addresses.[7] These addresses were mapped using Google Maps. Addresses that were incomplete (42), outside of Greater Mumbai (600), or could not be mapped (132) were removed from the sample. This left 531 and 532 control and treatment households, respectively. Table 2 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. Overall, however, I expect the 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 3, indeed shows that proportions of membership in certain categories in the mapped sample *are* significantly different from the original full sample obtained from MHADA. Importantly, there are relatively fewer Scheduled Tribe members and more General Population (e.g. Forward Castes) members in the mapped sample than in the full sample provided by MHADA. The mapped sample may thus have slightly higher socio-economic status than the full sample of applicants on average.

Given the lack of availability of pretreatment covariates, I cannot test for corruption in the lottery among the 1,848 addresses provided by MHADA. Once mapped, however, I can place households into state and municipal electoral wards and test for evidence of selection into the mapped treatment group by ward. Here, I conduct regressions of the treatment indicator on

[7] There are more than 300,000 economically weaker section applicants for roughly 300 spots, so I interviewed a random sample of applicants.

the state and municipal ward membership indicators and calculate of a heteroscedasticity-robust Wald statistic for the hypothesis that the coefficients on all of the indicators (other than block randomization dummies) are zero. The p-values for regressions on state and municipal ward membership are 0.35 and 0.46, respectively. These p-values do not allow me to reject the null hypothesis that members of any political constituency were not systematically more (or less) likely to be in the mapped treatment group.

From the mapped sample, I randomly selected 500 households from each treatment condition to interview. From September 2017-May 2018 (after the Mumbai municipal elections in February 2017), I worked with a Mumbai-based organization to contact the households and conduct interviews.[8] The process for contacting was as follows: The addresses and phone numbers provided by MHADA constituted the contact information for households at the time of application. Non-winners were attempted at these addresses. In cases where they had moved away, neighbors were asked for updated contact information, with which the enumerators once again attempted to contact non-winners. Winners resided at either the old addresses or new lottery buildings, as they were free to either inhabit their new property or rent it out. Lottery housing societies were thus first contacted to ascertain which of the winners were living at the apartments. Owner-occupiers were approached at the lottery apartments; landlords were approached at the addresses listed on the application using the procedure developed for non-winners. Care was taken to ensure that the same team and survey protocols were used to approach both winners and non-winners.

In all cases, we attempted to speak to the individual who had filled out application for the lottery home. The application required providing important and sensitive information such as PAN card numbers; as a result, 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 older children may be better able to use computers and the internet than their parents), enumerators were instructed to speak to the family's primary earner. Interviews were thus conducted on Sundays and weekday evenings. In my sample, 78%

[8]The organization hires its enumerators from local neighborhoods, which is a practice that was very 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.

Table 2: Proportion of members of each category in treatment and control groups after mapping with p-values for difference in proportions test.

| | Non-winners (C) | Winners (T) | p |
|---|---|---|---|
| *Caste/Occupation category* | | | |
| AR | 0.021 | 0.026 | 0.541 |
| CG | 0.021 | 0.019 | 0.829 |
| DF | 0.017 | 0.008 | 0.164 |
| DT | 0.008 | 0.011 | 0.524 |
| EX | 0.024 | 0.021 | 0.683 |
| FF | 0.006 | 0.015 | 0.129 |
| GP | 0.592 | 0.601 | 0.774 |
| JR | 0.021 | 0.032 | 0.249 |
| ME | 0.009 | 0.021 | 0.130 |
| MP/MLA/MLC | 0.002 | 0.008 | 0.179 |
| NT | 0.019 | 0.011 | 0.316 |
| PH | 0.030 | 0.023 | 0.447 |
| SC | 0.135 | 0.124 | 0.593 |
| SG | 0.062 | 0.047 | 0.284 |
| ST | 0.034 | 0.034 | 0.995 |
| | **1.00** | **1.00** | |
| *Lottery income category* | | | |
| EWS | 0.314 | 0.298 | 0.563 |
| LIG | 0.686 | 0.702 | 0.563 |
| | **1.00** | **1.00** | |
| *Apartment building #* | | | |
| 274 | 0.011 | 0.017 | 0.434 |
| 275 | 0.019 | 0.015 | 0.638 |
| 276 | 0.013 | 0.021 | 0.340 |
| 283 | 0.293 | 0.305 | 0.673 |
| 284 | 0.139 | 0.139 | 0.990 |
| 302 | 0.239 | 0.243 | 0.872 |
| 303 | 0.211 | 0.205 | 0.833 |
| 305 | 0.075 | 0.055 | 0.174 |
| | **1.00** | **1.00** | |

Table 3: Proportion of members of each category in full and mapped samples after mapping with p-values for difference in proportions test.

| | Full Sample | Mapped Sample | p |
|---|---|---|---|
| AR | 0.022 | 0.024 | 0.740 |
| CG | 0.021 | 0.020 | 0.886 |
| DF | 0.022 | 0.012 | 0.050 |
| DT | 0.014 | 0.009 | 0.250 |
| EX | 0.052 | 0.023 | 0.00 |
| FF | 0.028 | 0.010 | 0.00 |
| GP | 0.520 | 0.596 | 0.00 |
| JR | 0.028 | 0.026 | 0.779 |
| ME | 0.017 | 0.015 | 0.723 |
| MP/MLA/MLC | 0.004 | 0.005 | 0.883 |
| NT | 0.014 | 0.015 | 0.828 |
| PH | 0.026 | 0.026 | 0.947 |
| SC | 0.117 | 0.130 | 0.303 |
| SG | 0.053 | 0.055 | 0.902 |
| ST | 0.063 | 0.034 | 0.00 |
| | **1.00** | **1.00** | |
| *Lottery income category* | | | |
| EWS | 0.307 | 0.306 | 0.950 |
| LIG | 0.693 | 0.694 | 0.950 |
| | **1.00** | **1.00** | |
| *Apartment building #* | | | |
| 274 | 0.015 | 0.014 | 0.825 |
| 275 | 0.015 | 0.017 | 0.711 |
| 276 | 0.015 | 0.017 | 0.711 |
| 283 | 0.291 | 0.299 | 0.651 |
| 284 | 0.140 | 0.139 | 0.926 |
| 302 | 0.241 | 0.241 | 0.968 |
| 303 | 0.216 | 0.208 | 0.602 |
| 305 | 0.065 | 0.065 | 0.961 |
| | **1.00** | **1.00** | |

Table 4: Reasons for attrition with p-values for difference in proportions tests.

| | Control | Treatment | p |
|---|---|---|---|
| Surveyed | 413 | 421 | 0.373 |
| Address not found | 9 | 7 | 0.617 |
| Home demolished | 1 | 0 | 0.317 |
| Home locked | 5 | 11 | 0.131 |
| Respondent deceased | 1 | 0 | 0.373 |
| Refused | 14 | 20 | 0.294 |
| Unable to locate household that has moved | 19 | 10 | 0.090 |
| Incomplete survey | 37 | 31 | 0.453 |
| **Total** | **500** | **500** | - |

of respondents had filled out the application themselves.

## 5 The sample

The data collection process yielded a sample of 834, with 413 of the surveyed households in the control condition and 421 households in the treated condition. Full information on the number of households contacted in each stratum along with reasons for attrition can be found in Table 4. 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 0.395. Balance tests for fixed or baseline characteristics among the contacted sample can be found in Table 5. Importantly, there is an equal proportion of those belonging to the *Maratha* caste group, a dominant group in Mumbai and Maharashtra more generally.[9] In other words, winners and non-winners appear to be similar based on a number of fixed observable covariates and there is no evidence of corruption in the lottery or differential selection into the sample.[10]

Although these households fall into the EWS and LIG income categories for the housing lottery, a summary of the assets, housing quality, education levels, and tenure status of the control

[9] *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.

[10] In line with my pre-analysis plan, I also perform an omnibus test to judge whether observed covariate imbalance is larger than would normally be expected from chance alone. This test involves a regression of the treatment indicator on the covariates (Table A2) and calculation of a heteroscedasticity-robust Wald statistic for the hypothesis that all the coefficients on the covariates (other than block dummies) are zero. The p-value for this test is .39.

Table 5: Balance tests on household characteristics. The "Control" column presents means for winning households. The "Treatment" column presents the difference between winning and non-winning households estimated through an OLS regression of each variable on indicators for winning the lottery. Each regression includes an interaction with the centered block-level indicator for randomization groups. All regressions include HC2 errors. N=834.

| Variable | Control | Treatment | sd | Pr(>\|t\|) |
|---|---|---|---|---|
| OBC[1] | 0.150 | -0.021 | 0.035 | 0.543 |
| SC/ST[2] | 0.080 | -0.018 | 0.026 | 0.499 |
| Maratha[3] | 0.295 | 0.018 | 0.045 | 0.690 |
| Muslim | 0.090 | 0.006 | 0.029 | 0.852 |
| *Kutcha*[4]floor | 0.031 | 0.028 | 0.019 | 0.136 |
| *Kutcha*[4]roof | 0.039 | 0.001 | 0.018 | 0.945 |
| Originally from Mumbai | 0.809 | 0.062 | 0.039 | 0.114 |
| From the same ward as the apartment | 0.097 | 0.023 | 0.030 | 0.454 |

[1] Other backward class caste group members
[2] Scheduled caste or scheduled tribe groups, also known as Dalits.
[3] A dominant group in Mumbai and Maharashtra more generally.
[4] "*Kutcha*" means "rough" or "impermanent". Variable measured at time of application through recall.

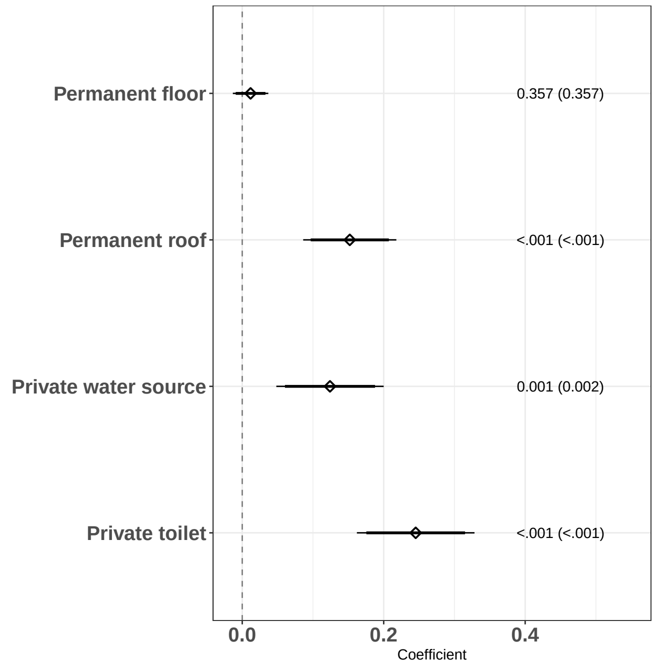
group respondents reveals that they should not be considered among the lowest income groups in the city (Table 6). They are educated, most have roughly 50% of the household employed and earning, and about 31% claim to have formal employment with either the government or private sector. Most live in dwellings with permanent floors and roofs. As none of the applicants, by rule, owns housing in the state of Maharashtra, they all living either in rental housing, homes with large families, or self-constructed homes to which they have no title. Many live in Mumbai chawls, or large buildings with shared taps and cheap, single room apartments. I thus describe the sample as lower-middle class and upwardly mobile. This description is corroborated by an interview conducted with the commissioner of the Mumbai Metropolitan Regional Development Authority, who saw the main beneficiaries the housing program to be lower-middle class households (Madan 2016). Citing experience from Latin American cities, Alan and Ward (1985, p 5) find that public housing interventions generally do not benefit a city's poorest citizens, as they simply cannot afford the requisite rent or mortgage.

Table 6: Summary of control group characteristics

| Variable | Control group mean[1] | (SD) |
|---|---|---|
| *Household Assets* | | |
| TV | 0.92 | (0.27) |
| Computer | 0.40 | (0.49) |
| Working refrigerator | 0.89 | (0.31) |
| Internet | 0.48 | (0.50) |
| Scooter/2 wheeler | 0.36 | (0.48) |
| Car | 0.06 | (0.24) |
| | | |
| *Housing quality* | | |
| Permanent floor | 0.97 | (0.18) |
| Semi-permanent roof | 0.17 | (0.38) |
| Permanent roof | 0.79 | (0.41) |
| Private tap | 0.73 | (0.45) |
| Private latrine | 0.62 | (0.49) |
| N Household members | 4.00 | 1.64 |
| | | |
| *Education and labor*[2] | | |
| Percentage of the household employed | 0.48 | (0.25) |
| Years of education | 11.73 | (3.85) |
| Unemployed | 0.03 | (0.18) |
| Wage laborer | 0.12 | (0.33) |
| Government employee | 0.18 | (0.38) |
| Private sector (informal)[3] | 0.43 | (0.50) |
| Private sector (formal) | 0.18 | (0.38) |
| | | |
| *Tenure status* | | |
| Migrants | 0.20 | (0.40) |
| Have always lived in Mumbai | 0.81 | (0.39) |
| Renting | 0.57 | (0.50) |
| Sharing/live in a joint family | 0.77 | (0.42) |

[1] Proportions may not add to 100% because of non-response to certain questions.
[2] Figures not referring to household means refer to the survey respondent.
[3] A job is considered to be in the formal sector if individuals are given letters, contracts, or notification of pension schemes upon being hired.

Figure 2: Treatment effects on enumerator observations of respondents' household quality



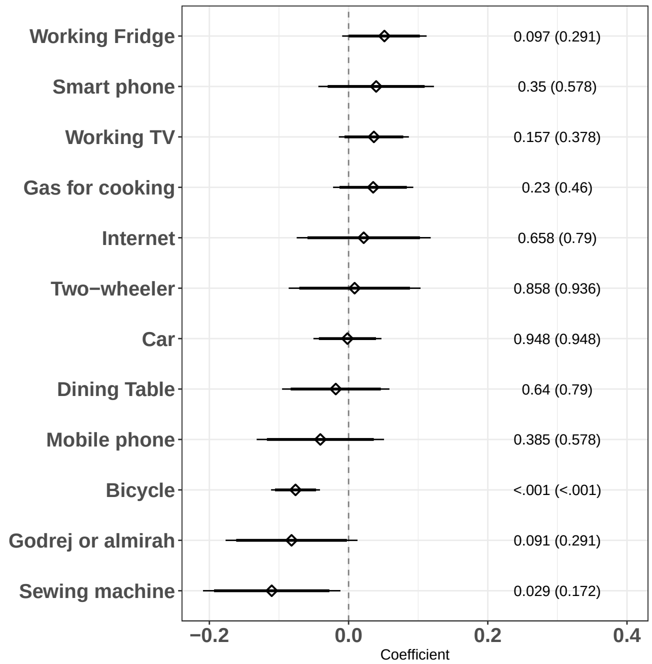

Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the false discovery rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Table A3.

# 6 Effects on wealth: housing quality and assets

As discussed, I first measure effects on common components of asset-based measures of wealth. Figure 2 shows improvements in household construction quality, with an estimated 15 percentage point increase in the incidence of permanent roofs, and roughly 12 percentage point and 24 percentage point decrease in the incidence of shared (vs. private) taps and toilets, respectively. The improvements could arise from relocation, or they could reflect the potential for winning households who do not relocate to use rental income to improve their own dwellings.

In contrast, I do not observe any detectable increase in ownership of small assets (Figure 3); in fact, I detect a small average decrease in bicycle ownership. Increases in ownership of these assets was hypothesized as a sign of the wealth transfer among those who chose the cash payout structure over the in-kind payout structure. This failure to detect an effect on ownership of small durable assets could be a result of high variance in the ways in which those making this choice spend the cash.

Figure 3: Treatment effects on asset ownership

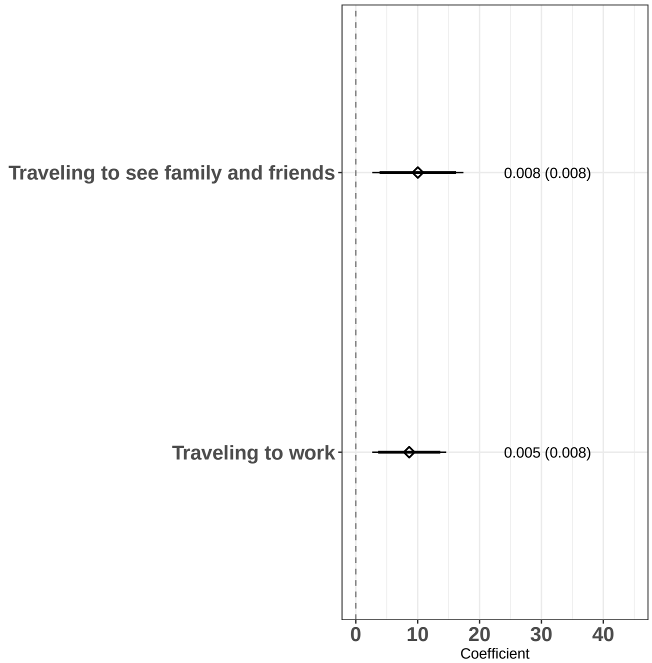



Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the false discovery rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Tables A4 and A5

## 7 Adverse effects: social isolation

Concerns that the program leads to social isolation are moderately supported. Figure 4 does indeed show an increase in the number of minutes taken to travel (one way) to work or to see family and friends. Reported times increase by about 8.6 and 9.7, minutes, respectively. It is possible that increased distances to work among owner-occupiers are offset by those who found new jobs, but only 10.5% of owner-occupiers report having sought new jobs after moving. Overall, then, the housing benefits seem accompanied by only minor changes in location, at least relative to the results reported by Barnhardt et al (2017), who study a relocation program in Ahmedabad, Gujarat. This may be due either to the relatively central location of the apartments in the MHADA program, or to the fact that those who find the new apartments too far from their

Figure 4: Treatment effects on reported travel times (minutes) to see closest friends and family and to go to work. Respondents were instructed to report the time based on the method of travel they normally use.



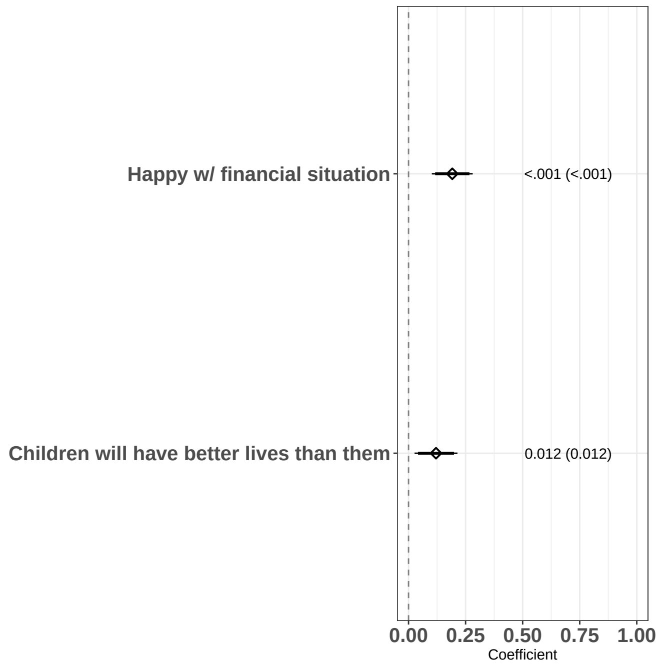

Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Holm (1979) step-down procedure for controlling for the false discovery rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Table A6.

baseline locations could simply choose not to relocate.

## 8 Effects on attitudes

Figure 5 next shows effects on self-reported attitudes and beliefs. First, I estimate a 19 percentage point increase in the likelihood that respondents will 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 18 percentage points more likely than non-winners to say that they believe their children will have better lives than them. These findings are complementary to research (e.g. Baird et al. 2013; Fernald et al. 2008; Haushofer and Fehr 2014; Haushofer and Shapiro 2013; Ozer et al. 2011; Ssewamala et al. 2009) that has found that income shocks can increase psychological well-being, happiness, and life satisfaction. They are somewhat sur-

Figure 5: Treatment effects on reported satisfaction with household financial situation and belief that children will have better lives than parents.




Bars show 90% and 95% confidence intervals. Group means available in Table **??**. P-values (with with p-values using a Benjamini-Hochberg correction for the false discovery rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Table A7.

prising, however, because winning households were, at the time of the survey, unable to realize the full wealth effect of the asset subsidy because of the rule prohibiting sale. The findings thus suggest that even gains of illiquid wealth may increase psychological well-being. The increase in socio-economic status associated with homeownership may also improve happiness and optimism about the future.

# 9 Effects on education

Next, I measure effects on educational attainment. First, I estimate that the household mean years of education increased by 0.464 years on average and that household maximum years of education increased by 0.570 years on average (Figure 6). Based on data from the Indian Human Development Survey II (IHDS-II) 2016), the intervention shifts households from roughly the 63rd to 73rd percentile of family-wise average years of education in Mumbai. The intervention shifts households from roughly the 81st to 84th percentile of family-wise average years of education in urban areas more generally.

Next, I use individual level data that is based on a census of every household member to estimate individual level treatment effects.[11] This dataset drops all individuals born *after* the household-relevant lottery was conducted. Table 7 shows that this sample remains reasonably balanced across treatment and control individuals.

Table 7: Balance tests on individual characteristics. The "Control" column presents means for control group individuals. The "Treatment" column presents the difference between control and treatment group individuals estimated through an OLS regression of each variable on indicators for winning the lottery. Each regression includes an interaction with the centered block-level indicator for randomization groups. Standard errors are clustered by families. N=3,127.

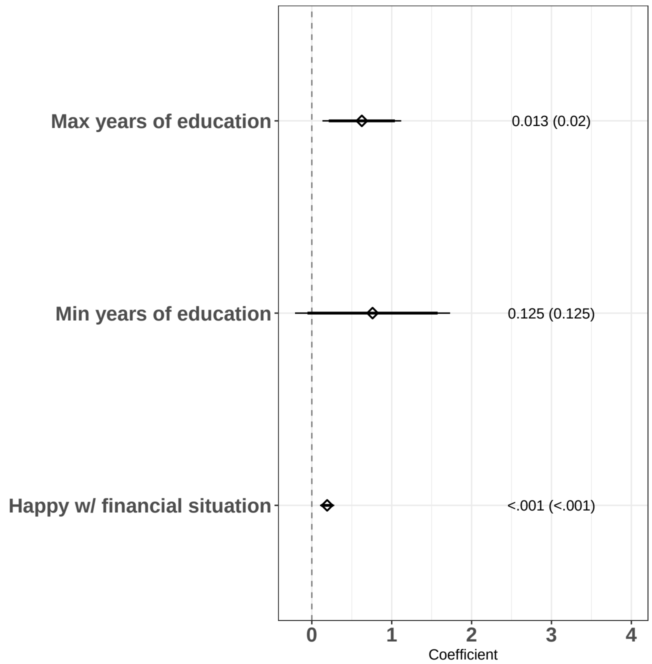
| Variable | Control | Treatment | sd | Pr(>\|t\|) |
|---|---|---|---|---|
| Age | 35.874 | 0.095 | 0.574 | 0.869 |
| Female | 0.485 | 0.00 | 0.011 | 0.998 |
| OBC[1] | 0.148 | -0.022 | 0.023 | 0.340 |
| SC/ST[2] | 0.084 | -0.029 | 0.021 | 0.165 |
| Maratha[3] | 0.292 | 0.024 | 0.032 | 0.457 |
| Muslim | 0.086 | 0.015 | 0.021 | 0.477 |
| *Kutcha*[4]floor | 0.028 | 0.030 | 0.023 | 0.188 |
| *Kutcha*[4]roof | 0.043 | 0.001 | 0.023 | 0.979 |
| From Mumbai | 0.812 | 0.051 | 0.026 | 0.052 |
| From the same ward as the apartment | 0.095 | 0.030 | 0.021 | 0.154 |

[1] Other backward class caste group members
[2] Scheduled caste or scheduled tribe groups, also known as Dalits.
[3] A dominant group in Mumbai and Maharashtra more generally.
[4] "*Kutcha*" means "rough" or "impermanent". Variable measured at time of application through recall.

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 7). The means at 0, 12, and 15 years likely represent barriers to beginning schooling, beginning post-secondary schooling, and beginning post-graduate schooling respectively. The mean at 10 years possibly represents the barriers to continuing education past 10th grade that are particularly high in this context. In India, students sit for the

[11] This individual-level analysis was not preregistered and can be considered exploratory.

Figure 6: Treatment effects on years of education in a household




Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the family-wise error rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Table A8.

All India Secondary School Examination (AISSE) at the end of grade 10. Only if they pass this exam can students continue with their studies. Those who pass also receive an AISSE Secondary School Completion Certificate, which is in itself a certification that may be used 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 in general; continuation of school after grade 10 should increase rates of both secondary school completion *and* rates of post-secondary school education.

I conduct an exploratory analysis to see whether winning the housing lottery increases the likelihood of overcoming each of these barriers (Table 8). I estimate regressions of completing one's education past these barriers on the treatment indicator. Belonging to a household that has won the lottery indeed increases the likelihood of moving past grades 10 and 12 and completing post-secondary education. It does not seem to have an effect on actually beginning one's education. I also include an interaction with the treatment indicator and an indicator for whether each individual turned 6, 16, and 18 in between being surveyed and the applicable lottery year. These

Figure 7: Distribution of individual years of education. “0" indicates membership in non-winning households and “1" indicates membership in winning households.

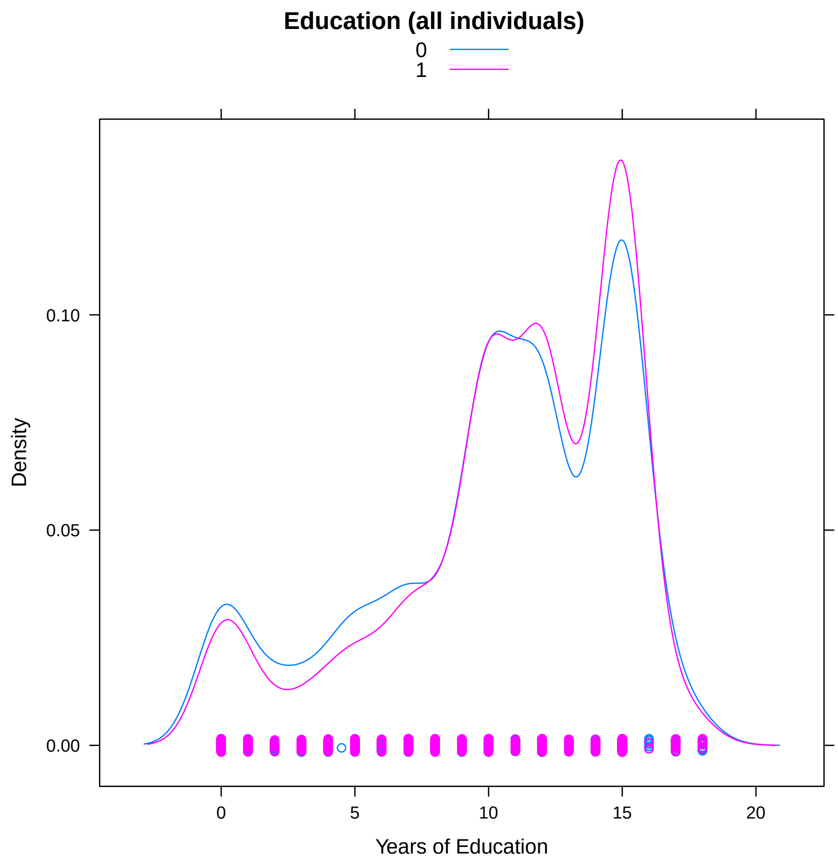
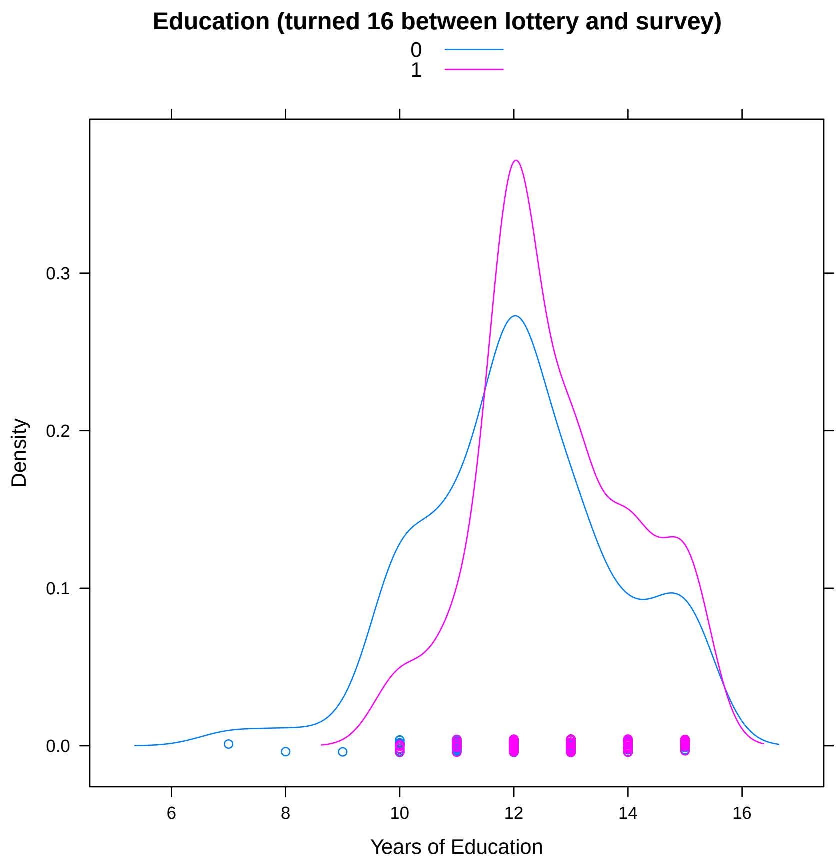


Figure 8: Distribution of individual years of education for those who turned 16 between the lottery and being surveyed. “0" indicates membership in non-winning households and “1" indicates membership in winning households.



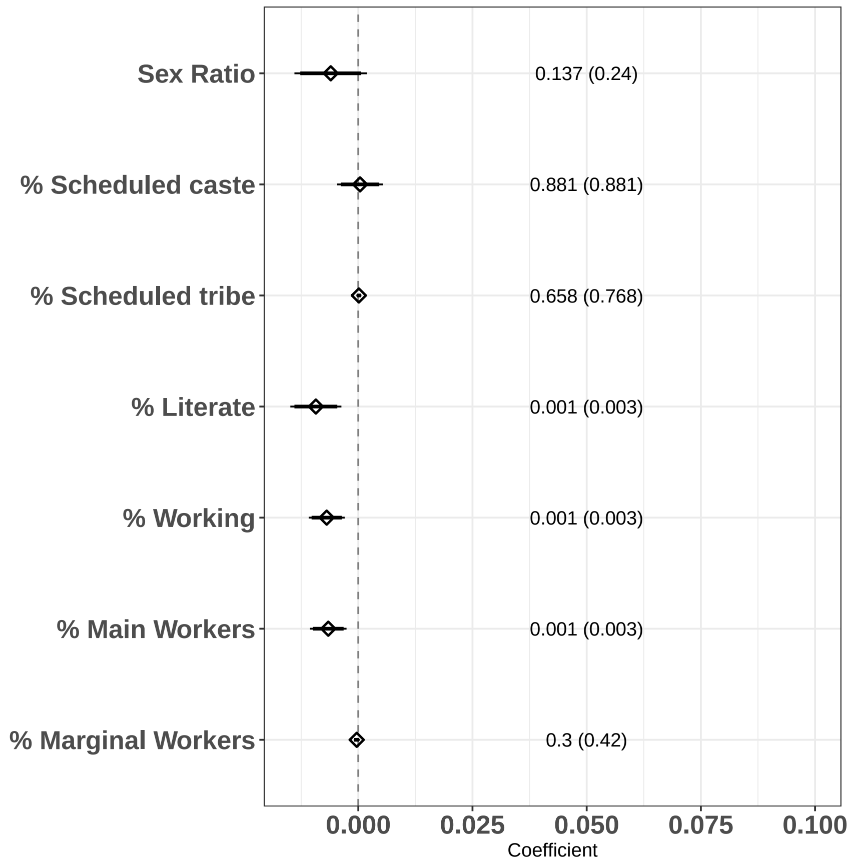

years were chosen with the assumption that most individuals complete 6, 16, and 18 years of age in their first, tenth, and twelfth years of education. In other words, I investigate whether the treatment effect is stronger for those who were the conventional ages for completing first, tenth, and twelfth grades in between the lottery and being surveyed. I see some evidence to suggest that the housing lottery's effect on completing grade ten is stronger among those who turned 16 after winning, but it is likely that the study is underpowered to detect each of the interaction effects. Nevertheless, Figure 8 clearly displays the 13% increase in the likelihood of completing grade 10 among members of winning households who turned 16 after the lottery.

The conceptual framework suggests that these increases in education are the result, through a variety of mechanisms, of gains in wealth. But the effects may also be driven by access to

Table 8: Regressions of individual completion of various years of education on the treatment indicator. All models include standard errors clustered at the household level and the treatment indicator interacted with mean centered block dummies. "TurnedX" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed.

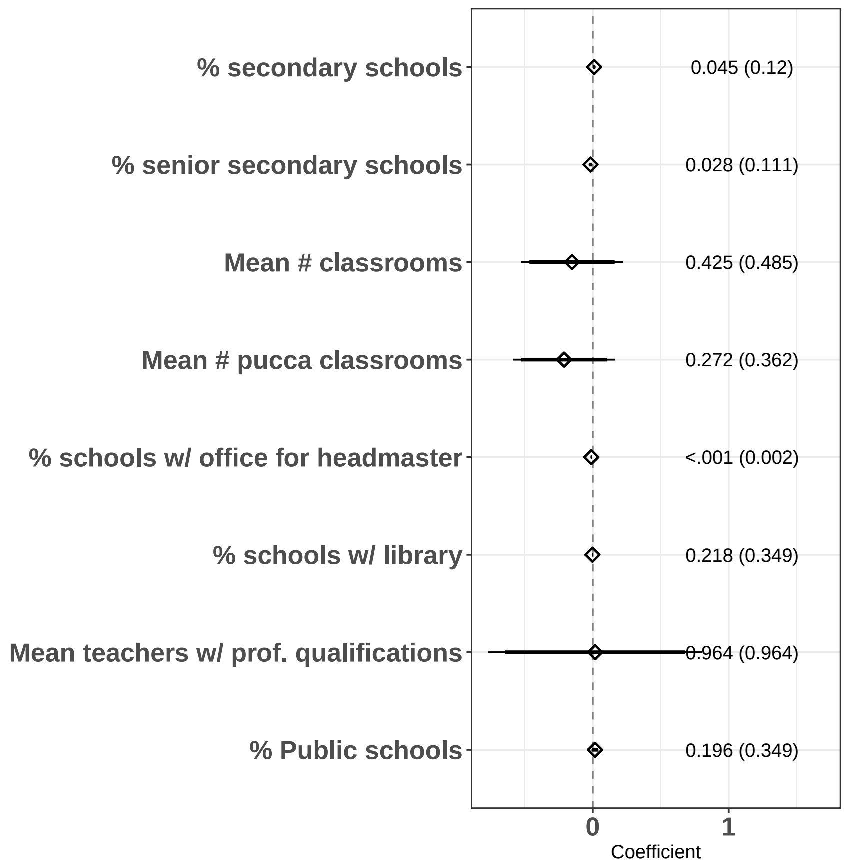
| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | I(>0 years) | | I(>10 years) | | I(>12 years) | | I(>=15 years) | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| T | 0.008 | 0.009 | $0.074^{***}$ | $0.059^{***}$ | $0.057^{***}$ | $0.040^{*}$ | $0.039^{**}$ | $0.036^{*}$ |
| | (0.008) | (0.009) | (0.018) | (0.019) | (0.020) | (0.021) | (0.019) | (0.020) |
| Turned6 | | $0.045^{***}$ | | | | | | |
| | | (0.016) | | | | | | |
| Turned16 | | | | $0.327^{***}$ | | | | |
| | | | | (0.042) | | | | |
| Turned18 | | | | | | $0.383^{***}$ | | |
| | | | | | | (0.051) | | |
| Turned21 | | | | | | | | $0.418^{***}$ |
| | | | | | | | | (0.048) |
| TXTurned6 | | −0.010 | | | | | | |
| | | (0.017) | | | | | | |
| TXTurned16 | | | | $0.093^{*}$ | | | | |
| | | | | (0.050) | | | | |
| TXTurned18 | | | | | | 0.105 | | |
| | | | | | | (0.067) | | |
| TXTurned21 | | | | | | | | 0.084 |
| | | | | | | | | (0.064) |
| Constant | $0.944^{***}$ | $0.942^{***}$ | $0.512^{***}$ | $0.494^{***}$ | $0.322^{***}$ | $0.302^{***}$ | $0.293^{***}$ | $0.264^{***}$ |
| | (0.006) | (0.006) | (0.013) | (0.013) | (0.013) | (0.014) | (0.013) | (0.014) |
| Observations | 3,127 | 3,127 | 3,127 | 3,127 | 3,127 | 3,127 | 3,127 | 3,127 |
| $R^2$ | 0.048 | 0.049 | 0.055 | 0.090 | 0.059 | 0.109 | 0.062 | 0.121 |
| Adjusted $R^2$ | 0.006 | 0.006 | 0.013 | 0.049 | 0.017 | 0.069 | 0.021 | 0.081 |
| Residual Std. Error | 0.224 (df = 2994) | 0.224 (df = 2992) | 0.494 (df = 2994) | 0.485 (df = 2992) | 0.474 (df = 2994) | 0.461 (df = 2992) | 0.459 (df = 2994) | 0.445 (df = 2992) |
| *Note:* | | | | | | | | $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01 |

Figure 9: Treatment effects on characteristics of wards in which households live.



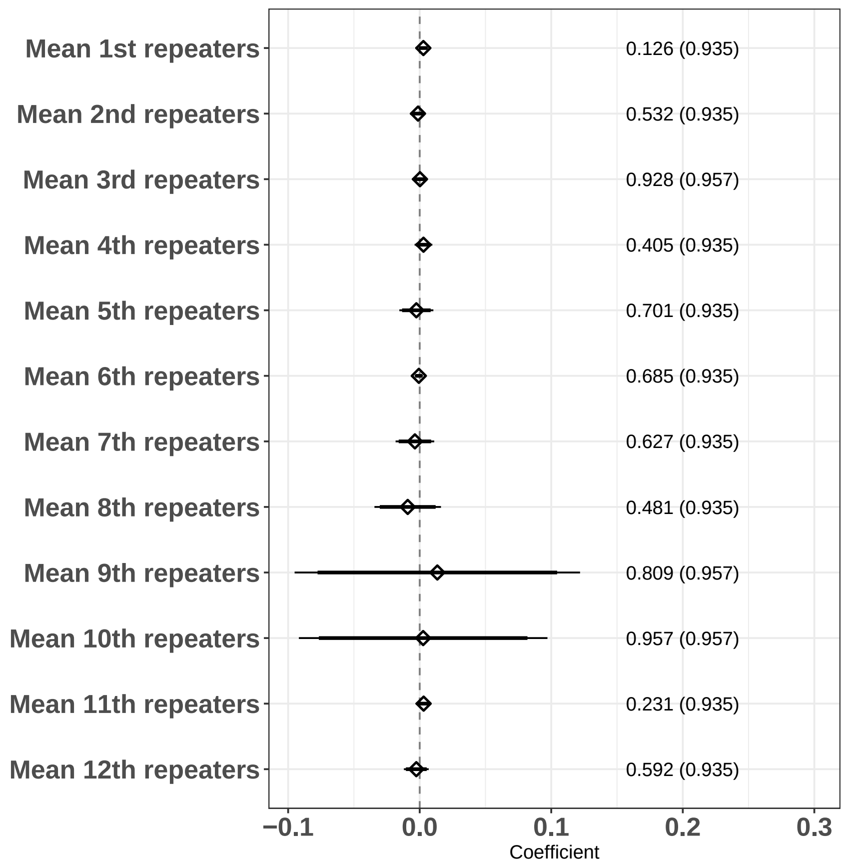

Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the family-wise error rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Tables **??**.

Figure 10: Treatment effects on characteristics of schools of postal codes in which households live.




Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the false discovery rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Tables A11 and A12.

Figure 11: Treatment effects on the rates of class repetition of schools in postal codes in which households live.




Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the family-wise error rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Tables A13 and A14.

better schools or increased access to schooling more generally among those who move. While Filmer and Pritchett (2001) find little variation in access to schools across geography in India, Chetty et al. (2016) find that moving to better neighborhoods improves educational outcomes among children who enjoy prolonged exposure to the new neighborhoods. The treatment effects on statistics (taken from India's 2011 Census) on the municipal ward in which households are located do not suggest that the intervention causes households to live in neighborhoods of higher socio-economic status or literacy rates; rather, households are living in areas with higher percentages of minorities, lower literacy rates, and fewer employed individuals (Figure 9). I also observe no treatment effects on various indicators of school quality calculated at the postal code level. These statistics were calculated by taking pin code level means of all of the schools in the Mumbai and Mumbai Suburban administrative districts.[12] In particular, the lottery does not cause households to live in pincodes with schools more likely to offer education through grades 10 or 12, or those with more amenities such as classrooms or libraries. In fact, it causes households to live in pincodes with schools that are 1 percentage point less likely to have offices for headmasters (an indicator for school size and formality)(Figure 10). Furthermore, the lottery does not cause households to live in neighborhoods with schools in which rates of repeating any grade level are higher or lower than the neighborhoods of non-winners (Figure 11). Overall, then, the lottery causes increases in educational attainment accompanied by no detectable changes in the type of schools available to winners.

## 10 Conclusion

In this paper, I propose that the main function of a subsidized housing sale program in Mumbai, India, is the transfer of wealth to eligible lower-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 is visible in improved housing quality among beneficiaries. Consistent with the idea that the program increases household wealth, the intervention also leads to more optimistic

[12]These data were generously provided by the Department of School Education and Literacy, Ministry of Human Resource Development, Government of India. Find more information at http://schoolreportcards.in/SRC-New/.

attitudes about the future and and increases educational attainment among beneficiaries.

The program evaluated is part of a larger set of policy instruments that subsidize the price of homes; for example, mortgage subsidies may be more familiar in the context of the United States. Housing subsidizes themselves can be seen as a type of asset transfer. Governments may often subsidize other durable assets such as tractors or sewing machines, or they may subsidize savings through programs such as college funds. The point is that a government may subsidize a certain asset because its use or ownership may be politically desirable or optimal fro a social planner's perspective. The subsidy essentially functions as a wealth transfer; the way in which this wealth transfer can be *measured* will depend on the vehicle for wealth, the asset, in question. If the subsidy is sizable, it may also have measurable effects on household attitudes and decision-making.

## References


(2014). *World urbanization prospects: The 2014 revision, highlights. department of economic and social affairs*. Technical report, United Nations.

(2015). *Slums in India: A Statistical Compendium*. Technical report, Ministry of Housing and Urban Poverty Alleviation, Government of India.

Alan, G. & Ward, P. (1985). *Housing, the state and the poor: policy and practice in three Latin American Cities*. Cambridge University Press, Cambridge.

Baird, S., De Hoop, J., & Ozler, B. (2013). Income shocks and adolescent mental health. *Journal of Human Resources*, 48(2), 370–403.

Banerjee, A., Duflo, E., Goldberg, N., Karlan, D., Osei, R., ParientÃl', W., Shapiro, J., Thuysbaert, B., & Udry, C. (2015). A multifaceted program causes lasting progress for the very poor: Evidence from six countries. *Science*, 348(6236), 1260799.

Barnhardt, S., Field, E., & Pande, R. (2017). Moving to Opportunity or Isolation? Network Effects of a Randomized Housing Lottery in Urban India. *American Economic Journal: Applied Economics*, 9(1), 1–32.

Burra, S. (2005). Towards a pro-poor framework for slum upgrading in Mumbai, India. *Environment and Urbanization*, 17(1), 67–88.

Chetty, R., Hendren, N., & Katz, L. F. (2016). The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment. *American Economic Review*, 106(4), 855–902.

Davila, R. L., McCarthy, A. S., Gondwe, D., HealthCare, B., Kirdruang, P., & Sharma, U. (2014). *Water, walls and bicycles: wealth index composition using census microdata*. Minnesota Population Center, University of Minnesota Minneapolis.

Desai, S. & Vanneman, R. (2016). National Council of Applied Economic Research, New Delhi. India Human Development Survey (IHDS), 2005. ICPSR22626-v11. *Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor]*, (pp. 02–16).

Di Tella, R., Galiant, S., & Schargrodsky, E. (2007). The formation of beliefs: evidence from the allocation of land titles to squatters. *The Quarterly Journal of Economics*, 122(1), 209–241.

Feder, G. & Feeny, D. (1991). Land Tenure and Property Rights: Theory and Implications for Development Policy. *The World Bank Economic Review*, 5(1), 135–153.

Fernald, L. C., Hamad, R., Karlan, D., Ozer, E. J., & Zinman, J. (2008). Small individual loans and mental health: a randomized controlled trial among South African adults. *BMC Public Health*, 8(1), 409.

Field, E. (2005). Property rights and investment in urban slums. *Journal of the European Economic Association*, 3(2-3), 279–290.

Filmer, D. & Pritchett, L. H. (2001). Estimating Wealth Effects without Expenditure Data-or Tears: An Application to Educational Enrollments in States of India. *Demography*, 38(1), 115–132.

Galiani, S. & Schargrodsky, E. (2010). Property rights for the poor: Effects of land titling. *Journal of Public Economics*, 94(9), 700–729.

Ganapati, S. (2010). Enabling Housing Cooperatives: Policy Lessons from Sweden, India and the United States. *International Journal of Urban and Regional Research*, 34(2), 365–380.

Gulyani, S. & Bassett, E. M. (2007). Retrieving the baby from the bathwater: slum upgrading in Sub-Saharan Africa. *Environment and Planning C: Government and Policy*, 25(4), 486–515.

Haushofer, J. & Fehr, E. (2014). On the psychology of poverty. *Science*, 344(6186), 862–867.

Haushofer, J. & Shapiro, J. (2013). Welfare effects of unconditional cash transfers: Evidence from a randomized controlled trial in Kenya. *Unpublished Working Paper*.

Holm, S. (1979). A Simple Sequentially Rejective Multiple Test Procedure. *Scandinavian Journal of Statistics*, 6(2), 65–70.

Imbens, G. W. & KolesÃąr, M. (2015). Robust Standard Errors in Small Samples: Some Practical Advice. *The Review of Economics and Statistics*, 98(4), 701–712.

Imparato, I. & Ruster, J. (2003). *Slum upgrading and participation: Lessons from Latin America*. The World Bank.

Lin, W. (2013). Agnostic notes on regression adjustments to experimental data: Reexamining FreedmanâĂŹs critique. *The Annals of Applied Statistics*, 7(1), 295–318.

MacKinnon, J. G. & White, H. (1985). Some heteroskedasticity-consistent covariance matrix estimators with improved finite sample properties. *Journal of econometrics*, 29(3), 305–325.

Madan, U. (2016). Personal interview with the chief commissioner of Mumbai's Metropolitan Region Development Authority.

Mani, A., Mullainathan, S., Shafir, E., & Zhao, J. (2013). Poverty impedes cognitive function. *science*, 341(6149), 976–980.

Maslow, A. H. (1943). A theory of human motivation. *Psychological review*, 50(4), 370.

Ozer, E. J., Fernald, L. C., Weber, A., Flynn, E. P., & VanderWeele, T. J. (2011). Does alleviating poverty affect mothersâĂŹ depressive symptoms? A quasi-experimental investigation of MexicoâĂŹs Oportunidades programme. *International Journal of Epidemiology*, 40(6), 1565–1576.

Pornchokchai, S. (2008). *Housing Finance Mechanisms in India*. UN-HABITAT. Google-Books-ID: YWbm7JK6dLEC.

Shinde, U. (2019). Interview with individuals working in the office of the comptroller and auditor general of India.

Ssewamala, F. M., Han, C.-K., & Neilands, T. B. (2009). Asset ownership and health and mental health functioning among AIDS-orphaned adolescents: Findings from a randomized clinical trial in rural Uganda. *Social Science & Medicine*, 69(2), 191–198.

Stegman, M. A., Quercia, R. G., & Davis, W. (2007). The wealth-creating potential of homeownership. *Chasing the American dream: New perspectives on affordable homeownership*, (pp. 171–191).

Sukumar, G. (2001). Institutional potential of housing cooperatives for low-income households: The case of India. *Habitat International*, 25(2), 147–174.

Vale, L. J. (2007). The ideological origins of affordable homeownership efforts. *Chasing the American dream: New perspectives on affordable homeownership*, (pp. 15–40).

van Dijk, W. (2019). The Socio-Economic Consequences of Housing Assistance. *Working Paper.*

# A Appendix

Table A1: Caste/occupation category codes

| Code | Category |
|---|---|
| AR | Artist |
| CG | Central govt. servant occupying staff qrts. |
| DF | Families of defense personall |
| DT | Denotified tripes |
| EX | Ex-servicemen and dependents |
| FF | Freedom fighters |
| GP | General public |
| JR | Journalists |
| ME | MHADA employees |
| MP/MLA/MLC | Ex-members of parliament, legislative assemblies, legislative councils |
| NT | Nomadic tribes |
| PH | Handicapped persons |
| SC | Scheduled castes |
| SG | State government employees who have retired |
| ST | Scheduled tribes |

Table A2: Regression of treatment indicator on the covariates

| Covariates[1] | Winning the housing lottery |
|---|---|
| OBC | $-0.053$ |
| | (0.057) |
| SCST | 0.060 |
| | (0.071) |
| *Maratha* caste member | $-0.041$ |
| | (0.046) |
| Muslim | 0.002 |
| | (0.066) |
| *Kutcha*[2]floor | $0.200^{*}$ |
| | (0.118) |
| *Kutcha*[2]roof | $-0.277^{**}$ |
| | (0.124) |
| From Mumbai | $-0.003$ |
| | (0.047) |
| From the same ward as the apartment building | 0.051 |
| | (0.061) |
| Block dummies? | Yes |
| F Statistic (df = 91; 742) | 1.2046 |
| N | 834 |
| $R^2$ | 0.120 |
| Adjusted $R^2$ | 0.015 |
| Residual Std. Error | 0.497 (df = 744) |

$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

[1] Unless otherwise specified, all covariates are dummy variables.

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

Table A3: Regression estimates of treatment effects on enumerator observations of respondents' household quality. All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Permanent floor | | Permanent roof | | Private water source | | Private toilet | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| T | −0.008 | 0.012 | 0.148*** | 0.152*** | 0.130*** | 0.124*** | 0.250*** | 0.245*** |
| | (0.017) | (0.013) | (0.035) | (0.034) | (0.039) | (0.039) | (0.043) | (0.042) |
| OBC | | 0.019 | | −0.021 | | 0.009 | | 0.009 |
| | | (0.015) | | (0.039) | | (0.045) | | (0.049) |
| SCST | | 0.018 | | 0.014 | | 0.089 | | 0.009 |
| | | (0.019) | | (0.050) | | (0.058) | | (0.063) |
| Maratha | | 0.002 | | −0.005 | | −0.012 | | 0.002 |
| | | (0.012) | | (0.031) | | (0.036) | | (0.040) |
| Muslim | | −0.003 | | 0.044 | | 0.031 | | 0.032 |
| | | (0.017) | | (0.046) | | (0.053) | | (0.058) |
| Kutcha floor | | −0.613*** | | −0.106 | | 0.041 | | −0.166 |
| | | (0.032) | | (0.083) | | (0.096) | | (0.105) |
| Kutcha roof | | −0.001 | | −0.467*** | | −0.231** | | −0.177 |
| | | (0.033) | | (0.087) | | (0.100) | | (0.110) |
| From Mumbai | | −0.021* | | −0.0001 | | 0.092** | | 0.113*** |
| | | (0.012) | | (0.033) | | (0.038) | | (0.041) |
| From same ward as apt | | −0.021 | | −0.022 | | 0.039 | | 0.100* |
| | | (0.016) | | (0.042) | | (0.048) | | (0.053) |
| Constant | 0.975*** | 1.000*** | 0.775*** | 0.791*** | 0.748*** | 0.669*** | 0.603*** | 0.510*** |
| | (0.011) | (0.014) | (0.023) | (0.038) | (0.026) | (0.044) | (0.028) | (0.048) |
| Observations | 834 | 834 | 834 | 834 | 834 | 834 | 834 | 834 |
| $R^2$ | 0.174 | 0.546 | 0.211 | 0.274 | 0.195 | 0.215 | 0.228 | 0.253 |
| Adjusted $R^2$ | 0.018 | 0.454 | 0.063 | 0.127 | 0.043 | 0.056 | 0.082 | 0.102 |
| Residual Std. Error | 0.169 (df = 701) | 0.126 (df = 693) | 0.343 (df = 701) | 0.331 (df = 693) | 0.384 (df = 701) | 0.382 (df = 693) | 0.423 (df = 701) | 0.418 (df = 693) |

*Note:* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table A4: Regression estimates of treatment effects on asset ownership (no covariates). All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Fridge | Smartphone | TV | Gas | Internet | 2whlr | Car | Dining tbl | Mobile | Bicycle | Almirah | Sewing mchn |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| T | −0.098** | −0.021 | 0.034 | 0.047 | 0.037 | 0.008 | 0.024 | −0.110** | −0.028 | 0.037 | 0.001 | 0.001 |
| | (0.049) | (0.039) | (0.026) | (0.031) | (0.029) | (0.033) | (0.049) | (0.050) | (0.047) | (0.042) | (0.025) | (0.048) |
| Constant | 0.711*** | 0.206*** | 0.914*** | 0.879*** | 0.886*** | 0.878*** | 0.379*** | 0.513*** | 0.696*** | 0.751*** | 0.064*** | 0.357*** |
| | (0.032) | (0.026) | (0.017) | (0.020) | (0.019) | (0.022) | (0.032) | (0.033) | (0.031) | (0.028) | (0.016) | (0.032) |
| Observations | 834 | 834 | 834 | 834 | 834 | 834 | 834 | 834 | 834 | 834 | 834 | 834 |
| $R^2$ | 0.140 | 0.188 | 0.167 | 0.132 | 0.188 | 0.167 | 0.171 | 0.166 | 0.166 | 0.179 | 0.171 | 0.158 |
| Adjusted $R^2$ | −0.022 | 0.035 | 0.010 | −0.032 | 0.035 | 0.010 | 0.015 | 0.009 | 0.008 | 0.025 | 0.015 | −0.0004 |
| Residual Std. Error (df = 701) | 0.481 | 0.390 | 0.255 | 0.308 | 0.291 | 0.328 | 0.486 | 0.495 | 0.464 | 0.417 | 0.246 | 0.480 |

*Note:* *p<0.1; **p<0.05; ***p<0.01

Table A5: Regression estimates of treatment effects on asset ownership (with covariate adjustment). All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Fridge | Smart phone | TV | Gas | Internet | Two-wheeler | Car | Dining table | Mobile phone | Bicycle | Almirah | Sewing machine |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| T | −0.082* | −0.018 | 0.036 | 0.051* | 0.035 | 0.010 | 0.022 | −0.110** | −0.041 | 0.040 | −0.002 | 0.009 |
| | (0.048) | (0.039) | (0.026) | (0.031) | (0.029) | (0.033) | (0.049) | (0.050) | (0.047) | (0.042) | (0.025) | (0.048) |
| OBC | 0.071 | 0.025 | 0.037 | 0.088** | 0.044 | 0.100*** | 0.024 | −0.049 | 0.035 | 0.088* | 0.038 | 0.058 |
| | (0.056) | (0.046) | (0.030) | (0.036) | (0.034) | (0.038) | (0.057) | (0.058) | (0.054) | (0.049) | (0.029) | (0.056) |
| SCST | 0.112 | −0.007 | 0.084** | 0.015 | 0.051 | 0.068 | 0.077 | −0.016 | −0.039 | 0.012 | −0.004 | 0.199*** |
| | (0.072) | (0.059) | (0.038) | (0.046) | (0.044) | (0.049) | (0.073) | (0.075) | (0.070) | (0.063) | (0.037) | (0.072) |
| Maratha | −0.076* | −0.022 | 0.033 | 0.019 | 0.012 | 0.048 | 0.057 | 0.014 | 0.050 | 0.028 | 0.023 | 0.091** |
| | (0.045) | (0.037) | (0.024) | (0.029) | (0.028) | (0.031) | (0.046) | (0.047) | (0.044) | (0.040) | (0.023) | (0.045) |
| Muslim | 0.044 | 0.108** | 0.074** | 0.067 | 0.057 | 0.045 | 0.034 | −0.033 | 0.078 | −0.003 | 0.010 | 0.114* |
| | (0.066) | (0.053) | (0.035) | (0.042) | (0.040) | (0.045) | (0.067) | (0.068) | (0.063) | (0.058) | (0.034) | (0.066) |
| kutchafloor | −0.053 | −0.165* | −0.028 | −0.165** | −0.090 | −0.048 | 0.014 | −0.086 | −0.043 | −0.054 | 0.013 | −0.121 |
| | (0.120) | (0.098) | (0.064) | (0.077) | (0.073) | (0.082) | (0.122) | (0.125) | (0.116) | (0.105) | (0.062) | (0.120) |
| kutcharoof | −0.114 | 0.100 | −0.052 | −0.009 | 0.025 | 0.018 | −0.065 | −0.014 | −0.053 | 0.025 | 0.013 | 0.053 |
| | (0.125) | (0.102) | (0.066) | (0.080) | (0.076) | (0.086) | (0.127) | (0.130) | (0.121) | (0.110) | (0.065) | (0.125) |
| always | −0.134*** | 0.061 | 0.026 | 0.042 | 0.074** | 0.030 | 0.091* | 0.036 | 0.132*** | −0.003 | 0.056** | 0.013 |
| | (0.047) | (0.038) | (0.025) | (0.030) | (0.029) | (0.032) | (0.048) | (0.049) | (0.046) | (0.041) | (0.024) | (0.047) |
| same | −0.046 | −0.090* | −0.080** | −0.033 | 0.041 | −0.020 | −0.109* | −0.038 | 0.180*** | 0.044 | −0.048 | −0.117* |
| | (0.060) | (0.049) | (0.032) | (0.039) | (0.037) | (0.041) | (0.061) | (0.063) | (0.058) | (0.053) | (0.031) | (0.060) |
| Constant | 0.816*** | 0.162*** | 0.873*** | 0.826*** | 0.806*** | 0.818*** | 0.291*** | 0.500*** | 0.559*** | 0.726*** | 0.013 | 0.294*** |
| | (0.055) | (0.044) | (0.029) | (0.035) | (0.033) | (0.037) | (0.055) | (0.057) | (0.053) | (0.048) | (0.028) | (0.055) |
| Observations | 834 | 834 | 834 | 834 | 834 | 834 | 834 | 834 | 834 | 834 | 834 | 834 |
| $R^2$ | 0.165 | 0.203 | 0.189 | 0.153 | 0.202 | 0.178 | 0.184 | 0.171 | 0.189 | 0.184 | 0.184 | 0.177 |
| Adjusted $R^2$ | −0.004 | 0.042 | 0.025 | −0.018 | 0.041 | 0.012 | 0.019 | 0.003 | 0.025 | 0.019 | 0.019 | 0.011 |
| Residual Std. Error (df = 693) | 0.477 | 0.388 | 0.253 | 0.305 | 0.290 | 0.327 | 0.485 | 0.497 | 0.460 | 0.418 | 0.246 | 0.477 |

*Note:* *p<0.1; **p<0.05; ***p<0.01

Table A6: Regression estimates of treatment effects on reported travel times (minutes) to see closest friends and family and to go to work. Respondents were instructed to report the time based on the method of travel they normally used. All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Traveling to see family and friends | | Traveling to work | |
| | (1) | (2) | (3) | (4) |
| T | 8.641** | 10.020*** | 8.250*** | 8.631*** |
| | (3.785) | (3.757) | (3.045) | (3.051) |
| OBC | | 2.896 | | 0.299 |
| | | (3.235) | | (3.370) |
| SCST | | −0.443 | | 2.296 |
| | | (4.006) | | (4.339) |
| Maratha | | 1.040 | | 2.714 |
| | | (2.645) | | (2.713) |
| Muslim | | 3.643 | | −5.989 |
| | | (3.466) | | (3.867) |
| Kutcha floor | | −2.296 | | −6.904 |
| | | (7.376) | | (7.336) |
| Kutcha roof | | −3.246 | | 5.620 |
| | | (6.347) | | (7.419) |
| From Mumbai | | −11.850*** | | −4.030 |
| | | (2.522) | | (2.817) |
| From same ward as apt | | −0.087 | | 9.311** |
| | | (3.010) | | (3.632) |
| Constant | 9.154*** | 16.790*** | 43.520*** | 45.040*** |
| | (2.198) | (3.119) | (2.041) | (3.315) |
| Observations | 419 | 419 | 780 | 780 |
| $R^2$ | 0.370 | 0.419 | 0.151 | 0.169 |
| Adjusted $R^2$ | 0.170 | 0.214 | −0.015 | −0.005 |
| Residual Std. Error | 18.480 (df = 317) | 17.970 (df = 309) | 27.620 (df = 652) | 27.490 (df = 644) |
| *Note:* | | | | *p<0.1; **p<0.05; ***p<0.01 |

Table A7: Regressions estimates for treatment effects on reported satisfaction with household financial situation and belief that children will have better lives than parents. All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Happy w/ financial situation | | Think children will have better lives | |
| | (1) | (2) | (3) | (4) |
| T | 0.200$^{***}$ | 0.192$^{***}$ | 0.122$^{**}$ | 0.120$^{**}$ |
| | (0.046) | (0.046) | (0.048) | (0.048) |
| OBC | | −0.066 | | 0.030 |
| | | (0.053) | | (0.056) |
| SCST | | −0.048 | | −0.141$^{**}$ |
| | | (0.068) | | (0.071) |
| Maratha | | 0.036 | | 0.087$^{*}$ |
| | | (0.043) | | (0.045) |
| Muslim | | 0.062 | | 0.005 |
| | | (0.062) | | (0.065) |
| Kutcha floor | | −0.124 | | 0.035 |
| | | (0.113) | | (0.119) |
| Kutcha roof | | −0.129 | | −0.080 |
| | | (0.118) | | (0.124) |
| From Mumbai | | 0.160$^{***}$ | | −0.011 |
| | | (0.045) | | (0.047) |
| From same ward as apt | | −0.037 | | −0.071 |
| | | (0.057) | | (0.060) |
| Constant | 0.596$^{***}$ | 0.483$^{***}$ | 0.561$^{***}$ | 0.563$^{***}$ |
| | (0.030) | (0.052) | (0.032) | (0.054) |
| Observations | 834 | 834 | 834 | 834 |
| $R^2$ | 0.165 | 0.195 | 0.193 | 0.209 |
| Adjusted $R^2$ | 0.008 | 0.033 | 0.041 | 0.049 |
| Residual Std. Error | 0.457 (df = 701) | 0.451 (df = 693) | 0.475 (df = 701) | 0.473 (df = 693) |
| *Note:* | | | | $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01 |

Table A8: Regression estimates for treatment effects on years of education in a household. All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Mean years education | | Min years education | | Max years education | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| T | 0.680** | 0.680** | 0.751 | 0.760 | 0.620** | 0.626** |
| | (0.284) | (0.283) | (0.493) | (0.495) | (0.254) | (0.252) |
| OBC | | 0.198 | | 0.100 | | 0.361 |
| | | (0.328) | | (0.574) | | (0.292) |
| SCST | | 0.894** | | 1.535** | | 0.658* |
| | | (0.422) | | (0.738) | | (0.375) |
| Maratha | | 0.658** | | 0.773* | | 0.612*** |
| | | (0.265) | | (0.464) | | (0.236) |
| Muslim | | 0.037 | | 0.262 | | 0.077 |
| | | (0.385) | | (0.673) | | (0.342) |
| Kutcha floor | | 0.373 | | 1.491 | | −0.717 |
| | | (0.703) | | (1.229) | | (0.624) |
| Kutcha roof | | −1.422* | | −0.584 | | −1.643** |
| | | (0.732) | | (1.282) | | (0.651) |
| From Mumbai | | 0.195 | | −0.227 | | 0.344 |
| | | (0.276) | | (0.484) | | (0.246) |
| From same ward as apt | | −0.538 | | −0.948 | | 0.137 |
| | | (0.354) | | (0.619) | | (0.315) |
| Constant | 10.240*** | 9.862*** | 5.848*** | 5.684*** | 13.660*** | 13.140*** |
| | (0.187) | (0.319) | (0.325) | (0.559) | (0.167) | (0.284) |
| Observations | 834 | 834 | 834 | 834 | 834 | 834 |
| $R^2$ | 0.159 | 0.182 | 0.156 | 0.168 | 0.143 | 0.180 |
| Adjusted $R^2$ | 0.001 | 0.017 | −0.003 | −0.0002 | −0.018 | 0.014 |
| Residual Std. Error | 2.816 (df = 701) | 2.793 (df = 693) | 4.893 (df = 701) | 4.887 (df = 693) | 2.522 (df = 701) | 2.482 (df = 693) |
| *Note:* | | | | | | *p<0.1; **p<0.05; ***p<0.01 |

Table A9: Regression estimates for treatment effects of characteristics of wards in which households live (no covariates). All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | | | | |
|---|---|---|---|---|---|---|---|
| | HH size | Sex ratio | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| T | −0.006 | 0.001 | 0.0001 | −0.010*** | −0.007*** | −0.007*** | −0.0003 |
| | (0.004) | (0.003) | (0.0003) | (0.003) | (0.002) | (0.002) | (0.0003) |
| Constant | 0.853*** | 0.064*** | 0.010*** | 0.813*** | 0.404*** | 0.381*** | 0.023*** |
| | (0.003) | (0.002) | (0.0002) | (0.002) | (0.001) | (0.001) | (0.0002) |
| Observations | 834 | 834 | 834 | 834 | 834 | 834 | 834 |
| $R^2$ | 0.278 | 0.253 | 0.335 | 0.370 | 0.273 | 0.287 | 0.281 |
| Adjusted $R^2$ | 0.142 | 0.113 | 0.210 | 0.251 | 0.136 | 0.152 | 0.145 |
| Residual Std. Error (df = 701) | 0.040 | 0.025 | 0.003 | 0.029 | 0.021 | 0.021 | 0.003 |

*Note:* $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01

Table A10: Regression estimates for treatment effects of characteristics of wards in which households live (with covariate adjustment). All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | | | | |
|---|---|---|---|---|---|---|---|
| | HH size | Sex ratio | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| T | −0.006 | 0.001 | 0.0001 | −0.010*** | −0.007*** | −0.007*** | −0.0003 |
| | (0.004) | (0.003) | (0.0003) | (0.003) | (0.002) | (0.002) | (0.0003) |
| OBC | 0.853*** | 0.064*** | 0.010*** | 0.813*** | 0.404*** | 0.381*** | 0.023*** |
| | (0.003) | (0.002) | (0.0002) | (0.002) | (0.001) | (0.001) | (0.0002) |
| Observations | 834 | 834 | 834 | 834 | 834 | 834 | 834 |
| $R^2$ | 0.278 | 0.253 | 0.335 | 0.370 | 0.273 | 0.287 | 0.281 |
| Adjusted $R^2$ | 0.142 | 0.113 | 0.210 | 0.251 | 0.136 | 0.152 | 0.145 |
| Residual Std. Error (df = 701) | 0.040 | 0.025 | 0.003 | 0.029 | 0.021 | 0.021 | 0.003 |

*Note:* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table A11: Regression estimates for treatment effects on average school quality by postal code of where interviewed households are living (no covariates). All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | | | | |
|---|---|---|---|---|---|---|---|
| | % secondary | % sr. secondary | # of classrooms | Mean # pucca classrooms | % w/ head office | % w/ library | # teachers w/ prof qual. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| T | 0.012** | −0.016** | −0.132 | −0.199 | −0.002 | 0.051 | 0.015 |
| | (0.005) | (0.007) | (0.190) | (0.190) | (0.002) | (0.400) | (0.013) |
| Constant | 0.307*** | 0.124*** | 8.279*** | 8.029*** | 0.985*** | 14.460*** | 0.325*** |
| | (0.004) | (0.005) | (0.124) | (0.125) | (0.001) | (0.262) | (0.008) |
| Observations | 832 | 832 | 832 | 832 | 832 | 832 | 832 |
| $R^2$ | 0.237 | 0.155 | 0.155 | 0.156 | 0.188 | 0.154 | 0.216 |
| Adjusted $R^2$ | 0.094 | −0.004 | −0.004 | −0.002 | 0.036 | −0.004 | 0.069 |
| Residual Std. Error (df = 700) | 0.053 | 0.071 | 1.880 | 1.889 | 0.016 | 3.964 | 0.128 |

*Note:* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table A12: Regression estimates for treatment effects on average school quality by postal code of where interviewed households are living (with covariate adjustment). All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | | | | |
|---|---|---|---|---|---|---|---|
| | % secondary | % sr. secondary | # of classrooms | Mean # pucca classrooms | % w/ head office | % w/ library | # teachers w/ prof qual. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| T | 0.011** | −0.016** | −0.152 | −0.211 | −0.002 | 0.018 | 0.017 |
| | (0.005) | (0.007) | (0.191) | (0.191) | (0.002) | (0.402) | (0.013) |
| OBC | −0.005 | 0.009 | 0.080 | 0.156 | 0.002 | 0.120 | 0.006 |
| | (0.006) | (0.008) | (0.221) | (0.222) | (0.002) | (0.466) | (0.015) |
| SCST | 0.0004 | −0.007 | 0.475* | 0.510* | 0.002 | 0.374 | 0.010 |
| | (0.008) | (0.011) | (0.284) | (0.285) | (0.002) | (0.599) | (0.019) |
| Maratha | −0.010* | 0.001 | −0.058 | −0.036 | 0.004*** | −0.452 | 0.016 |
| | (0.005) | (0.007) | (0.179) | (0.179) | (0.001) | (0.377) | (0.012) |
| Muslim | −0.004 | 0.001 | 0.100 | 0.103 | −0.002 | 0.009 | −0.002 |
| | (0.007) | (0.010) | (0.260) | (0.261) | (0.002) | (0.548) | (0.018) |
| Kutcha floor | 0.012 | −0.013 | 0.852* | 0.652 | −0.001 | 1.756* | −0.013 |
| | (0.013) | (0.018) | (0.473) | (0.475) | (0.004) | (0.997) | (0.032) |
| Kutcha roof | −0.004 | −0.001 | −0.089 | 0.001 | −0.002 | −0.579 | 0.025 |
| | (0.014) | (0.019) | (0.493) | (0.495) | (0.004) | (1.040) | (0.033) |
| From Mumbai | 0.004 | 0.001 | 0.133 | 0.174 | 0.002 | −0.128 | −0.010 |
| | (0.005) | (0.007) | (0.186) | (0.187) | (0.002) | (0.393) | (0.013) |
| From same ward as apt | 0.020*** | 0.002 | −0.045 | −0.188 | −0.003 | 0.428 | −0.037** |
| | (0.007) | (0.009) | (0.239) | (0.240) | (0.002) | (0.504) | (0.016) |
| Constant | 0.305*** | 0.123*** | 8.112*** | 7.823*** | 0.983*** | 14.560*** | 0.329*** |
| | (0.006) | (0.008) | (0.215) | (0.216) | (0.002) | (0.453) | (0.015) |
| Observations | 832 | 832 | 832 | 832 | 832 | 832 | 832 |
| $R^2$ | 0.255 | 0.158 | 0.164 | 0.165 | 0.209 | 0.163 | 0.225 |
| Adjusted $R^2$ | 0.105 | −0.011 | −0.003 | −0.002 | 0.051 | −0.005 | 0.070 |
| Residual Std. Error (df = 692) | 0.053 | 0.072 | 1.880 | 1.889 | 0.016 | 3.965 | 0.128 |
| *Note:* | | | | | | | $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$ |

Table A13: Treatment effects on the mean rates of class repetition of schools in pincodes in which households live (no covariates). All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1st | 2nd | 3rd | 4th | 5th | 6th | 7th | 8th | 9th | 10th | 11th | 12th |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| T | 0.003 | −0.001 | 0.00003 | 0.003 | −0.004 | −0.001 | −0.005 | −0.011 | 0.010 | 0.002 | 0.003 | −0.003 |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.007) | (0.002) | (0.007) | (0.013) | (0.055) | (0.048) | (0.003) | (0.005) |
| Constant | 0.002 | $0.003^{**}$ | $0.006^{***}$ | 0.003 | $0.014^{***}$ | $0.005^{***}$ | $0.016^{***}$ | $0.024^{***}$ | $0.355^{***}$ | $0.163^{***}$ | $0.009^{***}$ | $0.022^{***}$ |
| | (0.001) | (0.001) | (0.002) | (0.002) | (0.004) | (0.001) | (0.005) | (0.008) | (0.036) | (0.031) | (0.002) | (0.003) |
| Observations | 832 | 832 | 832 | 832 | 832 | 832 | 832 | 832 | 832 | 832 | 832 | 832 |
| $R^2$ | 0.199 | 0.193 | 0.183 | 0.171 | 0.274 | 0.258 | 0.276 | 0.281 | 0.242 | 0.239 | 0.303 | 0.164 |
| Adjusted $R^2$ | 0.049 | 0.042 | 0.030 | 0.016 | 0.138 | 0.119 | 0.141 | 0.147 | 0.100 | 0.097 | 0.172 | 0.007 |
| Residual Std. Error (df = 700) | 0.018 | 0.020 | 0.024 | 0.034 | 0.065 | 0.016 | 0.074 | 0.127 | 0.549 | 0.475 | 0.025 | 0.047 |

*Note:* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table A14: Treatment effects on the mean rates of class repetition of schools in pincodes in which households live (with covariate adjustment). All regressions include treatment indicator interactions with mean centered block dummies.

| | *Dependent variable:* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1st | 2nd | 3rd | 4th | 5th | 6th | 7th | 8th | 9th | 10th | 11th | 12th |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| T | 0.003 | −0.001 | 0.0002 | 0.003 | −0.003 | −0.001 | −0.004 | −0.009 | 0.013 | 0.003 | 0.003 | −0.003 |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.007) | (0.002) | (0.007) | (0.013) | (0.055) | (0.048) | (0.003) | (0.005) |
| OBC | 0.001 | −0.002 | 0.001 | 0.001 | 0.004 | 0.002 | 0.005 | 0.005 | −0.088 | −0.052 | 0.001 | 0.004 |
| | (0.002) | (0.002) | (0.003) | (0.004) | (0.008) | (0.002) | (0.009) | (0.015) | (0.064) | (0.056) | (0.003) | (0.006) |
| SCST | −0.00005 | −0.004 | −0.001 | −0.004 | 0.004 | 0.001 | 0.004 | 0.005 | 0.033 | 0.030 | −0.004 | 0.006 |
| | (0.003) | (0.003) | (0.004) | (0.005) | (0.010) | (0.002) | (0.011) | (0.019) | (0.082) | (0.072) | (0.004) | (0.007) |
| Maratha | 0.001 | −0.002 | 0.001 | −0.001 | 0.001 | 0.001 | 0.001 | −0.003 | −0.027 | −0.015 | −0.001 | 0.004 |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.006) | (0.002) | (0.007) | (0.012) | (0.052) | (0.045) | (0.002) | (0.005) |
| Muslim | 0.0001 | −0.001 | 0.001 | −0.001 | 0.005 | 0.002 | 0.006 | 0.009 | 0.031 | 0.054 | 0.002 | 0.004 |
| | (0.002) | (0.003) | (0.003) | (0.005) | (0.009) | (0.002) | (0.010) | (0.018) | (0.076) | (0.066) | (0.003) | (0.007) |
| Kutcha floor | −0.003 | −0.004 | −0.002 | −0.003 | −0.002 | −0.002 | −0.002 | 0.002 | −0.035 | 0.015 | −0.004 | −0.003 |
| | (0.005) | (0.005) | (0.006) | (0.009) | (0.016) | (0.004) | (0.019) | (0.032) | (0.137) | (0.120) | (0.006) | (0.012) |
| Kutcha roof | 0.001 | 0.014*** | 0.005 | 0.0004 | −0.010 | −0.001 | −0.012 | −0.023 | 0.258* | 0.157 | 0.007 | −0.002 |
| | (0.005) | (0.005) | (0.006) | (0.009) | (0.017) | (0.004) | (0.019) | (0.033) | (0.143) | (0.125) | (0.006) | (0.012) |
| From Mumbai | −0.001 | −0.001 | −0.001 | 0.0001 | −0.011* | −0.003** | −0.013* | −0.020 | 0.016 | 0.013 | −0.004 | 0.002 |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.006) | (0.002) | (0.007) | (0.013) | (0.054) | (0.047) | (0.002) | (0.005) |
| From same ward as apt | −0.002 | −0.001 | −0.004 | −0.003 | −0.010 | −0.003 | −0.011 | −0.015 | −0.212*** | −0.116* | 0.012*** | 0.002 |
| | (0.002) | (0.003) | (0.003) | (0.004) | (0.008) | (0.002) | (0.009) | (0.016) | (0.069) | (0.060) | (0.003) | (0.006) |
| Constant | 0.003 | 0.005** | 0.007** | 0.004 | 0.022*** | 0.006*** | 0.026*** | 0.040*** | 0.371*** | 0.164*** | 0.011*** | 0.018*** |
| | (0.002) | (0.002) | (0.003) | (0.004) | (0.007) | (0.002) | (0.008) | (0.015) | (0.062) | (0.054) | (0.003) | (0.005) |
| Observations | 832 | 832 | 832 | 832 | 832 | 832 | 832 | 832 | 832 | 832 | 832 | 832 |
| $R^2$ | 0.202 | 0.207 | 0.186 | 0.173 | 0.280 | 0.268 | 0.283 | 0.286 | 0.257 | 0.248 | 0.324 | 0.166 |
| Adjusted $R^2$ | 0.042 | 0.048 | 0.023 | 0.007 | 0.135 | 0.121 | 0.139 | 0.142 | 0.108 | 0.097 | 0.188 | −0.001 |
| Residual Std. Error (df = 692) | 0.018 | 0.020 | 0.024 | 0.034 | 0.065 | 0.016 | 0.074 | 0.127 | 0.546 | 0.476 | 0.025 | 0.047 |

*Note:* *p<0.1; **p<0.05; ***p<0.01