The intergenerational effects of subsidizing homeownership: evidence from a program in Mumbai*

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Abstract

Are there intergenerational effects of subsidizing homeownership? This wealth transfer to beneficiaries is implemented in many forms across the globe, including mortgage and home-price subsidies. This study uses a natural experiment in the form of a housing lottery in Mumbai and finds that three to five years after implementation, beneficiaries have higher levels of educational attainment than non-beneficiaries, with effects concentrated among school-age youth. Contrary to expectations that unearned income might decrease labor supply, the intervention increases rates of employment, particularly full-time employment among youth. Effects are accompanied by changes in winners' attitudes. They also occur in spite of the fact that winners tend to live in neighborhoods with poorer school quality and lower rates of literacy and employment than non-winners. The paper is among the first to analyze the household-level effects of a widespread policy and presents findings that differ significantly from other studies of wealth shocks.

I Introduction

Are there intergenerational effects of subsidizing homeownership? The initiative is implemented globally by governments in many forms, including mortgage and home-price subsidies. These subsidies constitute wealth transfers to beneficiaries experienced in any combination of three payout structures: through a stream of in-kind benefits for those who choose to live in the home, through cash benefits among those who choose to rent out the home, or lump-sum through resale. Aside from transferring wealth directly, these interventions also reduce barriers to ownership of a large asset that forms the cornerstone of wealth accumulation for many families. In fact, home equity has been seen by many as so fundamental to wealth that differential barriers to purchasing a home have been hypothesized to play a key role in intergenerational inequality across racial groups in the United States (Oliver and Shapiro 2013).

Do parents re-invest the wealth transfer in their children? A substantial literature has attempted to answer this question in the United States, where homeowners can deduct much, if not all, of their mortgage interest from taxes (e.g. Richman, 1974; Essen et al., 1978; Green & White, 1997; Haurin et al., 2002; Dietz & Haurin, 2003; Cairney, 2005; Barker & Miller, 2009). Findings have been mixed, and all studies suffer face the problem that those who select into homeownership may differ in many ways from those who do not.[1]

The present study provides some of the first causally identifiable estimates of the effects of subsidizing homeownership on beneficiaries' children. In particular, it estimates the effects of one policy configuration in India, the subsidized sale of government-constructed homes to middle-class households. Similar policies exist in Brazil, Uruguay, Nigeria, Kenya, Ethiopia, and elsewhere, but they have yet to be systematically studied. I take advantage of the fact that a program in Mumbai, a city of over 20 million residents and allocates the housing through a randomized lottery system.

From September 2017 to May 2018, I surveyed winners and non-winners of multiple lotteries that took place in 2012 and 2014 and found large effects on children's educational attainment and

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

employment outcomes.[2] 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. In other words, the intervention shifts households from roughly the 63rd to 73rd percentile of family-wise average years of education in Mumbai. This shift reflects an effect on individuals' likelihood of completing secondary and post-secondary education, with increases concentrated among school-age children, or youth. Among household members who turned 16 after the lottery, for example, the intervention increases the likelihood of continuing schooling past grade 10 by 13 percentage points. Among household members who turned 21 after the lottery, the intervention increases the likelihood of completing post-secondary education by 15 percentage points. Also, the intervention increases levels of employment among individuals by 4 percentage points; this effect size is 17.3 percentage points for youth who turned 21 after the intervention. The overall effects actually represent a a larger 7.4 percentage point increase in full-time labor offset by a *decrease* in rates of part-time, or casual, employment . I discuss many possible reasons for these effects, including changes in budget constraints, attitudes about the future, and the perceived returns to education. Relocation perhaps does not explain effects, as winners live on average in neighborhoods with poorer school quality and lower rates of literacy and employment than non-winners.

   Existing experimental work studies the effects of interventions that differ from the one studied here in important ways. Perhaps the closest related studies are those of rental subsidies. Rental, however, requires relocation to receive the transfer, which means that the location of housing can drive effects. Barnhardt et al. (2017) and van Dijk (2019) find, for example, that rental subsidies lead to broken social networks and differential effects on labor markets depending of the location of the housing. Studies of United States' Moving to Opportunity (MTO) program (Katz et al. 2001; Ludwig et al. 2001; Ludwig et al. 2013; Chetty et al. 2016) similarly find many positive effects of an intervention explicitly motivated by moving households to wealthier neighborhoods. Studies of the effects of large cash prize (Imbens, Rubin, and Sacerdote 2001; Cesarini et al. 2016; Cesarini et al. 2017) and land (Bleakley and Ferrie 2016) lotteries have

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[2]The design used here is the same as in AUTHOR 2019, which focuses on different outcomes.

found null or negative effects on human capital investment and employment, but these transfers are orders of magnitude larger than housing subsidies; their size could very well disincentivize human capital investment and employment. Also, these streams of income are uncertain; they may be reversed, cancelled, or changed in value by future administrations. The uncertainty itself could inhibit investment in human capital.

Overall, this paper is among the first to study the effects of a common policy. Subsidizing homeownership is an initiative pursued by governments in wealthy, low-, and middle-income countries alike, yet causal identification of the effects of these policies has generally been difficult until now. In low- and middle- income countries, other types of asset transfers such as urban land-titling (see e.g. Feder and Feeny 1991; Field 2005; Di Tella et al. 2007; Galiani and Schargrodsky 2010) and rural ultra-poor graduation programs (e.g. Banerjee et al. 2015) have received more attention, but both entail smaller wealth transfers than the program studied here. Furthermore, an urban apartment is much more easily sold or rented and also likely subject to greater appreciation in value than a home in a slum or livestock. For many families, owning a home in an expensive city is simply beyond the realm of possibility; subsidized housing programs, then, may facilitate asset accumulation and fundamentally change the family trajectories.

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

II The intervention[3]

The intervention studied here 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 2015). As demand for living space increases, poorer households are forced to live on the least de-

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[3]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.

sirable and cheapest housing in a city.[4] As a result, governments have also attempted to increase formal housing supply by encouraging private developers to build and by constructing housing themselves. In fact, state-level development boards have spearheaded programs that sell, rather than rent, subsidized units to eligible households in every major city in India.[5] Moreover, in 2015, India's federal government claimed a housing shortfall of over 18 million units to motivate a plan, Pradhan Mantri Awas Yojana (P-MAY, or "The Prime Minister's Housing Scheme), to build 20 million affordable homes by 2022. Grants to subsidize the construction and sale of low-income housing by local municipal boards remain a central component of this policy.

Formal housing programs may be particularly appealing to governments for political reasons. Alan and Ward (1985, 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. Providing homes may be a popular initiative among voters because 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.[6]

This study is based in Mumbai, Maharashtra, an area that attracts migrants from all over India.[7] I study the effects of an annual housing lottery run by the Mumbai Housing and Area

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

[5]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.

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

[7]Here, 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

Development Authority (MHADA), a subsidiary of the Maharashtra Housing and Area Development Authority that uses the same acronym. MHADA runs subsidized housing programs for economically weaker section (EWS) and low-income group (LIG)[8] 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 include lotteries that took place in 2012 and 2014. Information about the area, cost, and downpayment for the apartments in the included lotteries can be found in Table I.

Table I: Lottery apartments included in the sample

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

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

The lottery homes were sold at a government "fair price" that government officials claim was 30-60% of market prices at the time of sale. Table I shows winners could eventually hope for large gains; 3-5 years after the lottery, the difference between the apartment purchase price and list price for older MHADA apartments of the same size in the same neighborhood appears to

the Arabian Sea and that the airport lies near the center of the metropolitan area. Developers are thus incentivized to devote valuable central city square footage to higher end buildings, leading lower-income households to occupy slums, crowd into extremely small homes with friends and relatives, or live far from the city. One survey respondent, for example, claimed to have lived 2.5 hours by train from his place of work when he first moved to the city. According to the 2011 census, roughly 40% of the population of the Mumbai Metropolitan Area lives in slums (Government of India 2011).

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

lie anywhere between Rs.661,700 (about $10,300 at 2017 conversion rates) to Rs.2,869,015 (about $45,000). The differences also provide some sense of the cost, including the opportunity cost, of each program for the government. Housing was constructed on land obtained for free from the city's dismantled textile industry - this land 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 walking distance of the Mumbai suburban rail network, the main network that millions of city residents use to commute every day. Figure I shows the location of the 2012 and 2014 EWS and LIG MHADA apartment buildings and households in the sample at the time of application. Households were permitted to choose the building for which they submitted an application.

Figure I: Location of the addresses of households in the sample (small pink dots) along with the location of apartment buildings (large blue dots) at the time of application
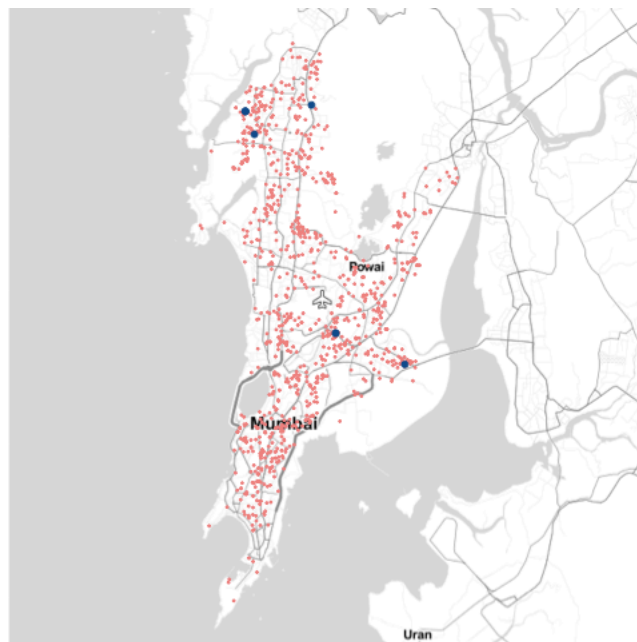
Resale of the apartments is not permitted until 10 years after purchase. This rule generally seems to be enforced, both by MHADA officials and homeowners' associations active in each lottery building. Conversations with building residents reveal that one or two owners have successfully sold their homes 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 the study have made this choice, and the median monthly rental income net of mortgage payments is Rs.2000, or roughly 30 USD. Finally, households do not pay taxes on their dwelling for five years after possession.

As mentioned above, beneficiaries were selected through a lottery process. In fact, the winning sample was stratified by caste and occupation group (Table A.I), 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.[9] Applicants also applied with their Permanent Account Numbers (PAN), which are linked to their bank accounts. Before conducting the lottery, MHADA officials used the PAN numbers to check both whether individuals had applied multiple times for the same lottery round and whether or not they met the criteria for eligibility.[10]

III   Data collection

I estimate treatment effects on all outcomes based on in-person household surveys of a sample of both winning (treatment) and non-winning (control) households. There are about 1,000 applicants for each apartment, so I interviewed a random sample of non-winning applicants. I procured from MHADA phone numbers and addresses for both winners and a random sample of applicants drawn in the same stratified method used for the selection of winners. One non-

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

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

winner was drawn for each winner, there were an equal number of treated and control units in each block, and I accessed a total of 1,862 pre-treatment addresses, or those used at the time of application to the lottery. In this way, both the sample of winners and non-winners was a random draw from the sample of applicants.

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

These addresses were first mapped by hand 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 A.II demonstrates that even after this mapping procedure, I was left with roughly equal proportions of winners and applicants in each caste/occupation category, lottery income category, and apartment building. Given the assumption that the lottery was truly randomized and the fact that I used pre-treatment addresses for the mapping exercise, there is no reason to expect the mapping exercise to systematically favor treatment or control units.

I expect the mapping procedure to have favored wealthier applicants because 1) addresses that could not be mapped often referred to informal settlements, and 2) to create a sample that I could feasibly survey, I also dropped all who lived outside of Greater Mumbai, limiting my sample to urban applicants. Table A.III 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 (or Forward Castes) members in the mapped sample than in the full

sample provided by MHADA. The mapped sample may thus have slightly higher socio-economic status than the full sample of applicants on average. While this issue may affect the external validity of the study, it should not impinge upon the internal validity or causal interpretation of results.

Given the lack of availability of pretreatment covariates, I cannot test for non-random selection into winning the lottery among the 1,862 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 electoral ward. Selection by ward would indicate that individuals from certain locations or with certain political representatives are more likely than others to win the lottery. Here, I estimate regressions of the treatment indicator on the state and municipal ward membership indicators and calculate a heteroscedasticity-robust Wald statistic for the hypothesis that the coefficients on all of the indicators (other than block randomization dummies) are zero. The p-values for regressions on state and municipal ward membership are 0.35 and 0.46, respectively. These p-values do not allow me to reject the null hypothesis that members of any electoral constituency were equally likely to be in the mapped treatment group.

From the mapped sample, I randomly selected 500 households from each treatment condition to interview. From September 2017-May 2018, I worked with a Mumbai-based organization to contact the households and conduct interviews.[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 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

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[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 II: 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**	-

were approached at the addresses listed on the application using the procedure developed for non-winners.

In all cases, we attempted to speak to the individual who had filled out application for the lottery home. 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.[12] Interviews were thus conducted on Sundays and weekday evenings. In my sample, 78% of respondents had reportedly completed the applications themselves.

The data collection process yielded a sample of 834, with 413 (82.6%) of the surveyed households in the control condition and 421 (84.2%) households in the treatment condition. Full information on the number of households contacted in each stratum along with reasons for attrition can be found in Table II. I do not see strong evidence of differential rates of contact for control and treated units; the p-value for the difference in proportion contacted is 0.395. Balance tests for fixed or baseline characteristics among the contacted sample can be found in Table III. Importantly, both treatment groups have an equal proportion of those belonging to the *Maratha* caste group, a dominant group in Mumbai and Maharashtra more generally.[13] This is among the most politically powerful caste groups in Mumbai, and therefore particularly likely to call

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[12]In the case a child had applied for the home (likely because the form could be completed online and youth may be better able to use computers and the internet than their parents), enumerators were instructed to speak to the family's primary earner.

[13]*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.

in a favor and "win" the lottery. Nevertheless, winners and non-winners appear to be similar based on a number of fixed observable covariates, limiting concerns of corruption in the lottery or differential selection into the treatment groups.[14]

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

Variable	Control	Treatment	sd	Pr(>|t|)
A: Household characteristics N=834				
OBC[1]	0.150	-0.021	0.035	0.543
SC/ST[2]	0.080	-0.018	0.026	0.499
Maratha[3]	0.295	0.018	0.045	0.690
Muslim	0.090	0.006	0.029	0.852
Kutcha[4] floor	0.031	0.028	0.019	0.136
Kutcha[4] roof	0.039	0.001	0.018	0.945
Originally from Mumbai	0.809	0.062	0.039	0.114
From the same ward as the apartment	0.097	0.023	0.030	0.454
B: Individual characteristics N=3,127				
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

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.
[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 IV provides a summary of the main outcome variables of interest among the surveyed

[14]In line with my pre-analysis plan, I also perform an omnibus test to judge whether observed covariate imbalance at the household level is larger than would normally be expected from chance alone. This test involves a regression of the treatment indicator on the covariates (Table A.IV) and calculation of a heteroscedasticity-robust Wald statistic for the hypothesis that all the coefficients on the covariates (other than block dummies) are zero. The p-value for this test is 0.39.

control group. The sample is reasonably well-educated and at about the 63rd percentile for mean years of education in Mumbai based on the India Human Development Survey- II (IHDS-II), which was conducted in 2010 (Desai and Vanneman 2016). Roughly half of each family is employed, compared to about 42% for Mumbai overall (2011 Census). 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.[15] 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.[16] EWS and LIG group membership entailed annual income caps of Rs.192,000 and Rs.480,000, placing the highest earners in each category in the 47th and 94th percentile of annual income in Mumbai as collected by IHDS-II.[17] I thus describe the sample as decidedly middle-class and upwardly mobile. This description is corroborated by an interview conducted with the commissioner of the Mumbai Metropolitan Regional Development Authority, who saw the main beneficiaries the housing program to be working class households (Madan 2016). Citing experience from Latin American cities, Alan and Ward (1985, 5), find that public housing interventions generally do not benefit a city's poorest citizens, as they simply cannot afford the requisite rent or mortgage. Recall, however, that the sample mapped and surveyed is somewhat wealthier than the entire pool of applicants on average.

IV   Estimation

For household level effects, I follow my pre-analysis plan and estimate the treatment effect on the pooled sample of lotteries, β, in the following equation where Y is the outcome (as measured through a survey), T is an indicator for treatment (winning the lottery), and $C_1...C_j$ is the group of fixed (or pre-treatment) covariates used for randomization checks. Given that randomization

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

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

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

Table IV: Summary of the control group.

Statistic	Mean	St. Dev.	Min	Max
Housing quality				
Permanent floor	0.96	0.19	0	1
Permanent roof	0.79	0.41	0	1
Private water source	0.73	0.45	0	1
Private toilet	0.62	0.49	0	1
Assets				
Dining table	0.20	0.40	0	1
TV	0.91	0.29	0	1
Fridge	0.87	0.33	0	1
Gas	0.88	0.33	0	1
Computer	0.39	0.49	0	1
Internet	0.47	0.50	0	1
Smartphone	0.73	0.44	0	1
Car	0.06	0.23	0	1
2 wheeler	0.36	0.48	0	1
Bicycle	0.04	0.20	0	1
Education				
Max years of education	13.85	2.66	0	20
Min years of education	5.83	4.87	0	18
Mean years of education	10.35	2.87	0.00	18.00
Public school (sons)	0.06	0.23	0.00	1.00
Public school (daughters)	0.05	0.22	0.00	1.00
Employment				
% of HH employed	0.48	0.25	0	1
Main earner salaried	0.80	0.40	0	1
Main earner has govt. job	0.18	0.38	0	1
Attitudes				
Happy w/ financial situation	0.63	0.48	0	1
Children will have better lives than them	0.56	0.50	0	1
Would never leave Mumbai	0.77	0.42	0	1
Trust others	0.73	0.45	0	1
Believe effort leads to success	0.85	0.36	0	1
Claim to make own decisions	0.15	0.35	0	1

happened within blocks, I treat each of the blocks as a separate lottery and include a set of dummies, $B_1...B_l$ for each. Following Lin (2013), I center these dummies and interact them with the treatment indicator :

$$Y = \alpha + \beta T + \sum_1^j \gamma_j C_j + \sum_1^l \eta_i (T * (B_i - \bar{B}_i)) \tag{1}$$

I label households as "treated" if they win the lottery in the specific year for which they appear in the sample.[18] Following Imbens and Kolesar (2015), I compute standard errors using the HC2 estimator (MacKinnon and White 1985). Also, I make Benjamini-Hochberg corrections for the false discovery rate within "families" of outcomes. 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.[19] When an outcome is not binary or categorical, treatment effects are reported in standard deviations of the control group.

For education and employment results, I also analyze individual-level data that is based on a census of every household member to estimate individual-level treatment effects. This dataset drops all individuals born *after* the household-relevant lottery was conducted. These individuals are dropped to exclude post-treatment bias arising due to treatment effects on fertility.[20] Regressions here include block-centered dummies as well.

Again, note that this paper estimates average treatment effects across the different types of payout structures chosen. This is mainly because this choice reveals a type, and types remain unknown among the control group.[21] 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. More generally, the study is underpowered to detect heterogeneous effects at the household level.

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[18]The possibility of households applying for multiple lotteries was addressed when discussing the sampling procedure.

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

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

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

V   Results

Figure II presents the household-level results for education and employment. This section discusses these and individual-level results in depth. Overall, effects are positive and concentrated among older youth; it is likely that average household effects will increase in size over time as beneficiaries' children become older and pass through school-age years.

V.A   Education

First, I estimate that parents of winners are about 8.4 percentage points less likely to report sending their children to public school than parents of non-winners; in India, asking children attend a public or "government" school is a more common way to draw the distinction between public and private schools than by asking if children attend private schools. This is like due to the extreme heterogeneity in the types of non-government providers of education in India; a private school can refer to a prestigious international school, or it could refer to a school run out of a private home (Harma 2011). In spite of this heterogeneity, public schools have the well-earned reputation of being significantly lower quality than than their private counterparts in urban India (Kingdon 1996; De and Drèze 1999).

The household mean for years of education differs among treatment and control households by 0.24 standard deviations on average (Figure II). These effects correspond to 0.68 and 0.62 year differences, respectively. Based on data from 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. There is no measurable effect on minimum years of education, suggesting that the intervention has no effect on beginning one's schooling. There is, however, a 0.24 standard deviation effect on the maximum number of years of education, suggesting that the intervention leads household members to continue their education.

Effects on individual-level education support this conclusion. 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

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

Education	Coefficient	P-value
Max years of education in HH (in SDs)		0.013 (0.029)
Min years of education in HH (in SDs)		0.125 (0.175)
English medium school (daughters)		0.788 (0.788)
English medium school (sons)		0.526 (0.613)
Public school (daughters)		<.001 (<.001)
Public school (sons)		<.001 (<.001)
Mean years education (in SDs)		0.017 (0.029)

Employment	Coefficient	P-value
Mean % working (in SDs)		0.113 (0.151)
Main earner salaried		0.038 (0.151)
Main earner govt job		0.313 (0.313)
Main earner has formal–sector job		0.102 (0.151)

Employment here means having worked one hour or more in the past week. Whether or not an individual has a "formal sector" job is proxied for by whether s/he received a letter or a contract at the beginning of the job. Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the false discovery rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Tables A.V-A.VI

Figure III: Distribution of individual years of education for the whole sample drawn using a Gaussian kernel.
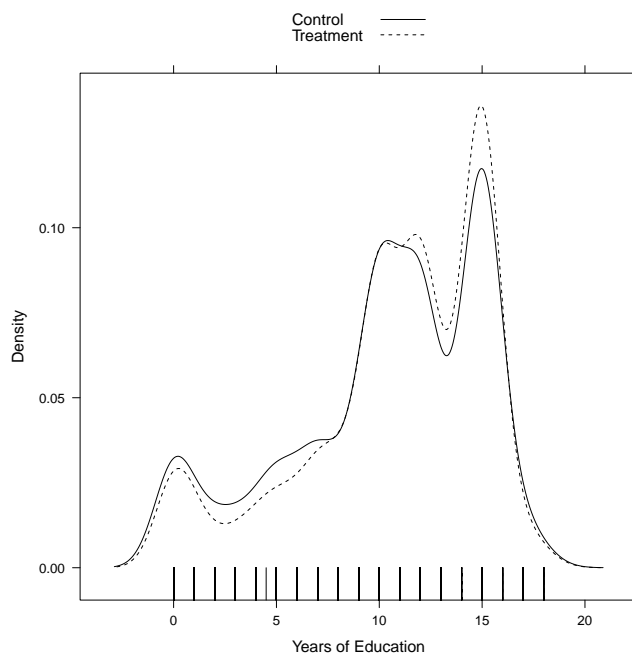
(Figure III). The means at 0, 12, and 15 years represent barriers to beginning schooling, beginning post-secondary schooling, and completing post-secondary schooling respectively.[22] The mean at 10 years possibly the barriers to continuing education past 10th grade that are particularly high in India. Here, students sit for the All India Secondary School Examination (AISSE) at the end of grade 10. Only if they pass this exam can students advance past grade 10. Those who pass also receive an AISSE Secondary School Completion Certificate, which is in itself a certification that may be used for 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 next explore whether winning the housing lottery increases the likelihood of overcoming each of these barriers (Table V). I estimate regressions of completing one's education past these barriers on the treatment indicator. Belonging to a household that has won the lottery indeed

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

increases the likelihood of moving past grades 10 and 12 and completing post-secondary education. It does not seem to have an effect on actually beginning one's education. I also include an interaction with the treatment indicator and an indicator for whether each individual turned 6, 16, 18, and 21 in between being surveyed and the applicable lottery year. These years were chosen with the assumption that most individuals complete 6, 16, 18, and 21 years of age in their first, tenth, twelfth, and fifteenth years of education. In other words, I investigate whether the treatment effect is stronger for those who were the conventional ages for completing one, ten, twelve, and fifteen years of education in between the lottery and being surveyed. I see some evidence to suggest that the housing lottery's effect on completing grades ten and college is stronger among those who turned 16 and 21 after winning, respectively. Figure IV clearly shows that effects are concentrated among individuals who were of secondary and post-secondary school age after the lottery, rather than younger or older individuals. In particular, the figure displays a 13 percentage point increase in the likelihood of completing grade 10 among members of winning households who turned 16 after the lottery and a 15 percentage point increase in the likelihood of completing post-secondary education among members of winning households who turned 21 after the lottery. The three panels for secondary and post-secondary school age children show a rightward shift in the distribution for educational attainment.

V.B   Employment

Table VI shows effects on individual employment. Here, being employed is defined as working one hour or more in the past week. Model (1) first shows that individuals become more likely to be employed as they become older; child labor is generally uncommon in this sample. It also shows that intervention increases the likelihood of employment by about 4 percentage points across all age groups. As shown in Model (6), among the age cohort that turned 21 or had the opportunity to pass through college since the lottery, the likelihood of being employed increases by 17.3 percentage points. This increase is in line with the finding that belonging to a winning family marginally increases the likelihood of this age cohort completing college; children are more likely to complete their education and, in turn, more likely to find jobs. The treatment

Table V: 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.

	Years of education	I(>0 years)		I(>10 years)		I(>12 years)		I(≥15 years)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T	0.618***	0.009	0.010	0.071***	0.056***	0.056***	0.039*	0.041**	0.036**
	(0.177)	(0.009)	(0.009)	(0.018)	(0.019)	(0.019)	(0.021)	(0.017)	(0.017)
Turned6			0.057***						
			(0.017)						
Turned16					0.333***				
					(0.042)				
Turned18							0.387***		
							(0.051)		
Turned21									0.351***
									(0.050)
TXTurned6			−0.017						
			(0.018)						
TXTurned16					0.093*				
					(0.050)				
TXTurned18							0.106		
							(0.067)		
TXTurned21									0.114*
									(0.068)
Constant	10.230***	0.935***	0.932***	0.505***	0.487***	0.318***	0.298***	0.258***	0.234***
	(0.131)	(0.006)	(0.007)	(0.013)	(0.013)	(0.013)	(0.014)	(0.012)	(0.012)
Observations	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.051	0.047	0.049	0.053	0.088	0.058	0.109	0.058	0.107
Adjusted R^2	0.010	0.006	0.007	0.012	0.048	0.017	0.069	0.018	0.068
Residual Std. Error	4.544 (df = 3037)	0.239 (df = 3037)	0.239 (df = 3035)	0.495 (df = 3037)	0.486 (df = 3035)	0.472 (df = 3037)	0.459 (df = 3037)	0.445 (df = 3037)	0.434 (df = 3035)

Dependent variable:

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

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Figure IV: Distribution of individual years of education by cohort drawn using a Gaussian kernel.
Vertical lines drawn to show 0, 10, 12, and 15 years.
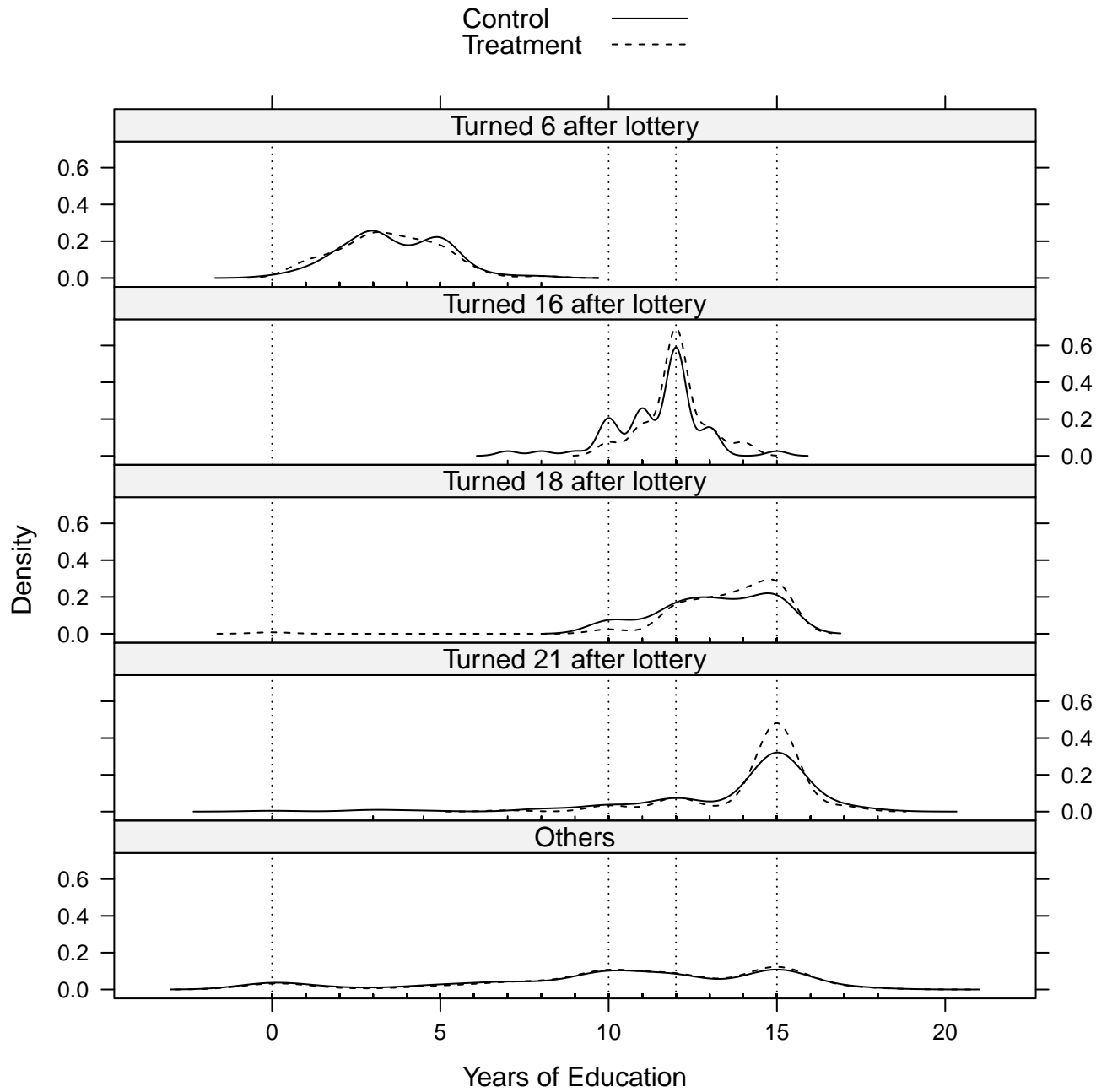
has no detectable effect on younger cohorts, likely because these individuals are still in school. There are also no detectable effects among older household members, indicating that effects are concentrated among children.

Tables VII and VIII break effects down for part-time (casual) and full-time employment, or working fewer than 5 days and working 5 or more days in a week.[23] This distinction can be thought of as a very rough proxy for wage and salaried labor. Effects on overall employment seem to be driven by increases in full-time employment, as patterns for effects on full-time work mirror those for effects on overall employment, but with larger coefficient sizes. In contrast, the intervention actually seems to decrease levels of casual employment. This breakdown complements positive but statistically insignificant household-level effects on the main earner being salaried or having a government job (Figure II). The "main" worker is defined as the family's highest earner; in several families, this individual was not the head of the household but rather the oldest child.

VI Discussion and mechanisms

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

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

One explanation for these results is that they are driven by owner-occupiers who relocate to a new neighborhood and experience better labor market and educational opportunities as a result. I explore this possibility by examining effects on characteristics of neighborhoods based on census block and postal-code averages.[24] As shown in Figure V, the intervention actually

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

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

Table VI: Regressions of individual employment 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. "Older" indicates individuals who were older than 21 at the time of the lottery.

	Dependent variable:					
	Employed					
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.040***	0.035**	0.050***	0.045***	0.034**	0.047
	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)	(0.029)
Turned6	−0.019	−0.469***				
	(0.012)	(0.014)				
Turned16	−0.004		−0.445***			
	(0.025)		(0.027)			
Turned18	0.118***			−0.216***		
	(0.035)			(0.052)		
Turned21	0.624***				0.152***	
	(0.036)				(0.045)	
Older	0.564***					0.409***
	(0.013)					(0.024)
TXTurned6		−0.022				
		(0.022)				
TXTurned16			0.037			
			(0.041)			
TXTurned18				0.021		
				(0.071)		
TXTurned21					0.139**	
					(0.069)	
TXOlder						−0.009
						(0.035)
Constant	0.009	0.475***	0.472***	0.460***	0.439***	0.164***
	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.020)
Observations	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.250	0.073	0.077	0.045	0.047	0.168
Adjusted R^2	0.216	0.032	0.036	0.002	0.005	0.132
Residual Std. Error	0.442 (df = 3032)	0.491 (df = 3035)	0.490 (df = 3035)	0.499 (df = 3035)	0.498 (df = 3035)	0.465 (df = 3035)

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

Table VII: Regressions of individual casual employment on the treatment indicator. Casual employment is defined as working fewer than five days a week. All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies. "TurnedX" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed. "Older" indicates individuals who were older than 21 at the time of the lottery.

	Dependent variable:					
	Employed (part-time)					
	(1)	(2)	(3)	(4)	(5)	(6)
T	−0.024*	−0.028**	−0.027**	−0.023*	−0.025**	−0.033
	(0.012)	(0.011)	(0.012)	(0.013)	(0.013)	(0.027)
Turned6	0.043	0.091**				
	(0.036)	(0.045)				
Turned16	0.018		0.093**			
	(0.032)		(0.043)			
Turned18	−0.041			0.063		
	(0.029)			(0.039)		
Turned21	−0.077***				−0.001	
	(0.029)				(0.029)	
Older	−0.111***					−0.101***
	(0.021)					(0.022)
TXTurned6		0.087				
		(0.070)				
TXTurned16			−0.010			
			(0.054)			
TXTurned18				−0.049		
				(0.047)		
TXTurned21					−0.003	
					(0.041)	
TXOlder						0.011
						(0.028)
Constant	0.169***	0.082***	0.082***	0.084***	0.087***	0.157***
	(0.022)	(0.009)	(0.009)	(0.009)	(0.009)	(0.020)
Observations	3,168	3,168	3,168	3,168	3,168	3,168
R^2	0.094	0.071	0.067	0.062	0.061	0.086
Adjusted R^2	0.053	0.030	0.026	0.021	0.019	0.046
Residual Std. Error	0.261 (df = 3030)	0.265 (df = 3033)	0.265 (df = 3033)	0.266 (df = 3033)	0.266 (df = 3033)	0.262 (df = 3033)

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

Table VIII: Regressions of individual full-time employment on the treatment indicator. Full-time employment is defined as working five or more days a week. All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies. "TurnedX" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed. "Older" indicates individuals who were older than 21 at the time of the lottery.

|			Dependent variable:			
			Employed (full-time)			
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.074***	0.069***	0.082***	0.078***	0.067***	0.075**
	(0.018)	(0.019)	(0.019)	(0.020)	(0.019)	(0.035)
Turned6	−0.020	−0.399***				
	(0.027)	(0.032)				
Turned16	−0.016		−0.383***			
	(0.029)		(0.037)			
Turned18	0.109***			−0.167***		
	(0.037)			(0.053)		
Turned21	0.569***				0.165***	
	(0.036)				(0.045)	
Older	0.472***					0.329***
	(0.021)					(0.026)
TXTurned6		−0.018				
		(0.051)				
TXTurned16			0.029			
			(0.050)			
TXTurned18				0.014		
				(0.074)		
TXTurned21					0.141**	
					(0.063)	
TXOlder						−0.002
						(0.037)
Constant	0.085***	0.478***	0.477***	0.465***	0.445***	0.227***
	(0.021)	(0.013)	(0.014)	(0.014)	(0.014)	(0.024)
Observations	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.211	0.082	0.086	0.061	0.068	0.143
Adjusted R^2	0.175	0.041	0.046	0.019	0.027	0.105
Residual Std. Error	0.454 (df = 3032)	0.490 (df = 3035)	0.489 (df = 3035)	0.495 (df = 3035)	0.493 (df = 3035)	0.473 (df = 3035)

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

leads winners to live, on average, in administrative wards that with 0.34 standard deviation lower rates of literacy and 0.33 standard deviation lower rates of full-time employment than non-winners. The lottery also causes households to live in postal codes with a lower percentage of senior secondary schools (those that offer education through grade 12), schools that are 0.22 standard deviations less likely to be be taught in English (a proxy for quality), and 0.38 standard deviations less likely to have offices for headmasters (a proxy for school size and formality). Unlike MTO, the intervention provides households with the opportunity to move to generally poorer neighborhoods. Generally, then relocation and exposure to better educational contexts or labor markets seem to be unlikely explanations for the positive education and employment results.

VI.B   Changes in the demand for education

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

While the rule prohibiting sale prevents households from fully realizing the value of the subsidy during the time of the sale, the effect on permanent-income can still lead households to update their consumption habits in the nearer term (Friedman 1957). For landlords, this shift would be facilitated by the additional rental income. Also, households may be able to borrow against the equity accumulated in the home. This possibility is supported by positive effects on the likelihood of reporting that families would turn to commercial banks in the case of a financial emergency (Figure A.I). Winners report being 5 percentage points more likely to ask commercial banks for loans in cases of emergency, reflecting perhaps some ability to borrow against the home

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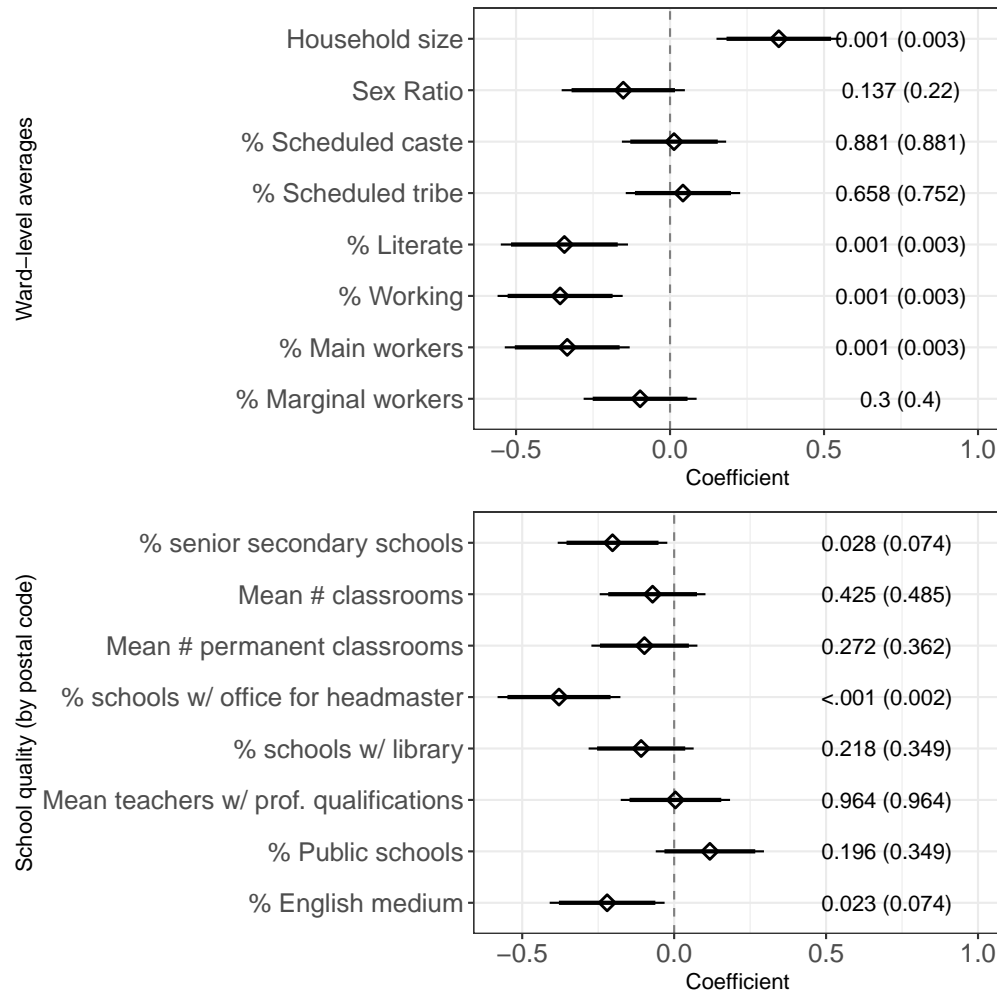
Bars show 90% and 95% confidence intervals. P-values (with with p-values using a Benjamini-Hochberg correction for the family-wise error rate in parentheses) are shown on the right. Full regression output with and without covariate adjustment available in Tables A.VII-A.X. All ward and zip-code level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

or better knowledge about financial institutions, but this effect is no longer statistically significant after accounting for multiple testing.

A large wealth transfer may facilitate investments in children not only because of effects on income, but also because of changing preferences. Not only are winners wealthier, but they can also expect the appreciation of home values and, therefore, household wealth. Beneficiaries seem to be well aware of this possibility; 91% of winning respondents are aware that the value of their properties had increased since purchase, 46% can place a value in INR on this increase, and 93.5% expect the value of the property to increase further in the future. Also, this increase in permanent income is relatively *certain*, unlike promises of pensions or cash payments, it cannot be revoked or changed by future administrations. Such a problem was encountered by beneficiaries of Mexico's conditional cash transfer program, *PROSPERA*, when many of its parameters were changed by the new administration in 2019.

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

These results are supported by qualitative evidence from informal interviews with winners and non-winners. Non-winners tended, in general to express a great deal of uncertainty about day-to-day life. "Anything can happen," said one interviewee. "Our area can flood, I could lose my job, or my mother could become ill. It may be easier to go back to our native place [village] where I have more family." In contrast, a winning interviewee said that the future of her family

was "set." "We have a house in Mumbai now. There is no going back to the past life. My children can have better jobs and marriages than I could," she said.

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

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

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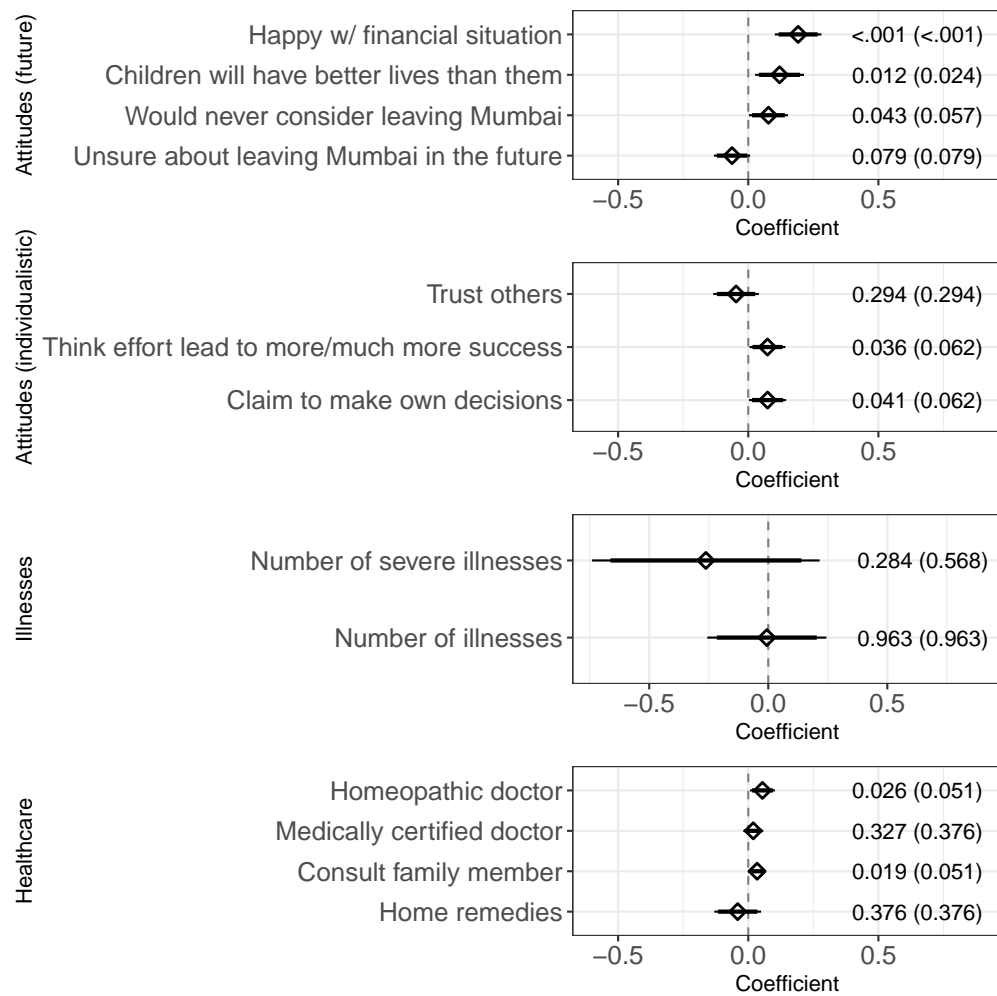
To be "happy" with one's financial situation means to select the highest level of a 3-point scale. To believe children will have better lives than one means to say "yes" (as opposed to no) when asked "Do you expect your children to have better lives than you?" To never consider leaving Mumbai means selecting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

The results in this study differ from those of other studies on the effects of wealth shocks on educational attainment. One reason for this could be differences in the margin at which effects are measured. Bleakley and Ferrie (2016), for example, find that winning a land lottery in Georgia, USA in 1832 did not increase the likelihood of *any* school attendance. I instead measure effects on years of education; indeed, Table V also shows that the Mumbai housing lottery has no effect on having more than 0 years of education. It is possible that among certain populations, barriers to beginning one's education are lower than barriers to continuing education after a certain point.

Differences in the size and permanence of shocks may also account for divergences from studies of cash transfers (e.g. Araujo et al. 2016; Hausofer and Shapiro 2016) that find only null to moderate effects on educational attainment. The vehicle for the wealth transfer may also affect results; the land lottery studied by Bleakley and Ferrie (2016) may increase the need for household labor on the farm, thereby increase the opportunity cost of sending one to school. Most importantly, the context and target population probably matter a great deal. Cesarini et al. (2016) find few human capital returns to a wealth shock in Sweden, but they argue that this is likely due in part to Sweden's strong social safety net, something which doesn't exist in urban Mumbai. Also, the returns to schooling vary greatly across time and space; this is demonstrated clearly by the large literature attempting to estimate these returns in different contexts (Psacharpoulos 1994; Psacharpoulous and Patrinos 2004).

VI.C   Effects on employment

I also observe an increase in full-time employment among precisely the same group of individuals exhibiting gains in educational attainment, namely older youth. If the gains in education are causing the effects on employment, then it would appear that increases in *post-secondary* education are affecting employment outcomes. These findings are somewhat surprising given the stylized fact that youth unemployment in India is highest among post-graduates.[25]

But of course, the relationship between educational attainment and employment is one that

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

will vary greatly across context and has yet to be fully explored in urban India, let alone Mumbai. Importantly, this study was conducted from mid-2017 to early 2018, a period which which saw a spike in unemployment rates among urban youth, particularly in the informal sector.[26] This spike has been attributed by many to the effects of a new national goods and services tax and a surprise "demonetization" initiative, which effectively cancelled a large portion of the national currency literally overnight. If the low returns to post-graduate education are due to the large size of India's informal labor market, returns may have been higher during this period that was particularly difficult for small and informal businesses.

Again, these results diverge from those of other studies on the effects of wealth shocks on employment and labor supply. A rental housing program studied by van Dijk (2019) finds that beneficiaries had worse labor outcomes than non-beneficiaries, an outcome attributed to distance from markets among those who relocate. The intervention here, however, does not force relocation. More significantly, these results diverge from those of studies finding that unearned income decreases labor supply in the United States (Imbens, Rubin, and Sacerdote 2001) and Sweden (Cesarini 2017). As with the education results, the context studied here differs substantially from that of these studies. It is possible that due to competition in the labor market, there are skills-based constraints here being hired; higher levels of educational attainment among winners may reflect a rational response to these constraints and subsequently be responsible for higher levels of employment. Moreover, the wealth shocks in the Mumbai lottery are small compared to those in the US and Sweden studies, and alone may not increase family wealth to the extent that children do not need to worry about working. For example, the largest wealth shock for the Mumbai lotteries appears to be about USD 50,000, whereas Cesarini et al.'s study features wealth shocks of about USD 140,000. Indeed, Imbens, Rubin, and Sacerdote (2001) find that winning smaller lottery prizes does not decrease labor supply.

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

VII 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 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 has intergenerational effects in the form of greater educational attainment and employment rates among youth in particular. Beneficiaries also possess both more optimistic and individualistic attitudes, which could be partially responsible for human capital investment and also suggest the possibility of longer-term effects. These effects occur even though beneficiaries tend to live in areas with lower levels of employment and worse schools.

The program evaluated is part of a larger set of policy instruments that subsidize the price of homes. Because homes are large assets, can appreciate substantially in value, and tend to be purchased by all types of families everywhere, understanding the effects of subsidizing homeownership is important to identifying important sources of human capital accumulation. Changes in human capital accumulation may occur not only within families, but across countries and time-periods witnessing large initiatives to promote homeownership. Given the fact that households must be able to purchase the unsubsidized portion of the apartment, however, the intervention may tend to benefit middle- or middle-class households over their poorer counterparts. This feature of the program along with its positive effects may exacerbate inequalities in a setting. By this same reasoning, barriers to homeownership that are particularly high for certain groups may also deepen economic inequalities.

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

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A   Appendix

A.1   Effects on assets and borrowing

Winning households do not appear to be consuming more durable assets than non-winning households. They do not appear more likely to own common components of many asset-based indices of wealth, such as computers and dining tables (Davila et al. 2014), even while control group ownership of these items is not particularly high (Table IV).

I also asked individuals a multiple choice question about the sources to which they would turn when faced with a shock such as a family illness. Winners are about 5 percentage points more likely to report turning to commercial banks or credit unions, but the effect is no longer statistically significant after correcting for multiple hypothesis testing.

Figure A.I: Treatment effects on asset ownership and reported likelihood of visiting commercial banks for loans.
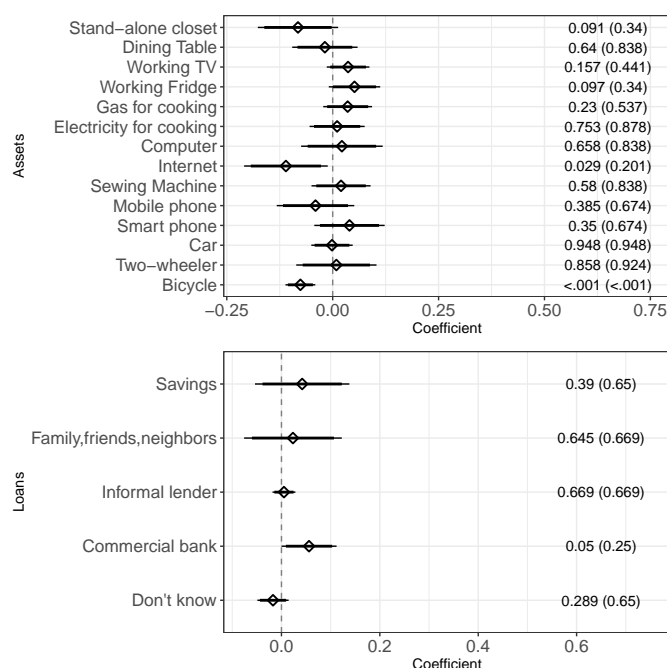
Treatment effects for loan activity are based on multiple choice responses to "If you have a financial emergency (such as an illness in the family), where do you think you will get the money?" Questions were open-ended, with the enumerator filling out the correct categories. "Informal lender" includes local politicians or leaders. 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 A.XV-A.XVII.

A.2 Tables

Table A.I: 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 A.II: 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 A.III: 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 A.IV: 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 A.V: Regression estimates of household-level educational outcomes. All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:									
	Mean years education (in SDs)		Min years education (in SDs)		Max years education (in SDs)		Public school (sons)		Public school (daughters)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
T	0.237**	0.237**	0.154	0.156	0.233**	0.235**	−0.086***	−0.084***	−0.089***	−0.084***
	(0.099)	(0.098)	(0.101)	(0.102)	(0.096)	(0.095)	(0.020)	(0.020)	(0.018)	(0.018)
OBC		0.069		0.020		0.136		−0.006		0.017
		(0.114)		(0.118)		(0.110)		(0.023)		(0.021)
SCST		0.311**		0.315**		0.247*		−0.011		0.048*
		(0.147)		(0.151)		(0.141)		(0.030)		(0.027)
Maratha		0.229**		0.159*		0.230***		0.022		0.005
		(0.092)		(0.095)		(0.089)		(0.019)		(0.017)
Muslim		0.013		0.054		0.029		0.047*		0.028
		(0.134)		(0.138)		(0.128)		(0.027)		(0.025)
Kutcha floor		0.130		0.306		−0.269		−0.032		−0.024
		(0.244)		(0.252)		(0.235)		(0.050)		(0.045)
Kutcha roof		−0.494*		−0.120		−0.617**		0.072		0.064
		(0.255)		(0.263)		(0.245)		(0.052)		(0.047)
From Mumbai		0.068		−0.047		0.129		−0.041**		−0.052***
		(0.096)		(0.099)		(0.092)		(0.020)		(0.018)
From same ward as apt		−0.187		−0.194		0.051		0.042*		0.039*
		(0.123)		(0.127)		(0.118)		(0.025)		(0.023)
Constant	3.562***	3.430***	1.200***	1.166***	5.133***	4.938***	0.095***	0.113***	0.088***	0.111***
	(0.065)	(0.111)	(0.067)	(0.115)	(0.063)	(0.107)	(0.013)	(0.023)	(0.012)	(0.021)
Observations	834	834	834	834	834	834	823	823	822	822
R^2	0.159	0.182	0.156	0.168	0.143	0.180	0.203	0.222	0.237	0.260
Adjusted R^2	0.001	0.017	−0.003	−0.0002	−0.018	0.014	0.050	0.062	0.090	0.107
Residual Std. Error	0.979 (df = 701)	0.971 (df = 693)	1.004 (df = 701)	1.002 (df = 693)	0.948 (df = 701)	0.933 (df = 693)	0.197 (df = 690)	0.196 (df = 682)	0.179 (df = 689)	0.177 (df = 681)

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

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Table A.VI: Treatment effects on the standardized percentage of the household employed and other employment outcomes. Employment here means having worked one hour or more in the past week. Having a formal sector job here means having received a letter or contract at the start of employment.

| | *Dependent variable:* | | | | | | | |
| | Mean % working (in SDs) | | Main earner salaried | | Main earner govt job | | Main earner formal job | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	0.146	0.158	0.079**	0.080**	0.038	0.039	0.053	0.056
	(0.100)	(0.100)	(0.039)	(0.039)	(0.039)	(0.039)	(0.034)	(0.034)
OBC		0.193*		0.034		−0.017		−0.045
		(0.116)		(0.045)		(0.045)		(0.039)
SCST		0.365**		0.165***		−0.002		0.076
		(0.149)		(0.057)		(0.058)		(0.051)
Maratha		0.223**		0.121***		0.082**		0.026
		(0.094)		(0.036)		(0.037)		(0.032)
Muslim		0.081		−0.130**		−0.136**		−0.047
		(0.136)		(0.052)		(0.053)		(0.046)
Kutcha floor		0.042		0.028		−0.114		0.003
		(0.248)		(0.096)		(0.097)		(0.084)
Kutcha roof		−0.132		−0.016		0.070		−0.064
		(0.258)		(0.100)		(0.101)		(0.088)
From Mumbai		−0.045		−0.017		−0.014		−0.050
		(0.098)		(0.038)		(0.038)		(0.033)
From same ward as apt		−0.212*		0.045		0.053		0.048
		(0.125)		(0.048)		(0.049)		(0.042)
Constant	1.876***	1.796***	0.782***	0.746***	0.181***	0.180***	0.096***	0.127***
	(0.066)	(0.113)	(0.026)	(0.043)	(0.026)	(0.044)	(0.022)	(0.038)
Observations	834	834	834	834	834	834	834	834
R^2	0.170	0.187	0.139	0.174	0.206	0.227	0.139	0.152
Adjusted R^2	0.013	0.022	−0.024	0.008	0.056	0.071	−0.023	−0.019
Residual Std. Error	0.990 (df = 701)	0.986 (df = 693)	0.386 (df = 701)	0.380 (df = 693)	0.388 (df = 701)	0.385 (df = 693)	0.336 (df = 701)	0.335 (df = 693)

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

Table A.VII: 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	−0.163	0.024	0.039	−0.367***	−0.378***	−0.355***	−0.093
	(0.101)	(0.086)	(0.095)	(0.109)	(0.108)	(0.108)	(0.093)
Constant	21.470***	2.166***	3.404***	30.030***	20.810***	19.330***	6.425***
	(0.067)	(0.056)	(0.063)	(0.072)	(0.071)	(0.071)	(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)	1.005	0.851	0.943	1.080	1.073	1.069	0.926
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 A.VIII: 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	−0.152	0.013	0.042	−0.343***	−0.357***	−0.334***	−0.097
	(0.102)	(0.086)	(0.095)	(0.105)	(0.104)	(0.103)	(0.094)
OBC	0.057	−0.107	−0.165	0.320***	0.152	0.205*	−0.303***
	(0.118)	(0.100)	(0.110)	(0.122)	(0.120)	(0.120)	(0.108)
SCST	−0.115	−0.058	0.118	0.023	0.109	0.123	−0.086
	(0.152)	(0.129)	(0.141)	(0.157)	(0.154)	(0.154)	(0.139)
Maratha	−0.043	−0.016	−0.156*	0.091	0.025	0.038	−0.072
	(0.096)	(0.081)	(0.089)	(0.099)	(0.097)	(0.097)	(0.088)
Muslim	−0.084	−0.100	−0.262**	−0.161	−0.094	−0.093	0.005
	(0.139)	(0.117)	(0.129)	(0.143)	(0.141)	(0.141)	(0.127)
Kutcha floor	−0.229	0.037	−0.198	−0.288	−0.472*	−0.420	−0.249
	(0.253)	(0.214)	(0.235)	(0.261)	(0.257)	(0.257)	(0.232)
Kutcha roof	−0.250	−0.086	−0.023	−0.263	−0.005	−0.040	0.195
	(0.264)	(0.223)	(0.245)	(0.273)	(0.268)	(0.268)	(0.242)
From Mumbai	−0.073	0.041	−0.044	0.151	0.308***	0.282***	0.118
	(0.100)	(0.084)	(0.093)	(0.103)	(0.101)	(0.101)	(0.091)
From same ward as apt	0.019	0.220**	0.374***	−0.797***	−0.947***	−0.908***	−0.138
	(0.128)	(0.108)	(0.118)	(0.132)	(0.130)	(0.129)	(0.117)
Constant	21.560***	2.152***	3.487***	29.940***	20.640***	19.160***	6.423***
	(0.115)	(0.097)	(0.107)	(0.119)	(0.117)	(0.117)	(0.106)
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)	1.007	0.852	0.935	1.039	1.022	1.021	0.923

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

Table A.IX: 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:							
	% sr. secondary	Mean # of classrooms	Mean # pucca classrooms	% w/ library	Mean # teachers w/ prof qual.	% Public	% w/ office for head	English medium
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	−0.206**	−0.062	−0.092	−0.106	0.012	0.105	−0.396***	−0.217***
	(0.091)	(0.088)	(0.089)	(0.088)	(0.091)	(0.090)	(0.096)	(0.013)
Constant	1.577***	3.858***	3.731***	54.990***	3.300***	2.279***	35.700***	3.145***
	(0.060)	(0.058)	(0.058)	(0.058)	(0.060)	(0.059)	(0.063)	(0.015)
Observations	832	832	832	832	832	832	832	832
R^2	0.155	0.155	0.156	0.188	0.154	0.216	0.365	0.229
Adjusted R^2	−0.004	−0.004	−0.002	0.036	−0.004	0.069	0.246	0.084
Residual Std. Error (df = 700)	0.904	0.876	0.878	0.877	0.905	0.896	1.029	0.951

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

Table A.X: 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:							
	% sr. secondary	Mean # of classrooms	Mean # pucca classrooms	% w/ library	Mean # teachers w/ prof qual.	% Public	% w/ office for head	% English medium
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	−0.203**	−0.071	−0.098	−0.109	0.004	0.117	−0.379***	−0.221**
	(0.092)	(0.089)	(0.089)	(0.088)	(0.092)	(0.091)	(0.103)	(0.096)
OBC	0.109	0.037	0.073	0.091	0.028	0.045	0.217*	0.055
	(0.107)	(0.103)	(0.103)	(0.102)	(0.106)	(0.105)	(0.120)	(0.112)
SCST	−0.094	0.221*	0.237*	0.098	0.085	0.072	0.254*	−0.163
	(0.137)	(0.132)	(0.133)	(0.131)	(0.137)	(0.135)	(0.154)	(0.144)
Maratha	0.010	−0.027	−0.017	0.238***	−0.103	0.111	0.130	0.025
	(0.086)	(0.083)	(0.083)	(0.083)	(0.086)	(0.085)	(0.097)	(0.090)
Muslim	0.012	0.047	0.048	−0.097	0.002	−0.011	−0.076	0.116
	(0.126)	(0.121)	(0.121)	(0.120)	(0.125)	(0.124)	(0.141)	(0.132)
Kutcha floor	−0.162	0.397*	0.303	−0.041	0.401*	−0.091	−0.355	−0.204
	(0.228)	(0.220)	(0.221)	(0.219)	(0.228)	(0.225)	(0.256)	(0.239)
Kutcha roof	−0.010	−0.042	0.0003	−0.136	−0.132	0.179	−0.403	−0.127
	(0.238)	(0.230)	(0.230)	(0.228)	(0.237)	(0.235)	(0.267)	(0.250)
From Mumbai	0.015	0.062	0.081	0.122	−0.029	−0.067	0.083	0.121
	(0.090)	(0.087)	(0.087)	(0.086)	(0.090)	(0.089)	(0.101)	(0.094)
From same ward as apt	0.023	−0.021	−0.087	−0.148	0.098	−0.257**	−0.196	−0.047
	(0.115)	(0.111)	(0.112)	(0.111)	(0.115)	(0.114)	(0.129)	(0.121)
Constant	1.556***	3.780***	3.636***	54.830***	3.324***	2.307***	35.580***	3.056***
	(0.104)	(0.100)	(0.100)	(0.099)	(0.103)	(0.102)	(0.116)	(0.109)
Observations	832	832	832	832	832	832	832	832
R²	0.158	0.164	0.165	0.209	0.163	0.225	0.386	0.236
Adjusted R²	−0.011	−0.003	−0.002	0.051	−0.005	0.070	0.263	0.083
Residual Std. Error (df = 692)	0.908	0.876	0.878	0.870	0.905	0.896	1.017	0.952

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

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Table A.XI: 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.

	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 A.XII: Regression estimates for reported individualistic attitudes. All regressions include treatment indicator interactions with mean-centered block dummies.

| | *Dependent variable:* | | | | | |
| | Trust others | | Effort leads to success | | Make own decisions | |
	(1)	(2)	(3)	(4)	(5)	(6)
T	−0.054	−0.047	0.072**	0.074**	0.067*	0.074**
	(0.045)	(0.045)	(0.035)	(0.035)	(0.036)	(0.036)
OBC		0.026		0.053		−0.021
		(0.052)		(0.041)		(0.042)
SCST		0.029		0.071		0.024
		(0.066)		(0.052)		(0.054)
Maratha		0.126***		0.085***		−0.010
		(0.042)		(0.033)		(0.034)
Muslim		0.017		0.046		0.038
		(0.061)		(0.048)		(0.049)
Kutcha floor		−0.306***		−0.101		0.039
		(0.111)		(0.087)		(0.091)
Kutcha roof		0.186		−0.004		0.004
		(0.115)		(0.091)		(0.095)
From Mumbai		0.047		0.018		−0.110***
		(0.044)		(0.034)		(0.036)
From same ward as apt		−0.131**		0.013		−0.020
		(0.056)		(0.044)		(0.046)
Constant	0.742***	0.675***	0.814***	0.758***	0.127***	0.212***
	(0.030)	(0.050)	(0.023)	(0.040)	(0.024)	(0.041)
Observations	834	834	834	834	824	824
R^2	0.188	0.217	0.178	0.191	0.191	0.205
Adjusted R^2	0.035	0.059	0.024	0.027	0.036	0.042
Residual Std. Error	0.446 (df = 701)	0.440 (df = 693)	0.347 (df = 701)	0.346 (df = 693)	0.358 (df = 691)	0.357 (df = 683)

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

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Table A.XIII: Regression estimates for reported illness in the last month and whether or not households report visiting the relevant individuals in the past month (no covariates). All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:					
	N Illnesses (in SDs)	N Severe Illnesses (SDs)	Homeopathic dr	Medically certified dr	Consult family member	Use home remedies
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.003	−0.206	0.052**	0.015	0.037**	−0.028
	(0.127)	(0.225)	(0.024)	(0.020)	(0.014)	(0.046)
Constant	0.373***	0.484***	0.036**	0.949***	0.004	0.315***
	(0.083)	(0.155)	(0.016)	(0.013)	(0.010)	(0.030)
Observations	825	258	819	819	819	834
R^2	0.122	0.314	0.142	0.235	0.156	0.159
Adjusted R^2	−0.045	0.015	−0.023	0.087	−0.007	0.0002
Residual Std. Error	1.252 (df = 692)	0.830 (df = 179)	0.240 (df = 686)	0.193 (df = 686)	0.143 (df = 686)	0.455 (df = 701)

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

Table A.XIV: Regression estimates for reported illness in the last month and whether or not households report visiting the relevant individuals in the past month (with covariates). All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:					
	N Illnesses (SDs)	N Severe Illnesses (SDs)	Homeopathic dr	Medically certified dr	Consult family member	Use home remedies
	(1)	(2)	(3)	(4)	(5)	(6)
T	−0.006	−0.262	0.055**	0.019	0.034**	−0.041
	(0.128)	(0.244)	(0.024)	(0.020)	(0.014)	(0.046)
OBC	0.045	0.226	−0.043	0.037	−0.011	0.007
	(0.149)	(0.205)	(0.028)	(0.023)	(0.017)	(0.053)
SCST	0.018	−0.184	−0.041	0.049*	−0.008	0.080
	(0.191)	(0.251)	(0.036)	(0.029)	(0.022)	(0.068)
Maratha	0.110	0.048	−0.005	0.037**	0.011	0.089**
	(0.120)	(0.157)	(0.023)	(0.018)	(0.014)	(0.043)
Muslim	−0.008	0.272	−0.043	0.007	−0.021	0.073
	(0.174)	(0.209)	(0.033)	(0.027)	(0.020)	(0.062)
Kutcha floor	0.390	0.007	0.043	−0.063	0.088**	0.091
	(0.320)	(0.565)	(0.063)	(0.051)	(0.037)	(0.114)
Kutcha roof	−0.324	−0.147	−0.009	0.022	−0.072*	−0.105
	(0.334)	(0.551)	(0.069)	(0.056)	(0.041)	(0.118)
From Mumbai	−0.081	0.202	−0.053**	−0.029	−0.016	0.154***
	(0.125)	(0.161)	(0.024)	(0.019)	(0.014)	(0.045)
From same ward as apt	0.177	−0.078	−0.050	0.037	0.055***	0.012
	(0.161)	(0.213)	(0.031)	(0.025)	(0.019)	(0.057)
Constant	0.381***	0.337	0.097***	0.946***	0.013	0.156***
	(0.144)	(0.213)	(0.028)	(0.022)	(0.016)	(0.052)
Observations	825	258	819	819	819	834
R^2	0.127	0.334	0.156	0.248	0.178	0.182
Adjusted R^2	−0.051	−0.002	−0.018	0.093	0.009	0.017
Residual Std. Error	1.256 (df = 684)	0.837 (df = 171)	0.239 (df = 678)	0.193 (df = 678)	0.142 (df = 678)	0.451 (df = 693)

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

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

| | 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)
							Dependent variable:						
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

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

	Almirah (1)	Dining tbl (2)	TV (3)	Fridge (4)	Gas (5)	Computer (6)	Intrnt (7)	Swngmchn (8)	Mobile (9)	Smrtphone (10)	Car (11)	2whlr (12)	Bicycle (13)
							Dependent variable:						
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
R²	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 R²	−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 A.XVII: Treatment effects for responses to "If you have a financial emergency (such as an illness in the family), where do you think you will get the money?" Questions were multiple choice and open-ended, with the enumerator filling out the correct categories. "Informal lender" includes local politicians or leaders. All regressions include treatment indicator interactions with mean-centered block dummies.

| | Dependent variable: | | | | | | | | | |
| | Savings | | Family,friends,neighbors | | Informal lender | | Commercial bank | | Don't know | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
T	0.033	0.042	0.030	0.023	0.005	0.005	0.058***	0.056**	−0.021	−0.017
	(0.049)	(0.049)	(0.050)	(0.051)	(0.012)	(0.012)	(0.028)	(0.029)	(0.016)	(0.016)
OBC		−0.014		−0.123**		0.020		−0.025		0.022
		(0.057)		(0.059)		(0.014)		(0.033)		(0.019)
SCST		−0.051		−0.058		−0.014		−0.059		0.013
		(0.073)		(0.076)		(0.018)		(0.043)		(0.024)
Maratha		−0.036		0.014		0.011		−0.032		−0.025*
		(0.046)		(0.048)		(0.011)		(0.027)		(0.015)
Muslim		−0.011		0.002		0.012		−0.040		−0.003
		(0.067)		(0.069)		(0.016)		(0.039)		(0.022)
Kutcha floor		−0.128		0.193		−0.003		0.098		−0.028
		(0.123)		(0.127)		(0.030)		(0.072)		(0.041)
Kutcha roof		−0.109		0.030		−0.010		−0.085		−0.050
		(0.128)		(0.132)		(0.031)		(0.075)		(0.042)
From Mumbai		−0.138***		−0.033		−0.002		0.007		−0.024
		(0.048)		(0.050)		(0.012)		(0.028)		(0.016)
From same ward as apt		0.099		0.009		0.019		−0.067*		−0.004
		(0.062)		(0.064)		(0.015)		(0.036)		(0.020)
Constant	0.597***	0.718***	0.548***	0.589***	0.012	0.007	0.049***	0.074**	0.036***	0.059***
	(0.032)	(0.056)	(0.033)	(0.058)	(0.008)	(0.014)	(0.019)	(0.032)	(0.011)	(0.018)
Observations	824	824	824	824	824	824	824	824	824	824
R²	0.172	0.190	0.151	0.164	0.205	0.211	0.124	0.136	0.211	0.225
Adjusted R²	0.013	0.024	−0.011	−0.008	0.053	0.049	−0.043	−0.041	0.060	0.066
Residual Std. Error	0.484 (df = 691)	0.482 (df = 683)	0.499 (df = 691)	0.498 (df = 683)	0.117 (df = 691)	0.117 (df = 683)	0.281 (df = 691)	0.281 (df = 683)	0.160 (df = 691)	0.159 (df = 683)

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