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

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Abstract

This paper uses original survey data from a housing lottery in Mumbai to examine the household-level effects of the subsidized sale of housing to lower-middle class urban families 3-5 years after program implementation. The intervention consists of a wealth transfer manifested, for some, through a stream of housing benefits. Beneficiaries can also rent out or eventually sell the housing to receive in-kind or lump sum benefits. The program leads individuals to express greater optimism about the future and increases educational attainment, particularly at the secondary and post-secondary levels. Effects on educational attainment occur in spite of the fact that winners live in areas with poorer school quality and lower levels of literacy. More generally, beneficiaries experience some adverse effects of relocation. This is among the first studies of the effects of the subsidized sale of an asset in a developing country.

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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 very common in India, the subsidized sale of homes to lower-middle class households.

This intervention is motivated in part by a growing urban population; in India, 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.[1] As a result, governments have also attempted to increase formal housing supply by encouraging private developers to build and by constructing housing themselves.[2] In fact, state-level development boards have spearheaded programs that sell, rather than rent, subsidized units to eligible households in every major city in India.[3] Moreover, in 2015, India's federal government claimed a housing shortfall of over 18 million units to motivate a plan, Pradhan Mantri Awas Yojana (P-MAY), 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. Similar policies exist in Brazil, Uruguay, Nigeria, Kenya, and elsewhere.

The subsidy in these programs constitutes a wealth transfer to beneficiaries. Households experience this transfer in any combination of three payout structures: through a stream of in-

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

[2]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.

[3]These boards were 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, thereby motivating the sale and not rental of units in 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 central government's development plans moved towards policies favoring the facilitation of private construction after the economic liberalization of the 1990s.

kind benefits enjoyed when living in the home, through cash benefits when renting the home out, or lump-sum through sale. The possibility of receiving cash benefits makes these programs very different from rental or relocation programs such as the one studied by Barnhardt et al. (2017) or the United States' Moving to Opportunity program (Chetty et al. 2016; Ludwig et al. 2001; Ludwig et al. 2013; Katz et al. 2001) that offer the first option only.

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 intervention has positive effects on an asset-based index of wealth; I further find that this effect is likely driven by improved housing quality among winners.

Winners also report feeling happier about their financial situations, expect better lives for their children, and are more likely to plan to stay in the city permanently. 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; beneficiaries report attitudes markedly differing from those of non-beneficiaries even though they have not realized the wealth transfer that the subsidized asset sale entails.

I also estimate effects on household consumption of healthcare and education. The intervention increases the likelihood of households seeking medical advice, even without a detectable increase in the likelihood of illness. Furthermore, the average number of years of education among winning households is 0.24 standard deviations, or 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 see these effects in spite of the fact that winners tend to live in less literate neighborhoods with lower school quality. These findings are consistent with research showing that wealth predicts educational attainment.

Finally, given that the in-kind payment structure is bundled with relocation, winners do experience some adverse effects. As mentioned, they tend to live in worse neighborhoods than non-winners. They also live further from work and are marginally less likely to have friends or family in their neighborhoods.

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. Furthermore, this program entails a particularly large wealth transfer, and this study focuses on a wide array of outcomes. 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.[4] 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 Devel-

[4] 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.

opment Authority (MHADA), a subsidiary of the Maharashtra Housing and Area Development Authority that uses the same acronym. MHADA runs subsidized housing programs for economically weaker section (EWS) and low-income group (LIG)[5] urban residents who 1) do not own housing, and 2) who have lived in the state of Maharashtra for at least 15 continuous years within the 20 years prior to the sale. Winners have access to loans from a state-owned bank and most take out 15-year mortgages. While the downpayment and mortgage leave this program out of the reach of many of the city's poorest residents, it gives eligible 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 #	N winners	Lottery Year	Group	Neighborhood	Area[1]	Cost[2]	Downpayment[3]
274	14	2012	LIG	Charkop	402	2,725,211	15,050
275	14	2012	LIG	Charkop	462	3,130,985	15,050
276	14	2012	LIG	Charkop	403	2,731,441	15,050
283	270	2012	LIG	Malvani	306	1,936,700	15,050
284	130	2012	LIG	Vinobha Bhave Nagar	269	1,500,000	15,050
302	227	2014	EWS	Mankhurd	269	1,626,500	15,200
303	201	2014	LIG	Vinobha Bhave Nagar	269	2,038,300	25,200
305	61	2014	EWS	Magathane	269	1,464,500	15,200

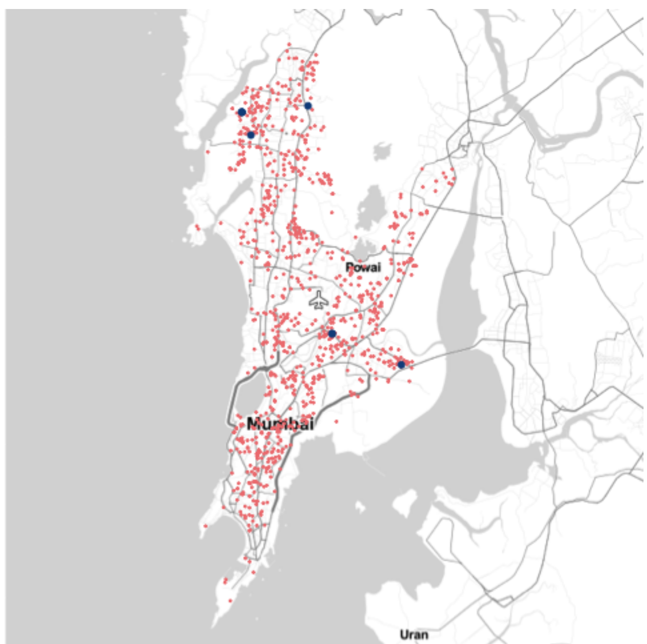
[1] In square feet. Refers to "carpet area", or the actual apartment area and excludes common space.
[2] In INR with the cost stated in the lottery year.
[3] In INR with the cost stated in the lottery year. Includes application fee of Rs.200.

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 near major highways and transit lines. Each is within

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

walking distance of the Mumbai suburban rail network, the main network that millions of city residents use to commute every day. Figure 1 shows the location of the 2012 and 2014 EWS and LIG MHADA apartment buildings and households in the sample at the time of application. Households were permitted to choose the building for which they submitted an application. 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. These parameters, of course, are likely to vary with the specific program in question.

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



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, and the median monthly rental income net of mortgage payments is Rs.2000, or roughly 30 USD. Finally, households do not pay taxes on their dwelling for five years after possession.

As mentioned above, beneficiaries were 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. In fact, a handful control group respondents complained about paying brokers who claimed to be able to help “fix” the lottery and were subsequently never heard from again. Applicants also applied with their Permanent Account Numbers (PAN), which are linked to their bank accounts. Before conducting the lottery, MHADA officials used the PAN numbers to check both whether individuals had applied multiple times for the same lottery round and whether or not they met the criteria for eligibility.[6]

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.[7] Households can make this choice based a wide variety of parameters. This paper is agnostic about the choice and instead seeks to estimate

[6]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.

[7]As discussed Beneficiaries are technically not permitted to take advantage of this last option during the time of the study.

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 an asset-based measure of wealth (Filmer and Pritchett 2001). I also estimate treatment effects on individual components of this index. This is because 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.

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.[8] Thus the specific vehicle of this wealth transfer, a home, also has the potential to yield improvements in psychological well-being among beneficiaries.

To better understand the extent to which the wealth transfer affects household decision-making, I also estimate effects on educational attainment and healthcare consumption. Multiple

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

mechanisms are potentially at work here. As households grow wealthier, they may be willing to invest more into healthcare and education. Moreoverthey may derive greater utility from gains from education and health that are higher on Maslow's (1943) hierarchy of needs, such as self-actualization. As discussed above, the intervention may also improve psychological well-being, which in itself may decrease discount rates and increase investments into one's future.

Finally, I estimate potential adverse effects of this particular in-kind transfer, particularly on travel times to work, neighborhood quality, and the presence of friends and family nearby. Studies of home rental programs that effectively provide the option of an in-kind transfer only have found that the resulting relocation can lead to broken social networks (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 thus involve greater constraints on the way in which transfers are used, which is potentially a reason for their adverse effects. For this reason, I expect to find relatively small effects here, as the subsidized sale programs allow greater flexibility in their payout structures.

For household level effects, I follow my pre-analysis plan and estimate the treatment effect, β, 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:

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

When an outcome is not binary or categorical, treatment effects are reported for the standardized variables. Also, 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. 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.[9] 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.

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.[10] In other words, one non-winner was drawn for each winner, there were an equal number of treated and control units in each block, and I accessed total 1,848 addresses. These addresses were mapped using Google Maps. Addresses that were incomplete (42), outside of Greater Mumbai (600), or could not be mapped (146) 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 mapping procedure to have favored wealthier applicants be-

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

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

cause 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 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, I worked with a Mumbai-based organization to contact the households and conduct interviews.[11] 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

[11]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**	

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.

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**	-

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% of respondents had reportedly completed the applications 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 treatment 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.[12] In other words, winners and non-winners appear to be similar based on a number of fixed observable covariates.[13]

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.

Table 6 provides a summary of the main outcome variables of interest among the surveyed control group. The sample is reasonably well-educated, roughly half of each family is employed, and most live in dwellings with permanent floors and roofs. In addition to these outcome statistics, about 31% of respondents claim that the household's main earner has formal employment with either the government or private sector. About 43% of respondents claim that the household's main earner has informal employment with the private sector.[14] As none of the applicants, by rule, owns housing in the state of Maharashtra, 57% claim to live in rental housing, and 77% report living in homes shared with with extended families. I thus describe the sample as lower-

[12] *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.

[13] 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.

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

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. Recall, however, that the sample mapped and surveyed is likely wealthier than the entire pool of applicants on average.

6 Results

This section presents treatment effects as outlined by the conceptual framework. For reference, control group summary statistics for all outcomes can be found in Table 6.

6.1 Effects on wealth

Following Filmer and Pritchett (2001), I estimate treatment effects on an index of asset ownership, or the first component of a principal components analysis of the housing quality and asset ownership variables listed in Table 6.[15] As shown by Figure 2, there is a positive treatment effect on this variable of about 0.43, or 0.25 standard deviations.

To better understand and interpret this treatment effect, I next measure effects on the components of this index. Figure 3 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 25 percentage point increase in the incidence of private (vs. shared) taps and toilets, respectively. The improvements likely arise from relocation, but could also reflect the potential for winning households who do not relocate to use rental income to improve their own dwellings.

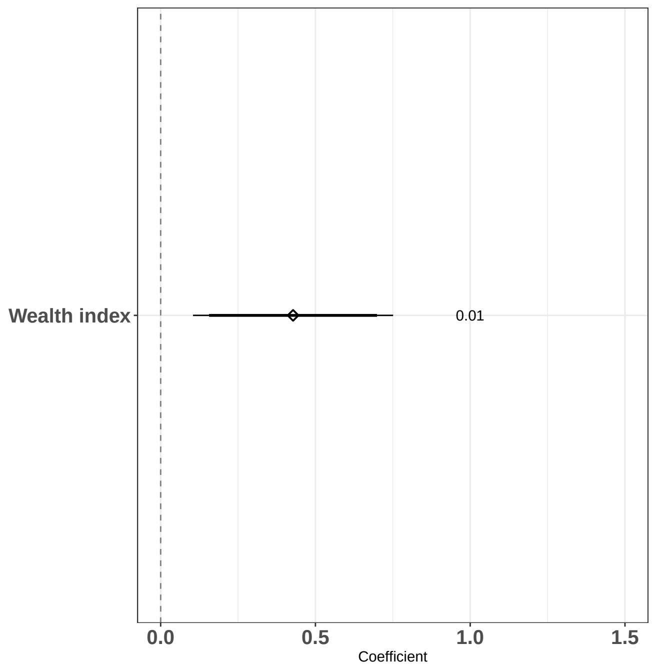
In contrast, I do not observe any detectable increase in ownership of small assets (Figure 4); in fact, I detect a small decrease in the likelihood of bicycle ownership (low even among control

[15]The eigenvalue and cumulative proportion of variance explained by this first component are 2.87 and 0.17, respectively.

Table 6: Summary of outcome distributions control group members

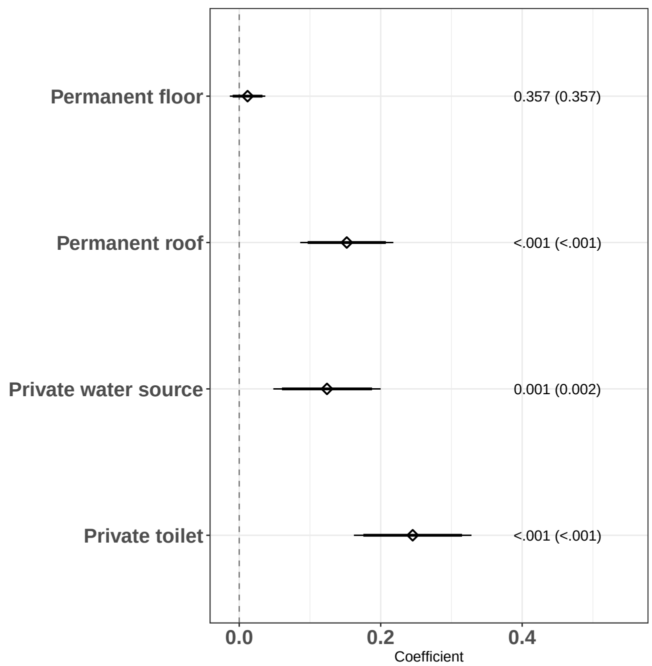
Statistic	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Wealth index	−0.270	1.879	−6.894	−1.255	0.892	3.479
Housing quality						
Permanent floor	0.964	0.187	0	1	1	1
Permanent roof	0.787	0.410	0	1	1	1
Private water source	0.729	0.445	0	0	1	1
Private toilet	0.620	0.486	0	0	1	1
Asset ownership						
Almirah	0.692	0.462	0	0	1	1
Dining table	0.203	0.403	0	0	0	1
TV	0.908	0.289	0	1	1	1
Fridge	0.874	0.332	0	1	1	1
Gas	0.877	0.329	0	1	1	1
Computer	0.390	0.488	0	0	1	1
Internet	0.472	0.500	0	0	1	1
Sewing machine	0.150	0.358	0	0	0	1
Mobile	0.673	0.470	0	0	1	1
Smartphone	0.731	0.444	0	0	1	1
Car	0.058	0.234	0	0	0	1
2 wheeler	0.356	0.479	0	0	1	1
Bicycle	0.044	0.204	0	0	0	1
Attitudes						
Happy w/ financial situation	0.630	0.484	0	0	1	1
Children will have better lives than them	0.559	0.497	0	0	1	1
Would never leave Mumbai	0.772	0.420	0	1	1	1
Education						
Max years of education	13.847	2.662	0	12	15	20
Min years of education	5.834	4.875	0	0	10	18
Mean years of education	10.346	2.875	0.000	8.750	12.250	18.000
Health outcomes and healthcare in the past month						
N illnesses	0.667	1.946	0.000	0.000	1.000	30.000
% visiting homeopathic doctor	0.032	0.177	0.000	0.000	0.000	1.000
% visiting medically certified doctor	0.950	0.218	0.000	1.000	1.000	1.000
% consulting family members	0.005	0.070	0.000	0.000	0.000	1.000
% conducting home remedies	0.293	0.456	0	0	1	1
Location based outcomes						
% of HH employed	0.481	0.252	0	0.3	0.7	1
Minutes taken to get to work	47.058	29.523	0.000	30.000	60.000	240.000
Feel safe/very safe in neighborhood	0.944	0.230	0	1	1	1
Have family/friends in neighborhood	0.562	0.497	0	0	1	1

Figure 2: Treatment effects on an asset-based index of wealth.



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.

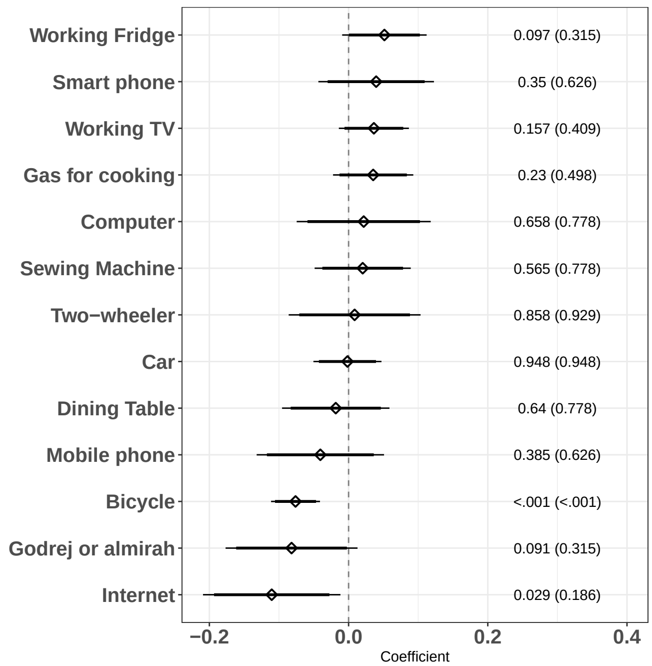
Figure 3: Treatment effects on aspects of respondents' household quality as observed by enumerators.



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 A4.

group members). 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 variation in the ways in which those making this choice spend the cash.

Figure 4: Treatment effects on asset ownership

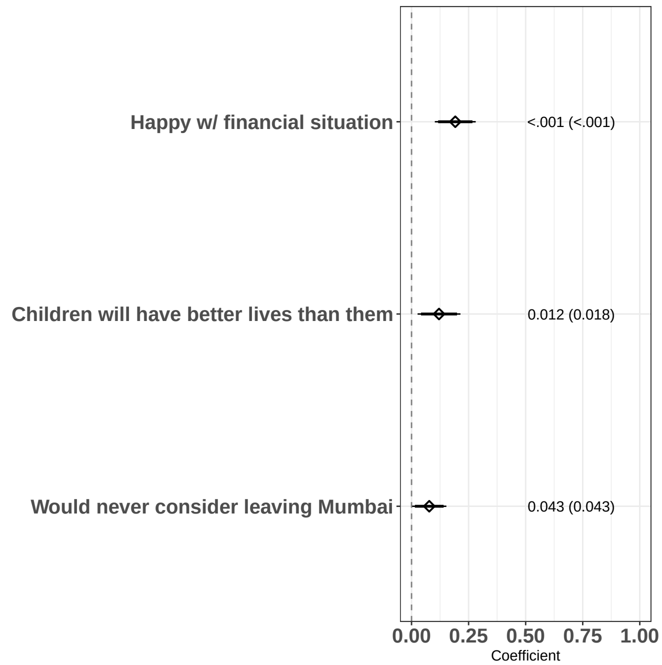


Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the false discovery rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Tables A5 and A6

6.2 Effects on attitudes

Figure 5 next shows effects on self-reported attitudes and beliefs. First, I estimate that winners are 19 percentage points more likely than non-winners to claim to be happy with the financial situation of the household. Winners also appear to believe they will pass on their good fortune to their children, as they are roughly 12 percentage points more likely than non-winners to say that they believe their children will have better lives than them. Finally, they are roughly 8 percentage points more likely than non-winners to respond that they “would never leave" (as opposed to responding that they “plan to leave in the future" or “might leave in the future") when asked

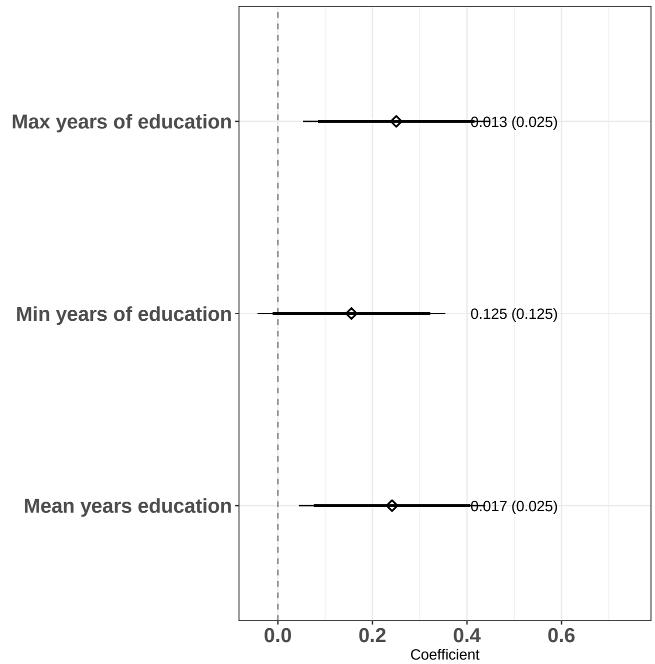
Figure 5: Treatment effects on likelihood of respondents saying they are “happy" (the highest level on a 3-point scale) satisfaction with the household financial situation, say “yes" when asked if their children will have better lives than them, and say “never" (as opposed to “maybe" or “planning to leave") when asked if they would ever leave Mumbai.



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 A7.

if would ever consider relocating from Mumbai, suggesting increased time horizons. These findings are complementary to research (e.g. Baird et al. 2013; Fernald et al. 2008; Haushofer and Fehr 2014; Haushofer and Shapiro 2013; Ozer et al. 2011; Ssewamala et al. 2009) that has found that income shocks can increase psychological well-being, happiness, and time horizons. They are somewhat surprising, 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.

Figure 6: Treatment effects on years of education in a household. All dependent variables are standardized.



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.

6.3 Effects on education

Next, I measure effects on educational attainment. First, I estimate that the household mean years of education differs among treatment and control households by 0.24 standard deviations on average and that household maximum years of education increased by 0.25 years on average (Figure 6). These effects correspond to .68 and .62 year differences, respectively. Based on data from the Indian Human Development Survey 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.[16] This dataset drops all individuals born *after* the

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

household-relevant lottery was conducted. Table 7 shows that this sample remains reasonably balanced across treatment and control individuals.

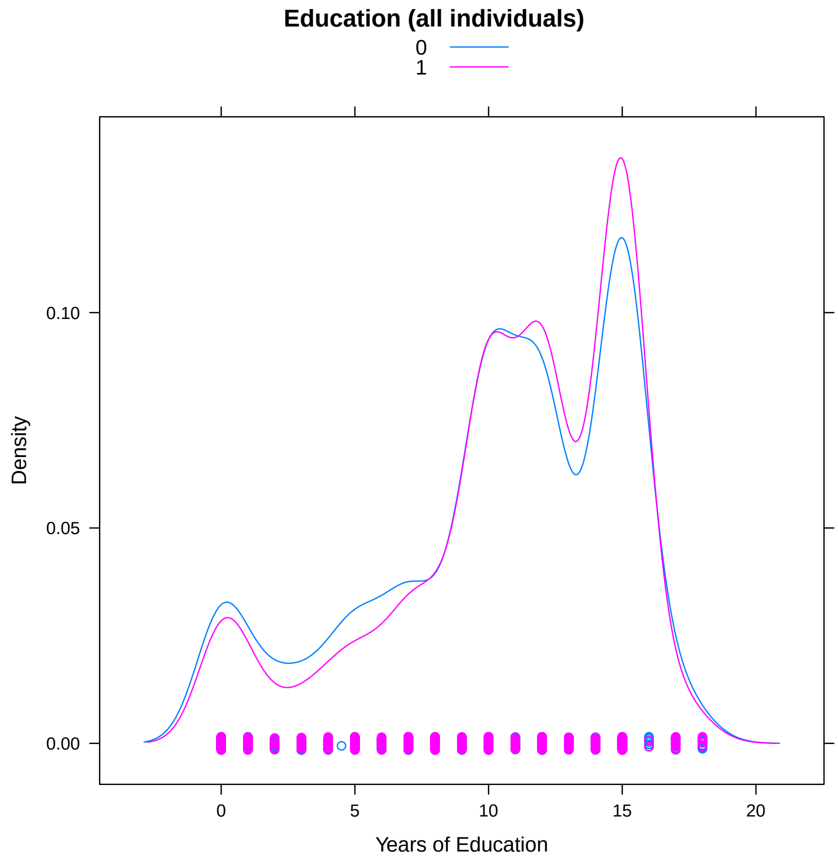
Table 7: Balance tests on individual characteristics. The "Control" column presents means for control group individuals. The "Treatment" column presents the difference between control and treatment group individuals estimated through an OLS regression of each variable on indicators for winning the lottery. 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 India. Here, students sit for the All India Secondary School Examination (AISSE) at the end of grade 10. Only if they pass this exam can students 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

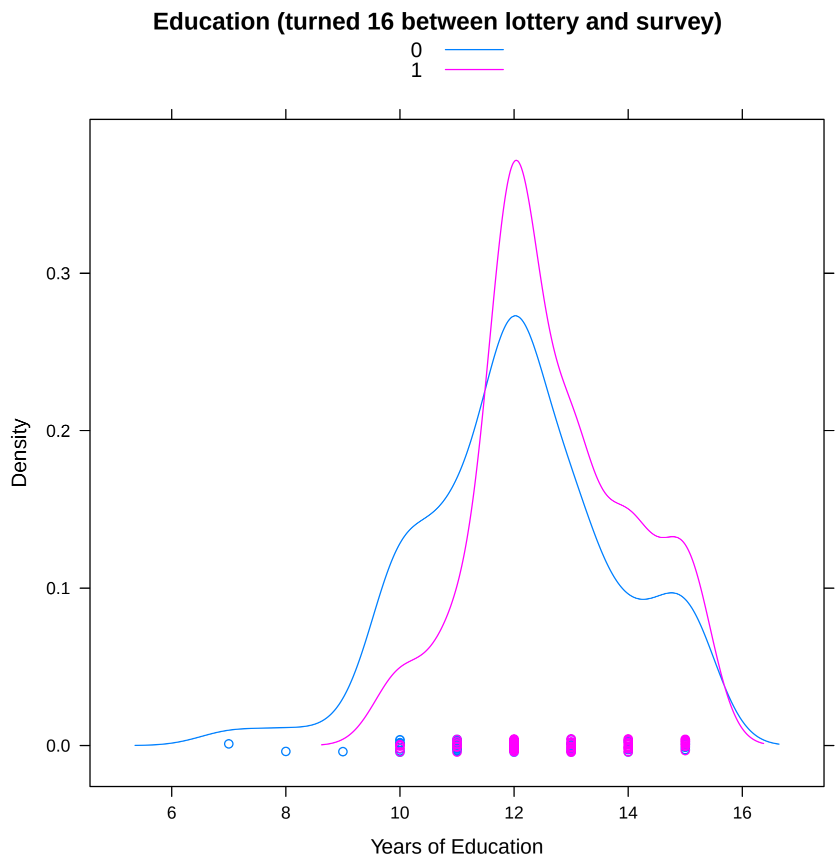
Figure 7: Distribution of individual years of education. "0" indicates membership in non-winning households and "1" indicates membership in winning households.



both secondary school completion *and* rates of post-secondary school education.

I next explore 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 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

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.



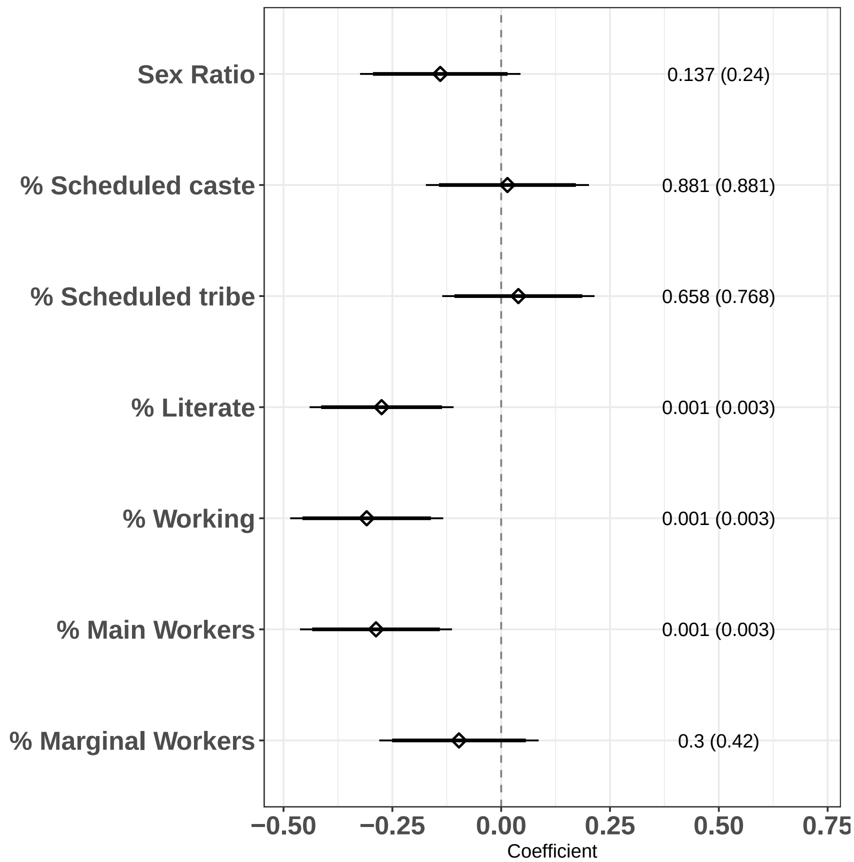
in between the lottery and being surveyed. I see some evidence to suggest that the housing lottery’s effect on completing grade ten is stronger among those who turned 16 after winning, but it is likely that the study is underpowered to detect each of the interaction effects. Nevertheless, Figure 8 clearly 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 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

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.

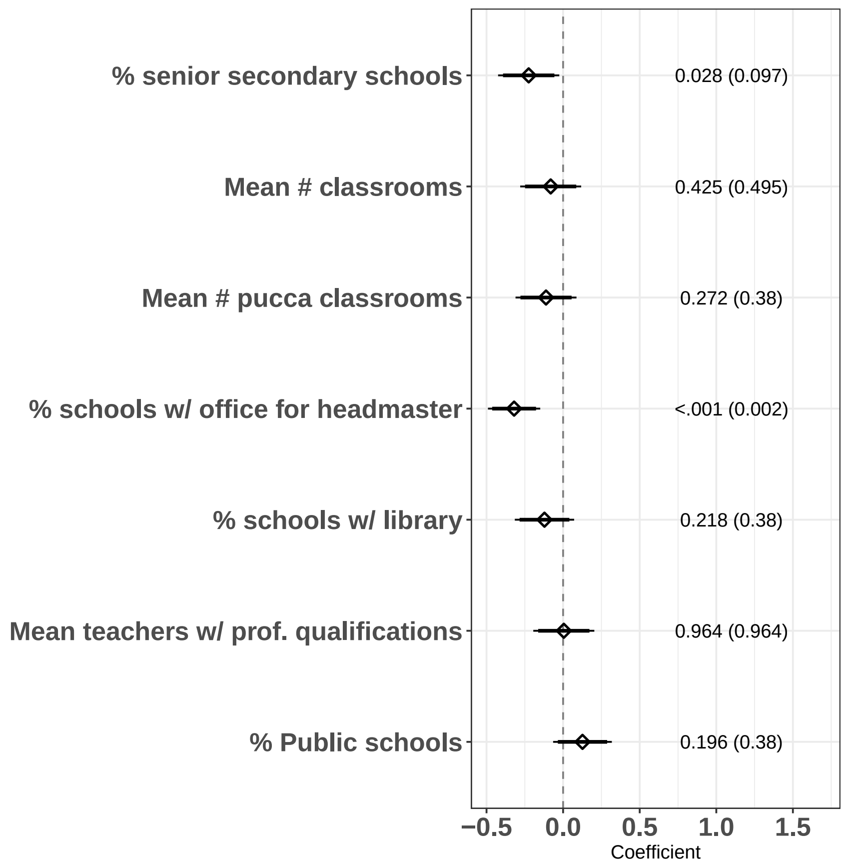
	Dependent variable:							
	I(>0 years)		I(>10 years)		I(>12 years)		I(>=15 years)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	0.008	0.009	0.074^{***}	0.059^{***}	0.057^{***}	0.040^{*}	0.039^{**}	0.036^{*}
	(0.008)	(0.009)	(0.018)	(0.019)	(0.020)	(0.021)	(0.019)	(0.020)
Turned6		0.045^{***}						
		(0.016)						
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. All outcomes are standardized. A “marginal" worker is one who has worked for fewer than six months out of the past year. A main worker is one who has worked for six months or more out of the past year.



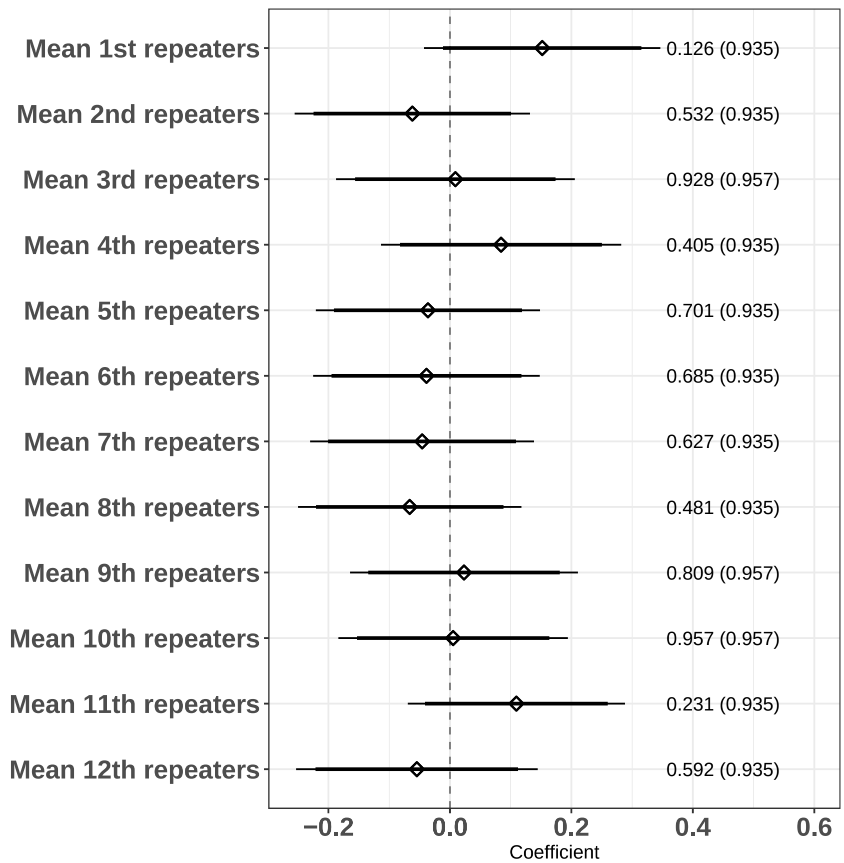
Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the family-wise error rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Tables **??**.

Figure 10: Treatment effects on characteristics of schools of postal codes in which households live. All dependent variables are standardized.



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. All dependent variables are standardized.



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.

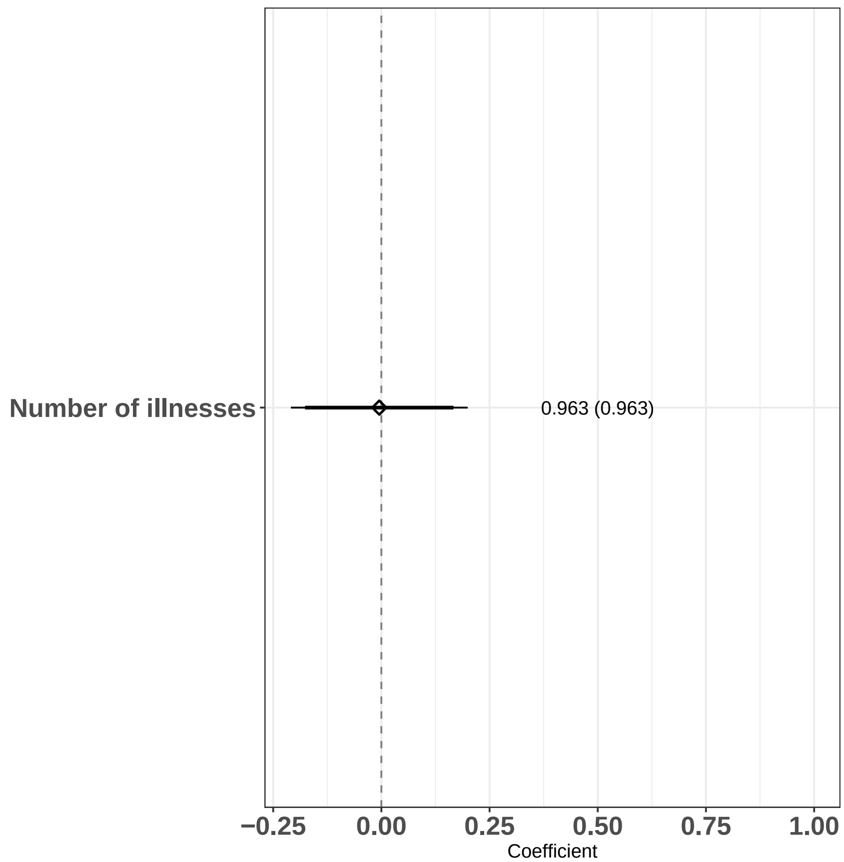
among children who enjoy prolonged exposure to the new neighborhoods. The treatment effects on statistics (taken from India's 2011 Census) on the municipal ward in which households are located do not suggest that the intervention causes households to live in neighborhoods of higher socio-economic status or literacy rates; rather, households are living in areas with *lower* literacy rates and fewer employed individuals (Figure 9). I also estimate treatment effects on various indicators of school quality calculated at the postal code level. These statistics were calculated by taking postal code level means of all of the schools in the Mumbai and Mumbai Suburban administrative districts.[17] In particular, the lottery does not cause households to live in postal codes with a higher percentage of senior secondary schools, or those that offer education through grade 12; in fact, households live in areas with a *a smaller* percentage of these schools. Nor does it cause households to live in postal codes with schools more amenities such as classrooms or libraries. In fact, it causes households to live in postal codes 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 slightly *lower* quality schools available to winners.

6.4 Effects on healthcare consumption

Figure 12 first shows that control and treatment households experience no detectable difference in the incidence of 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 non-medically certified individuals such as homeopathic doctors and family members that are common throughout India (Das and Hammer 2014). Given the high rates of visiting medically certified healthcare providers among the control group (Table 6) , it is possible that effects are driven primarily by those not seeking any healthcare at all prior to the intervention.

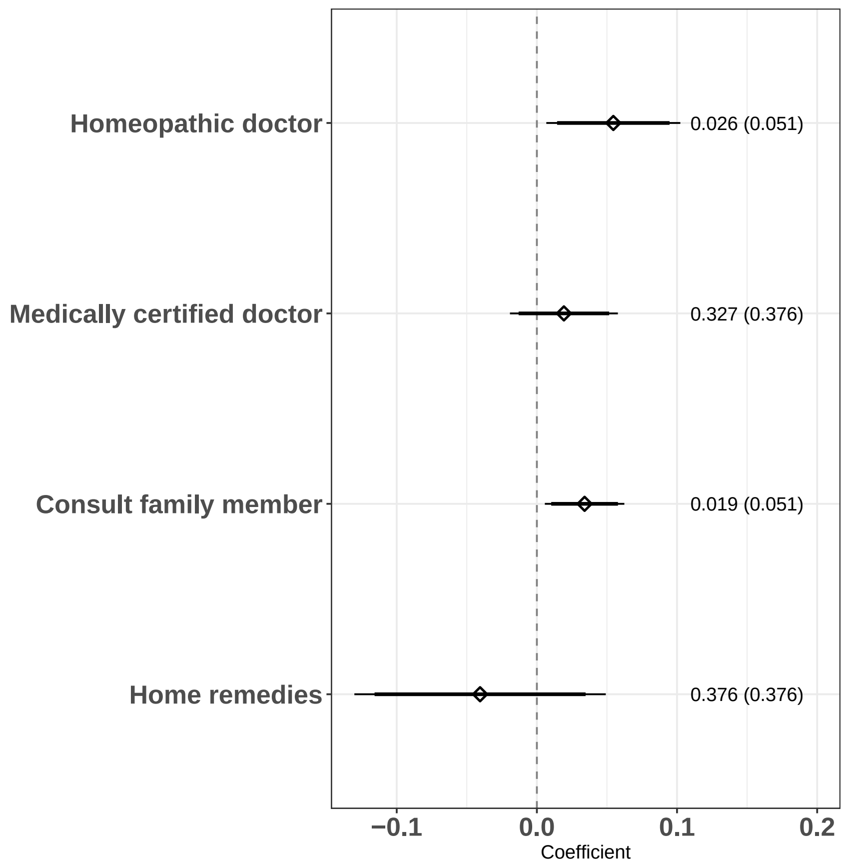
[17]These data were generously provided by the Department of School Education and Literacy, Ministry of Human Resource Development, Government of India. Find more information at http://schoolreportcards.in/SRC-New/.

Figure 12: Treatment effects on the (standardized) reported incidence of household illness in the past month.



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 A15

Figure 13: Treatment effects on the reported visits to different healthcare providers in the past month.



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 A15

6.5 Adverse effects

Of course, it is also possible for this program to have harmful effects on beneficiaries, particularly those choosing to move into the homes. As shown in Figure 9, the intervention leads winners to live, on average, in administrative wards with lower levels of literacy and employment than non-winners. As shown in Figure 10, winners also live in areas with poorer quality schools than non-winners. Results based on survey responses paint a slightly brighter picture. Figure 14 shows that the intervention does not have any measurable effect on the percentage of the household working, suggesting that it may not have negative labor market effects, unlike the intervention studied by van Dijk (2019); in fact, this point estimate is positive. Winners do report about a 0.31 standard deviation increase in the minutes taken for one-way travel time to work. This increase is not negligible, and represents roughly an 8.6 minute increase over the control mean time of about 47.6 minutes (Table 6). Additionally, there are no discernible differences between treatment and control units on whether individuals feel “safe" or “very safe" in their neighborhoods. I do estimate a negative but only marginally statistically significant effect for having friends and family in one’s neighborhood. Overall, however, the fact that the in-kind transfer is bundled with relocation does have some adverse effects for beneficiaries.

7 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 attitudes about the future and and increases bot health care consumption and educational attainment among beneficiaries. The intervention does, however, lead to some location-based adverse effects, likely driven by those choosing the in-kind housing benefits.

The program evaluated is part of a larger set of policy instruments that subsidize the price of

Figure 14: Treatment effects on location-based outcomes

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 REF and REF.

homes; for example, mortgage subsidies may be more familiar in the context of the United States. Housing subsidies themselves can be seen as a type of asset transfer. Governments may often subsidize other durable assets such as tractors, camels, 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 from 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.

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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 an index of wealth. All regressions include treatment indicator interactions with mean centered block dummies.

	Dependent variable:	
	Wealth index	
	(1)	(2)
T	0.410**	0.428***
	(0.168)	(0.013)
OBC		0.333***
		(0.015)
SCST		0.406***
		(0.019)
Maratha		0.142***
		(0.012)
Muslim		0.445***
		(0.017)
Kutcha floor		−1.133***
		(0.032)
Kutcha roof		−0.672***
		(0.033)
From Mumbai		0.441***
		(0.012)
From same ward as apt		−0.164***
		(0.016)
Constant	−0.194*	−0.650***
	(0.111)	(0.014)
Observations	834	834
R^2	0.189	0.235
Adjusted R^2	0.036	0.080
Residual Std. Error	1.665 (df = 701)	1.627 (df = 693)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A4: 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 A5: Regression estimates of treatment effects on asset ownership (no covariates). All regressions include treatment indicator interactions with mean centered block dummies.

	Dependent variable:												
	Almirah	Dining tbl	TV	Fridge	Gas	Computer	Internet	Sewing machine	Mobile	Smartphone	Car	2 whlr	Bicycle
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
T	-0.098^{**}	-0.021	0.034	0.047	0.037	0.024	-0.110^{**}	0.022	-0.028	0.037	0.001	0.001	-0.079^{***}
	(0.049)	(0.039)	(0.026)	(0.031)	(0.029)	(0.049)	(0.050)	(0.035)	(0.047)	(0.042)	(0.025)	(0.048)	(0.018)
Constant	0.711^{***}	0.206^{***}	0.914^{***}	0.879^{***}	0.886^{***}	0.379^{***}	0.513^{***}	0.127^{***}	0.696^{***}	0.751^{***}	0.064^{***}	0.357^{***}	0.078^{***}
	(0.032)	(0.026)	(0.017)	(0.020)	(0.019)	(0.032)	(0.033)	(0.023)	(0.031)	(0.028)	(0.016)	(0.032)	(0.012)
Observations	834	834	834	834	834	834	834	834	834	834	834	834	834
R^2	0.140	0.188	0.167	0.132	0.188	0.171	0.166	0.155	0.166	0.179	0.171	0.158	0.191
Adjusted R^2	-0.022	0.035	0.010	-0.032	0.035	0.015	0.009	-0.005	0.008	0.025	0.015	-0.0004	0.039
Residual Std. Error (df = 701)	0.481	0.390	0.255	0.308	0.291	0.486	0.495	0.348	0.464	0.417	0.246	0.480	0.177

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

Table A6: Regression estimates of treatment effects on asset ownership (with covariate adjustment). All regressions include treatment indicator interactions with mean centered block dummies.

	Dependent variable:												
	Almirah	Dining tbl	TV	Fridge	Gas	Computer	Internet	Swng mchn	Mobile	Smartphone	Car	2 whlr	Bicycle
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
T	−0.082*	−0.018	0.036	0.051*	0.035	0.022	−0.110**	0.020	−0.041	0.040	−0.002	0.009	−0.076***
	(0.048)	(0.039)	(0.026)	(0.031)	(0.029)	(0.049)	(0.050)	(0.036)	(0.047)	(0.042)	(0.025)	(0.048)	(0.018)
OBC	0.071	0.025	0.037	0.088**	0.044	0.024	−0.049	−0.035	0.035	0.088*	0.038	0.058	0.008
	(0.056)	(0.046)	(0.030)	(0.036)	(0.034)	(0.057)	(0.058)	(0.041)	(0.054)	(0.049)	(0.029)	(0.056)	(0.021)
SCST	0.112	−0.007	0.084**	0.015	0.051	0.077	−0.016	−0.089*	−0.039	0.012	−0.004	0.199***	−0.023
	(0.072)	(0.059)	(0.038)	(0.046)	(0.044)	(0.073)	(0.075)	(0.053)	(0.070)	(0.063)	(0.037)	(0.072)	(0.027)
Maratha	−0.076*	−0.022	0.033	0.019	0.012	0.057	0.014	−0.063*	0.050	0.028	0.023	0.091**	−0.017
	(0.045)	(0.037)	(0.024)	(0.029)	(0.028)	(0.046)	(0.047)	(0.033)	(0.044)	(0.040)	(0.023)	(0.045)	(0.017)
Muslim	0.044	0.108**	0.074**	0.067	0.057	0.034	−0.033	−0.034	0.078	−0.003	0.010	0.114*	−0.018
	(0.066)	(0.053)	(0.035)	(0.042)	(0.040)	(0.067)	(0.068)	(0.048)	(0.063)	(0.058)	(0.034)	(0.066)	(0.024)
Kutcha floor	−0.053	−0.165*	−0.028	−0.165**	−0.090	0.014	−0.086	−0.041	−0.043	−0.054	0.013	−0.121	−0.035
	(0.120)	(0.098)	(0.064)	(0.077)	(0.073)	(0.122)	(0.125)	(0.089)	(0.116)	(0.105)	(0.062)	(0.120)	(0.045)
Kutcha roof	−0.114	0.100	−0.052	−0.009	0.025	−0.065	−0.014	0.165*	−0.053	0.025	0.013	0.053	0.069
	(0.125)	(0.102)	(0.066)	(0.080)	(0.076)	(0.127)	(0.130)	(0.093)	(0.121)	(0.110)	(0.065)	(0.125)	(0.046)
From Mumbai	−0.134***	0.061	0.026	0.042	0.074**	0.091*	0.036	0.011	0.132***	−0.003	0.056**	0.013	−0.012
	(0.047)	(0.038)	(0.025)	(0.030)	(0.029)	(0.048)	(0.049)	(0.035)	(0.046)	(0.041)	(0.024)	(0.047)	(0.018)
From same ward as apt	−0.046	−0.090*	−0.080**	−0.033	0.041	−0.109*	−0.038	0.065	0.180***	0.044	−0.048	−0.117*	−0.025
	(0.060)	(0.049)	(0.032)	(0.039)	(0.037)	(0.061)	(0.063)	(0.045)	(0.058)	(0.053)	(0.031)	(0.060)	(0.022)
Constant	0.816***	0.162***	0.873***	0.826***	0.806***	0.291***	0.500***	0.147***	0.559***	0.726***	0.013	0.294***	0.096***
	(0.055)	(0.044)	(0.029)	(0.035)	(0.033)	(0.055)	(0.057)	(0.040)	(0.053)	(0.048)	(0.028)	(0.055)	(0.020)
Observations	834	834	834	834	834	834	834	823	834	834	834	834	834
R^2	0.165	0.203	0.189	0.153	0.202	0.184	0.171	0.170	0.189	0.184	0.184	0.177	0.198
Adjusted R^2	−0.004	0.042	0.025	−0.018	0.041	0.019	0.003	−0.001	0.025	0.019	0.019	0.011	0.036
Residual Std. Error	0.477 (df = 693)	0.388 (df = 693)	0.253 (df = 693)	0.305 (df = 693)	0.290 (df = 693)	0.485 (df = 693)	0.497 (df = 693)	0.350 (df = 682)	0.460 (df = 693)	0.418 (df = 693)	0.246 (df = 693)	0.477 (df = 693)	0.177 (df = 693)

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

Table A7: Regression estimates for treatment effects on reported satisfaction with household financial situation, belief that children will have better lives than parents, and whether or not the respondent thinks the family would ever leave Mumbai. All regressions include treatment indicator interactions with mean centered dummies.

[!htbp]

	Dependent variable:					
	Happy w/ financial situation		Think children will have better lives than them		Would never leave Mumbai	
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.200***	0.192***	0.122**	0.120**	0.087**	0.078**
	(0.046)	(0.046)	(0.048)	(0.048)	(0.039)	(0.038)
OBC		−0.066		0.030		−0.015
		(0.053)		(0.056)		(0.044)
SCST		−0.048		−0.141**		−0.048
		(0.068)		(0.071)		(0.057)
Maratha		0.036		0.087*		0.067*
		(0.043)		(0.045)		(0.036)
Muslim		0.062		0.005		−0.049
		(0.062)		(0.065)		(0.052)
Kutcha floor		−0.124		0.035		−0.136
		(0.113)		(0.119)		(0.095)
Kutcha roof		−0.129		−0.080		0.132
		(0.118)		(0.124)		(0.099)
From Mumbai		0.160***		−0.011		0.172***
		(0.045)		(0.047)		(0.037)
From same ward as apt		−0.037		−0.071		0.031
		(0.057)		(0.060)		(0.048)
Constant	0.596***	0.483***	0.561***	0.563***	0.774***	0.632***
	(0.030)	(0.052)	(0.032)	(0.054)	(0.025)	(0.043)
Observations	834	834	834	834	834	834
R^2	0.165	0.195	0.193	0.209	0.168	0.205
Adjusted R^2	0.008	0.033	0.041	0.049	0.011	0.045
Residual Std. Error	0.457 (df = 701)	0.451 (df = 693)	0.475 (df = 701)	0.473 (df = 693)	0.384 (df = 701)	0.378 (df = 693)
Note:						*p<0.1; **p<0.05; ***p<0.01

Table A8: Regression estimates for treatment effects on standardized 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	0.241**	0.241**	0.154	0.156	0.248**	0.250**
	(0.101)	(0.101)	(0.101)	(0.101)	(0.102)	(0.101)
OBC		0.070		0.020		0.145
		(0.117)		(0.118)		(0.117)
SCST		0.318**		0.314**		0.263*
		(0.150)		(0.151)		(0.150)
Maratha		0.234**		0.158*		0.245***
		(0.094)		(0.095)		(0.094)
Muslim		0.013		0.054		0.031
		(0.137)		(0.138)		(0.137)
Kutcha floor		0.132		0.305		−0.287
		(0.249)		(0.252)		(0.250)
Kutcha roof		−0.505*		−0.120		−0.657**
		(0.260)		(0.262)		(0.260)
From Mumbai		0.069		−0.046		0.138
		(0.098)		(0.099)		(0.098)
From same ward as apt		−0.191		−0.194		0.055
		(0.126)		(0.127)		(0.126)
Constant	−0.124*	−0.258**	−0.072	−0.105	−0.132**	−0.339***
	(0.066)	(0.113)	(0.066)	(0.114)	(0.067)	(0.114)
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	1.000 (df = 701)	0.992 (df = 693)	1.001 (df = 701)	1.000 (df = 693)	1.009 (df = 701)	0.993 (df = 693)
Note:						*p<0.1; **p<0.05; ***p<0.01

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

	Dependent variable:						
	Sex ratio	% SC	% ST	% Literate	% Working	% Main Workers	% Marg Workers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
T	−0.150	0.026	0.037	−0.294***	−0.327***	−0.306***	−0.093
	(0.093)	(0.095)	(0.090)	(0.087)	(0.094)	(0.093)	(0.093)
Constant	0.069	−0.025	−0.0003	0.163***	0.176***	0.167***	0.038
	(0.061)	(0.063)	(0.059)	(0.057)	(0.062)	(0.061)	(0.061)
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)	0.926	0.942	0.889	0.865	0.929	0.921	0.924

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

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

	Dependent variable:						
	Sex ratio	% SC	% ST	% Literate	% Working	% Main Workers	% Marg Workers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
T	−0.140	0.014	0.039	-0.275^{***}	-0.309^{***}	-0.288^{***}	−0.097
	(0.094)	(0.096)	(0.089)	(0.084)	(0.090)	(0.089)	(0.093)
OBC	0.052	−0.119	−0.156	0.256^{***}	0.132	0.177^{*}	-0.302^{***}
	(0.109)	(0.111)	(0.103)	(0.098)	(0.104)	(0.103)	(0.108)
SCST	−0.106	−0.064	0.112	0.019	0.094	0.106	−0.085
	(0.140)	(0.142)	(0.133)	(0.126)	(0.134)	(0.133)	(0.139)
Maratha	−0.039	−0.018	-0.147^{*}	0.073	0.022	0.033	−0.071
	(0.088)	(0.090)	(0.084)	(0.079)	(0.084)	(0.084)	(0.088)
Muslim	−0.077	−0.111	-0.247^{**}	−0.129	−0.081	−0.080	0.005
	(0.128)	(0.130)	(0.121)	(0.115)	(0.122)	(0.121)	(0.127)
Kutcha floor	−0.211	0.041	−0.186	−0.231	-0.409^{*}	−0.362	−0.248
	(0.233)	(0.237)	(0.221)	(0.209)	(0.223)	(0.221)	(0.232)
Kutcha roof	−0.231	−0.095	−0.022	−0.211	−0.004	−0.035	0.195
	(0.243)	(0.247)	(0.231)	(0.218)	(0.232)	(0.231)	(0.242)
From Mumbai	−0.067	0.045	−0.042	0.121	0.267^{***}	0.243^{***}	0.118
	(0.092)	(0.093)	(0.087)	(0.082)	(0.088)	(0.087)	(0.091)
From same ward as apt	0.017	0.244^{**}	0.352^{***}	-0.638^{***}	-0.820^{***}	-0.782^{***}	−0.138
	(0.118)	(0.120)	(0.112)	(0.105)	(0.112)	(0.111)	(0.117)
Constant	0.150	−0.041	0.078	0.090	0.028	0.022	0.036
	(0.106)	(0.108)	(0.101)	(0.095)	(0.101)	(0.101)	(0.105)
Observations	834	834	834	834	834	834	834
R^2	0.284	0.260	0.355	0.424	0.349	0.357	0.293
Adjusted R^2	0.139	0.110	0.225	0.307	0.217	0.227	0.151
Residual Std. Error (df = 693)	0.928	0.943	0.881	0.832	0.885	0.879	0.922
Note:							$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

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

	Dependent variable:							
	% scndry	% sr. scndry	Mean # of classrms	Mean # pucca classrms	% w/ library	Mean # teachers w/ prof qual.	% Public	% w/ office for head
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	-0.229^{**}	-0.070	-0.105	-0.119	0.013	0.113	-0.334^{***}	
	(0.101)	(0.101)	(0.101)	(0.099)	(0.101)	(0.097)	(0.088)	
Constant	0.129^{*}	0.035	0.055	0.062	-0.001	-0.062	0.180^{***}	
	(0.066)	(0.066)	(0.066)	(0.065)	(0.066)	(0.064)	(0.057)	
Observations	832	832	832	832	832	832	832	
R^2	0.155	0.155	0.156	0.188	0.154	0.216	0.365	
Adjusted R^2	-0.004	-0.004	-0.002	0.036	-0.004	0.069	0.246	
Residual Std. Error (df = 700)	1.002	1.002	1.001	0.982	1.002	0.965	0.868	

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

Table A12: Regression estimates for treatment effects on standardized school quality variables measured 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:							
	% scndry	% sr. scndry	Mean # of classrms	Mean # pucca classrms	% w/ library	Mean # teachers w/ prof qual.	% Public	% w/ office for head
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	−0.225**	−0.081	−0.112	−0.122	0.005	0.126	−0.010***	
	(0.102)	(0.102)	(0.101)	(0.099)	(0.102)	(0.098)	(0.003)	
OBC	0.121	0.043	0.083	0.102	0.030	0.048	0.006*	
	(0.118)	(0.118)	(0.118)	(0.115)	(0.118)	(0.113)	(0.003)	
SCST	−0.104	0.253*	0.270*	0.109	0.094	0.077	0.007*	
	(0.152)	(0.151)	(0.151)	(0.147)	(0.151)	(0.146)	(0.004)	
Maratha	0.011	−0.031	−0.019	0.267***	−0.114	0.119	0.004	
	(0.096)	(0.095)	(0.095)	(0.093)	(0.095)	(0.092)	(0.003)	
Muslim	0.013	0.053	0.055	−0.109	0.002	−0.012	−0.002	
	(0.139)	(0.139)	(0.138)	(0.135)	(0.139)	(0.133)	(0.004)	
Kutcha floor	−0.180	0.454*	0.345	−0.046	0.444*	−0.098	−0.010	
	(0.253)	(0.252)	(0.252)	(0.245)	(0.252)	(0.243)	(0.007)	
Kutcha roof	−0.011	−0.048	0.0003	−0.153	−0.146	0.192	−0.011	
	(0.264)	(0.263)	(0.263)	(0.256)	(0.263)	(0.253)	(0.007)	
From Mumbai	0.016	0.071	0.092	0.137	−0.032	−0.072	0.002	
	(0.100)	(0.099)	(0.099)	(0.097)	(0.099)	(0.096)	(0.003)	
From same ward as apt	0.025	−0.024	−0.100	−0.166	0.108	−0.277**	−0.005	
	(0.128)	(0.127)	(0.127)	(0.124)	(0.128)	(0.123)	(0.004)	
Constant	0.106	−0.054	−0.054	−0.112	0.026	−0.032	0.972***	
	(0.115)	(0.114)	(0.114)	(0.111)	(0.114)	(0.110)	(0.003)	
Observations	832	832	832	832	832	832	832	
R^2	0.158	0.164	0.165	0.209	0.163	0.225	0.386	
Adjusted R^2	−0.011	−0.003	−0.002	0.051	−0.005	0.070	0.263	
Residual Std. Error (df = 692)	1.006	1.002	1.001	0.974	1.003	0.964	0.028	
Note:						*p<0.1; **p<0.05; ***p<0.01		

Table A13: Treatment effects on the standardized mean rates of class repetition of schools in postal codes 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	0.140	−0.065	0.001	0.080	−0.052	−0.060	−0.062	−0.079	0.017	0.004	0.109	−0.055
	(0.098)	(0.099)	(0.099)	(0.100)	(0.094)	(0.095)	(0.093)	(0.093)	(0.096)	(0.096)	(0.092)	(0.100)
Constant	−0.057	0.041	0.010	−0.030	0.025	0.033	0.029	0.036	−0.004	−0.002	−0.061	0.023
	(0.065)	(0.065)	(0.065)	(0.066)	(0.061)	(0.062)	(0.061)	(0.061)	(0.063)	(0.063)	(0.060)	(0.066)
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)	0.975	0.979	0.985	0.992	0.929	0.938	0.927	0.924	0.949	0.950	0.910	0.996

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

Table A14: Treatment effects on the standardized mean rates of class repetition of schools in postal codes 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	0.152	−0.062	0.009	0.084	−0.036	−0.039	−0.046	−0.066	0.023	0.005	0.109	−0.054
	(0.099)	(0.099)	(0.100)	(0.101)	(0.094)	(0.095)	(0.094)	(0.094)	(0.096)	(0.096)	(0.091)	(0.101)
OBC	0.031	−0.114	0.033	0.019	0.054	0.118	0.065	0.039	−0.152	−0.104	0.020	0.092
	(0.115)	(0.115)	(0.116)	(0.117)	(0.109)	(0.110)	(0.109)	(0.109)	(0.111)	(0.112)	(0.106)	(0.118)
SCST	−0.003	−0.215	−0.042	−0.118	0.056	0.060	0.056	0.036	0.057	0.059	−0.134	0.129
	(0.148)	(0.147)	(0.149)	(0.150)	(0.140)	(0.142)	(0.140)	(0.140)	(0.143)	(0.144)	(0.136)	(0.151)
Maratha	0.051	−0.106	0.056	−0.039	0.010	0.078	0.018	−0.019	−0.047	−0.030	−0.021	0.088
	(0.093)	(0.093)	(0.094)	(0.095)	(0.088)	(0.089)	(0.088)	(0.088)	(0.090)	(0.090)	(0.086)	(0.095)
Muslim	0.006	−0.043	0.032	−0.025	0.074	0.095	0.079	0.066	0.053	0.108	0.078	0.078
	(0.135)	(0.135)	(0.137)	(0.138)	(0.129)	(0.130)	(0.128)	(0.128)	(0.131)	(0.131)	(0.125)	(0.138)
Kutcha floor	−0.159	−0.174	−0.085	−0.091	−0.025	−0.148	−0.020	0.015	−0.061	0.031	−0.131	−0.054
	(0.246)	(0.245)	(0.249)	(0.251)	(0.234)	(0.236)	(0.233)	(0.233)	(0.238)	(0.239)	(0.227)	(0.252)
Kutcha roof	0.053	0.675***	0.210	0.011	−0.148	−0.032	−0.157	−0.166	0.446*	0.313	0.259	−0.050
	(0.257)	(0.256)	(0.259)	(0.261)	(0.244)	(0.246)	(0.243)	(0.243)	(0.248)	(0.249)	(0.236)	(0.262)
From Mumbai	−0.079	−0.034	−0.055	0.003	−0.164*	−0.194**	−0.166*	−0.145	0.028	0.026	−0.140	0.041
	(0.097)	(0.097)	(0.098)	(0.099)	(0.092)	(0.093)	(0.092)	(0.092)	(0.094)	(0.094)	(0.089)	(0.099)
From same ward as apt	−0.131	−0.059	−0.148	−0.096	−0.142	−0.167	−0.143	−0.111	−0.366***	−0.231*	0.421***	0.035
	(0.125)	(0.124)	(0.126)	(0.127)	(0.118)	(0.119)	(0.118)	(0.118)	(0.120)	(0.121)	(0.115)	(0.127)
Constant	−0.002	0.132	0.043	0.001	0.144	0.146	0.146	0.150	0.025	−0.001	0.011	−0.069
	(0.112)	(0.111)	(0.113)	(0.114)	(0.106)	(0.107)	(0.106)	(0.106)	(0.108)	(0.109)	(0.103)	(0.114)
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)	0.979	0.976	0.989	0.996	0.930	0.938	0.928	0.926	0.944	0.950	0.901	1.001

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

Table A15: Treatment effects on the standardized incidence of illness and contacting healthcare providers in the month. All regressions include treatment indicator interactions with mean centered block dummies.

	Dependent variable:									
	N Illnesses		Homeopathic doctor		Certified doctor		Family member		Home remedies	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
T	0.002	−0.005	0.052**	0.055**	0.015	0.019	0.037**	0.034**	−0.028	−0.041
	(0.103)	(0.104)	(0.024)	(0.024)	(0.020)	(0.020)	(0.014)	(0.014)	(0.046)	(0.046)
OBC		0.037		−0.043		0.037		−0.011		0.007
		(0.121)		(0.028)		(0.023)		(0.017)		(0.053)
SCST		0.015		−0.041		0.049*		−0.008		0.080
		(0.156)		(0.036)		(0.029)		(0.022)		(0.068)
Maratha		0.090		−0.005		0.037**		0.011		0.089**
		(0.098)		(0.023)		(0.018)		(0.014)		(0.043)
Muslim		−0.007		−0.043		0.007		−0.021		0.073
		(0.142)		(0.033)		(0.027)		(0.020)		(0.062)
Kutcha floor		0.319		0.043		−0.063		0.088**		0.091
		(0.261)		(0.063)		(0.051)		(0.037)		(0.114)
Kutcha roof		−0.265		−0.009		0.022		−0.072*		−0.105
		(0.272)		(0.069)		(0.056)		(0.041)		(0.118)
From Mumbai		−0.067		−0.053**		−0.029		−0.016		0.154***
		(0.102)		(0.024)		(0.019)		(0.014)		(0.045)
From same ward as apt		0.145		−0.050		0.037		0.055***		0.012
		(0.131)		(0.031)		(0.025)		(0.019)		(0.057)
Constant	0.004	0.010	0.036**	0.097***	0.949***	0.946***	0.004	0.013	0.315***	0.156***
	(0.068)	(0.118)	(0.016)	(0.028)	(0.013)	(0.022)	(0.010)	(0.016)	(0.030)	(0.052)
Observations	825	825	819	819	819	819	819	819	834	834
R^2	0.122	0.127	0.142	0.156	0.235	0.248	0.156	0.178	0.159	0.182
Adjusted R^2	−0.045	−0.051	−0.023	−0.018	0.087	0.093	−0.007	0.009	0.0002	0.017
Residual Std. Error	1.022 (df = 692)	1.025 (df = 684)	0.240 (df = 686)	0.239 (df = 678)	0.193 (df = 686)	0.193 (df = 678)	0.143 (df = 686)	0.142 (df = 678)	0.455 (df = 701)	0.451 (df = 693)

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

Table A16: Treatment effects on the standardized percentage of the household employed, standardized minutes taken to travel to work, and whether respondents feel safe in the neighborhood. All regressions include treatment indicator interactions with mean centered block dummies.

	Dependent variable:							
	% of HH employed		Min. taken traveling to work		Feel safe/very safe in neighborhood		Friends/family in neighborhood	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	0.146	0.159	0.301***	0.315***	0.003	0.006	−0.090*	−0.081
	(0.100)	(0.100)	(0.111)	(0.111)	(0.023)	(0.023)	(0.050)	(0.050)
OBC		0.194*		0.011		0.024		−0.092
		(0.116)		(0.123)		(0.027)		(0.058)
SCST		0.366**		0.084		0.033		−0.010
		(0.149)		(0.158)		(0.035)		(0.075)
Maratha		0.223**		0.099		0.033		−0.122***
		(0.094)		(0.099)		(0.022)		(0.047)
Muslim		0.081		−0.218		0.066**		−0.036
		(0.136)		(0.141)		(0.031)		(0.068)
Kutcha floor		0.042		−0.252		−0.122**		−0.183
		(0.249)		(0.268)		(0.057)		(0.124)
Kutcha roof		−0.133		0.205		0.047		0.196
		(0.259)		(0.271)		(0.060)		(0.129)
From Mumbai		−0.046		−0.147		0.005		−0.107**
		(0.098)		(0.103)		(0.023)		(0.049)
From same ward as apt		−0.213*		0.340**		0.014		0.119*
		(0.125)		(0.132)		(0.029)		(0.063)
Constant	−0.082	−0.162	−0.174**	−0.118	0.946***	0.919***	0.565***	0.686***
	(0.066)	(0.113)	(0.074)	(0.121)	(0.015)	(0.026)	(0.033)	(0.056)
Observations	834	834	780	780	834	834	834	834
R^2	0.170	0.187	0.151	0.169	0.118	0.132	0.162	0.189
Adjusted R^2	0.013	0.022	−0.015	−0.005	−0.048	−0.043	0.005	0.025
Residual Std. Error	0.993 (df = 701)	0.989 (df = 693)	1.007 (df = 652)	1.003 (df = 644)	0.229 (df = 701)	0.228 (df = 693)	0.499 (df = 701)	0.494 (df = 693)

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