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How welfare shapes beneficiaries' political and economic behavior: evidence from two programs in India

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How welfare shapes beneficiaries' political and economic behavior: evidence from two programs in India

by

Tanu Kumar

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Committee in charge:

Professor Pradeep Chhibber, Chair
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Abstract

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How do welfare programs shape the economic and political behavior of recipients? This dissertation is a series of three empirical articles that examine the effects of two programs in India. The first paper finds that a subsidized housing program increases the educational attainment and employment prospects of beneficiaries' children. The second finds that this same program increases everyday local level political participation to improve public goods and services. The third paper finds that a national rural employment guarantee decreases participation in state-level elections among the target beneficiary group. Overall, the dissertation finds that welfare programs can alter beneficiaries' economic and political behavior in important and unexpected ways, thereby providing a vast open agenda for future research.

To my family

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Acknowledgments

I have been pleasantly surprised to learn that dissertation research ends up being a collaborative endeavor. In the past seven years, I have had the good fortune to build this project with many mentors, peers, research partners, friends, and family.

Each of my committee members played a unique role. This project has generated a number of complex methodological problems and questions. Joel Middleton not only answered them patiently, but also helped me to build the knowledge and confidence to begin tackling them on my own. Joel, you have been a consistent voice of support while encouraging me to stay way from "razzle dazzle" and just make sure I understand what it is I am estimating.

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A reader will feel Alison Post's influence through every page of the dissertation. Way back in 2014, you and Isha Ray took a chance on a clueless first-year student and offered me an opportunity to work on your experiment on water intermittency in Bangalore. This project gave me the tools I need to do the field research that was so fundamental to this project. Since then, our close collaborative work has led to many shared research interests, ways of thinking, and real conversations. You have also been a key source of support and happiness throughout my degree.

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Overview

This dissertation explores how India's many welfare programs affect everyday political participation. I refer in particular to citizens' demands to improve services delivered by local government. This behavior is important to study because where officials have incentives to be responsive, it can lead to real improvements in governance and service provision at the local level. I frame this everyday participation to improve local services as a collective action problem wherein the receipt of welfare benefits can change how individuals choose whether or not they choose to participate.

The project is centered around the idea that many types of welfare programs, such as pensions, work guarantees, or affordable housing, deliver streams of income over time. Programs that transfer large benefits have the potential to change attitudes and motivations. They may decrease the cost of demand-making activity by extending recipients' time horizons and improving their perception of themselves. I argue further that beneficiaries will be particularly motivated to protect the value or quality of their benefits. For example, recipients of food ration cards may demand better stocked shops, or those receiving pensions may demand systems to monitor corruption among bureaucrats who demand bribes for the delivery of payments. If complaint-making to protect the value of group welfare benefits is seen as a collective action problem, welfare transfers both increase the benefit and decrease the cost of participation.

In other words, as discussed by those who study the United States and Europe, policies have important feedback effects. With this scholarship as a starting point, I develop and empirically support an argument focused on local civic engagement in developing countries. While the policy feedback literature emphasizes voter mobilization and lobbying to affect policy-making at the national level, I focus on local-level demands to improve the benefits. In settings where governing responsibilities are decentralized, local bodies play an important role in distributing and maintaining benefits. In the first two papers, I illustrate these dynamics through a natural experiment, a lottery-based subsidized homeownership program in Mumbai. The project combines qualitative fieldwork and an original survey that entailed locating and surveying 1,000 applicants of multiple housing lotteries 3-5 years after program implementation.

The first paper establishes that the affordable housing programs studied provided large benefits that have substantial and intergenerational effects on the economic lives of beneficiaries. I find that the program increased household wealth, optimism about the future, time horizons, educational attainment, and rates of employment among beneficiaries.

The second paper finds that these programs also affect beneficiaries' political behavior at the local level. I find that winning an apartment increases both reported political participation to improve neighborhoods and knowledge about local politics. Winners who choose to rent out the apartments also report taking action to improve neighborhoods in which they own apartments but do not live, suggesting that effects are not the result of social pressure alone.

The third paper examines how welfare programs might affect political participation at other levels of government. It uses a difference-in-differences design to find that implementation of India's National Rural Employment Guarantee Scheme decreased turnout in state-level elections in Uttar Pradesh and Punjab. Using multiple rounds of state- and national-level survey data, I argue that we see this effect because NREGS empower central and local-level governance structures, thereby decreasing the salience of the state-level for voters. The paper complements the first two by considering the political effects of welfare in the context of multi-level governance; even while we see a certain effect at the local level, effects at other levels of governance may differ.

1

The human capital effects of subsidized homeownership

Abstract: Government programs in countries across all income levels subsidize homeownership and transfer wealth to households. Do beneficiaries further invest their newfound wealth in *human* capital? Using subsidized housing lotteries in Mumbai, I identify the human capital effects of such a transfer and find that 3-5 years later, winners are more educated than non-winners, with effects concentrated among school-age youth. The intervention also increased rates of employment, particularly among older youth who have had the chance to complete their education. Effects are unlikely to be driven by relocation to areas with better opportunities, as at the time of being surveyed, winners live in neighborhoods with poorer school quality and lower rates of literacy and employment than non-winners. I instead explore the possibilities that the wealth transfer both shifts out inter-temporal budget constraints and changes underlying preferences for investment in human capital.

1.1 Introduction

What are the effects of subsidizing homeownership on household economic trajectories? Governments in many countries use a variety of tools to make homeownership more affordable for citizens, including mortgage and home-price subsidies. These subsidies constitute wealth transfers to beneficiaries experienced in any combination of three payout structures: 1) through a stream of in-kind benefits for those who choose to live in the subsidized home; 2) through cash benefits among those who choose to rent it out; 3) or lump-sum through resale. Aside from transferring wealth directly, these interventions also facilitate the purchase of an asset that forms the cornerstone of wealth accumulation for many families. In fact, home equity may be so fundamental to wealth that differential barriers to purchasing a home have been hypothesized to play a key role in intergenerational inequality (Oliver and Shapiro 2013).

As the home itself represents gains to physical capital, do beneficiaries further invest their newfound wealth in *human* capital? Investment in human capital is fundamental to passing these fortunes onto the next generation. Bleakley and Ferrie (2016), for example, find that winners of a plot of land in Antebellum Georgia did not invest more in their children, and beneficiaries' descendants therefore did not have measurably different economic outcomes than those of non-beneficiaries. A substantial literature has attempted to estimate human capital effects of home subsidies in the United States, where homeowners can deduct much, if not all, of their mortgage interest from taxes (e.g. Richman, 1974; Essen et al., 1978; Green & White, 1997; Haurin et al., 2002; Dietz & Haurin, 2003; Cairney, 2005; Barker & Miller, 2009). Findings have been mixed, and all studies face the problem that those who select into homeownership may differ in many ways from those who do not.[1] Moreover, this question has yet to be investigated in other countries and contexts where subsidizing homeownership is common, particularly low- and middle-income countries.

The present study provides some of the first causally identifiable estimates of the effects of subsidizing homeownership on beneficiaries' educational attainment, an important component of human capital. It further estimate effects on beneficiaries' employment outcomes. It studies one policy configuration common in India, the subsidized sale of government-constructed homes to middle-class households. These programs are found in every major city in India, including Delhi, Mumbai, Bengaluru, Kolkata, Chennai, Hyderabad, Ahmedabad, and are frequently offered in some form across smaller cities as well. Similar policies exist in Brazil, Uruguay, Nigeria, Kenya, Ethiopia, and elsewhere. They have not, however, been systematically studied.[2] I take advantage of the fact that a program in Mumbai, a city of over 20 million residents, allocates the housing through a randomized lottery system.

From September 2017 to May 2018, I surveyed 834 total households of winners and non-winners of multiple lotteries that took place in 2012 and 2014 and found modest effects on educational attainment and employment outcomes. On average, individuals in winning households have 0.13 standard deviations, or over a half year, more education than those living in non-winning households. In other words, the intervention shifts individuals from roughly the 61st to 63rd percentile of average years of education in Mumbai.

An exploratory analysis shows that this shift reflects an effect on individuals' likelihood of completing secondary and post-secondary education, with large increases concentrated among school-age children (youth). Among household members who turned 16 after the lottery, for example, the intervention increases the likelihood of continuing

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

[2]Franklin 2019 studies causes and effects of relocation motivated by one such program in Ethiopia.

schooling past grade ten by 14 percentage points. Among household members who turned 21 after the lottery, the intervention increases the likelihood of completing post-secondary education by 15 percentage points.

Also, the intervention increases levels of employment among individuals by 4.2 percentage points; this effect size is 21.7 percentage points for youth who turned 21 after the intervention, or those who had the chance to complete their education. The overall employment effects actually represent a larger 7.5 percentage point increase in full-time labor offset by a *decrease* in rates of part-time employment. These patterns suggest the possibility of the employment increases occurring as a result of better education.

I explore four possible mechanisms for these effects, including relocation, shifts in budget constraints, changes in attitudes about the future, and changes in the perceived returns to education. Relocation to areas with better education and employment opportunities perhaps does not explain effects, as winners live on average in neighborhoods with poorer school quality and lower rates of literacy and employment than non-winners. It is more likely that the intervention changes behavior through changes to household budget constraints. I also find evidence to suggest that the intervention leads to changes in attitudes and preferences. Winners report feeling happier about their financial situations, expect better lives for their children, are more likely to plan to stay in the city permanently, and have slightly more "individualistic" attitudes. Attitudes and beliefs potentially have important effects on households' investment decisions. Recent work (e.g. Mani et al. 2013; Haushofer and Fehr 2014) has found that the insecurity created by poverty can make it difficult to focus on long-term goals and lead to short-sighted behavior.

Existing experimental work studies the effects of interventions that differ from the one studied here in important ways. One set of related studies is those of housing or rental subsidies that require relocation to receive the transfer, which means that the location of housing can drive effects.[3] Barnhardt et al. (2017) and van Dijk (2019) find, for example, that rental subsidies lead to broken social networks and differential effects on labor market outcomes depending on the location of the housing. 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 some long-term positive effects of an intervention explicitly motivated by moving households to wealthier neighborhoods. In the intervention I study, winners may rent out their homes and are not required to relocate.

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 found null or negative effects on human capital investment and employment, but these transfers have been in different institutional contexts and entail different vehicles. In low- and

[3]Barnhardt et al. (2017, 7), for example, state that "failure to pay monthly rent resulted in the occupant losing the legal right to remain in the property."

middle- income countries, other types of asset transfers such as urban land-titling (see e.g. Feder and Feeny 1991a; Field 2005a; Di Tella et al. 2007; Galiani and Schargrodsky 2010a) 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. The long-term effects of small cash transfers on educational attainment have, so far, appeared to be null or modest (e.g. Araujo, Bosch, and Schady 2016; Haushofer and Shapiro 2018; see Bouguen et al. (2018) for a review) with a few exceptions (e.g. Mollina Millán et al. 2020). Streams of income through cash transfers are uncertain; they may be reversed, cancelled, or changed in value by future administrations. The uncertainty itself could inhibit investment in human capital.

This paper is among the first to study the effects of a common policy that may facilitate asset accumulation and fundamentally change the trajectories of families. Subsidizing homeownership is an initiative pursued by governments in wealthy, low-, and middle-income countries alike, yet causal identification of the effects of these policies is difficult. Like the home mortgage interest deduction in the United States (see Glaeser and Shapiro 2003), these programs often benefit middle-class households rather than the poor; studying their effects is thus also essential to understanding the growth of inequality.

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

1.2 The Intervention

The intervention studied here is motivated in part by the growing demand for urban housing; in India, about 404 million people are expected to migrate to cities by 2050 (UN World Urbanization Prospects 2015). As a result, governments have also attempted to increase the 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. Moreover, in 2015, India's federal government claimed a housing shortfall of over 18 million units to motivate a plan, Pradhan Mantri Awas Yojana (P-MAY, or "The Prime Minister's Housing Scheme), to build 20 million affordable homes by 2022. Grants to subsidize the construction and sale of low-income housing by local municipal boards remain a central component of this policy.

This study is based in Mumbai, Maharashtra, an area that attracts migrants from all over India.[4] I study the effects of an annual housing lottery run by the Mumbai

[4]The population growth rate for Mumbai from 2010-2018 was approximately 13%. The private sec-

Table 1.1: Lotteries included in the sample

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

[1] In square feet. Refers to "carpet area", or the actual apartment area and excludes common space.
[2] Price at which winners purchased the home in INR with the cost stated in the lottery year. In 2017, about 64 rupees made up 1 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.

Housing and Area Development Authority (MHADA), a subsidiary of the Maharashtra Housing and Area Development Authority that uses the same acronym. MHADA runs subsidized housing programs for economically weaker section (EWS) and low-income group (LIG)[5] urban residents who 1) do not own housing, and 2) who have lived in the state of Maharashtra for at least 15 continuous years within the 20 years prior to the sale. Winners have access to loans from a state-owned bank, and most take out 15-year mortgages. Interests rates are high, hovering between 10 and 15%. While the downpayment and mortgage leave this program out of the reach of many of the city's poorest residents, it gives eligible middle-class families without property the opportunity to purchase heavily subsidized apartments. I include lotteries that took place in 2012 and 2014. Information about the area, cost, and downpayment for the apartments in the included lotteries can be found in Table 2.3.

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 2.3 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 lie anywhere between Rs.661,700 (about $10,300 at 2017 conversion rates) to Rs.2,869,015 (about $45,000). These prices do not account for untaxed informal payments made above the list price, so can be thought of as a lower bound on the potential value of the lottery homes.

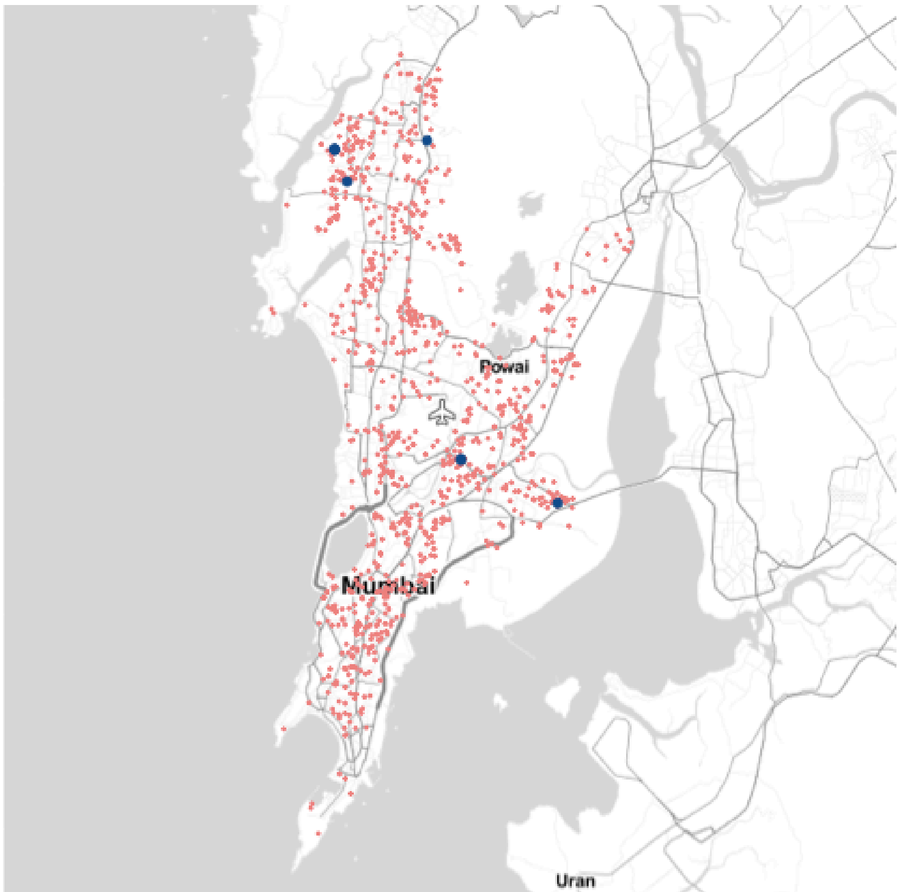
The differences also provide some sense of the opportunity cost of each program for the government. Housing was constructed on land obtained for free from the city's dismantled textile industry - this land has been earmarked specifically for "social" projects and cannot be used for other purposes (Madan 2016). This means that the homes for sale do not lie on the city's outskirts, but are within the major metropolitan area and near major highways and transit lines. Each is within walking distance of the Mumbai suburban rail network, the main network that millions of city residents use to commute every day. Figure B.1 shows the location of the 2012 and 2014 EWS and LIG MHADA apartment buildings and households in the sample at the time of application. Households were permitted to choose the building for which they submitted an application.[6]

tor has been unable to meet the resulting growth in housing demand for one main reason: supply is constrained by a strict building height-to-land ratio. This rule originally stems from the facts that much of the city occupies land reclaimed from the Arabian Sea and that the airport lies near the center of the metropolitan area. Developers are thus incentivized to devote valuable central city square footage to higher end buildings, leading lower-income households to occupy slums, crowd into extremely small homes with friends and relatives, or live far from the city. One survey respondent, for example, claimed to have lived 2.5 hours by train from his place of work when he first moved to the city. According to the 2011 census, roughly 40% of the population of the Mumbai Metropolitan Area lives in slums (Government of India 2011).

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

[6]The centrality of program housing will likely vary across cities. Nevertheless, development authorities in other major cities, such as Delhi and Bangalore, offer housing areas that are similarly somewhat

Figure 1.1: Location of the addresses of households in the sample (small pink dots) along with the location of apartment buildings (large blue dots) at the time of application



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 apartment prices are sticky and early sales create a low "benchmark" for future sales in the same apartment complex.

Households can, however, put the apartments up for rent.[7] Fifty percent of house-

central such as Vasant Kunj (Delhi) and Koramangala (Bangalore).

[7]The ability of households to legally rent out lottery housing will likely vary across cities as well. Households will always, however, be able to rent homes out illegally. Development boards in other cities also may have other provisions to increase the flexibility of beneficiaries. The Bangalore Development Authority, for example, allows beneficiaries to swap allotted houses.

holds in the study have made this choice, and the median monthly rental income net of mortgage payments is Rs.3000, or roughly 50 USD. Finally, households do not pay taxes on their dwelling for five years after possession.

As mentioned earlier, beneficiaries were selected through a lottery process. In fact, the winning sample was stratified by caste and occupation group (Table B.1), as each lottery had quotas for these groups within which random selection occurred. The lottery 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 the allotment process was fair, or truly randomized. First of all, in response to extreme scrutiny over the selection process and concerns about corruption, the lottery was conducted using a protected computerized process that was implemented in 2010.[8] 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 number and whether or not they met the criteria for eligibility. Note that applicants are able to apply for multiple lotteries within the same year, but only once per lottery.[9]

1.3 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.[10] All winners from the relevant year were included in the sampling frame. There are about 1,000 applicants for each apartment; for this reason, I interviewed a random sample of non-winning applicants. I procured from MHADA phone numbers and addresses for both winners and a random sample of applicants drawn in the same stratified method used for the selection of winners. One non-winner was drawn for each winner within each block. Recall that a block consists of a caste/occupation group within each 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 (either within a year or across years), they would have a higher likelihood of appearing in either the sample of treatment or control households. The sampling procedure explicitly allowed for the possibility of the same household being drawn multiple times, and I had planned to include multiple rows for the household in question in this situation. For

[8] 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.

[9] 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.

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

example, if a household won lottery A but also was drawn in the sample of non-winners for lottery B, its data would have been included as a set of outcomes under treatment for lottery A and under control for lottery B. Ultimately, no households were drawn more than once, likely reflecting the fact that being sampled from the pool of applicants is an extremely rare event.[11]

I accessed a total of 1,862 pre-treatment addresses, or those used at the time of application to the lottery. These addresses were first mapped by hand using Google Maps. Addresses that were incomplete (42), outside of Greater Mumbai (611), or could not be mapped (146) were removed from the sample. This left 531 and 532 control and treatment households, respectively. Table B.2 demonstrates that even after this mapping procedure, I was left with roughly equal proportions of winners and applicants in each caste/occupation category, lottery income category, and apartment building. Given the assumption that the lottery was truly randomized and the fact that I used pre-treatment addresses for the mapping exercise, there is no reason to expect the mapping exercise to systematically favor treatment or control units.

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 B.3 indeed shows that proportions of membership in certain categories in the mapped sample are significantly different from the original sample obtained from MHADA. Importantly, there are relatively fewer Scheduled Tribe members and more General Population (or Forward Castes) members in the mapped sample than in the full sample provided by MHADA.[12] 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. [13]

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

[11] As described by de Chaisemartin and Benhaghel (2015), this is a case of a randomized waiting list. I use what they describe as the consistent "Initial Offer" estimator. Not that given the size of this lottery, however, the bias of the "Ever Offer" estimator may be approaching zero.

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

[13] Once mapped, I can place households into state and municipal electoral wards to test for evidence of selection into the mapped treatment group by electoral ward. Selection by ward would indicate that individuals from certain locations or with certain political representatives are more likely than others to win the lottery. Here, I estimate regressions of the treatment indicator on the state and municipal ward membership indicators and calculate a heteroscedasticity-robust Wald statistic for the hypothesis that the coefficients on all of the indicators (other than 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 leave me unable to reject a null hypothesis that members of any electoral constituency were equally likely to be in the mapped treatment group.

organization to contact the households and conduct interviews.[14] The process for contacting was as follows: The addresses and phone numbers provided by MHADA constituted the contact information for households at the time of application. Non-winners were attempted at these addresses. In cases where they had moved away, neighbors were asked for updated contact information, with which the enumerators once again attempted to contact non-winners. Winners were initially approached at either the old addresses or new lottery buildings, based on whether or not lottery apartment "society" chairmen reported that they were renting out their units.[15] Lottery housing societies were thus first contacted to ascertain which of the winners were living at the apartments. Owner-occupiers were approached at the lottery apartments; landlords were approached at the addresses listed on the application using the procedure developed for non-winners.

In all cases, we attempted to speak to the individual who had filled out application for the lottery home. Applicants applied with their Permanent Account Numbers (PAN), which are linked to their bank accounts.[16] Given the sensitive nature of the information required for application, I assumed that the individual applying was most likely to be the head of the household. In the case a child had applied for the home (likely because the form could be completed online and youth may be better able to use computers and the internet than their parents), enumerators were instructed to speak to the household head. Interviews were conducted on Sundays and weekday evenings, or times when this individual was mostly likely to be home. In my sample, 78% of respondents had reportedly completed the applications themselves.

To recap, here is a timeline of the events relevant to the study:

May 25, 2012: Winners of 2012 lottery announced

May 2013: Winners of 2012 lottery begin taking possession

June 25, 2014: Winners of 2014 lottery announced

June 2015: Winners of 2014 lottery begin taking possession

September 15, 2017-May 15, 2018: Surveys

Table 1.2: Reasons for attrition with p-values for difference in proportions tests.

	Control	Treatment	p
Surveyed	413	421	0.6
Address not found	9	7	0.8
Home demolished	1	0	1
Home locked	5	11	0.2
Respondent deceased	1	0	1
Refused	14	20	0.4
Unable to locate household that has moved	19	10	0.1
Incomplete survey	37	31	0.5
Total	**500**	**500**	-

The sample

The data collection process yielded a sample of 834, with 413 (82.6% contact rate) of the surveyed households in the control condition and 421 (84.2% contact rate) households in the treatment condition. Full information on the number of households contacted in each stratum along with reasons for attrition can be found in Table B.4. I do not see strong evidence of differential rates of contact for control and treated units; the p-value for the difference in proportion contacted is 0.8. Balance tests for fixed or baseline characteristics among the contacted sample can be found in Table 2.4. Winners and non-winners appear to be similar based on a number of fixed observable covariates, limiting concerns of corruption in the lottery or differential selection into the treatment groups. Importantly, both treatment groups have an equal proportion of those belonging to the *Maratha* caste group, a dominant group in Mumbai and Maharashtra more generally.[17] This is among the most politically powerful caste groups in Mumbai, and therefore particularly likely to call in a favor and "win" the lottery. In line with my pre-analysis plan, I also perform an omnibus test to judge whether observed covariate imbalance at the household level is larger than would normally be expected from chance alone. This test involves a regression of the treatment indicator on the covariates (Table B.5) 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

[14] The organization hires its enumerators from local neighborhoods, which is a practice that was important to the success of contacting my sample households. More information about the firm, Partners for Urban Knowledge Action Research (PUKAR), can be found here.

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

[16] A PAN is issued by the Indian Income Tax Department to all eligible for an income tax. Its stated purpose is to minimize tax evasion. It has evolved to become a unique identifier for financial transactions and is mandatory for actions such as opening a bank account or receiving a taxed salary.

[17] *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.

test is 0.39.[18]

Table 1.3: 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=3,170				
Age	35.874	0.095	0.574	0.869
Female	0.485	0.000	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, with errors clustered at the household level for individual results.

[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 1.4 provides a summary of the main outcome variables of interest among the surveyed control group. The sample is at about the 61st percentile for mean years of education in Mumbai based on the India Human Development Survey- II (IHDS-II), which was conducted in 2010 (Desai and Vanneman 2016). Most live in dwellings with permanent floors and roofs. 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

[18]Other balance tests are available in appendix.

main earner has informal employment with the private sector.[19] None of the applicants, by rule, owns housing in the state of Maharashtra, and 57% claim to live in rental housing, while 77% report living in homes shared with extended families.[20] EWS and LIG group membership is defined by annual income caps of Rs.192,000 and Rs.480,000, placing the highest earners in each category in the 47th and 94th percentile of annual income in Mumbai as collected by IHDS-II.[21] 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 of the housing program to be working class households (Madan 2016). Citing experience from Latin American cities, Alan and Ward (1985, 5), find that public housing interventions generally do not benefit a city's poorest citizens, 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.

1.4 Estimation

I follow my pre-analysis plan[22] and estimate the treatment effect β, on i households or individuals across the pooled sample of lotteries. In the following equation, Y_i is the outcome (as measured through a survey), T_i is an indicator for treatment (winning the lottery), and $C_1...C_j$ is the group of fixed (or pre-treatment) covariates used for randomization checks, and ϵ_i is an error term. Given that randomization happened within blocks, I treat each of the blocks as a separate lottery and include a set of centered dummies, $B_1...B_l$ for each. Following Lin (2013), I allow for heterogeneous effects within the blocks by centering the block dummies and interacting them with the treatment indicator:

$$Y_i = \alpha + \beta T_i + \sum_1^j \gamma_j C_j + \sum_1^l \omega_l B_l + \sum_1^l \eta_l (T \times B_l) + \epsilon_i \tag{1.1}$$

I label households as "treated" if they win the lottery in the specific year for which they appear in the sample. While this study potentially suffers from two-sided noncompliance (8% of treated units did not purchase homes), I simply conduct an intent-to-treat (ITT) analysis.[23] β can thus be interpreted as a weighted average of block-specific intent-

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

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

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

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

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

Table 1.4: 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
Household education and employment variables				
Public school (sons)	0.06	0.23	0.00	1.00
Public school (daughters)	0.05	0.22	0.00	1.00
After school tuition (sons)	0.20	0.39	0.00	1.00
After school tuition (daughters)	0.19	0.38	0.00	1.00
Main earner salaried	0.80	0.40	0	1
Main earner has govt. job	0.18	0.38	0	1
Individual education and employment variables				
Years of education	10.31	4.67	0	18
Working	0.46	0.48	0	1
Working full-time	0.47	0.49	0	1
Working part-time	0.09	0.30	0	1
Attitudes (main earner)				
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

to-treat effects. 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. When an outcome is not binary or categorical, treatment effects are reported in standard deviations of the control group. The full regression output with and without covariates can be found in Appendix.

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.[24] Regressions here include block-centered dummies, covariates, and errors clustered at the household level.

Again, note that this paper estimates average treatment effects across the different types of payout structures chosen. This is mainly because this choice reveals a type, and types remain unknown among the control group.[25] As a result, it is not possible to measure the effects conditional on this choice, let alone the effect of this choice itself, without additional modeling assumptions. Predictors of moving can be found in Table A.24. More generally, the study is not powered to detect heterogeneous effects at the household level.

1.5 Results

Figure 1.2 presents results for education and employment related variables measured at the individual and household levels. Household-level employment effects refer to the household's main earner. Household-level educational investment effects refer to whether an outcome holds for *any* of the sons or daughters. In an exploratory analysis, I find that positive effects on education and employment are particularly large among older youth. I also find evidence to suggest that gains in employment arise from a higher likelihood of having full-time or even salaried work.

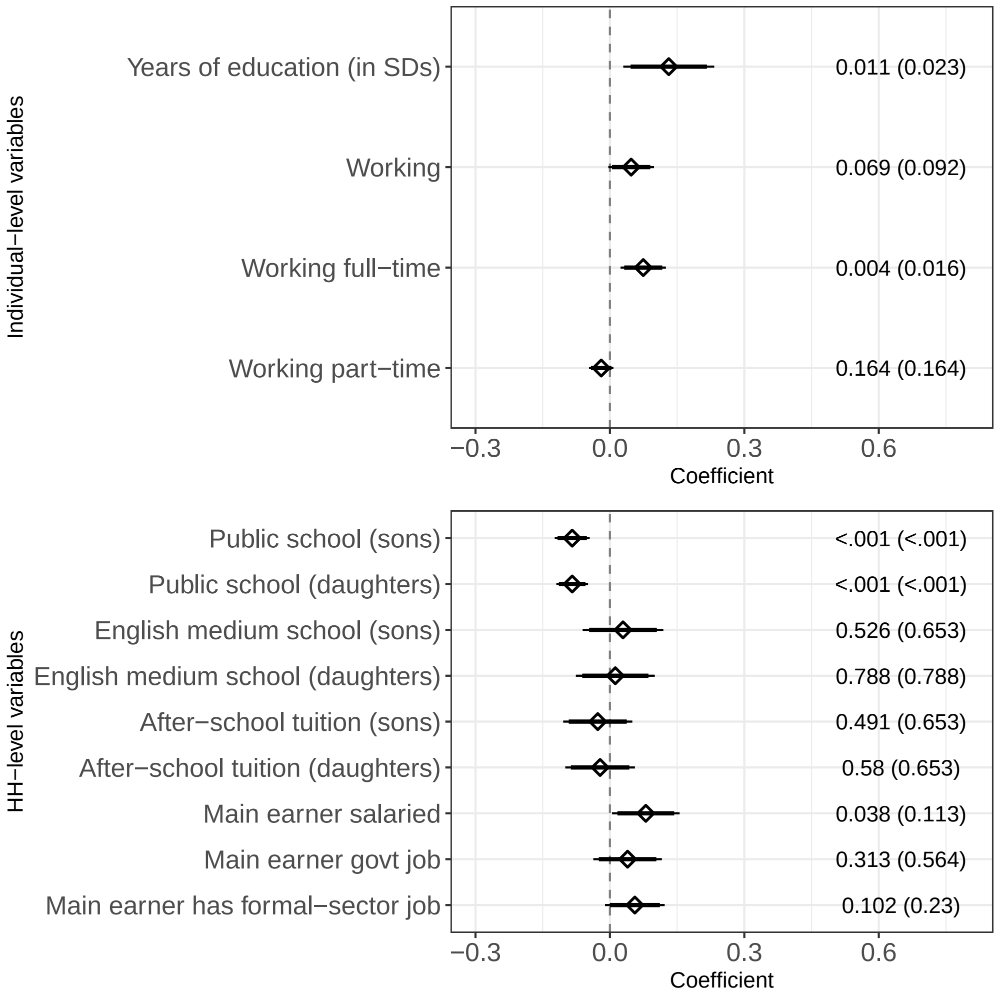
Education

First, I estimate that the average number of years of education among winners is 0.13 standard deviations, or about 0.60 years, greater than among non-winners. On average, non-winners report having completed 10.31 years of education; based on data from

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

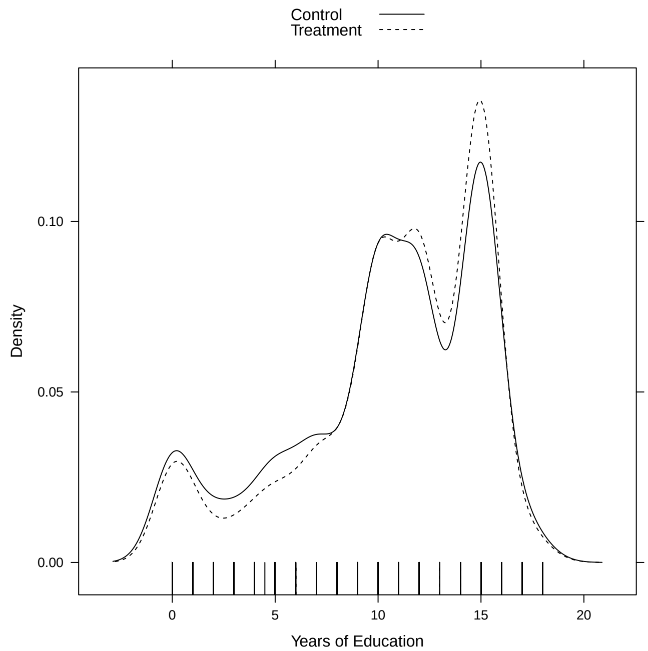
[25]Control group households do not seem to be good at describing their counterfactual behavior. In the survey, I asked them whether they would have chosen the in-kind transfer and moved into the homes had they won. About 95% said that they would, but only 50% of winning households chose the in-kind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

Figure 1.2: Treatment effects on household educational attainment and employment outcomes.



Employment here means having worked one hour or more in the past week. Full-time employment means working five days or more in the past week, while part-time employment refers to working fewer than five days in the past week. Household-level employment effects refer to the household's main earner. 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 intables A.10- A.12

Figure 1.3: Distribution of individual years of education for the whole sample drawn using a Gaussian kernel.



IHDS-II (2016), the intervention shifts individuals from roughly the 61st to 63rd percentile for educational attainment in Mumbai. The intervention shifts individuals from roughly the 75th to 78th percentile of educational attainment in urban areas more generally. At the household level, 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 if children attend a public ("government") school is a more common way to draw the distinction between public and private schools than by asking if children attend private schools.[26] This is likely due to the extreme heterogeneity in the types of non-government providers of education in India; a private school can refer to a prestigious international school, or it could refer to a school run out of a private home (Harma 2011). In spite of this heterogeneity, public schools are free and have the well-earned reputation of being significantly lower quality than their private counterparts in urban India (Kingdon 1996; De and Drèze 1999). These results are not accompanied by any measurable effects on a common practice in India, namely sending children to after school tuition. Note also that effects do not differ for sons and daughters, but this may be due to social desirability bias in responses.

At what margin does this shift occur? The distribution of the individual years of education for those living in winning and non-winning households shows a multimodal

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

distribution of educational attainment, with means at 0, 10, 12, 15 years of education (Figure 1.3). The modes at 0, 12, and 15 years represent barriers to beginning schooling, beginning post-secondary schooling, and beginning graduate schooling respectively.[27] The mode at 10 years possibly reflects the barriers to continuing education past 10th grade that are particularly high in India. Here, students sit for national or state board exams (depending on their school's affiliation) at the end of grade 10. Only if they pass this exam can students advance past grade 10. Those who pass also receive a Secondary School Certificate, which is in itself a certification that is often required for certain jobs. Stopping one's education at grade 10 can be the result of a failure to pass the exam or the decision to discontinue schooling; continuation of school after grade 10 should increase rates of both secondary school completion *and* rates of post-secondary school education.

In an exploratory analysis, I next consider whether winning the housing lottery increases the likelihood of overcoming each of these barriers (Table 1.5).[28] I estimate regressions of completing one's education past these barriers on the treatment indicator. Belonging to a household that has won the lottery indeed increases the likelihood of moving past grades 10 and 12 and completing post-secondary education. It does not seem to have an effect on actually beginning one's education. I also include an interaction with the treatment indicator and an indicator for whether each individual turned 6, 16, 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 at the conventional ages for completing one, ten, twelve, and fifteen years of education in between the lottery and being surveyed.[29]

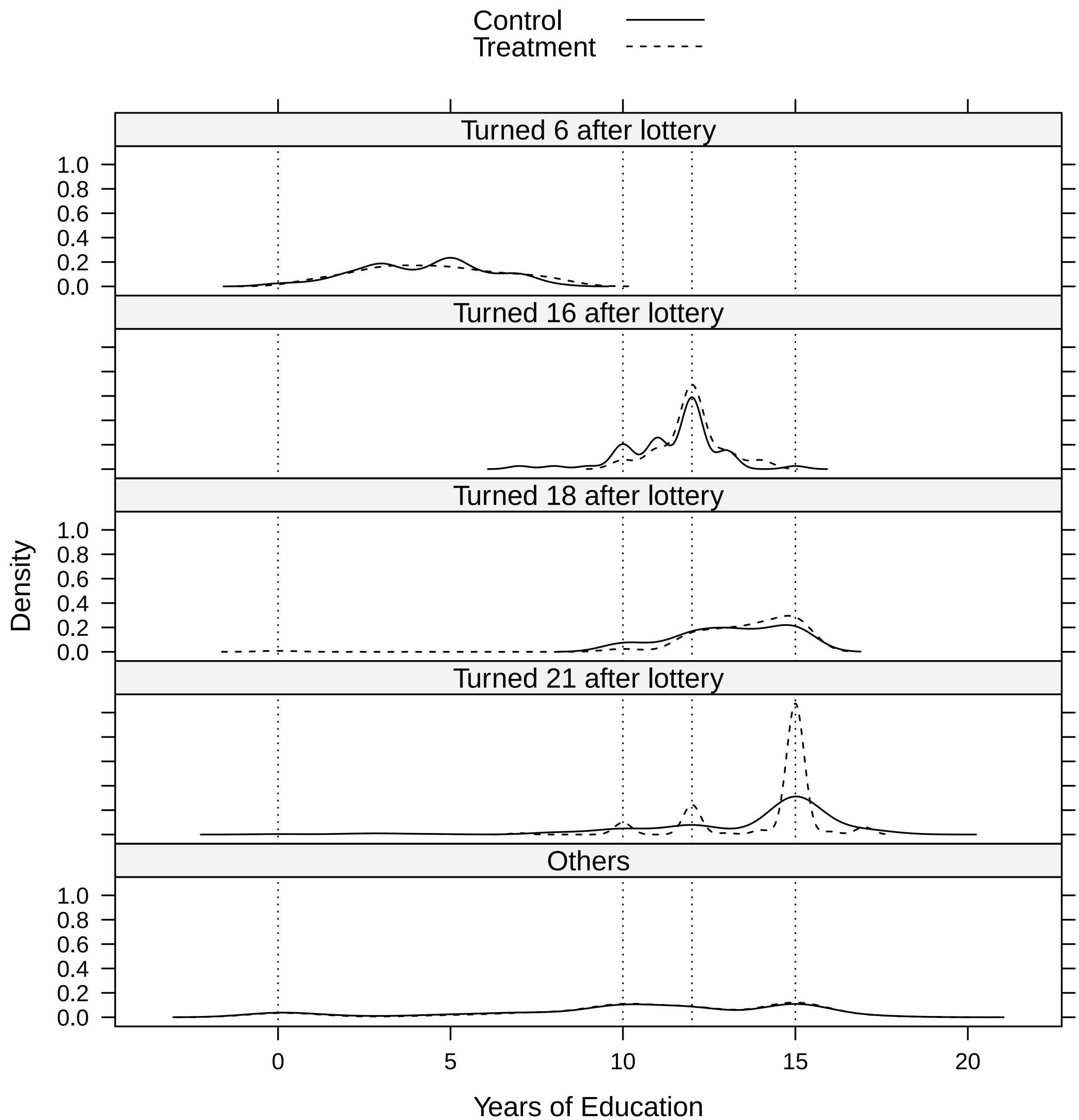
I see some evidence to suggest that the housing lottery's effect on completing grades ten and college is stronger among those who turned 16 and 21 after winning, respectively (Table 1.5) . Figure 1.4 clearly shows that effects are concentrated among individuals who

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

[28] This analysis was not preregistered.

[29] As the survey did not collect information on dates of birth or age at the time of the lottery but age at the time of the survey only, this coding was done using the following logic: For applicants to the 2012 and 2014 lotteries, surveys were conducted 5 years and some fraction of a year or 3 years and some fraction of a year after the lotteries, respectively. Suppose an individual was age_s on the date of the survey in 2017, s, and participated in the 2012 lottery. On date s in 2012, she would be exactly $age_s - 5$. If her birthday had occurred between the lottery and the survey, she would have have been $age_s - 6$ at the time of the lottery. If her birthday had occurred before the lottery that year, she would be $age_s - 5$ at the time of the lottery. This same logic holds for participants of the 2014 lottery, except the lottery age could be either $age_s - 3$ or $age_s - 4$. In this way, one can code two possible ages age_l for individuals at the time of the lottery using age_s, which we will call $age_{\bar{l}}$ and $age_{\underline{l}}$ to correspond to the older and younger possible options. Individuals are further coded to have turned X years old ($Turned_X$) after the lottery if age_s is greater than or equal to X and age_l is less than X. Given the two possible values for age_l, there are also two values for $Turned_X$. For simplicity, tables in the text present results assuming all individuals were $age_{\bar{l}}$ at the time of the lottery. Results using $age_{\underline{l}}$ are similar and presented in appendix.

Figure 1.4: Distribution of individual years of education by cohort drawn using a Gaussian kernel.



Vertical lines drawn to show 0, 10, 12, and 15 years.

Table 1.5: Regressions of individual completion of various years of education on the treatment indicator.

	Dependent variable:								
	Years of education	I(>0 years)		I(>10 years)		I(>12 years)		I(≥15 years)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T	0.618	0.009	0.010	0.071	0.056	0.056	0.039	0.041	0.036
	(0.177)	(0.009)	(0.009)	(0.018)	(0.019)	(0.019)	(0.021)	(0.017)	(0.017)
$Turned_6$[1]			0.057						
			(0.017)						
$Turned_{16}$					0.333				
					(0.042)				
$Turned_{18}$							0.387		
							(0.051)		
$Turned_{21}$									0.351
									(0.050)
T$\times Turned_6$			−0.017						
			(0.018)						
T$\times Turned_{16}$					0.093				
					(0.050)				
T$\times Turned_{18}$							0.106		
							(0.067)		
T$\times Turned_{21}$									0.114
									(0.068)
Constant	10.230	0.935	0.932	0.505	0.487	0.318	0.298	0.258	0.234
	(0.131)	(0.006)	(0.007)	(0.013)	(0.013)	(0.013)	(0.014)	(0.012)	(0.012)
Observations	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.051	0.047	0.049	0.053	0.088	0.058	0.109	0.058	0.107
Adjusted R^2	0.010	0.006	0.007	0.012	0.048	0.017	0.069	0.018	0.068

All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies.

[1] "$Turned_X$" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using $age_{\bar{i}}$, or each individual's oldest possible age.

were of secondary and post-secondary school age after the lottery, rather than younger or older individuals. In particular, the figure displays a roughly 14 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.

Imbalance in the age distribution for the relevant cohorts cannot account for these results. Table 2.4 shows that winners are slightly older than non-winners. As shown in Table A.5, this difference appears to be concentrated among older individuals, but is not statistically significant for any age group.

Table 1.6: Regressions of individual employment on the treatment indicator.

	Dependent variable:					
	Employed					
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.042	0.038	0.051	0.045	0.035	0.058
	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)	(0.029)
$Turned_6$[1]	−0.016	−0.470				
	(0.012)	(0.014)				
$Turned_{16}$	0.001		−0.446			
	(0.025)		(0.027)			
$Turned_{18}$	0.138			−0.217		
	(0.035)			(0.052)		
$Turned_{21}$	0.644				0.160	
	(0.036)				(0.045)	
Older	0.566					0.406
	(0.013)					(0.024)
T×$Turned_6$		−0.023				
		(0.021)				
T×$Turned_{16}$			0.058			
			(0.041)			
T×$Turned_{18}$				0.065		
				(0.071)		
T×$Turned_{21}$					0.164	
					(0.068)	
T×Older[2]						−0.021
						(0.035)
Constant	0.005	0.475	0.473	0.461	0.439	0.166
	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.020)
Observations	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.249	0.072	0.074	0.042	0.049	0.163
Adjusted R^2	0.216	0.031	0.034	0.0001	0.007	0.126

All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies.

[1] "$Turned_X$" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using $age_{\bar{I}}$, or each individual's oldest possible age.

[2] "Older" is an indicator for an individual being older than 21 at the time of the lottery.

Employment

Table 1.6 and Figure 1.2 show that these gains in educational attainment are accompanied by effects on individual employment. Employment here means having worked one hour or more in the past week. Tables 1.7 and 1.8 break down effects for part-time and full-time employment, or working fewer than 5 days and working 5 or more days in a week.[30] Individuals in winning households are 4.2 percentage points more likely to be employed than those living in non-winning households. This effect can further be broken down into a 7.5 percentage point positive effect on full-time work offset by a negative effect on part-time 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 may actually decrease levels of part-time employment. As increases in employment are concentrated among the same cohort experiencing gains in education, the fact that they are better educated may help them secure full-time jobs for which there is likely greater competition or higher skills requirements than part-time labor. If the distinction between part-time and full-time labor is a rough proxy for wage and salaried labor, this breakdown in results complements positive estimates of household-level effects on the main earner being salaried or having a government job (Figure 1.2). The "main" worker is defined as the family's highest earner.

I conduct an exploratory analysis to determine whether the likelihood of employment occurs among the same groups that benefitted from gains in educational attainment. Model 1 in 1.6 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. Models 2-6 further conduct an exploratory analysis to see whether effects are concentrated among certain cohorts. 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 21.7 percentage points. This increase is in line with the finding that belonging to a winning family increases the likelihood of this age cohort completing college; children are more likely to complete their education and, in turn, more likely to find jobs.

1.6 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 to areas with better opportunities. I instead propose that effects on education are driven by increases in permanent income that shift budget constraints and preferences in the medium term.

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

One explanation for these results could be that they are driven by owner-occupiers who relocate to a new neighborhood and experience better labor market and educational op-

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

Table 1.7: Regressions of individual part-time employment on the treatment indicator.

	Dependent variable:					
	Employed (part-time)					
	(1)	(2)	(3)	(4)	(5)	(6)
T	−0.021	−0.024	−0.025	−0.020	−0.021	−0.021
	(0.012)	(0.011)	(0.012)	(0.013)	(0.013)	(0.027)
$Turned_6$[1]	0.038	0.091				
	(0.036)	(0.045)				
$Turned_{16}$	0.027		0.093			
	(0.032)		(0.043)			
$Turned_{18}$	−0.039			0.063		
	(0.030)			(0.039)		
$Turned_{21}$	−0.091				−0.008	
	(0.029)				(0.028)	
Older[2]	−0.116					−0.098
	(0.022)					(0.022)
T×$Turned_6$		0.084				
		(0.071)				
T×$Turned_{16}$			0.017			
			(0.055)			
T×$Turned_{18}$				−0.036		
				(0.049)		
T×$Turned_{21}$					−0.010	
					(0.040)	
T×Older						−0.0003
						(0.028)
Constant	0.172	0.082	0.082	0.083	0.087	0.155
	(0.022)	(0.009)	(0.009)	(0.009)	(0.009)	(0.020)
Observations	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.096	0.070	0.068	0.061	0.059	0.086
Adjusted R^2	0.055	0.028	0.026	0.019	0.018	0.046

Part-time 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.

[1] "$Turned_X$" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using $age_{\bar{l}}$, or each individual's oldest possible age.

[2] "Older" is an indicator for an individual being older than 21 at the time of the lottery.

Table 1.8: Regressions of individual full-time employment on the treatment indicator.

	Dependent variable:					
	Employed (full-time)					
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.075	0.071	0.082	0.077	0.069	0.082
	(0.018)	(0.019)	(0.019)	(0.020)	(0.019)	(0.035)
$Turned_6$[1]	−0.017	−0.400				
	(0.027)	(0.032)				
$Turned_{16}$	−0.008		−0.384			
	(0.030)		(0.037)			
$Turned_{18}$	0.122			−0.168		
	(0.037)			(0.053)		
$Turned_{21}$	0.588				0.181	
	(0.036)				(0.044)	
Older[2]	0.473					0.325
	(0.021)					(0.026)
T×$Turned_6$		−0.018				
		(0.051)				
T×$Turned_{16}$			0.051			
			(0.051)			
T×$Turned_{18}$				0.049		
				(0.074)		
T×$Turned_{21}$					0.148	
					(0.062)	
T×Older						−0.009
						(0.038)
Constant	0.083	0.479	0.477	0.466	0.445	0.231
	(0.021)	(0.013)	(0.014)	(0.014)	(0.014)	(0.024)
Observations	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.211	0.082	0.084	0.059	0.071	0.138
Adjusted R^2	0.175	0.041	0.044	0.018	0.030	0.100

Full-time employment is defined as working five or more days a week. All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies.

[1] "$Turned_X$" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using $age_{\bar{i}}$, or each individual's oldest possible age.

[2] "Older" is an indicator for an individual being older than 21 at the time of the lottery.

portunities as a result.[31] I explore this possibility by examining effects on characteristics of neighborhoods based on census block and postal-code averages.[32] As shown in Figure 1.5, the intervention actually leads winners to live, on average, in administrative wards with 0.34 standard deviation lower rates of literacy and 0.33 standard deviation lower rates of full-time employment than non-winners. The lottery also causes households to live in postal codes with a lower percentage of senior secondary schools (those that offer education through grade 12), schools that are 0.22 standard deviations less likely to be taught in English (a proxy for quality), and 0.38 standard deviations less likely to have offices for headmasters (a proxy for school size). Unlike MTO, the intervention provides households with the opportunity to move to generally poorer neighborhoods. Generally, then, relocation and exposure to better educational contexts or labor markets seem to be unlikely explanations for the positive education and employment results.

Changes in the demand for education

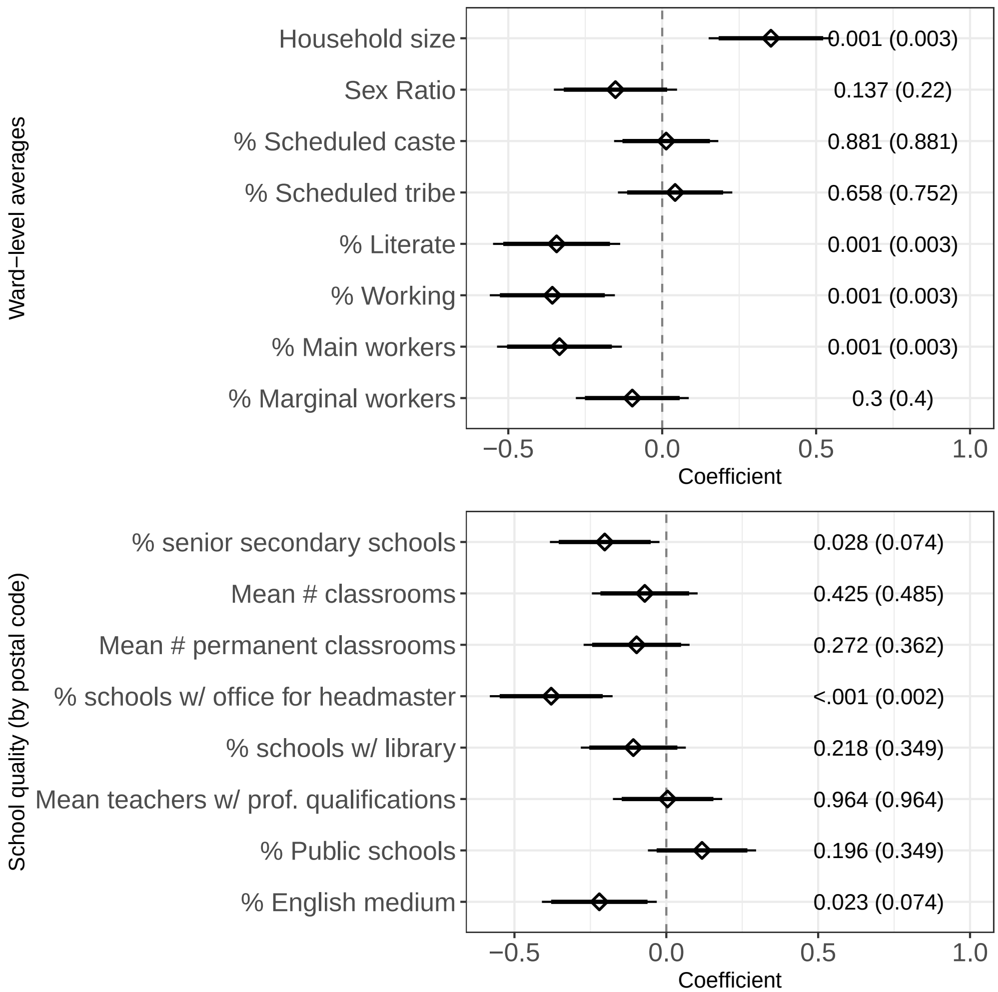
There are, however, many reasons to expect that the intervention has an effect on the demand for education. One is that the wealth transfer may shift out household budget constraints. That this shift will lead to increased educational attainment seems particularly likely given the correlation between 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; 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; see appendix for positive but imprecisely measured effects on reported monthly 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.2). Winners report being 5 percentage points more likely to ask commercial banks for loans in cases of emergency, reflecting perhaps some ability to borrow against the home or better

[31] Appendix Table A.24 presents predictors of moving. Across all models, those who relocate are less likely to be SC/ST or *Marathas*, and more likely to have had impermanent floors at the time of lottery application, and more likely to be from the same ward as the lottery apartment.

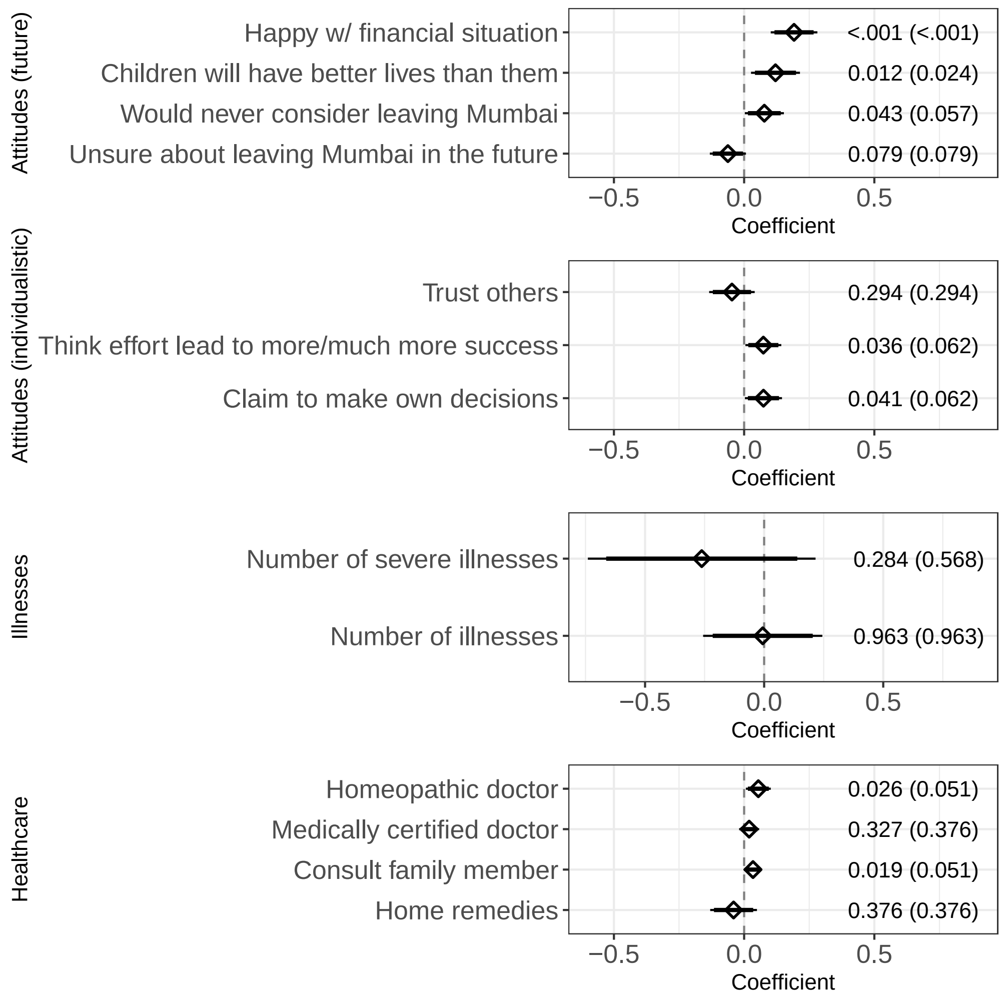
[32] Ward-level data were taken from the 2011 Indian Census. Postal-code level data for 2017 were provided by the Department of School Education and Literacy, Ministry of Human Resource Development, Government of India. Find more information at http://schoolreportcards.in/SRC-New/. Education-related variables were selected to reflect those values that could be verified by surveyors working for the Government of India, rather than school self-reported values such as pass rates.

Figure 1.5: 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 intables A.13-A.16. 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/.

Figure 1.6: 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 intables A.17-A.20. 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.

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

A large wealth transfer may increase the demand for education not only because of effects on income, but also because of changing underlying preferences. In particular, their attitudes about the future and time horizons may change. Not only are winners wealthier, but they can also expect the appreciation of home values and, therefore, household wealth. Winners seem to be well aware of this possibility; 91% of winning respondents are aware that the value of their properties had increased since purchase, 46% can place a value in INR on this increase, and 93.5% expect the value of the property to increase further in the future. Also, this increase in permanent income is relatively *certain*, unlike promises of pensions or cash payments, it cannot be revoked or changed by future administrations. 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 1.6 shows effects on the household head's self-reported attitudes and beliefs about the future alongside individualistic 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 "yes" when asked if their children will have better lives than them. Finally, they are about 8 percentage points more likely than non-winners to respond that they "would never leave" when asked if would ever consider relocating from Mumbai, suggesting increased time horizons. These findings are complementary to research (e.g. Baird et al. 2013; Fernald et al. 2008; Haushofer and Fehr 2014; Haushofer and Shapiro 2016; Ozer et al. 2011; Ssewamala et al. 2009) that has found that income shocks can increase psychological well-being, happiness, and time horizons.

These results are supported by qualitative evidence from informal interviews with winners and non-winners. Non-winners did, in general, 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 1.6). 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). Some non-medically certified doctors known as "bengali doctors" often have no formal training and provide services based on superstition or non-scientific information. They generally charge lower fees than medical doctors and, in the case of family members, may even be free. Thus changes in this reported behavior may reflect changes in preferences rather than simply shifts in budget constraints. 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.

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 1.5 also shows that the Mumbai housing lottery has no effect on having more than 0 years of education, or beginning one's education. It is possible that among certain populations, barriers to beginning one's education are either lower or higher 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 increasing the opportunity cost of sending one to school. Most importantly, the context and target population probably matter a great deal. Cesarini et al. (2016) find few human capital returns to a wealth shock in Sweden, but they argue that this is likely due in part to Sweden's strong social safety net, 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).

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.[33]

But the relationship between educational attainment and employment is one that will vary greatly across context and has yet to be fully explored in urban India, let alone Mumbai. Importantly, this study was conducted from mid-2017 to early 2018, a period which saw a spike in unemployment rates among urban youth, particularly in the informal sector.[34] 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. This conjecture is supported by the results on full-time and salaried work.

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 to 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.

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

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

1.7 A transfer to the middle class

Overall, this study finds modest educational and employment effects of a substantial housing subsidy for middle class households. How might we think about the size of these effects relative to other interventions? One benchmark is provided by Baird, McIntosh, and Özler 2019, who find that a conditional cash transfer program that gave $10 a month for two years to adolescent women increased school attainment by over than 0.6 years (over a base of 7 years) and the likelihood of completing primary school by 8 percentage points (over a base of 37%). These effect sizes are comparable to the effects on education reported in the present study at a lower cost.

One reason for modest effects of a large intervention may be issues with targeting. The program studied here is, essentially, a large transfer to fairly well-off families. It is possible that this study's sampling strategy of interviewing only applicants with coherent addresses simply drops poor winners, but it appears that the program reaches wealthier citizens *by design*. Its income thresholds and requirement that winners pay a 15 year mortgage certainly keep it out of the reach of a city's poorest residents. As a result, it is unlikely that these households are credit constrained.

These parameters are not unique to the program in Mumbai, as most similar programs in other cities entail mortgages and similar income categories. A mortgage subsidy similarly favors the wealthy (Glaeser and Shapiro 2003).[35] While it is beyond the scope of this paper to fully understand why these programs are targeting in this way, it seems possible that not only are effects small among this population, but that large transfers to the middle class in the form of housing subsidies actually exacerbate inequalities within a city. It is essential for future studies to measure both effects of similar programs on the poorer groups of applicants dropped from this study and the effects of different policy configurations that may more effectively target the poor.

1.8 Conclusion

In this paper, I propose that the main function of a subsidized housing program in Mumbai, India is the transfer of wealth to eligible middle-class households. Through a survey of winners and non-winners of multiple housing lotteries that occurred in 2012 and 2014, I find this wealth transfer increases educational attainment and employment rates, particularly among youth. Winners also possess both more optimistic and individualistic attitudes, which could be partially responsible for human capital investment and also suggest the possibility of longer-term effects. These effects occur even though

[35] Moreover, cities such as Mumbai actually have separate relocation and rehabilitation programs for poor households living in slums. These programs often require relocation; as discussed by Barnhardt *et al.* 2017 and van Dijk 2019, mandatory relocation may have adverse effects on social networks and labor market access that could offset other benefits.

winners tend to live in areas with lower levels of employment and worse schools, and are accompanied by changes in winners' attitudes about the future.

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

The program evaluated is part of a larger set of policy instruments that subsidize the price of homes. Because homes are large assets, can appreciate substantially in value in rapidly growing urban areas, and tend to be purchased by all types of families everywhere, understanding the effects of subsidizing homeownership is important to identifying important sources of human capital accumulation. These effects on human capital accumulation have implications not only for families, but also for countries and time-periods witnessing large initiatives to promote homeownership. Given the fact that households must be able to purchase the unsubsidized portion of the apartment, however, the intervention may tend to benefit 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.

2

The non-electoral political effects of welfare policies

Abstract: Welfare policies have the potential to fundamentally alter citizens' relationships with government. How do they affect an outcome unstudied in low- and middle-income countries, namely recipients' *non-electoral* political behavior? In these contexts, citizens are often described as making demands of governments primarily to access resources, suggesting that receiving resources through benefits may decrease this demand-making. Yet research on "policy feedback" in the U.S. shows that resources from benefits increase recipients' capacity for political action and motivate new demands to shape national-level policy-making and protect these resources. I study the effects of a common policy, subsidized home-prices, on demand-making in India. A natural experiment of multiple program lotteries shows that winning increases reported demand-making and knowledge about local government, even among those who rent out homes. Possible mechanisms include changes in attitudes and an increased interest in improving communities. This study highlights how motivations, beliefs, and aspirations, rather than simply need, may shape citizens' demands for resources. It also shows the potential for important policy feedback effects at the local level, where policies may not be made, but are implemented and experienced.

2.1 Introduction

Governments in many low- and middle-income countries (LMICs) devote nontrivial portions of their budgets to social welfare spending. In India, central and state governments spend on numerous policies, including pensions, electrification, employment, financial inclusion, and subsidized home-price programs. These policies often provide programmatic benefits that reach hundreds of millions. By 2015, for example, about 180 million individuals had benefitted from a rural employment program rolled out in 2005 (*India Today* 2015). The high likelihood of becoming a welfare beneficiary in India suggests that the proliferation of these programs has the potential to change the economic, social, and political landscape of the country.

Seeking to understand the political motivations for spending on such initiatives, many (e.g. Bechtel and Hainmueller 2011; De la O 2013; Diaz-Cayeros *et al.* 2016; Imai *et al.* 2019; Manacorda *et al.* 2011; Pop-Eleches and Pop-Eleches 2012; Zucco 2013) have investigated the electoral returns to welfare programs. The study of whether beneficiaries reward implementing politicians can be seen as part of a broader understanding of politics as an exchange of votes for resources, or clientelism (Kitschelt and Wilkinson 2007). Yet activity beyond voting, such as demands placed with politicians, bureaucrats, and brokers for state-provided goods and services, particularly those that are provided collectively, forms a cornerstone of political participation in many countries (Auyero 2001; Jha, Rao, and Woolcock 2007; MacLean 2011; Kruks-Wisner 2018; Bussell 2019). These everyday demands can occur even among those who engage in *quid pro quo* voting at election time. How does receiving government benefits affect the likelihood of making demands for everyday goods and services among beneficiaries?

Welfare benefits may decrease the need to take part in such activity either by providing services themselves or increasing one's capacity to procure private alternatives. Benefitting from a pension program, for example, may allow one to pay for a private water tanker rather than ask an elected official to resume a community's tardy water supply. It is possible that citizens may simply exit the non-electoral demand-making arena once they receive substantial government benefits. This prediction is supported by research on clientelism finding that citizens' dependence on state-provided resources decreases with wealth (e.g. Brusco, Nazareno, and Stokes 2004; Calvo and Murillo 2004; Dixit and Londregan 1996; Hicken 2011; Nathan 2016; Stokes *et al.* 2013) and that government benefits can break clientelistic ties with the leaders who once fulfilled these needs (Larreguy *et al.* 2015b; Ramìrez-Àlvarez 2019; Bobonis *et al.* 2017).

Yet we know from a literature on policy feedback from the United States and Europe (see Campbell 2012 for a review) that welfare policies have the potential to *increase* political participation among beneficiaries by changing their interests, capacities, and beliefs. First, they can make beneficiaries wealthier, thereby improving their self-perceived status and increasing their time horizons, both of which may facilitate political participation. Second, they can motivate beneficiaries to make *new* demands to protect this wealth, even once other needs are fulfilled. The policy feedback literature focuses mainly on beneficiaries' attempts to shape future policy-making, particularly at the national level, to do so.

Where policy implementation is decentralized and local-level administrative capacity varies, I argue that protecting benefits entails making demands for improvements at the benefits at the local level. For example, recipients of disability programs may demand more timely payments, or those participating in an employment guarantee program may wish to influence the types of projects on which they work. In other words, benefits can increase demand-making through two simultaneous channels by affecting citizens' *aspirations* and *capacity* to make demands (Kruks-Wisner 2018, 29).

To date, it has been difficult to empirically assess whether becoming a welfare recipient increases or decreases demands of the government. It is likely that beneficiaries are by nature simply more politically active than non-beneficiaries. Researchers have used the staggered or uneven rollout of programs to get around this problem and identify causal effects on other outcomes, such as turnout and vote share. Yet demand-making is rarely measured in the administrative data that such studies rely upon.

I provide some of the first empirical evidence on the effects of welfare benefits on demand-making by studying a subsidized home-price program. This welfare policy is widespread not only in India, but in LMICs and high-income countries alike. I use a natural experiment to study the effects of receiving an untaxed subsidized home for purchase in Mumbai, India on local political participation and demand-making. The program is implemented through a lottery system, allowing causal identification of its effects. I conducted original interviews of 834 winning and non-winning applicants of multiple subsidized home-price lotteries conducted in Mumbai in 2012 and 2014 to estimate its effects on local demand-making.

On average, winners are 29 percentage points more likely than non-winners to report attending municipal ward-level meetings where local communities' improvements are discussed. They are also 14 percentage points more likely to report individually approaching bureaucrats and politicians to demand improvements to their communities, 11 percentage points more likely to report doing so in groups, and 11 percentage points more likely to be able to correctly name a local elected official. Effects are accompanied by changes in attitudes. These include an increased sense of status relative to authority figures and an increased interest in local-level issues as demonstrated by reported reasons for candidate choice in local elections. They also occur in spite of increased satisfaction with local services among beneficiaries, suggesting that having one's needs met does not preclude demand-making.

This local-level participation is not confined only to those living in the new apartment buildings. Winners are not obligated to relocate to the homes, but can rent them out. Even so, landlords, or those who rent out the homes, may seek to improve communities to increase the rental or resale values of the homes. Fifty-nine percent of landlords travel considerable distances to the lottery homes to participate in collective action in the communities in which they own homes but do not live, suggesting strong incentives for organizing that are separate from the effects of social pressure within a community.

Subsidizing homeownership thus creates an interest group of beneficiaries able and motivated to protect their welfare benefits. I suggest that under certain conditions, other welfare programs providing a sustained stream of benefits over time can be thought of as providing assets that 1) make beneficiaries wealthier and 2) whose value is affected by government actions, and may thus also generate demand-making among beneficiaries.

This proposition is supported by research on a rural employment program in India (Jenkins and Manor 2017) and education and healthcare in African countries (MacLean 2011).

These empirical findings are among the first sets of causally identified effects of any government welfare policy on local-level demand making. Studying local-level demand making is particularly important in contexts with poor or variable local-level service delivery as it has the potential to play a role in mitigating these deficiencies. The findings also suggest that welfare policies have the potential to change the motivations, beliefs, and actions of beneficiaries, thereby pointing to an important avenue besides *quid pro quo* voting through which programmatic policies can affect electoral behavior in LMICs. Furthermore, the identification of these important determinants of behavior suggests that individual political decision-making may change over time in spite of fixed characteristics such as ethnicity or religion that have received more attention in studies of political behavior in India and other LMICs.

2.2 Welfare spending and demand-making in India

Since its independence, the Indian government has enacted numerous policies dedicated to supporting its founders' stated goals of poverty alleviation (Varshney 2014, 7). Such "schemes" (programs) affect the lives of millions. Table 2.1 shows the fraction of respondents of a nationally representative survey who claimed to have benefitted from various programs in 2011 and 2012 (India Human Development Survey- II (IHDS-II) 2016). Because India's population is over one billion, even the Annapurna scheme, a food security program for the elderly from which only 0.2% of the population reportedly benefits (Table 2.1), will reach more than two million citizens. Moreover, administrations are continuously seeking to create new and innovative welfare policies; in the 2019 general elections, for example, creating a Universal Basic Income program formed a key component of the Indian National Congress platform (Safi 2019).

Given that these schemes reach so many citizens, learning about their effects is fundamental to understanding long-term political trends. How do such programs shape the political behavior of beneficiaries? To date, much of the analysis of Indian politics has been through the lens of clientelism, wherein public goods and services are seen to be distributed in exchange for votes (Kitschelt and Wilkinson 2007).[1] As described in this literature, an absence of baseline service provision can create opportunities for rent-seeking among those who govern allocation.[2] As a result, a natural way to think about the political effects of welfare spending is to study the electoral returns to various

[1]See Thachil (2011) for a study of how privately provided goods may generate electoral returns.

[2]For example, representatives at India's municipal, state, and national levels receive "area development funds" to respond to requests made by constituents, and several have found that the use of these funds can be strategically targeted to win votes (Jensenius and Chhibber 2018).

Table 2.1: Fraction of respondents to a nationally representative survey reporting that they benefit from a given program.

Benefit	Fraction
Old age pension	0.0908
Widows' pension	0.0511
Maternity scheme	0.0287
Disability scheme	0.0131
Annapurna (food security) scheme[1]	0.0023
Sanitary latrines	0.0509
Kisan credit card[2]	0.0513
Indira Awas Yojana[3]	0.0514
NREGA[4]	0.2844

[1] Food security for senior citizens.
[2] Credit scheme for farmers.
[3] Rural subsidized housing program.
[4] Mahatma Gandhi National Rural Employment Guarantee Act.

Source: IHDS-II (2011-2012) N= 42,152

programs. Indeed, this is the approach taken by several who study the political effects of the Mahatma Gandhi National Rural Employment Guarantee Act (NREGA, Dasgupta 2015) in India and cash transfers (De la O 2013; Imai *et al.* 2019; Manacorda *et al.* 2011; Zucco 2013) in other countries.

Yet political engagement extends well beyond voting. An emerging literature in Indian politics focuses on citizens' everyday interactions with the state. Scholars describe efforts to access to goods and services such as jobs, roads, and lighting (Jha, Rao, and Woolcock 2007; Auerbach 2016; Bussell 2019; Kruks-Wisner 2018). Beyond simply voting for those who help them, individuals negotiate with intermediaries and place pressure on bureaucrats and officials to get what they need.

Much of this everyday demand-making is action taken to improve the provision of *collective* goods and services, as opposed to requests for individual items such as jobs or voter cards. This activity is important to study because it can alert governments to deficiencies in service provision, thereby allowing them to be addressed. For example, even while much of the literature on public goods provision highlights incentives and discretion in responsiveness, recent literature has found that politicians in India may effectively deliver constituency service to those who approach them (Bussell 2019) and that participation in government meetings is an important part of "deliberative democracy" (Sanyal and Rao 2018).

Moreover, Mumbai, the site of this study, has a process for making and receiving responses to demands for improvements to communities. This is part of a larger trend wherein several state and municipal governments in India have developed a bureaucratic process to handle complaints about government infrastructure and services. In areas governed by the Municipal Corporation of Greater Mumbai (MCGM)[3], citizens can place complaints with their local administrative units (wards) over the phone, in person, through an app, or online. The local administrative ward then assigns each complaint with a number that one can use to track its progress as it is passed to the appropriate department. Bureaucrats in the ward office mark the complaint as "closed" once it has been resolved or a reason has been given for why it cannot be resolved.[4] I scraped the website through which one makes and tracks complaints and found that 87,395 complaints were registered in 2017.[5] As shown in Figure 2.1, 89.5% of these complaints were resolved, with the resolution rate approaching 100% for several categories designated by the municipality.[6] This data is supported by qualitative interviews with lottery winners who said that the municipal government was responsive to their complaints.[7]

While potentially effective, these demands for collective benefits require organization and also entail the problem of freedridership; members of any group can defect from participation in such action yet still reap the benefits of participation by others. In a 651 household survey of slum-dwellers in Delhi, only 37% of households claiming that the sanitation condition in their neighborhood was "Bad" or "Very bad" reported making a complaint to anybody about neighborhood sanitation conditions.[8] Moreover, according to IHDS-II, only about 30% of households report ever having attended a ward or village level meetings where complaints, service delivery, and the use of development funds are discussed. Existing literature seeking to understand variation in levels of public goods provision often points to the connection between ethnic homogeneity and the provision of public goods through a variety of potential mechanisms, particularly the ability of in-group members to sanction one another for free-riding (Alesina, Baqir, and Easterly 1999; Miguel, Gugerty, and Kay 2005; Baldwin and Huber 2010).

Yet coethnicity cannot be the only mechanism responsible for participation in collective demand-making, as even diverse metropolitan communities too have developed means of cooperation; indeed, Auerbach (2017) describes participation in extremely di-

[3]Also known as the Brihanmumbai Municipal Corporation, or BMC.

[4]The modal remark for a complaint about garbage, for example, is "garbage has been lifted."

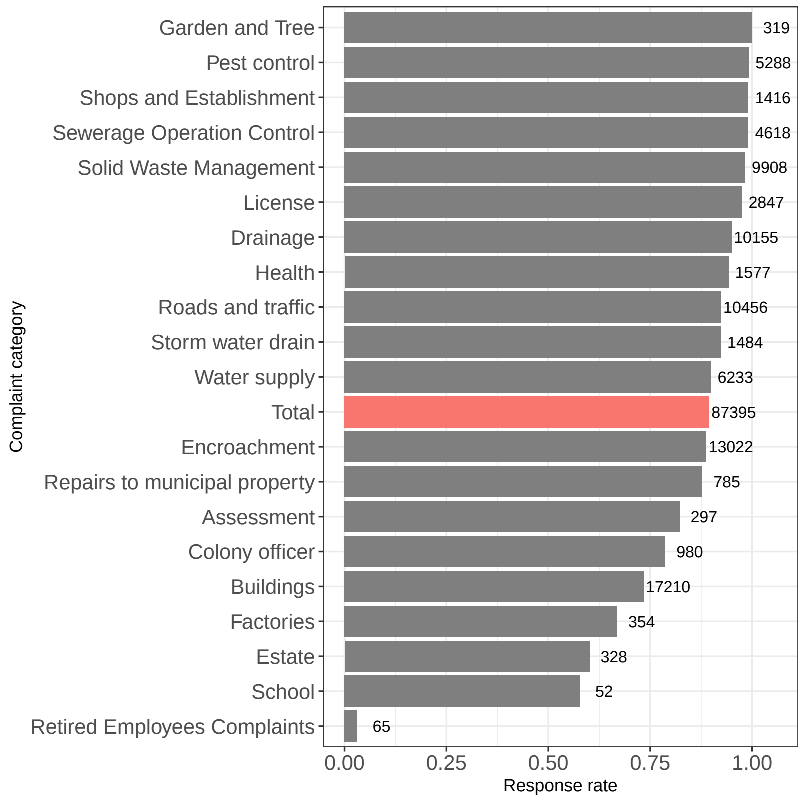
[5]In this website, one can look up a complaint by entering the ward, category, and date under which it was filed. If one enters all the possible combinations of these items, it is possible to download a complete set of complaints filed for a given time period. The website is here (https://portal.mcgm.gov.in/portal/) .

[6]Of course, there are certain types of complaints that entail costly system-wide repairs or political tradeoffs that do not receive satisfactory responses. Complaints about water pressure or poor timing, for example, often receive the reply "False complaint" or "Water reservoirs have low supply." But the point remains that there is some accessible bureaucratic process in place to ensure that once a complaint is made, it is heard and (sometimes) resolved, particularly for simple problems.

[7]During my fieldwork, I found those working in the office to be candid about the fact that the government is much less responsive to the complaints of those squatting illegally.

[8]This survey was conducted by Lokniti CSDS in Delhi in 2012.

Figure 2.1: Complaints made to and resolved by the Municipal Corporation of Greater Mumbai in 2017.



Collected from https://portal.mcgm.gov.in/. Names of categories are as they appear on the website. Numbers to the right of the bars reflect the total number of complaints made in each category.

verse urban neighborhood development societies. How might becoming a welfare beneficiary affect participation in this cooperative behavior?

The effects of welfare policies on demand-making: two potential directions

Because effective welfare programs can make beneficiaries wealthier through either in-kind or cash transfers, receiving benefits may *decrease* participation in demand-making. In Mexico, Larreguy *et al.* (2015b) find that insecure property rights create opportunities for political intermediation by municipal agents as residents seek access to titles, ways to provide proof of residence, or protection from eviction. They further find that a program issuing land titles to squatters reduce clientelistic voting for the municipal government as households' need for political intermediation disappeared. Bobonis *et al.* (2017) similarly find that building water cisterns in drought-prone areas of Brazil decreases requests of politicians, especially among citizens likely to be in what they define as clientelistic relationships.

It logically follows that welfare would also decrease the utility of non-electoral political participation among beneficiaries as well. Several programs themselves provide services to beneficiaries, thereby precluding the need for making demands for goods and services. For example, a slum rehabilitation program providing water and electricity connections could eliminate the need to organize to demand these very same items. Wealth gains from government programs may also decrease incentives to participate in demand-making by facilitating the purchase of private counterparts to state-provided services, such as water from tankers or private education. If this is true, then perhaps demand-making truly is "poor people's politics" (Auyero 2001) and welfare programs cause beneficiaries to exit this political arena. Evidence to support this proposition would be in line with claims by Chatterjee (2004) and Harris (2006) that it is urban India's poorest citizens who make everyday demands of the state.

Yet if demand-making is a function of not only need but of other variables, such as attitudes and the existence of interest groups, then becoming a welfare beneficiary might *increase* participation in demand-making. A literature on policy feedback from the United States and Europe shows that benefitting from government social welfare can generate the resources necessary for action (Campbell 2012; Lowi 1964; Mettler and Soss 2004). The fact that welfare policies effectively make beneficiaries wealthier may facilitate civic engagement in the context studied here through two main channels, namely greater *capacity* and *aspirations* to make *new* demands (Kruks-Wisner 2018, 29).

First, welfare benefits may increase beneficiaries' capacity for action. Scholarship in development economics (see Haushofer and Fehr 2014) has found that poverty can create stress and lead to short-sighted behavior; increasing household wealth could decrease discount rates and increase the mental bandwidth (Mani *et al.* 2013) to participate in demand-making. Similarly, resources may also allow households to prioritize other "higher" items on Maslow's (1943) hierarchy of needs such as belonging and self-esteem, both of which may be fulfilled by political participation. Increases in income could also change an individual's sense of her status in a community, thereby increasing the perceived likelihood of success when making a complaint. Wealth may further decrease the relative opportunity cost of participating in collective action by decreasing the value of wages relative to the individual's overall wealth. Indeed, during my fieldwork, I observed that non-beneficiaries of the program I study appeared far too stressed to think about demand-making beyond their most immediate needs.

Second, welfare beneficiaries have greater aspirations for demand-making because may be motivated to protect this newfound wealth by improving levels of service provision. Those who study the United States and Europe argue that benefitting from government social welfare can encourage political participation to ensure either the continued or increased receipt of program benefits (e.g. Campbell 2012; Mettler and Soss

2004; Pierson 1993). In India, a welfare benefit is no different from any other government provided good or service in that it may be insufficient, of poor quality, or not reach those to whom it is promised (Post *et al.* 2018). Gulzar and Pasquale (2017, 165) clearly display the huge variation in implementation quality of NREGA. Demand-making can increase the quality, and therefore real value, of a welfare benefit. Pension recipients, for example, may demand an improvement in the timeliness of service delivery. Welfare programs may thus induce new demands even if they suppress others. Such requests are for improvements in *collective* services in that they affect all beneficiaries of the program.

Increases in demand-making may be particularly visible at the *local* level. Even while the US-based policy feedback literature focuses on the effects of welfare programs on national-level policymaking, effects on local demand-making are important in places that have seen a devolution of policy implementation to lower levels of government. While many welfare programs in India are crafted at the state or national levels, local governments are often responsible for the implementation of welfare programs. For example, Roy (2015) finds that the postmaster in Bihar's Sargana locality once wielded extreme discretion over the timing of payments to NREGA workers.[9] Local officials are also likely more visible or accessible to ordinary citizens than officials at higher levels (Corbridge *et al.* 2005).[10] As a result, local officials may both appear responsible for the implementation of welfare benefits and naturally be the first individuals to whom individuals make demands related to their welfare benefits.

It thus appears that government benefits have the potential to both increase and decrease the likelihood of making demands of the local government. In support of the hypothesis that benefits generally increase this type of local political participation, Table 2.2 shows that beneficiaries of various Indian welfare programs report greater attendance of local public meetings wherein they make make complaints or demands of local government than non-beneficiaries. But of course, this pattern could simply be a result of fundamental differences between program beneficiaries and non-beneficiaries, rather than any effect of the programs themselves. It is highly plausible that program beneficiaries are simply more politically active than non-beneficiaries in the first place.

2.3 Case: subsidized home prices in Mumbai

In this study, I present the effects of one type of government benefit, namely a program that subsidizes home purchase prices. This program has been implemented in many cities globally, including those in middle, low-income, and OECD countries, and is particularly common in India. More generally, subsidizing homeownership is an initiative that exists in many forms across the globe, including mortgage subsidies. Subsidized

[9]NREGA is a program that guarantees all rural Indian citizens up to 100 days of paid work a year.

[10]See Bussell 2019 for an explanation of why motivated members of minority groups may, however, seek out higher level officials.

Table 2.2: Welfare beneficiaries and political participation

Program	Beneficiaries	Non-beneficiaries	p[1]
Old age pension	0.35	0.28	0.00
Widows' pension	0.29	0.29	0.92
Maternity scheme	0.33	0.29	0.01
Disability scheme	0.38	0.29	0.00
Annapurna scheme[2]	0.28	0.29	0.84
Sanitary latrines	0.44	0.28	0.00
Kisan credit card[3]	0.43	0.28	0.00
Indira Awas Yojana	0.44	0.28	0.00
NREGA[4]	0.44	0.23	0.00

Fraction of program beneficiaries and non-beneficiaries who report having attended a public meeting called by the village panchayat (gram sabha) / nagarpalika / ward committee in the last year. Source: IHDS-II (2011-2012) N= 42,152.

[1] P-value from a two-tailed t-test.

[2] Food security for senior citizens.

[3] Credit scheme for farmers.

[4] Rural subsidized housing program.

[4] Mahatma Gandhi National Rural Employment Guarantee Act.

home-price programs are expensive, extremely common, and their policy feedback effects remain virtually unstudied, even in the United States.

The program studied provides households with a government-constructed home at a highly subsidized price. Households can enjoy benefits even without moving; they can rent out the homes and consume the asset as a stream of payments (rental income net of mortgage) instead.[11] Such programs have been spearheaded in major Indian cities by state level development boards to build low-income housing. Moreover, in 2015, India's federal government announced a plan, Pradhan Mantri Awas Yojana (P-MAY, roughly translated as "The Prime Minister's Dwelling Scheme"), to build 20 million affordable homes by 2022.[12] Part of the program entails central transfers to subsidize state level housing programs. The government has demonstrated a financial commitment to subsidizing housing programs; in 2003-2004, for example, the central government claimed to have spent roughly 1.65% of GDP on this type of program (Nayar 2009, 99).

[11]The program is distinct from a housing program wherein beneficiaries receive subsidized rent (e.g. Barnhardt *et al.* 2017). We can think of the latter policy as *relocation* programs, as households receive benefits only if they choose to relocate. It is also different from land titling (Di Tella *et al.* 2007; Feder and Feeny 1991b; Field 2005b; Galiani and Schargrodsky 2010b) and slum rehabilitation (e.g. Burra 2005), programs that are intended to resolve issues of informality and poor service delivery in slums.

[12]This program is an extension of what used to be known as Indira Awas Yojana, which provided mostly rural homes.

In this case too it is possible that the program either increases or decreases demand-making. The subsidy has potentially large economic effects for households, and could preclude the need for households to ask local officials for assistance with individual or group-level items.[13] At the same time, however, the subsidy and becoming a homeowner might extend beneficiaries' time horizons and improve their sense of status. Moreover, as argued by those who study the effects of homeownership on political participation in the United States (DiPasquale and Glaeser 1999; Einstein 2017; Fischel 2009; Hall and Yoder 2018), owning a home, the particular welfare benefit associated with this program, should lead to local demand-making to improve communities and protect the value of the asset. In other words, this welfare program should increase local demand-making not only through wealth and attitude effects, but also because it makes local issues particularly salient for beneficiaries.

Nevertheless, positive results on demand-making for collective benefits in particular would be surprising. This is partly because these beneficiaries would face high costs of collective action as they do not know each other and have no existing stock of social capital. Furthermore, the intervention might entail relocation and remove some beneficiaries from their social networks, a phenomenon Gay (2012) finds leads to decreased political participation among beneficiaries of the Moving to Opportunity program in the United States.

It is also possible that relocation, a unique feature of this type of government benefit, is solely responsible for any observed effects. I address this concern by observing the behavior of beneficiaries who do not relocate and simply rent the homes out.

2.4 The natural experiment

Using observational evidence to learn about the feedback effects of welfare programs may generate misleading conclusions due to the fact that beneficiaries are likely to be very different from beneficiaries on a number of dimensions, making it difficult to attribute differences in behavior to the welfare benefit alone. For example, it is likely that those who are politically active are predisposed to seeking out and accessing welfare benefits. For this reason, I make use of a natural experiment wherein the allocation of subsidized home-prices is randomized among applicants in Mumbai, India to identify the effects of welfare programs on recipients' local demand-making.[14]

The Mumbai Housing and Area Development Authority (MHADA)[15] runs subsidized home-price lotteries for economically weaker section (EWS) and low-income group

[13]See Kumar 2019 for a study of the economic effects of this program.

[14]Kumar 2019 uses the same design but different outcomes.

[15]The agency is a subsidiary of the Maharashtra Housing and Area Development Authority that uses the same acronym. The state development board was formed in 1977 by the Maharashtra Housing and Area Development Act and was preceded by the Bombay Housing Board, established in 1948. The name of the older agency was something of a misnomer, as its jurisdiction spread across the state.

(LIG)[16] 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. In 2012 and 2014, the EWS group could purchase a 180 square foot apartment for about Rs.1,500,000 (about 23,500 USD at the time), while the LIG group could purchase a 320 square foot apartment for about Rs.2,000,000 (about 31,000 USD).

The homes were sold at a government "fair price" that was 30-60% of market prices. Table 2.3 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 lie anywhere between Rs.661,700 (about $10,300 at 2017 conversion rates) to Rs.2,869,015 (about $45,000). 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). Figure B.1 shows the location of the 2012 and 2014 EWS and LIG MHADA apartment buildings and households in the sample at the time of application. At the time of application, households were permitted to choose the building for which they submitted an application. Resale of the apartments is not permitted until 10 years after purchase, but households can put the apartments up for rent. Fifty percent of households in my sample have done so. Finally, households do not pay taxes on their dwelling for five years after they move in.

All applications required a refundable fee of Rs.200 (about 3 USD). At the time of purchase, a downpayment of about 1-2% was required.[17] Winners had access to loans from a state owned bank and most took out 15 year mortgages. While the downpayment and mortgage left this program out of the reach of many of the city's poorest residents, it gave eligible lower middle-class families without property the opportunity to purchase heavily subsidized apartments. This segment of the urban population was comprised mainly of renters and large extended families sharing small homes.

As mentioned above, beneficiaries were selected through a lottery process. In fact, the winners were selected within caste and occupation groups (Table B.1), as each apartment building had quotas for these groups within which randomization occurred. Because randomization occurred within these socio-economic groups, the program can be thought of as a stratified randomized experiment. The building/caste-occupation group within which randomization occurred will be referred to as "blocks" from now on. There are several reasons to believe that this process was fair, or truly randomized. First of all, after facing a great deal of scrutiny over allegations of corruption in the 1990s and early 2000s, the lottery was implemented using a protected computerized process starting in 2010. Applicants also applied with their Permanent Account Numbers (PAN), which are linked to their bank accounts.[18] Before conducting the lottery, MHADA officials used the PAN numbers to check both whether individuals had applied multiple times

[16]Members of the EWS earn up to 3,200 USD/year. Members of the LIG earn up to 7400 USD/year.

[17]Prices and downpayments vary by year and apartment location.

[18]A PAN is issued by the Indian Income Tax Department to all eligible for an income tax. Its stated

Table 2.3: Lottery apartments included in the study

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. In 2017, about 64 rupees made 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.

for the same lottery round and whether they met the criteria for eligibility.[19] Finally, I provide randomization checks by demonstrating balance on covariates across winners and non-winning applicants.

Data collection

This study is based on both qualitative interviews and a quantitative survey. Prior to the survey, I spent five months conducting qualitative interviews with bureaucrats and citizens who had participated in the housing lottery in years not included in the study. As advocated by Thachil (2018), this research helped me to design and pilot the survey used in the large scale data collection. After the survey was complete, I conducted additional qualitative interviews with this same set of citizens and bureaucrats to clarify the mechanisms behind the effects I measure. While the main findings of this paper are based on the results of the survey, I include insights from this fieldwork to illustrate the argument.

I estimate treatment effects for all outcomes based on in-person household surveys of both winning (treatment) and non-winning (control) households. I aimed to interview 500 treatment and 500 control households that were members of a sample drawn as follows: For the 2012 and 2014 lotteries, I procured from the MHADA phone numbers and addresses for winners and a random sample of applicants Because there are more than 300,000 economically weaker section applicants for roughly 300 spots, I interviewed a random sample of applicants rather than all of the applicants. This sample of applicants was drawn in the same stratified sampling method used for the selection of winners. There were an equal number of treated and control units in each block or stratum, and I accessed a total of 1,862 addresses.[20]

I next located the addresses of these households on Google Maps. Addresses that were incomplete (42), outside of Greater Mumbai (611), or could not be mapped (146) were removed from the sample. This left 531 and 532 control and treatment households, respectively. Table B.2 demonstrates that even after this mapping procedure, I was left with roughly equal proportions of winners and applicants in each caste/occupation category, lottery income category, and apartment building. Given the assumption that the

purpose is to minimize tax evasion. It has evolved to become a unique identifier for financial transactions and is mandatory for actions such as opening a bank account or receiving a taxed salary.

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

[20] 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 duplicate observations for the household in question in this situation. 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 a rare event.

lottery was truly randomized and the fact that I used pre-treatment addresses for the mapping exercise, there is no reason to expect it to systematically favor treatment or control units. Overall, however, 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 B.3 indeed shows that proportions of membership in certain categories in the mapped sample *are* significantly different from the original full sample obtained from MHADA. Importantly, there are relatively fewer Scheduled Tribe members and more General Population (e.g. Forward Castes) members in the mapped sample than in the full sample provided by MHADA. The mapped sample may thus have slightly higher socio-economic status than the full sample of applicants on average, but I detect no such differences *between* treatment and control groups.

Once mapped, I identify which state and municipal wards households are located in and test for evidence of selection into the mapped treatment group by electoral ward. A higher likelihood of certain ward members to be treated would indicate that individuals from certain locations or with certain political representatives are more likely than others to win the lottery. Here, I conduct 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 assembly constituency 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 this set of mapped households, I randomly selected 500 of the mapped households from each treatment condition to interview. From September 2017-May 2018 (after the Mumbai municipal elections in February 2017), I worked with a Mumbai-based organization to contact individuals in the households and conduct interviews.[21] The addresses and phone numbers provided by MHADA constituted the contact information for households at the time of application. Non-winners were contacted at these addresses. In cases where they had moved away, neighbors were asked for updated contact information. Winners resided at either the old addresses or new lottery buildings, as they were free to either inhabit their new property or rent it out. Lottery housing cooperative societies were thus first contacted to ascertain which of the winners were living at the apartments. Owner-occupiers were approached at the lottery apartments; landlords were approached at the addresses listed on the application using the procedure developed for non-winners. The survey firm used the same team and survey protocols to approach both winners and non-winners.

[21]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 (http://www.pukar.org.in).

In all cases, we attempted to speak to the individual who had filled out the application for the lottery home. The application required providing important and sensitive information such as personal account numbers; as a result, I assumed that the individual applying was most likely to be the head of the household.[22] In the case that a child had applied for the home (likely because the form could be completed online and older children may be better able to use computers and the internet than their parents), enumerators were instructed to speak to the family's primary earner. Given this aim of speaking to individuals who were likely to be working full-time jobs, interviews were conducted on Sundays and weekday evenings. In my sample, 78% of respondents had filled out the application themselves.

The sample

The data collection process yielded a sample of 834, with 413 of the surveyed households in the control condition and 421 households in the treated condition. Full information on the number of households contacted in each stratum along with reasons for attrition can be found in Table B.4. I do not see strong evidence of differential rates of contact for control and treated units; the p-value for the difference in proportion contacted is 0.8. Balance tests for fixed or baseline characteristics among the contacted sample can be found in Table 2.4. Importantly, there is an equal proportion of those belonging to the *Maratha* caste group, a dominant group in Mumbai and Maharashtra more generally.[23] In other words, winners and non-winners appear to be similar based on a number of fixed observable covariates and there is no compelling evidence of corruption in the lottery or differential selection into the sample.[24]

Although these households fall into the EWS and LIG income categories for the housing lottery, a summary of the assets, housing quality, education levels, and tenure status of the control, or policy target group, respondents reveals that they should not be considered among the lowest income groups in the city (Table 2.5). They are educated, most have roughly 50% of the household employed and earning, and about 31% claim to have formal employment with either the government or private sector. Most live in dwellings

[22]A personal account number is issued by the Indian Income Tax Department to all eligible for an income tax. Its stated purpose is to minimize tax evasion. It has evolved to become a unique identifier for financial transactions and is mandatory for actions such as opening a bank account or receiving a taxed salary.

[23]*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.

[24]In line with my pre-analysis plan, I also perform an omnibus test to judge whether observed covariate imbalance is larger than would normally be expected from chance alone. This test involves a regression of the treatment indicator on the covariates (Table B.5) 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.

Table 2.4: Balance tests on household characteristics

Variable	Control mean	Treatment effect	sd	Pr(>|t|)
OBC[1]	0.150	-0.021	0.035	0.543
SC/ST[2]	0.080	-0.018	0.026	0.499
Maratha[3]	0.295	0.018	0.045	0.690
Muslim	0.090	0.006	0.029	0.852
Rough[4] floor	0.031	0.028	0.019	0.136
Rough[4] roof	0.039	0.001	0.018	0.945
From Mumbai	0.097	0.023	0.030	0.454
From the same ward as the apartment	0.097	0.017	0.022	0.446

The "Control mean" column presents means for winning households. The "Treatment effect" column presents the difference between winning and non-winning households estimated through an OLS regression of each variable on indicators for winning the lottery. Each regression includes an interaction with the centered block level indicator for randomization groups. All regressions include HC2 errors. N=834.
[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] "Rough" here is a translation of the word "*Kutcha*." Variable measured at time of application through recall.

with permanent floors and roofs. As none of the applicants, by rule, owns housing in the state of Maharashtra, they are all living either in rental housing, homes with large families, or self-constructed homes to which they have no title. Many live in Mumbai chawls, or large buildings with shared taps and cheap, single room apartments. I thus describe the sample as middle class and upwardly mobile.[25]

Estimation

I estimate effects of winning the lottery within the contacted sample on reported local civic action, attitudes, knowledge of local politics, and motivations for vote choice. I follow my pre-analysis plan and estimate the treatment effect β, on i households or individuals across the pooled sample of lotteries. In the following equation, Y_i is the outcome (as measured through a survey), T_i is an indicator for treatment (winning the lottery), and $C_1 ... C_j$ is the group of fixed (or pre-treatment) covariates used for randomization checks, and ϵ_i is an error term. Given that randomization happened within blocks, I treat each of the blocks as a separate lottery and include a set of centered dummies, $B_1 ... B_l$ for each. Following Lin (2013), I allow for heterogeneous effects within the blocks by interacting the centered block dummies with the treatment indicator:

[25]This description is corroborated by an interview conducted with the commissioner of the Mumbai Metropolitan Regional Development Authority, who saw the main beneficiaries the housing program to be lower-middle class households (Madan 2016).

Table 2.5: Summary of control group characteristics

Variable	Control group mean[1](SD)
Household Assets	
TV	0.91 (0.29)
Computer	0.39 (0.49)
Working refrigerator	0.87 (0.33)
Internet	0.47 (0.50)
Scooter/2 wheeler	0.36 (0.48)
Car	0.06 (0.23)
Housing quality	
Permanent floor	0.96 (0.19)
Semi-permanent roof	0.17 (0.38)
Permanent roof	0.79 (0.41)
Private tap	0.73 (0.45)
Private latrine	0.62 (0.49)
Education and labor[2]	
Percentage of the household employed	0.48 (0.25)
Years of education (HH mean)	10.35 (2.87)
Unemployed	0.03 (0.18)
Wage laborer	0.12 (0.33)
Government employee	0.18 (0.38)
Private sector (informal)[3]	0.43 (0.50)
Private sector (formal)[3]	0.18 (0.38)
Tenure status	
Migrants	0.20 (0.40)
Have always lived in Mumbai	0.81 (0.39)
Renting	0.57 (0.50)
Sharing/live in a joint family	0.77 (0.42)

[1] Proportions may not add to 100% because of non-response to certain questions.

[2] Figures not referring to household means refer to the survey respondent.

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

$$Y_i = \alpha + \beta T_i + \sum_1^j \gamma_j C_j + \sum_1^l \omega_l B_l + \sum_1^l \eta_l (T * B_l) + \epsilon_i \quad (2.1)$$

I label households as "treated" if they win the lottery in the specific year for which they appear in the sample. While this study potentially suffers from two-sided noncompliance (8% of treated units did not purchase homes), I simply conduct an intent-to-treat (ITT) analysis.[26] β can thus be interpreted as a weighted average of block-specific intent-to-treat effects. 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.

2.5 Results: demand-making and knowledge

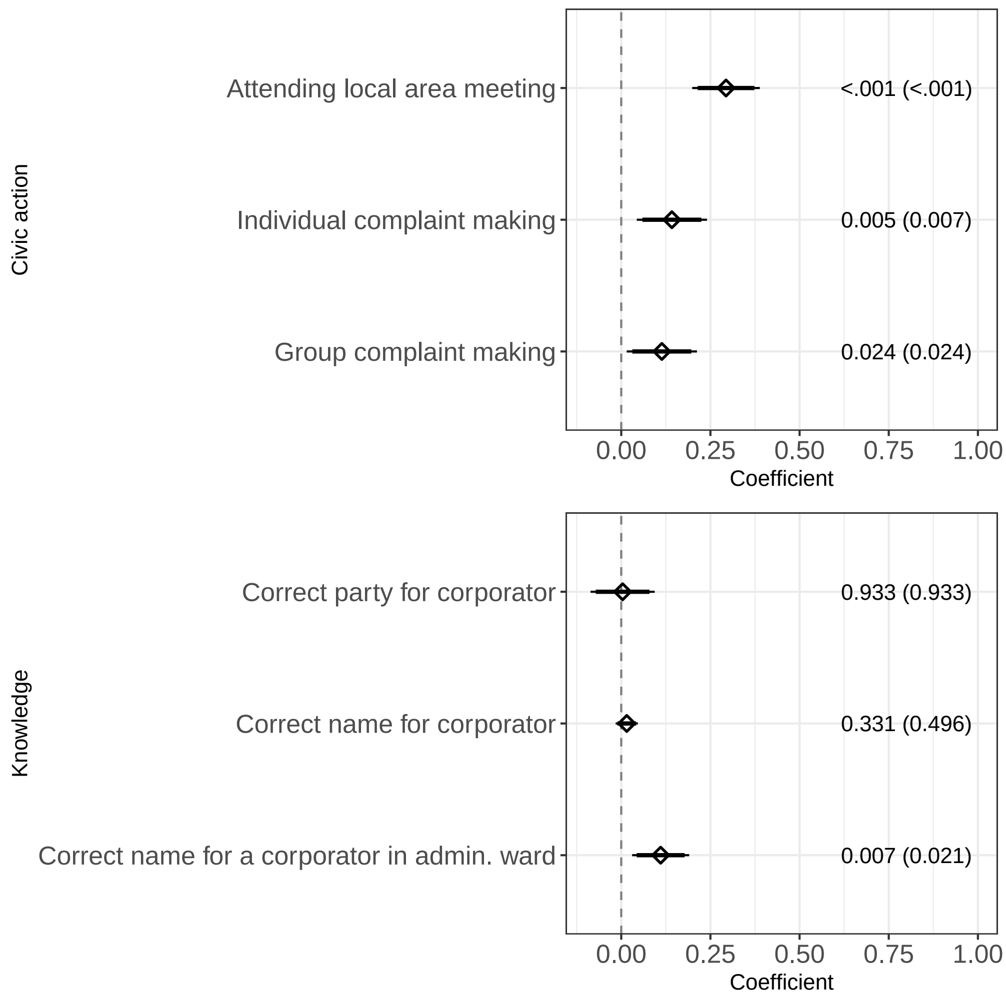
First, I measure effects on the extent to which respondents report taking action to improve their communities. Winners are about 29 percentage points more likely than non-winners to report that someone in the household has attended a local ward committee meeting in the last month. During the time of the survey, these meetings were very much preoccupied with discussions surrounding the Mumbai Draft Development Plan, or a document outlining MCGM's plan for land use in the city.

I also asked about how often they participate in both individual and group petitioning of politicians and bureaucrats for something benefitting the community. I estimate that lottery winners are 14 and 11 percentage points more likely to report making complaints individually and in groups, respectively, for "something" benefitting their communities. Based on my qualitative fieldwork, these complaints were often related to problems with water scarcity and encroachment by hawkers and squatters.

Of course, these treatment effects measure changes in reported behavior only. I also tested respondents' knowledge of local politics, with the assumption that greater local political engagement leads to greater knowledge. An individual who reports contacting a politician to ask for community improvements is more likely to know the name of the politician than one who has not claimed to contact a politician. In Mumbai, the municipal government is responsible for neighborhood problems, as demonstrated by its responsiveness to complaints about local services (Table 2.1). The election of 227 ward representatives, or corporators, to the MCGM occurred in February 2017, roughly six months prior to the survey. I therefore asked respondents for the name and party for the corporator for the electoral ward in which they lived at the time of the survey. The ward was determined using the GPS coordinates for baseline addresses for non-winners and winning landlords, and using lottery apartment addresses for winning

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

Figure 2.2: Treatment effects for main outcomes of interest



Bars show 90% and 95% confidence intervals. Full regression output with and without covariate adjustment available in Tables B.6-B.7. P-values (with p-values using Benjamini-Hochberg corrections for the false discovery rate in parentheses) are shown on the right. Treatment effects for demand-making first show the likelihood of respondents reporting attending a local area development meeting in the past month. They next show the likelihood of respondents choosing "often" or "sometimes" (as opposed to "rarely" or "never") when asked "How often in your community do you [individidually]/[in a group] petition government officials and political leaders for something benefitting your community?" Knowledge outcomes are based on respondents correctly identifying names and parties of elected officials for the electoral and administrative wards in which they live.

owner occupiers.[27] After determining the appropriate electoral ward for each household, I hand-coded responses for corporator party and name as either "correct" or "incorrect." Overall, knowledge is low; only about 2% of the control group can name the relevant corporator correctly. As seen in Figure 2.2, I do not detect treatment effects for knowing

[27] GIS maps for Mumbai's electoral wards were generously provided by the Urban Design Research Institute of Mumbai, India. More information about the organization can be found here (http://www.udri.org).

the name or party of the corporator for the ward in which respondents live.

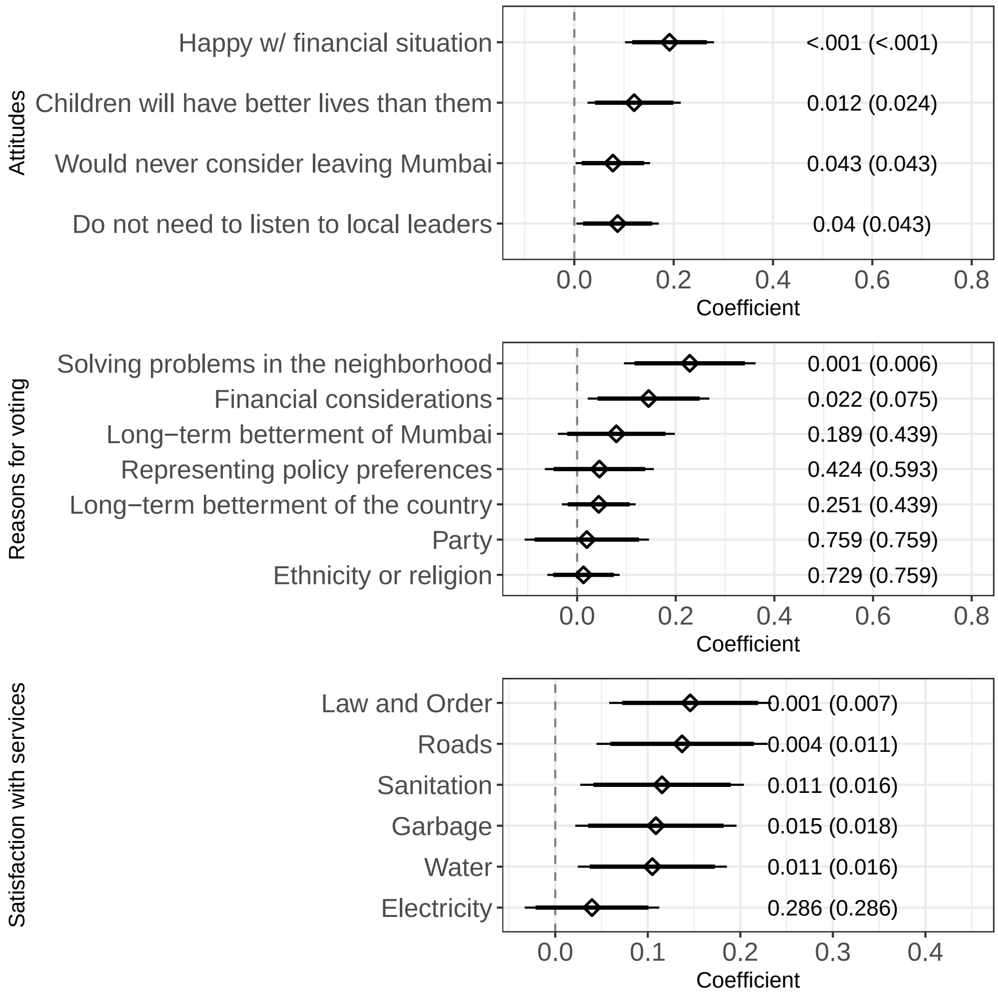
But in Mumbai, electoral wards are grouped into 24 larger administrative wards (Figure B.2) It is the administrative ward office, not the electoral ward office, that is responsible for handling complaints. Mumbai residents therefore think in terms of administrative wards, not electoral wards.[28] As a result, we might not expect complaint-making to increase knowledge of the names of corporators but we would expect complaint making to increase knowledge of the names of *any* of the corporators at the higher administrative ward-level. Within an administrative ward, certain corporators may be more active or responsive than others; a respondent may simply think that the active corporators are their representatives even when they are from a different electoral ward. Indeed, during my visits to ward offices, one or two corporators, but not all, were present to speak to constituents on a given day. I coded responses for corporator names as either belonging to the list of corporators within an administrative ward or not. Indeed, control group members are over seven times more likely to correctly name a corporator from their administrative wards than give the correct name of the corporator for their electoral wards. I therefore estimate treatment effects for correctly providing the name for a corporator from the administrative ward within which the respondent lived at the time of the interview. Correct responses among the treatment group occur at almost twice the rate of the control group (Figure 2.2 and Table B.7). Increases in reported complaint-making to benefit neighborhoods are accompanied by real increases in knowledge of local politics. These effects are particularly striking as outcomes were measured a mere six months after municipal elections, suggesting that beneficiaries actively seek up-to-date information about local government.

Mechanisms: attitudes, status, and motivations

One channel through which government benefits might lead to increases demand-making is by making recipients feel wealthier and altering their time horizons (Figure 2.3). 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 "yes" when asked if their children will have better lives than them. They are about 8 percentage points more likely than non-winners to respond that they "would never leave" when asked if would ever consider relocating from Mumbai, suggesting increased time horizons. Given the argument that welfare policies make recipients wealthier, 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 in-

[28]As a quick check of this claim, I asked 15 individuals on the street in different administrative wards about their ward membership. Four respondents did not know which ward they belonged to, and the remaining 11 gave the names of their administrative wards.

Figure 2.3: Treatment effects for proposed mechanisms



Bars show 90% and 95% confidence intervals. Full regression output with and without covariate adjustment available in Tables B.8-B.12. 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 means to say "yes" 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?" Results on attitudes also appear in Kumar 2019. To not need to listen to local leaders means to respond "no" when asked "Do you/people like you need to listen to what leaders in the area say?" Treatment effects for reasons for voting show responses to"How did you make your vote choice for the municipal elections?" Respondents were asked an open ended question, and enumerators were instructed to select all responses that applied. To be satisfied with one's services means to say "satisfied" rather than"neither satisfied nor dissatisfied" or "dissatisfied" when asked "How satisfied are you with the following services in your community?"

crease psychological well-being, happiness, and time horizons. These effects may reduce the cognitive or time cost of action. Indeed, a winning respondent in his fifties claimed he felt less stressed about his children's future after winning, giving him the energy to "focus on other things." In contrast, a non-winning mother laughed when asked if she attended local meetings. "Who has the time to do such things? I need to look after my family and children."

Welfare benefits may also alter a beneficiary's and perception of her own status. I estimate an 8.9 percentage point increase in the likelihood of respondents selecting "No," when asked "Do you/people like you need to listen to what leaders in the area say?" I interpret this effect as an increase in respondents' perceptions of their own status or efficacy. During my interviews, I observed that respondents usually fell into two categories: those who appeared to be afraid of authority figures, and those who did not. The intervention appears to have shifted winners into the latter category. These effects are complementary to beneficiaries' near universal claim in qualitative interviews that they "now have some status." These effects may further enable citizens to actually make demands of elected officials they may have once feared.

Finally, welfare programs can create interest groups of beneficiaries who are particularly motivated to work together to protect their benefits. To illustrate this mechanism, I also show effects on stated motivations for another form of local political participation, namely voting in local elections (Figure 2.3). I first estimate treatment effects for reported voting in the past municipal elections and state elections. I do not detect a treatment effect for reported voting. This could be for many reasons, particularly that all respondents may feel social pressure to claim that they did, in fact, vote. Control means (the constant estimates in models (1) and (2) in Table B.13) do show high rates of reported voting for the control group. I next asked respondents how they made their choice in the most recent municipal election. Relative to non-winners, I estimate that winners are 22 percentage points more likely to state neighborhood problems as a reported reason for voting, thus supporting increased interest in local problems as a mechanism for my findings.[29]

Alternative explanations

It is possible that effects on local demand-making are driven mainly by social norms in the new apartment buildings. To understand whether or not this is the case, I look at the actions of those who chose to move into apartments (owner-occupiers) and those who did not (landlords). All of these questions for the main results were phrased to understand winners' actions in the places in which they *live*, whether or not it is in the lottery apartments. Note that this paper estimates average treatment effects across both owner-occupiers and landlords. This is mainly because this choice reveals a type, and types remain unknown among the control group. As a result, it is not possible to measure the effects conditional on this choice, let alone the effect of this choice itself,

[29]Here, I used a question in which respondents were not prompted with options and all of their responses were selected by enumerators from a multiple choice list. I attempted to make an exhaustive list of multiple choice options based on responses to a pilot survey I conducted in March 2017. Those who did not vote are simply assumed to have found none of the listed reasons important enough to motivate a vote, addressing concerns about post-treatment bias.

without additional modeling assumptions. Nevertheless, Table 2.6 shows that outcomes for landlords and owner occupiers are similar, especially when compared to the control group.

Table 2.6: Mean outcomes for landlords, owner-occupiers, and the control group.

	Landlords	Owner-occupiers	Control group
Individual complaints	0.52	0.61	0.45
Group complaints	0.52	0.54	0.41
Can name corporator in admin. ward	0.25	0.29	0.14

I did, moreover, ask whether landlords had attended homeowners' association (commonly known as "society") meetings in the neighborhood of the lottery home in the past month. The range of issues being discussed in these meetings is enormous and includes water supply, sidewalk construction, water leakages in apartment buildings, local safety, and, of course, the occasional birthday party.

Fifty-five reported that they did so "Often" or "Sometimes," a figure only slightly lower than the 65% attendance rate reported by owner-occupiers. The attendance of meetings in the lottery home neighborhoods is particularly surprising as going to these meetings can be very costly in terms of time; 68% of the landlords work 6 or more days a week, and the travel time (one way via transit) to the lottery building neighborhoods takes 1.1 hours on average.[30] Finally, the percentages of meeting attendance may actually be underestimates of participation because, according to interviews with development meeting leaders, some landlords also communicate their wishes through WhatsApp or by phone.

Why do we see participation among landlords in the communities where they own apartments but do not live? Even though landlords do not benefit from the quality of life improvements that may result from changes in the community, they will benefit from home value appreciation that may occur as a result of improved neighborhoods. This phenomenon may motivate owner-occupiers to participate as well.

An important prerequisite for this argument is that homeowners must be aware of changes to home values and have some idea of what causes these changes. In my survey, I randomly asked half of the sample of winners about their home prices. All respondents were able to provide a figure for the value of the homes. About 16% of respondents were unsure about whether the value of the property had changed since the purchase, and about 80% claimed it had increased.[31] Furthermore, 88% of respondents claimed that

[30] Travel times are calculated using the Google Maps API and households' addresses at the time of application. The travel time was calculated for a Sunday morning, the time at which I observed most neighborhood improvement society meetings occur.

[31] The remainder was equally split between refusals and those who claimed that the value had not changed.

they expected the values to increase in the future. Finally, when presented with an open-ended question about what they thought affected the values of their properties, about 83% of the responses were similar to "the property value of the surrounding areas," 25% included answers mentioning government policies and actions, 15% mentioned individual actions, and only 11% mentioned God or luck. About 9% claimed not to know. Winners are, in fact, aware of the property values and that they can change and even increase over time.

Evidence from qualitative interviews suggests that landlords' participation in demand-making in their *own* communities arises from developing new habits surrounding the lottery apartments. One respondent, for example, said that "we just pay attention to what is happening with the BMC [MCGM]." Another respondent claimed that after visiting some MCGM ward offices, she had developed a new interest in how the municipal government works. "I now just like to know what is going on, even where I live," she claimed.

Increased participation in local demand-making may also be the result of dissatisfaction with service delivery. Owner-occupiers experiencing worse services in the new buildings could organize to demand improvements in their new communities; landlords who have seen better services in the apartment buildings could be organizing to demand improvements in their baseline communities. To see whether increased participation is driven by dissatisfaction, I look at responses to questions that ask if individuals are satisfied with services in the neighborhoods in which they live (Figure 2.3). I see no evidence for this mechanism; in fact, I see greater satisfaction with the delivery of most services among lottery winners, making increased levels of local demand-making particularly surprising.

It is also possible that effects are driven by disgruntled members of the control group who no longer want to participate in local politics after failing to win the lottery. This seems rather unlikely, however, as the program is truly seen as a lottery; indeed, 74% and 79% of control and treatment respondents, respectively, respond that "Luck" is responsible for deciding who wins. Only 1.6% and 0.4% of the control and treatment groups believe that the MCGM is responsible. Moreover, applicants apply to lottery repeatedly, much like someone in the US can repeatedly buy Powerball tickets or put quarters into a slot machine. Non-winners may be unhappy about not winning, but it is unlikely that this unhappiness extends so far as to affect their impressions of local government capacity and responsiveness.

2.6 The effects of other policies

To what extent should there exist similar effects for other types of policies? Based on the mechanisms proposed here, namely wealth increases and motivations to protect these increases, similar policy feedback effects may exist for other programs that make beneficiaries wealthier over time. The relevant policies seem to be those entailing sustained use or sustained delivery of benefits over time. Small one-time cash transfers do not fall in either category. In contrast, policies such as pensions or employment guarantees entail sustained delivery over time, while public hospitals or programs such as those that construct sanitary latrines allow the sustained use of toilet or hospital facilities over time, respectively. All of these types of policies provide streams of in-kind benefits over time.[32] As a result, recipients may seek to ensure that the value of benefits increases or simply does not decrease over the lifetime of the benefit.

Many welfare benefits including, but not limited to, home price subsidies can thus be considered to be wealth or asset shocks that recipients will seek to protect. Importantly, the existence of these dynamics seems plausible even when beneficiaries are allocated through some process of clientelistic, rather than programmatic or rule-based, policymaking. As described by Olson (1965), the extent to which participation in local collective demand-making is inhibited by free-riding in collective action problems will likely be based on the size and nature of the group of beneficiaries; those benefitting from a large public hospital may have a more difficult time organizing than homeowners or a small group of pension beneficiaries in a village. Also, the likelihood of such welfare policies generating demand-making may depend on the size of the transfer, the ability of beneficiaries to protect the value of the transfer, and the strength of existing institutions for engagement with local government. Subsidized home-prices in Mumbai score highly in each of these areas.

There is some evidence for the existence of similar effects of other major welfare programs in India and other low- and middle-income countries as well. Local-level protests to improve such sustained welfare benefits are common in India. In January 2019, for example, beneficiaries of the NREGA program in Kashmir organized to demand the release of wages that had been delayed for two years. In another example, in May 2018, beneficiaries of Kisan Credit Card loans in a village in Rajasthan protested the mistakenly high interest rates charged by the local branch of the State Bank of India (Jain 2018).

Jenkins and Manor (2017, 166-181), moreover, find that NREGA increases political capacity and the "assertion of citizenship" among Indian villagers in order to demand the full and adequate delivery of benefits promised by the program. In fact, they argue that NREGA has actually strengthened the accountability of local village governance across India by economically empowering villagers and focusing their attention on the local officials' actions. There is also evidence for similar effects in other countries; MacLean (2011), for example, finds that citizens of African countries benefitting from public schools and clinics are more likely to engage in acts of everyday citizenship to improve the quality of schools and clinics.

[32] In cases where benefits may be easily transferred to others, they may provide cash benefits as well. As this paper shows, subsidized homes may be rented out. As shown in the Bollywood film *Sui Dhaga*, even items such as sewing machines may be rented or re-sold.

2.7 Discussion

These findings adjust the theoretical expectations generated by existing scholarship in several different ways. They contribute to a small but growing literature studying non-clientelistic political participation in India (e.g. Auerbach 2017; Kruks-Wisner 2018). As it becomes more institutionalized, this type of behavior is becoming an important means of participation in the actual policy-making process throughout urban India, particularly among the middle class (Chakrabarti 2007; Fernandes 2006, 137-173; Ghertner 2011; Harriss 2006; Sami 2013). The positive effects on local demand-making contradict the idea that need alone drives this participation in local politics and demand-making; if this were the case, the increased access to state resources would decrease the need for participation.

The results on attitudes and reasons for vote choice also illuminate new mechanisms by which programmatic policies may change the the political fortunes of implementers. Those studying the electoral effects of programmatic policies (e.g. De La O 2013; Manacorda *et al.* 2011; Zucco 2013) find that such policies increase the electoral support for incumbents. The proposed mechanism (to which Imai *et al.* (2019) point out theoretical objections) is that beneficiaries reward implementers at the ballot box. This study, along with Di Tella *et al.* (2007), shows that welfare programs might actually alter how beneficiaries think and *what they want*, in turn potentially affecting electoral behavior in ways that may (or may not) reward implementing parties and politicians at election time. These changes may affect strategic behavior among local politicians and even parties when implementing policies and crafting policy platforms.

Finally, low- and middle-income are sites of rapid innovation in policies aiming to mitigate poverty and inequality, including universal basic income, conditional cash transfers, microcredit, and continuous attempts to improve publicly provided healthcare and education. This study extends to these countries a literature on policy feedback that has, until now, focused mainly on the United States. Aside from the setting, a key of point of departure from this existing literature is that I argue that welfare policies have feedback effects that not only affect future policymaking, but also affect demand-making that can improve governance at the local level.

2.8 Conclusion

In this paper, I propose that welfare policies in India and other middle- and low- income countries potentially have important effects on beneficiaries' non-electoral political behavior. Moving beyond studies of turnout and vote choice, I focus on their propensity to make everyday demands of the government. I exploit a natural experiment in the form of a housing lottery in Mumbai to find that benefitting from subsidized home-prices leads individuals to increase their reported participation in collective demand-making and knowledge of local government. These results may arise from beneficiaries' newfound wealth and their desire to protect this wealth. Beneficiaries indeed report greater financial satisfaction, longer time horizons, increased perception of their own status, and greater interest in local issues when making voting choices. Supported by evidence from other studies, I suggest that welfare programs entailing the sustained delivery of benefits may similarly be understood as assets with values that are affected by local government actions and that beneficiaries will seek to protect. I thus build upon Kruks-Wisner's (2018) argument that the act of making demands is partly produced by interactions with the state itself.

As demonstrated by the fact that subsidized home-price beneficiaries make demands to improve communities in which non-beneficiaries live as well, the effect of welfare programs on complaint-making activities can lead to spillovers for all citizens in general. This will be particularly true if aspects of governance affecting the quality of welfare programs affect services that reach non-beneficiaries as well. Like the work of any interest group, beneficiaries' actions may have positive or negative effects on others; this will depend on the extent to which they control local agendas. If the subsidized home-price beneficiaries control the local policy-making agenda, then the needs and preferences of non-beneficiaries might be ignored. Studies of homeownership in the United States, for example, have focused on a resulting "not-in-my-backyard" attitude that leads homeowners to defect from city level public goods such as landfills and homeless shelters due to the costs they impose on local communities (Portney 1991; Dear 1992; Fischel 2001; Schively 2007; Hankinson 2018).

Particularly because of their potential to affect other citizens, outcomes related to demand-making are important in developing contexts wherein researchers have found have found deep inadequacies in both the access to and quality of many government services, including water (Bjorkman 2015), electricity (Min and Golden 2014), sanitation (Spears et al. 2013), and education (Chaudhury et al. 2006). Kapur and Nangia (2015) have, in fact, argued that the Indian government allocates greater spending to welfare programs than the provision of basic goods and services. While the effects of other programs may differ, the evidence from this paper suggests that at least some welfare programs may themselves affect the provision of basic goods and services through their effects on local demand-making.

3

When welfare decreases turnout: the effects of NREGS on state-level elections

Abstract: How does enacting welfare programs affect the political behavior of recipients in multi-level democracies? While existing research shows that benefits can increase political participation at the levels at which policies are enacted and benefits are distributed, we know less about effects at other levels. This paper uses a difference-in-differences design to find that implementation of India's National Rural Employment Guarantee Scheme decreased turnout in state-level elections in Uttar Pradesh and Punjab. Using multiple rounds of state- and national-level survey data, I argue that we see this effect because NREGS empower central and local-level governance structures, thereby decreasing the salience of the state-level for voters. These results run counter almost all of the existing literature on the political effects of welfare programs and open new questions in the study of political behavior, decentralization, and welfare implementation in multi-level democracies.

3.1 Introduction

Over the past 20 years, low- and middle- income countries have steadily increased their spending on public social protection, or welfare, programs such as unconditional cash transfers, conditional cash transfers, public health insurance, and work guarantees (World Bank 2018). Several studies have found that these programs increase the vote share of implementing parties and politicians (e.g. Bechtel and Hainmueller 2011; De la O 2013 2013; Manacorda *et al.* 2011; Zucco 2013), in part by mobilizing voters and increasing turnout (Nichter 2008; De La O 2013; Baez et al. 2012; Galiani et al. 2019). Existing research also suggests that welfare programs might increase political participation at the level at which benefits are distributed. A literature on "policy feedback" (e.g. Pierson 1993, Mettler 2004) in the United States suggests that welfare beneficiaries are motivated to participate in politics at the level of program receipt to protect or increase the value of their benefits. Kumar (2019), MacLean (2011), and Jenkins and Manor (2017) empirically support this argument for housing in India, health and education in African countries, and work guarantees in India, respectively.

Yet turnout might not increase at other levels of government not responsible for either program enactment or benefit distribution. Building upon research on "second-order elections" in Europe (e.g. Reif and Schmitt 1980; Marsh 1998; Carruba and Timpone 2005; Golder et al. 2017, 67-85), I argue that welfare programs might *decrease* turnout at these levels. Existing research and nationally representative survey data demonstrate that at least one motivation for political participation in India is to gain access to public resources. I further use survey data to show that citizens perceive different levels of government to be differentially able to deliver these resources. When elections are non-concurrent and voting incurs non-zero costs on voters, citizens in multi-level democracies should vote in some elections and not others. In India, for example, state level governments are often held responsible for major welfare programs, and turnout in state-level elections is correspondingly higher in state elections than national elections.

This paper shows how the receipt of India's 2005 Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS) altered this pattern of participation in state elections in Punjab and Uttar Pradesh. NREGS is an Indian welfare program that, most simply, aims to improve rural livelihoods by guaranteeing at least 100 days of wage labor per year to all volunteers in rural India. A centrally funded program that reaches roughly 50 millions households each year, it was enacted by an Indian National Congress-led coalition government in 2005. At the same time, wages are distributed by India's three-tiered local rural governance system and is often described as the largest devolution of funds to local governments to date (e.g. Jenkins and Manor 2017). In other words, it was enacted by the central government and benefits are distributed by local-level governance structures; existing research has correspondingly found evidence to suggest that, in some places, NREGS increased political participation at these levels. Even while state level governance remains an important variable in understanding patterns of the program's implementation and success, the program is, according to survey data, associated less with the state level than other welfare programs. Other welfare programs, such as Brazil's Bolsa Familia conditional cash transfer program, have similarly relied on a central-local partnership for the distribution of benefits (Fenwick 2009).

Taking advantage of the fact that NREGS was rolled out in a staggered fashion at the district level, I use a difference-in-differences design to estimate the effects of program implementation in two states, Punjab and Uttar Pradesh. I find that the first wave of program implementation *decreased* electoral turnout in participating districts without differentially affecting any major party. I argue that we observe this negative effect on turnout because NREGS decreased the salience for citizens of state-level government. I support this argument by turning to two waves of individual-level data for Uttar Pradesh. I first replicate the electoral results for low-income rural voters and find that NREGS decreased reported and verified turnout in Vidhan Sabha, or state, elections in implementing districts. I also find that a lack of interest is the main reported reason for abstention from voting. Additionally, the program decreased reported interest in any election campaign activities; a failure to take interest in campaign activities suggests that citizens are less interested in collecting information on candidates, or receiving favors from them, in the first place. Finally, survey evidence shows that reported local-level political participation *increases* with NREGS earnings, making it unlikely that effects are driven by wealth gains and a corresponding reduction in the need to participate in material welfare-driven voting.

To my knowledge, these results run counter to almost all of the existing literature on how welfare programs affect beneficiaries' electoral participation. They first demonstrate the importance of considering multi-level governance structures when seeking to understand these political effects. They further contribute to the literature on decentralization by highlighting the important role of large-scale welfare policies in both driving, or even reversing, the phenomenon. Indeed, the effects may explain political strategies for program implementation at the state level. These implications are relevant for those who study multi-level political systems in high, middle, and low-income countries alike.

3.2 Multi-level governance and turnout

Few who live in democracies elect officials at only one level of government. By 1999, 95% of all democracies had some form of subnational or supranational elected government (Yusuf 1999). Different levels within a country – the center, states or provinces, municipalities, towns or villages – will have different administrative, fiscal and political responsibilities. Chhibber and Kollman (2009) find that the balance of these responsibilities affects party consolidation across time. How does it shape voters' behavior at election time?

In a democratic system with multiple levels of government, citizens may find elections at one level more salient or potentially impactful to their lives than those at another. Many who study the European Union, for example, argue that European Parliamentary elections are "second-order" in importance, relative to "first-order" national-level elections, resulting in lower turnout in the supranational elections (Reif and Schmitt 1980 ; Marsh 1998; Hobolt and Witrock 2011; Carruba and Timpone 2005; Golder et al. 2017, 67-85). Heath et al. (1999) extend this argument to subnational governance by demonstrating that in Britain, voters believe less is at stake in local elections than in national elections, and turnout in local elections therefore tends to be lower.

Why might perceptions of differential importance translate into different levels of turnout? In some cases, elections might be non-concurrent. In the United States, electoral turnout is generally highest at the national level. Research from the United States (e.g. Patterson and Caldeira 1983; Hajnal, Lewis, and Louch 2002; Hess 2002) has found lower turnout in non-concurrent elections at the subnational level. As in the literature on second-order elections in Europe, an important mechanism driving turnout in these studies is voters' perception of the utility of their vote and the costliness of getting out the door to vote. Even when elections are concurrent, there may be costs to learning about candidates and positions. Generally, then, given some nonzero costs to voting or collecting information, voters might have some fixed amount of time or energy they are willing to expend on elections and prioritize turnout at certain levels over others.

How might the introduction of a welfare program affect citizens' perceptions of the importance of different levels of government and their resulting propensity to vote? In the second-order election framework, a citizen's perceived importance of an election is increasing in the power of the relevant elected officials (Golder et al. 2017, 62-67). The introduction of a new welfare program should lead citizens to favorably update their beliefs about the power of the enacting level of government to pass legislation that will affect their lives. Existing research (De La O 2013; Baez et al. 2012; Galiani et al 2019) has found that new welfare programs increase turnout in elections at the enacting level, with voters presumably motivated to reward or keep in place politicians they view favorably. Furthermore, a new welfare program entails the large-scale distribution of funds, and therefore consolidates administrative and fiscal authority at the enacting level. Faletti (2015) claims that building administrative and fiscal authority can increase the power and autonomy of a certain level of government. Another strand of research, then, finds that welfare programs can increase political participation to protect and gain access to benefits at the level of distribution (Kumar 2019; MacLean 2011; and Jenkins and Manor 2017).

In contrast, the relative power, or perceived power, of levels at which the program is not enacted by politicians or received by beneficiaries might decrease. To date, there is little evidence, to my knowledge, on the effects of welfare programs on these other levels of governance. More generally, even while decentralization has been shown how the process might *increase* political participation at certain levels (Campbell 2003; Wampler and Avritzer 2004; Agrawal and Gupta 2005; Goldfrank 2007; Faguet 2014), we know less about whether this increase adversely affects participation at other levels. The following section describes electoral turnout at different levels of government in India, and considers how NREGS may have altered existing patterns of political participation.

3.3 Political participation at the state, central, and local levels in India

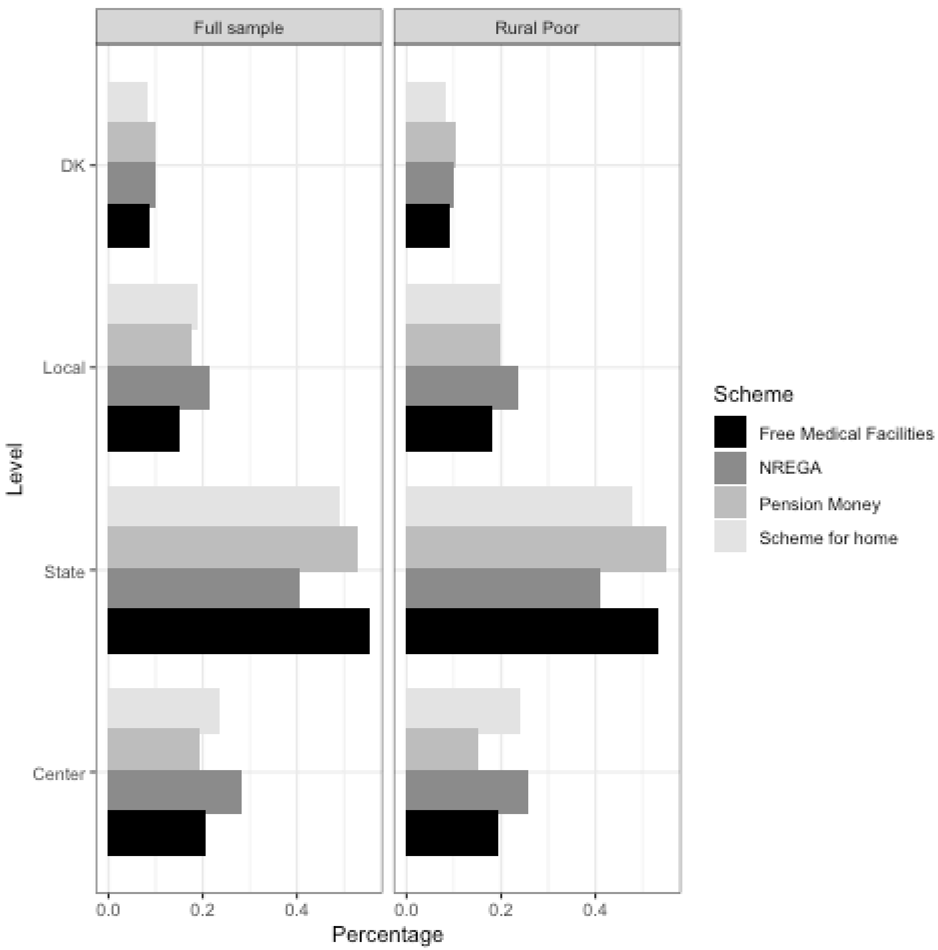
To understand patterns of political participation in India, it is important to first understand the motivations for turnout in India. One line of research finds that voting is the symbolic exercise of one's rights (Banerjee 2017; Ahuja and Chhibber 2012). Another one of its functions is to secure material interests, either through *quid pro quo* exchanges with politicians (Chandra 2004), gaining access to patronage networks (Jha et al. 2007; Auerbach 2016; Thachil 2014), through preferred policy instruments (Tandon 2012; Chhibber and Verma 2018), or simply placing in power the actors one believes will be most likely to bring resources to an area (Chhibber and Nooruddin 2004; Cole et al. 2012; Vaishnav 2017). This research is supported by a larger literature on economic voting (e.g. Powell and Whitten 1993; Lewis-Beck and Stegmaier 2008; Whitten and Palmer 1999; Anderson 2000; Lewis-Beck and Paldam 2000). While there is a debate on the extent to which these patterns are driven by the effects of material benefits on voter *turnout* (core-voter theory) rather than *choice* among existing voters (swing-voter theory), researchers from India have found evidence to support the core voter theory (Chandra 2004; Min 2015), particularly among actors from certain types of parties (Jensenius and Chhibber 2018). Similarly, a literature on mobilization and turnout-buying (e.g. Nichter 2008; Gans-Morse et al. 2014; Weschle 2014; Larreguy et al. 2015a; Casas 2018) finds that promising or delivering material benefits is an important strategy in persuading at least some citizens to vote in the first place. Let us assume, then, that gaining access to material resources is an important motivation for turnout among some subset of voters in this context.

There is variation across a multi-level government in the ability of actors to actually direct material resources to citizens. In India, this is demonstrated most clearly by comparing state and central-level governments. Federalism in India is defined by the Union list, State list, and Concurrent list, or a list of central, state, and shared responsibilities, respectively. Many duties associated with economic development, such as public health initiatives, agriculture, infrastructure, development, and local administration are on the state list. Important functions of economic and social planning are on the concurrent list, but central ministries involved in these endeavors tend to transfer funds to state governments to administer. Expenditure is also higher at the state level; roughly 80% of spending on social services is incurred by states alone (Kaur et al. 2013). Chhibber and Kollman 2009, 144 also clearly document the increase in public sector employment and spending among state governments relative to the central government from the 1970s to 1991. Deshpande et al. (2017) thus describe state-level governments as the true 'laboratories" of social welfare in India, as states exercise a great deal of discretion in how policies are implemented.

In recent years, the Indian government has made efforts to insitutionalize the role of local government as well. In rural areas, institutions known as gram panchayats form the lowest-level of the three-tier Panchayati Raj (PR) system of rural governance created by the 73rd Constitutional Amendment of 1993.[1] Gram sabhas are deliberative bodies of all eligible voters in a village, while gram panchayats are councils of elected leaders from

[1]Note, however, that several of the bodies within the PR system have existed in some form throughout the country for much longer.

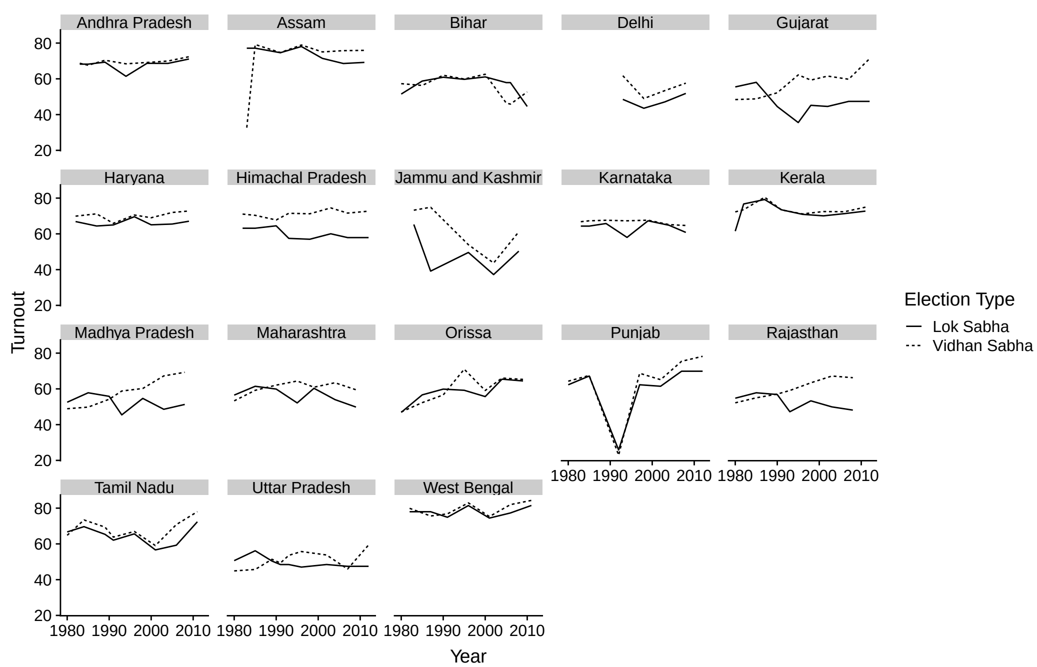
Figure 3.1: Assignment of credit for welfare programs by citizens to various levels of government



Source: Lokniti National Election Study 2014, conducted by the Centre for the Study of Developing Societies. N=22,295 a village or group of villages. In an effort to promote grassroots-level self-governance, the 73rd Amendment details the devolution of responsibilities of economic planning, social justice, and program implementation to local government. It also mandates that gram panchayats hold elections every five years and provide seats reserved for scheduled castes, scheduled tribes and women.

Nevertheless, the state level is *perceived* by voters as being responsible for social welfare, either because of the size of expenditures or due to some other reasons related to the visibility of different levels of government. As shown in Figure 3.1, the majority of beneficiaries responding to a nationally-representative survey in 2014 held state-level governments responsible for national-level policies. This is true for both the population as a whole and the rural poor, who are generally the target population for most welfare programs. Citizens attribute less responsibility for these programs to local-level governments.

Figure 3.2: Turnout in national (Lok Sabha) and state (Vidhan Sabha) level elections in India by state.



Data source: Vaishnav and Guy 2018, based on figures from the Election Commission of India.

Why might citizens abstain from voting? In a 2004 National Election Study conducted by Lokniti-CSDS, 4.5% of respondents reported abstaining from voting because they were away during the election, 3.0% reported being unwell, 2% claimed they had no interest, and 2.5% were prevented from a fear of violence or lack of an ID card. In villages, these figures were 3.8%, 3.2%, 2.1%, and 2.3%, respectively. In other words, some citizens might find voting costly for various reasons; for example, they have to get to the voting station, possibly meet voter intimidation tactics, and provide some form of ID. Additionally, for those who voters who are making a choice (rather than just voting based on party or ethnicity), they must participate in campaign activities to collect some amount of information about candidates. Denny (2017) further finds that the very poor often lack the mental bandwidth to participate in elections.

As in the United States, state and central (national) elections in India are non-current. A voter for whom the costs of voting are high might choose to vote in some elections and not others. The survey data above shows that voters in India attribute more credit to state governments than central governments for the social policies on the basis of which many vote. In the Indian context, then, we would expect greater turnout in state-level elections than national-level elections. As shown in Figure 3.2, this is true in most states, particularly beginning in the 1990s, a time at which Chhibber and Kollman (2009) claim marks the beginning of a trend of political decentralization in India.

NREGS: Consolidating central and local-level authority

NREGS is the result of the National Rural Employment Guarantee Act passed by the Indian Parliament in August 2005.[2] Very simply, the program legally guarantees 100 days per year of manual labor at minimum wage to every rural household. Beneficiaries may work on a range of infrastructure projects, including laying roads, power lines, and water pipes. The program is self-targeted in that there are no eligibility criteria for beneficiaries; it provides the "right to work" to all who wish to volunteer. Employment must be provided within 15 days of application and within 5 km of the worker's home. The program's stated aims are to provide social protection, rural livelihood security, and to create durable infrastructure.

Beginning in February 2006, the program was implemented in a series of three phases at the district level, with all districts enrolled by April 2008.[3] Since this roll-out, researchers have identified the program's effects on a range of outcomes, including wages and employment (e.g. Azam 2011; Berg et al. 2012; Imbert and Papp 2015), poverty (Klonner and Oldiges 2014), children's outcomes (Afridi et al. 2012; Islam and Sivasankar 2015), coping with income shocks (Johnson 2009) and insurgency-related violence (Khanna and Zimmerman 2014). Studies have found mixed but positive effects on household welfare, with a great deal of variation in implementation and the incidence of corruption across states (Dreze and Khera 2009; Dutta et al. 2014; Niehaus and Sukhuntar 2013a; Niehaus and Sukhuntar 2013b).

[2] In 2009, the act was renamed as the Mahatma Gandhi Employment Guarantee Act, so readers may have seen the use of other acronyms when referring to this policy as well. These may include MGNREGA, MGNREGS, or NREGA.

[3] In India, states and territories are further divided into administrative districts.

NREGS wages are almost entirely funded by the central government. The program is part of a collection of centrally sponsored flagship welfare programs implemented by the Congress-led governing coalition in power from 2004-2014, the United Progressive Alliance (UPA). Other programs in this collection include the National Rural Health Mission (for rural healthcare provision), the Midday Meal Scheme (for the supply for fortified food to children in primary school), Bharat Nirman (for the creation of rural infrastructure), and an expansion of the National Social Assistance Program (pension for the elderly, widowed, and disabled). But NREGS is by far the largest of these, with 20-50 million annual household beneficiaries and the central government allocating between 1.5 to 4% of its total expenditure on the program between 2006 and 2014 (Ehmke 2016). Given the role of the central government in enacting and funding the program, it has been strongly associated with the UPA government, for better and worse. Zimmerman (2020), for example, finds that in areas with strong implementation, longer exposure to the program caused greater support for the UPA government in national elections. In areas with poor implementation, support for the UPA decreased with exposure. Better implementation is also associated with higher voter turnout. the connection between implementation quality and UPA support suggest that voters do, in fact, attribute responsibility for the program to the UPA government. Given that the Gandhi family is typically associated with the Congress party, this attribution was likely strengthened after the program was renamed as the “Mahatma Gandhi National Rural Employment Guarantee Scheme" in 2009.

NREGS also creates a unique and powerful role for gram sabhas and gram panchayats, or the lowest levels of local rural governance in India.[4] These institutions are extremely important to the distribution of NREGS benefits for four reasons. First, NREGS is a *demand-driven* program, meaning that funds and projects are allocated based on need. Volunteers must submit applications for work to gram panchayats, rather than at the higher PR levels, namely the block or district. Of course, this process is rife with corruption. As described in the program’s founding Act, the gram panchayat "shall allocate employment opportunities among the applicants and ask them to report for work." It is clear (see e.g. Jenkins and Manor 2017) that village-level leaders exercise discretion in allocating jobs in order to build political power, and many (Dasgupta 2016; Bhattacharya, Kar, and Nandi 2016; Chau, Liu, and Soundararajan 2018; Maiorano, Das, and Silvero 2018; Bardhan et al. 2019) have found that the distribution of program benefits can follow a clientelistic pattern. Marcesse (2018) further finds that leaders may also manipulate the demand for jobs in order to generate surplus wage payments that can either be collected by themselves or higher level bureaucrats as rent.

Second, the creating Act requires at least half of all program funds to be placed under the control of the gram panchayat; in certain states, such as Madhya Pradesh, Karnataka,

[4]Andhra Pradesh, where NREGS is implemented by a centralized state bureaucracy, is the only exception here.

and Kerala, almost 90% of funds are managed at this level (Jenkins and Manor 2017). This devolution represents the largest injection of funds into local bodies in India since its independence. Third, the gram panchayat has the authority to submit, within guidelines, the development projects to be supported by NREGS labor to block level offices for approval. Finally, the gram sabha also plays an important role: it is the arena for citizens to provide recommendations on on priorities for the local public works *and* to conduct audits of completed and ongoing labor projects. While the actual implementation of each of these program features varies, their combined effect has generally been to increase the power of local-level governance structures to both channel resources to citizens and to increase local-level political participation and capacity (Jenkins and Manor 2017; Fischer and Ali 2019). While higher-level officials often take credit for the delivery of local goods and services (Thachil 2011), NREGS institutionalizes the role of the local level. Moreover, the fact that it is demand-based ensures that citizens are aware of the role of local levels in the distribution of funds. Furthermore, the decision-making role of the officials on gram sabhas ensures that citizens cannot, as described by Bussell (2019), seek recourse from higher level officials in case they are denied access to benefits by local officials.

Note that the state level is not "bypassed" by any means. As with all other welfare programs, each state is tasked with implementing NREGS within the local context and making a number of decisions, including whether and how to implement audits and other transparency mechanisms, how to respond to patterns of implementation, and the inclusion of civil society organizations. A substantial portion of the research on NREGS therefore focuses on state-level variation in outcomes and implementation (e.g. Maoirano 2014; Sukhtankar 2016; Jenkins and Manor 2017; Joshi et al. 2017; Fischer and Ali 2019).

It is not that state-level governments did not have important roles to play in NREGS implementation. Rather, the amount of central funding and local-level distribution of resources made central- and local-levels *relatively* more salient to voters than they were previously. Figure 3.1 indeed shows that beneficiaries of NREGS give relatively more credit to central and state levels of government for the policy than do beneficiaries of other policies. In contrast, they give relatively less credit to state level government for this policy. A natural question that emerges from these patterns is: how did this program affect political participation at the state level?

3.4 Effects of NREGS on turnout in assembly constituency elections

I estimate the effect of NREGS implementation on turnout in state electoral constituencies, or assembly constituencies using 2002 and 2007 data from the Socioeconomic High-resolution Rural-Urban Geographic Dataset on India (Asher et al. 2019). I take advantage of the fact that NREGS was rolled out in a staggered manner at the district level to identify effects.[5] In February 2006, 200 districts received the program as part of Phase I of implementation. In April 2007, 130 districts received the program as part of Phase II of implementation. Phase II implementation occurred in May 2007 in Uttar Pradesh due to the incidence of state elections in April. All remaining districts received the program as part of Phase III of implementation in April 2008.

I include in my sample the states that held assembly constituency elections in the year after Phase I but before Phase II, namely Punjab and Uttar Pradesh. The cumulative population of these two states in 2011 was 227,555,679–greater than that of all countries other than Indonesia, China, the United States, and India itself. I drop Gujarat from this list as elections were held in December 2007, after Phase II implementation. Of the 519 assembly constituencies in this sample, 135 had implemented NREGS as part of Phase I by the election time. I use a difference-in-differences framework to compare the changes in electoral turnout across two time periods between these two groups.

Estimation and assumptions

I estimate β_3 in the following equation:

$$Y_{cdst} = \beta_0 + \beta_1 NREGS_{cd} + \beta_2 \mathbb{I}_{t=2} + \beta_3 NREGS_{cd} \times \mathbb{I}_{t=2} + \overrightarrow{\gamma}\mathbb{C}_{cds} + \overrightarrow{\eta}\mathbb{D}_{ds} + \overrightarrow{\delta}\mathbb{S}_s + \epsilon_{cdst} \tag{3.1}$$

where Y_{cdst} is the outcome variable for constituency c in district d in state s at time $t \in 1, 2$. $NREGS_{cd}$ is an indicator that takes on the value 1 for constituencies c in districts d that were part of the Phase I implementation of NREGS, and 0 otherwise. $\mathbb{I}_{t=2}$ is an indicator that takes on the value 1 for observations measured in time period $t = 2$. Finally, $\mathbb{C}_{cds}$, $\mathbb{D}_{ds}$, $\mathbb{T}_t$, and $\mathbb{S}_s$ are matrices for indicators that observations share a common constituency, district, and state and $\overrightarrow{\gamma}$, $\overrightarrow{\eta}$, and $\overrightarrow{\delta}$ are vectors of coefficients. Given that program rollout occurred at the district level, I adjust all estimates for correlation of ϵ_{cdst} over time within districts by clustering standard errors at the district-level.[6]

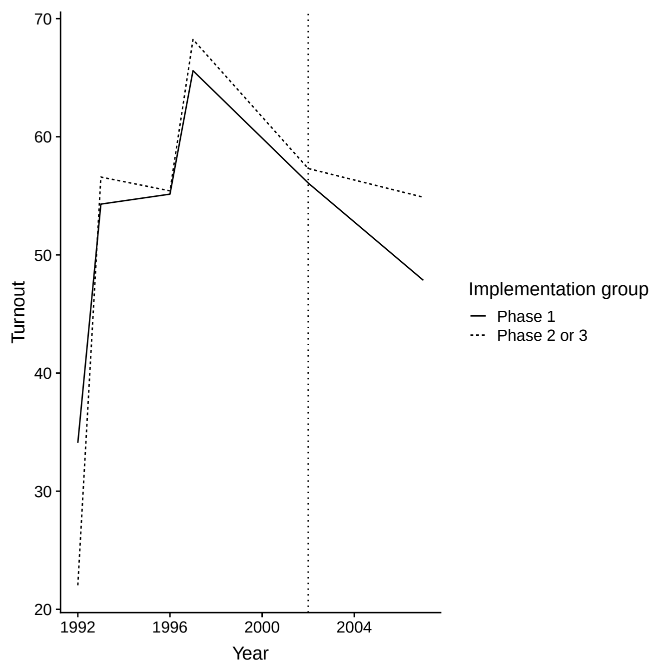
This research design estimates the causal effect of Phase I implementation on a handful of different outcomes. The main identifying assumption required here is *not* that that rollout and selection into Phase I was random. Assignment to NREGS phases was officially based on district ranks in an index-based development score, with poorer districts assigned to be in earlier phases. It is also possible that political connections at the district level may have affected the timing of implementation. It is very possible, then, that Phase I assignment is correlated with the main outcome of interest, namely turnout.

[5] Assembly constituencies were placed into districts as defined prior to the 2007 delimitation using GIS maps by Jensenius (2013).

[6] Because each of the assembly constituencies has only one treatment assignment status during the time period of the study, this design is less likely to suffer from the concerns raised by Goodman-Bacon (2019).

The main identifying assumption of the design, however, is that districts that received the program in different phases were not exhibiting different *trends* in turnout before program implementation. This assumption can first be visually verified in Figure 3.3. Following Imbert and Papp (2015), I further check this parallel trends assumption by estimating β_3 for multiple time periods prior to the implementation of NREGS. For example, instead of estimating comparing changes in electoral turnout from 2002 to 2007 between Phase I and non-Phase I constituencies, I can compare changes in electoral turnout from 1997 to 2002 (Table 3.1).

Figure 3.3: Turnout in Phase I and non-Phase I constituencies before and after the last election (2002) prior to NREGS implementation (2005).



Results

As shown by looking at the coefficients for $\mathbb{I}_{t=2} \times$ NREGS in Table 3.1, there is no statistically significant evidence that NREGS constituencies exhibited different trends when compared to non-NREGS constituencies prior to the 2007 election (Models 1-3). But in the 2007 election, after program implementation, we see that turnout in NREGS constituencies decreased by almost 6 percentage points when compared to non-NREGS constituencies (Model 4).

As shown in Table 3.2, these effects do not appear to be the result of changes in vote share for a single party. In Model 1, I first estimate effects on the vote share for the Indian National Congress (INC), or the party in power at the central level when NREGS was

Table 3.1: Difference in difference estimates for elections occurring before and after NREGS implementation.

	Year in which t=2 (UP/Punjab)			
	1993/1992	1996/1997	2002	2007
	(1)	(2)	(3)	(4)
$\mathbb{I}_{t=2}$[1]	-41.886^{***}	7.648^{***}	-2.428	-2.417
	(3.534)	(0.998)	(1.878)	(1.786)
NREGS[2]	-14.470^{***}	-16.042^{***}	-6.863^{***}	-8.739^{***}
	(0.915)	(1.140)	(1.878)	(1.164)
Uttar Pradesh	53.783^{***}	6.969^{***}	-7.571^{***}	-9.675^{***}
	(1.828)	(1.976)	(0.939)	(0.000))
$\mathbb{I}_{t=2} \times$ NREGS	1.780	1.324	1.023	-5.822^{**}
	(1.830)	(2.280)	(1.878)	(2.327)
Constant	59.293^{***}	56.192^{***}	73.009^{***}	77.928^{***}
	(1.767)	(2.198)	(0.939)	(0.893)
Observations	1,037	1,043	1,042	1,039
R^2	0.360	0.459	0.931	0.871
Adjusted R^2	-0.296	-0.091	0.856	0.741

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

Coefficients on difference in difference estimates for turnouts in two elections. All models include state, district, year, and assembly constituency dummies. All standard errors are clustered at the district level.

[1] Indicator for whether or not the observation occurred in the later election year in the pair.

[2] Indicator for whether the assembly constituency belongs to a Phase I district.

Table 3.2: Difference-in-difference estimates for party vote shares in elections in Uttar Pradesh and Punjab in 2002 and 2007.

	Party:				
	INC	SP	BSP	INC	SAD
	(1)	(2)	(3)	(4)	(5)
$\mathbb{I}_{t=2}$[1]	0.693	-0.736	7.671***	4.519	4.519
	(1.231)	(1.214)	(0.979)	(3.358)	(3.358)
NREGS[2]	-40.868^{***}	4.421***	6.000***	-4.270^{**}	-4.270^{**}
	(0.962)	(0.858)	(0.871)	(1.679)	(1.679)
$\mathbb{I}_{t=2} \times$ NREGS	0.466	2.519	-0.830	-3.901	-3.901
	(1.924)	(1.715)	(1.741)	(3.358)	(3.358)
Constant	49.073***	15.568***	23.425***	47.160***	47.160***
	(0.615)	(0.607)	(0.489)	(1.679)	(1.679)
States included	Both	UP	UP	Punjab	Punjab
Observations	1,309	806	806	233	233
R^2	0.856	0.766	0.771	0.545	0.917
Adjusted R^2	0.711	0.530	0.541	0.074	0.831

*p<0.1; **p<0.05; ***p<0.01

Coefficients on difference in difference estimates for turnouts in two elections. All models include district, year, and assembly constituency dummies. Model 1 includes state dummies as well. All standard errors are clustered at the district level.

[1] Indicator for whether or not the observation occurred in 2007 (as opposed to 2002).

[2] Indicator for whether the assembly constituency belongs to a Phase I district.

implemented. This model is estimated across both states In Models 2 and 3, I estimate effects on the vote share for the Samajwadi Party (SP) and the Bahujan Samaj Party (BSP) in Uttar Pradesh. These two parties formed the ruling coalition in Uttar Pradesh at the time. In Models 4 and 5, I estimate effects on the vote share for the INC and the Shiromani Akali Dal (SAD) in Punjab. These two parties formed the ruling coalition in Punjab at the time. Across all of these models, we see no measurable evidence to suggest that decreased turnout differentially affected any party in power at the state or central level.

3.5 NREGS decreases the salience of state-level government

Why did NREGS cause a decrease in turnout at the state level? I have argued that turnout in India is associated with the real or perceived ability for a certain level of government to provide access to resources relative to other levels. Why might the passage and implementation of NREGS decrease this real or perceived ability at the state level? Recall that NREGS is a program strongly associated with both the central government and local PR institutions. It is possible that its passage decreased the relative salience of state governments. I support this theory with individual-level post-election survey data from Uttar Pradesh in 2002 and 2007 to confirm these results and better understand the mechanisms behind them.[7] I use a similar estimation strategy here to the one used above, and estimate β_3 in the following equation:

$$Y_{cdt} = \beta_0 + \beta_1 NREGS_{cd} + \beta_2 \mathbb{I}_{t=2} + \beta_3 NREGS_{cd} \times \mathbb{I}_{t=2} + \overrightarrow{\gamma}\mathbb{C}_{cd} + \overrightarrow{\eta}\mathbb{D}_d + \epsilon_{cdt} \quad (3.2)$$

The only difference between equations 1 and 2 is that equation 2 drops the state-level indicators on account of the data being limited to one state only.

Tables 3.3 and 3.4 show difference-in-difference estimates for reported and verified turnout among poor villagers in Phase I and non-Phase I districts in Uttar Pradesh. Table 3.3 shows these results using the survey firm's own asset-based index of poverty. Results are similar when using a crude proxy for poverty, namely whether or not respondents live in single-room (as opposed to multiple-room) homes (Tables C.1 and C.2). We can see that reported turnout decreased in Phase I districts. So too did voting verified by the enumerator looking for a voting mark on the respondent's finger. As shown in Figure 3.2, 2007 was a very low turnout year in Uttar Pradesh; the results show that turnout was even lower in NREGS assembly constituencies.[8]

[7]This data has been made by available by Lokniti-CSDS.

[8]See Verniers (2016) for a discussion of why turnout may have decreased in 2007 more generally.

These results, complementary to those found using administrative data above, are important in clarifying mechanisms for a few reasons. First, it is possible that the negative turnout effects seen in Table 3.1 are driven by wealthier community members. Breitkreutz et al. (2017), for example, find that NREGS forced landowners to increase their wages to the minimum wage; this segment of society may have reduced their turnout in protest of the policy. Yet the results shown in Table 3.3 are estimated among the poor, or NREGS's target beneficiaries. Second, it is possible that decreased turnout among target beneficiaries is the result of voters simply being away at work during an election. Yet the Lokniti-CSDS surveys are conducted on election days, and thus show results among those who are available on election day.

The survey data also allows us to better learn *why* turnout decreased among this segment of the population. Table 3.4 shows difference-in-difference estimates for reported reasons for failing to vote in the 2002 and 2007 elections.[9] The only reason for abstention for which we see a non-zero treatment effect is "No Interest." Moreover, all respondents, voters and non-voters alike, were asked about their interest in the campaign. Model 5 shows a negative treatment effect for reported interest in the election. In other words, we see that NREGS increases the proportion of voters who did not *even try* to vote. Furthermore, we see that citizens are less likely to participate in the campaign-related activities that might allow them to make a voting decision or collect promises of favors in exchange for votes. Finally, we see little evidence that low turnout in NREGS constituencies was driven by voter ID rules, a factor that has been argued to generate low turnout in UP in 2007 more generally.[10]

Overall, then, we see that NREGS decreased reported and demonstrated turnout among poor voters, or the target beneficiary group, among assembly constituencies in implementing districts in UP. Furthermore, this effect on turnout is accompanied by negative effects on interest in the post-NREGS election activities and positive effects on a lack of interest as the reported reason for not voting. This decreased interest in the election supports the argument that NREGS decreased state-level electoral turnout because the relative importance of state-level politics for beneficiaries.

Are effects driven by a decreased need for material benefits?

An alternative explanation consistent with these empirical findings is that the program makes beneficiaries wealthier and reduces their need to vote on the basis of material interests. Research on clientelism finds that the utility of public resources decreases with income (e.g. Calvo and Murillo 2004; Dixit and Londregan 1996; Hicken 2011; Nathan 2016; Stokes et al. 2013) and that participation in clientelistic politics sometimes

[9]To avoid issues of post-treatment bias, I assume that none of these issues is a sufficiently large barrier to voting for those who *do* vote; voters are thus marked as "0" for each of these reasons.

[10]https://www.firstpost.com/politics/uttar-pradesh-election-2017-with-marginal-improvement-voter-turnout-in-state-higher-than-2012-3324728.html

Table 3.3: Difference-in-difference estimates for reported voting and confirmed voting in Uttar Pradesh in 2002 and 2007.

	Dependent variable:	
	Voted	Voted (mark found)
	(1)	(2)
$\mathbb{I}_{t=2}$[1]	0.023	0.113
	(0.054)	(0.083)
NREGS[2]	0.126^{***}	0.008
	(0.034)	(0.061)
$\mathbb{I}_{t=2} \times$ NREGS	-0.237^{***}	-0.208^{**}
	(0.064)	(0.103)
Constant	0.955^{***}	0.955^{***}
	(0.000)	(0.000)
Observations	3,206	3,206
R^2	0.110	0.145
Adjusted R^2	0.069	0.105

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

Coefficients on difference in difference estimates for turnouts in two elections. All models include district, year, and assembly constituency dummies. All standard errors are clustered at the district level.

Only poor respondents living in villages are included here. "Poor" is defined as those demarcated as "very poor" or "poor" by Lokniti-CSDS based on an index of asset ownership.

[1] Indicator for whether or not the observation occurred in 2007 (as opposed to 2002).

[2] Indicator for whether the assembly constituency belongs to a Phase I district.

Table 3.4: Difference-in-difference estimates for reasons for not voting and reported interest in campaign interest in Uttar Pradesh in 2002 and 2007.

	Reason for not voting:					
	Out of station	Unwell	Did not know I was a voter	No Interest	Lack of ID	Interest in campaign[1]
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{I}_{t=2}$[2]	0.030	−0.024	0.001	−0.063*	−0.004	0.039
	(0.020)	(0.083)	(0.004)	(0.035)	(0.002)	(0.068)
NREGA[3]	−0.096***	0.008	−0.010	−0.031**	0.124***	0.196**
	(0.027)	(0.061)	(0.010)	(0.014)	(0.010)	(0.083)
$\mathbb{I}_{t=2} \times$ NREGA	0.020	0.016	0.009	0.094**	0.013	−0.350***
	(0.034)	(0.103)	(0.011)	(0.038)	(0.010)	(0.107)
Constant	0.045***	0.000	0.000	0.000	0.000***	0.182***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	3,206	3,206	3,206	3,206	3,206	3,206
R^2	0.057	0.038	0.082	0.117	0.080	0.212
Adjusted R^2	0.013	−0.006	0.039	0.076	0.037	0.176

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

Coefficients on difference in difference estimates for turnouts in two elections. All models include district and assembly constituency dummies. All standard errors are clustered at the district level.

Only poor respondents living in villages are included here. "Poor" is defined as those demarcated as "very poor" or "poor" by Lokniti-CSDS based on an index of asset ownership.

[1] Dependent variable is whether or not respondent says "somewhat" or "a great deal" when asked "Now I would like to talk with you about the election campaigning during this elections. How much interest did you have this time?" much interest did you have this time - a great deal or somewhat or not at all?

[2] Indicator for whether or not the observation occurred in 2007 (as opposed to 2002).

[3] Indicator for whether the assembly constituency belongs to a Phase I district.

also decreases with benefitting from a program that increases beneficiaries' resources (Larreguy et al. 2015a; Bobonis et al. 2017).

If this explanation is indeed true, we would expect political participation to be decreasing with program benefits. I investigate this possibility by studying the relationship with NREGS income and political participation at the panchayat level as reported in the IHDS-II (2011-2012) nationally representative survey. While this survey does not measure reported turnout in state-level elections, the alternative theory would imply that a decreased need to participate in politics should apply to all levels of government.

I measure this relationship through a series of linear probability models estimating correlates of the likelihood of any household member reporting attending panchayat meetings (Table 3.5). Model 1 first shows that the relationship between reported attendance and NREGS income (with district-level weights) is not negative, but actually positive. Given that the survey was conducted across 2011 and 2012, it is possible that seasonality is confounding the measurement; during the summer, for example, the heat might prevent NREGS work and suppress meeting attendance. The number of household members may also affect measurement; more members means potentially more labor supply along with more individuals with time to attend meetings. Models 2, 4, and 6 thus include month and year dummies along with the number of women and men in a household. Women and men are included separately as they are likely to have different propensities to work and attend meetings. As most of the sample does not actually benefit from NREGS (Figure C.1), Models 3-6 conduct the analysis on those receiving some nonzero NREGS benefits. The coefficient for NREGS Income in Models 1-4 is positive, yet substantively small. This could be because of both the wide range and long tail on the distribution of wages even among beneficiaries.[11] Models 5 and 6 thus use a logged measure of NREGS income. Across all the models, we see a consistent positive, rather than negative relationship between NREGS income and reported participation.

These regression models are not attempting to estimate the causal effect NREGS income on participation. They simply aim to show the direction of a correlation while accounting for problems with measurement. Given the extremely political process of negotiation with elites often needed to access benefits in the first place (Dasgupta 2016; Bhattacharya, Kar, and Nandi 2016; Chau, Liu, and Soundararajan 2018; Maiorano, Das, and Silvero 2018; Bardhan et al. 2019), it is possible that this relationship is somewhat endogenous and that that greater political participation leads to more NREGS benefits. In fact, this is an intuitive reason why we might not expect NREGS to decrease the need for political participation: they rarely simply fall upon the beneficiary from the sky, but must rather be obtained through constant political negotiation and monitoring, mainly at the local level.

[11]One Indian Rupee, after all, was about 1/50th of a 1 USD at the time of the survey, and it would be surprising if a one rupee increase in wages was correlated with a substantively meaningful difference in behavior.

Table 3.5: NREGS earnings and political participation

	Dependent variable:					
	Local meeting participation					
	(1)	(2)	(3)	(4)	(5)	(6)
NREGS Income	0.00002***	0.00002***	0.00001***	0.00001***		
	(0.00000)	(0.00000)	(0.00000)	(0.00000)		
N Adults (F)		0.005*		−0.008		−0.007
		(0.003)		(0.009)		(0.009)
N Adults (M)		0.033***		0.050***		0.050***
		(0.003)		(0.008)		(0.008)
ln(NREGS Income)					0.036***	0.031***
					(0.007)	(0.007)
Constant	0.146***	0.060	0.345***	0.255**	0.079	0.043
	(0.016)	(0.039)	(0.056)	(0.123)	(0.078)	(0.134)
Month/Year dummies?	No	Yes	No	Yes	No	Yes
Observations	42,001	41,869	6,260	6,252	6,260	6,252
R^2	0.130	0.139	0.215	0.227	0.215	0.227
Adjusted R^2	0.129	0.138	0.211	0.221	0.212	0.222

*p<0.1; **p<0.05; ***p<0.01

Linear probability models estimating the correlation between NREGS earnings in the last year and the likelihood of household member reporting having attended a public meeting called by the village panchayat (gram sabha) in the last year.

Data from IHDS 2011-2012.

All models include district dummies and HC2 standard errors. Models 2-6 include non-zero earners only.

3.6 Discussion

This paper has used a difference-in-differences design to find that the implementation of NREGS had a negative effect on turnout at state-level elections in Punjab and Uttar Pradesh. I suggest that the reason for this effect is that NREGS decreased the salience of state-level elections for voters. The estimated effect is in the opposite direction of those in existing research finding that welfare program implementation increases political participation at the levels of policy enactment or benefit distribution. The results suggest the importance of examining multiple levels of government when estimating the political effects of any policy.

Substantively, the results also shed light on the processes of decentralization and centralization in federal systems. Chhibber and Kollman (2009, 79) argue that that "as national governments increase their authority over policies that voters care about, voters and candidates will become increasingly concerned with having a voice in national legislatures." On the other hand, they suggest that decentralization will lead to a greater interest in more regional or local elections. I argue that welfare programs can be important factors in these processes of nationalization and decentralization of policies, and therefore, politics.

The effects on state-level politics also suggest that politicians might have reasons to be strategic in their implementation of welfare programs. Andhra Pradesh, for example, is the sole example of a state not using gram panchayats to distribute NREGS benefits–they are distributed through the state bureaucracy instead. The given reason for this choice is generally that panchayats in the state are weak (Maiorano 2014). But the evidence from this study suggests that this strategy may have also preserved the power and relevance of state level political institutions. Similarly, while Fenwick 2009 claims that state governors were "bypassed" in the implementation of Brazil's Bolsa Familia in order to reach implementation goals, this choice may have had played a role in the program's central-level electoral returns for the president (Hunter and Power 2007). More generally, strategic concerns at different levels might affect welfare program implementation across democracies with multi-level governance.

Finally, the results should change the way we think about voting and political participation in low- and middle-income countries. First, much has been written about voter behavior in these contexts; it will be useful to learn more about how voters perceive and behave differently towards elected officials at different levels of government. In this vein, Bussell (2019) studies when citizens will approach higher-level officials as opposed to local ones. This work, along with the present study, motivates future research on what citizens might perceive to be the roles of different levels of government, and how these perceptions might shape strategic, and even clientelistic, behavior. The variation in turnout studied here also highlights the importance of learning about costs to voting, particularly among the poor. Even while it is a stylized fact that the poor vote at high rates in India, Denny (2017) also finds that voting is also particularly costly for the poor because of the time, mental bandwidth, and foregone wages that it incurs.

Of course, the generalizability of these findings is limited. They are restricted to only two states, and estimate effects immediately after program implementation. We already know that program implementation varies greatly across states. Several important reforms to the program, including linking Aadhar cards to accounts and the use of the electronic Fund Management System, have changed program implementation over time as well (Banerjee et al. 2016). As a result, there is likely variation in how the program shapes citizens' political behavior as well. The broadest implication of this study, then, is that welfare programs can alter political behavior in important and unexpected ways, thereby providing a vast open agenda for future research.

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Appendix A

Supplemental information for paper 1

A.1 Deviations from the pre-analysis plan

After the pre-analysis plan was filed, the survey was shortened in order to make sure respondents were paying attention throughout its duration. Several questions on the following topics were cut:

- Expenditure on education
- Psychological well-being
- Belief in market values
- The expenditures for which borrowing occurred

As a result, I am unable to report effects on these outcomes.

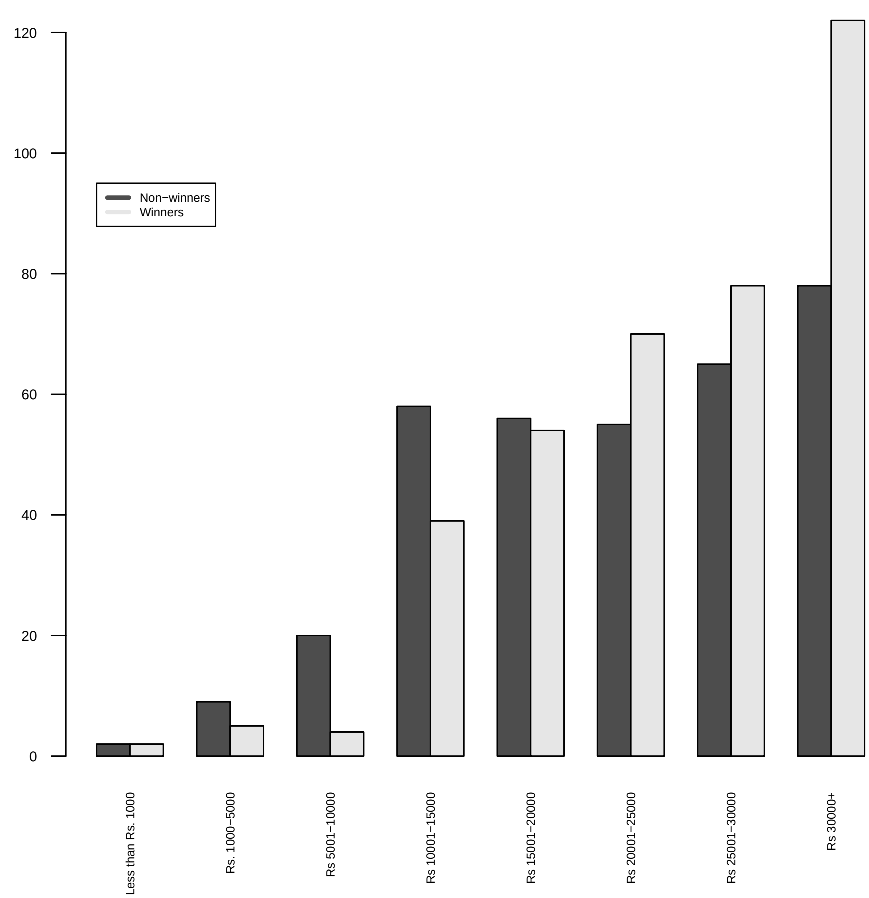
Also, I had planned on a) using a split-sample strategy to select hypotheses for testing as recommended by Anderson and Magruder 2017 and Olken 2015 and b) reporting effects for indices of outcomes. I had intended to take these steps to reduce the number of hypotheses tested and therefore decrease the number of multiple-testing adjustments required. Instead, I tested all of the hypotheses reported in the pre-analysis plan and made multiple-testing adjustments within families of hypotheses; this choice should lead to more conservative p-values.

I also do not report heterogeneous effects on income group, lottery year, and whether the lottery building is in the same ward as the original home due to insufficient power to detect these effects.

A.2 Effects on income, assets, and borrowing

Respondents were generally unable to provide numbers for monthly earnings, but preferred to provide ranges instead. Enumerators thus placed respondents into income bins. The bins used, unfortunately, appear to not capture the full range of the income distribution but rather only the left tail. Even so, a rightward shift in the distribution shows that winners clearly are earning more than non-winners. The p-value for a KS-test comparing these two distributions is 0.001.

Figure A.1: The reported income distribution for winners and non-winners.



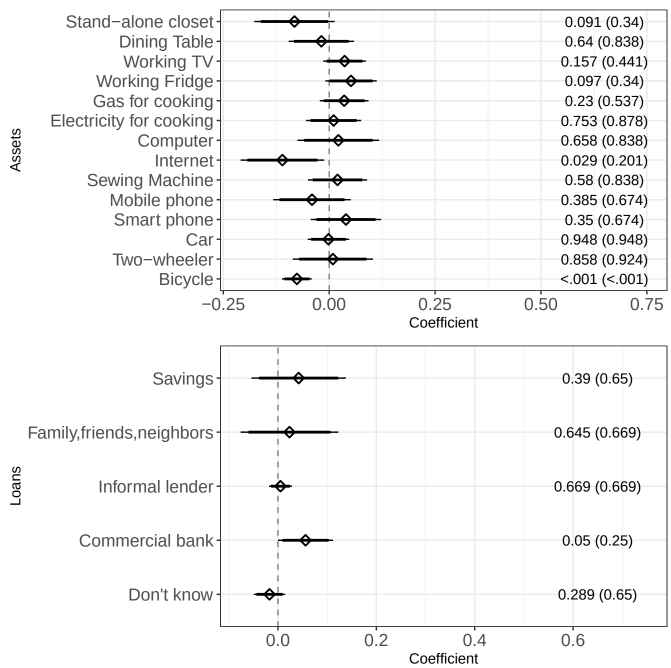
Bars represent the frequency of households in each income bin.

Winning households do not appear to be consuming more durable assets than non-winning households (Figure A.2). 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 1.4).

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.2: 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.21-A.23.

A.3 Balance tests

Table A.1: 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.2: Proportion of members of each category in treatment and control groups after mapping with p-values for difference in proportions test.

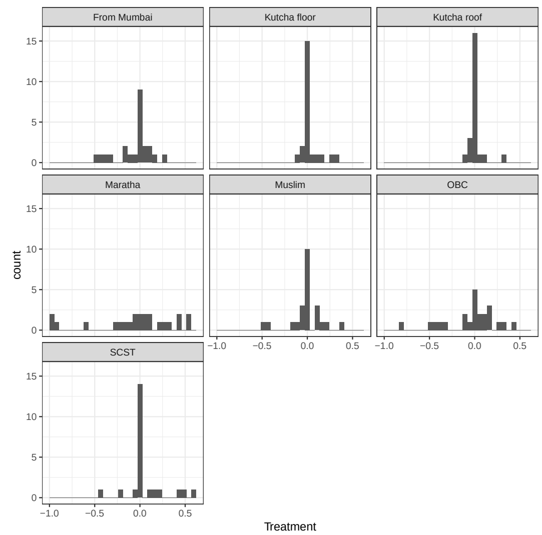
	Non-winners (C)	Winners (T)	p
Caste/Occupation category			
AR	0.021	0.026	0.541
CG	0.021	0.019	0.829
DF	0.017	0.008	0.164
DT	0.008	0.011	0.524
EX	0.024	0.021	0.683
FF	0.006	0.015	0.129
GP	0.592	0.601	0.774
JR	0.021	0.032	0.249
ME	0.009	0.021	0.130
MP/MLA/MLC	0.002	0.008	0.179
NT	0.019	0.011	0.316
PH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

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

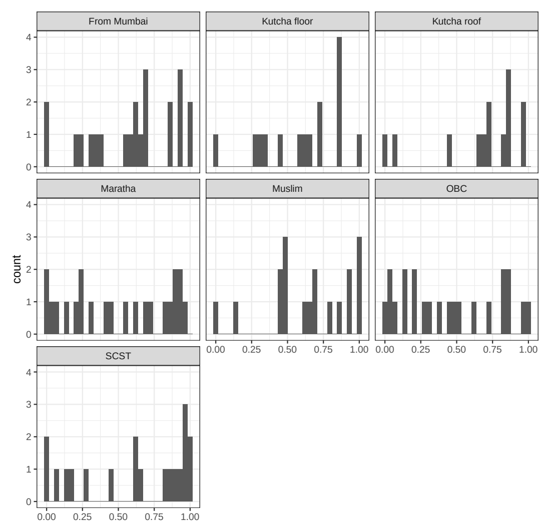
	Full Sample	Mapped Sample	p
AR	0.022	0.024	0.740
CG	0.021	0.020	0.886
DF	0.022	0.012	0.050
DT	0.014	0.009	0.250
EX	0.052	0.023	0.00
FF	0.028	0.010	0.00
GP	0.520	0.596	0.00
JR	0.028	0.026	0.779
ME	0.017	0.015	0.723
MP/MLA/MLC	0.004	0.005	0.883
NT	0.014	0.015	0.828
PH	0.026	0.026	0.947
SC	0.117	0.130	0.303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

Figure A.3: Distribution of (a) treatment effects and (b) p-values of those tests on fixed characteristics across Mumbai's 24 administrative wards.

(a) Estimated treatment effects



(b) Estimated p-values



The Treatment effect estimated is 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. I also conduct balance tests *within* each of Mumbai's municipal wards. The indicator for being from the same ward as the one in which the lottery is held is removed here. One ward (A) is dropped due to low sample size. Figure A.3 presents the distribution of the 24 estimated treatment effects along with the estimated 24 p-values. Consistent with the null hypothesis, the distributions of the estimated treatment effects appear roughly centered at 0, and the p-curves appear to take on a roughly uniform distribution.

Table A.4: 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

[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.5: Treatment effects on age by cohort.

Cohort	Control	Treatment	sd
$Turned_6$	9.454	-0.067	0.227
$Turned_{16}$	19.228	-0.107	0.340
$Turned_{18}$	21.175	-0.242	0.308
$Turned_{21}$	23.638	-0.099	0.218
Older	44.859	0.259	0.505

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. All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies. "$Turned_X$" is an indicator for membership in the cohort of individuals that completed X years of age in between the lottery and being surveyed, using $age_{\bar{l}}$, or each individual's oldest possible age. "Older" is an indicator for being in the cohort of individuals older than 21 at the time of the lottery.

A.4 Results using alternative age indicator

Table A.6: Regressions of individual completion of various years of education on the treatment indicator.

	Dependent variable:								
	Years of education	I(>0 years)		I(>10 years)		I(>12 years)		I(=15 years)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T	0.618	0.008	0.009	0.071	0.058	0.056	0.039	0.041	0.029
	(0.183)	(0.009)	(0.009)	(0.018)	(0.019)	(0.019)	(0.021)	(0.017)	(0.017)
$Turned_6$			0.045						
			(0.019)						
$Turned_{16}$					0.358				
					(0.036)				
$Turned_{18}$							0.411		
							(0.044)		
$Turned_{21}$									0.327
									(0.048)
TX$Turned_6$			−0.003						
			(0.020)						
TX$Turned_{16}$					0.068				
					(0.046)				
TX$Turned_{18}$							0.074		
							(0.061)		
TX$Turned_{21}$									0.111
									(0.066)
Constant	10.230	0.935	0.931	0.505	0.478	0.318	0.291	0.258	0.232
	(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	0.033	0.047	0.049	0.053	0.098	0.051	0.121	0.058	0.112
Adjusted R^2	0.007	0.005	0.007	0.012	0.010	0.017	0.082	0.018	0.073

All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies. "$Turned_X$" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using $age_{\underline{l}}$, or each individual's oldest possible age. "Older" is an indicator for an individual being older than 21 at the time of the lottery.

Table A.7: Regressions of individual employment on the treatment indicator.

	Dependent variable:					
	Employed					
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.043	0.040	0.051	0.039	0.030	0.069
	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)	(0.030)
$Turned_6$	−0.057	−0.495				
	(0.015)	(0.014)				
$Turned_{16}$	−0.027		−0.371			
	(0.029)		(0.033)			
$Turned_{18}$	0.092			−0.182		
	(0.035)			(0.050)		
$Turned_{21}$	0.619				0.180	
	(0.035)				(0.041)	
Older	0.531					0.379
	(0.016)					(0.025)
TX$Turned_6$		−0.024				
		(0.021)				
TX$Turned_{16}$			0.003			
			(0.048)			
TX$Turned_{18}$				0.105		
				(0.069)		
TX$Turned_{21}$					0.143	
					(0.060)	
TXOlder						−0.030
						(0.036)
Constant	0.036	0.486	0.477	0.462	0.436	0.190
	(0.015)	(0.011)	(0.012)	(0.011)	(0.011)	(0.021)
Observations	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.249	0.093	0.074	0.040	0.054	0.146
Adjusted R^2	0.215	0.053	0.033	−0.003	0.012	0.109

All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered block dummies. "$Turned_X$" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using $age_{\underline{l}}$, or each individual's oldest possible age. "Older" is an indicator for an individual being older than 21 at the time of the lottery.

Table A.8: Regressions of individual part-time employment on the treatment indicator.

	Dependent variable:					
	Employed (part-time)					
	(1)	(2)	(3)	(4)	(5)	(6)
T	−0.021	−0.023	−0.025	−0.019	−0.024	−0.013
	(0.012)	(0.011)	(0.012)	(0.013)	(0.013)	(0.026)
$Turned_6$	0.041	0.105				
	(0.034)	(0.041)				
$Turned_{16}$	0.028		0.081			
	(0.032)		(0.035)			
$Turned_{18}$	−0.028			0.070		
	(0.029)			(0.034)		
$Turned_{21}$	−0.075				−0.019	
	(0.028)				(0.024)	
Older	−0.109					−0.091
	(0.023)					(0.021)
TX$Turned_6$		0.049				
		(0.063)				
TX$Turned_{16}$			0.021			
			(0.047)			
TX$Turned_{18}$				−0.050		
				(0.044)		
TX$Turned_{21}$					0.033	
					(0.037)	
TXOlder						−0.013
						(0.027)
Constant	0.164	0.079	0.081	0.082	0.088	0.149
	(0.023)	(0.009)	(0.009)	(0.009)	(0.009)	(0.019)
Observations	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.096	0.072	0.068	0.062	0.059	0.087
Adjusted R^2	0.055	0.031	0.026	0.020	0.018	0.047

Part-time 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. “$Turned_X$” is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using $age_{\bar{l}}$, or each individual’s oldest possible age. “Older” is an indicator for an individual being older than 21 at the time of the lottery.

Table A.9: Regressions of individual full-time employment on the treatment indicator.

	Dependent variable:					
	Employed (full-time)					
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.075	0.072	0.082	0.072	0.066	0.087
	(0.018)	(0.019)	(0.020)	(0.020)	(0.019)	(0.035)
$Turned_6$	−0.042	−0.419				
	(0.029)	(0.032)				
$Turned_{16}$	−0.040		−0.323			
	(0.034)		(0.038)			
$Turned_{18}$	0.092			−0.135		
	(0.036)			(0.050)		
$Turned_{21}$	0.556				0.199	
	(0.035)				(0.040)	
Older	0.445					0.299
	(0.022)					(0.026)
TX$Turned_6$		−0.003				
		(0.049)				
TX$Turned_{16}$			0.006			
			(0.054)			
TX$Turned_{18}$				0.086		
				(0.070)		
TX$Turned_{21}$					0.105	
					(0.057)	
TXOlder						−0.010
						(0.038)
Constant	0.108	0.488	0.481	0.467	0.442	0.252
	(0.023)	(0.013)	(0.014)	(0.014)	(0.014)	(0.025)
Observations	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.211	0.095	0.084	0.057	0.073	0.127
Adjusted R^2	0.175	0.055	0.044	0.016	0.033	0.089

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. "$Turned_X$" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using $age_{\underline{l}}$, or each individual's oldest possible age. "Older" is an indicator for an individual being older than 21 at the time of the lottery.

A.5 Regression output for figures

Table A.10: Regression estimates for individual-level education and employment effects.

	Dependent variable:							
	Years of education (in SDs)		Working		Working full-time		Working part-time	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	0.135	0.131	0.044	0.047	0.077	0.074	−0.021	−0.019
	(0.052)	(0.052)	(0.026)	(0.026)	(0.026)	(0.026)	(0.014)	(0.014)
OBC		0.064		0.065		0.015		0.005
		(0.061)		(0.031)		(0.030)		(0.016)
SCST		0.126		0.081		−0.038		0.029
		(0.078)		(0.039)		(0.039)		(0.021)
Maratha		0.165		0.068		0.044		−0.002
		(0.049)		(0.025)		(0.024)		(0.013)
Muslim		−0.036		0.020		−0.013		0.014
		(0.070)		(0.035)		(0.035)		(0.019)
Kutcha floor		0.195		0.013		0.040		−0.021
		(0.138)		(0.069)		(0.069)		(0.037)
Kutcha roof		−0.295		−0.009		0.012		0.017
		(0.133)		(0.067)		(0.066)		(0.036)
From Mumbai		0.047		−0.007		−0.039		−0.003
		(0.051)		(0.026)		(0.026)		(0.014)
From same ward as apt		−0.095		−0.041		0.043		−0.016
		(0.064)		(0.032)		(0.032)		(0.017)
Constant	2.246	2.159	0.450	0.419	0.457	0.474	0.087	0.086
	(0.034)	(0.059)	(0.017)	(0.030)	(0.017)	(0.029)	(0.009)	(0.016)
Observations	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170
R^2	0.051	0.059	0.034	0.039	0.054	0.058	0.059	0.060
Adjusted R^2	0.010	0.016	−0.007	−0.006	0.013	0.014	0.018	0.017

Full-time employment is defined as working five or more days a week, while part-time employment is defined as working fewer than 5 days a week. All standard errors are clustered at the family level.

Table A.11: Regression estimates of household-level educational outcomes.

	Dependent variable:											
	Public school (sons)		Public school (daughters)		English school (sons)		English school (daughters)		Tuition (sons)		Tuition (daughters)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
T	−0.086	−0.084	−0.089	−0.084	0.022	0.029	0.009	0.012	−0.037	−0.027	−0.031	−0.022
	(0.020)	(0.020)	(0.018)	(0.018)	(0.026)	(0.018)	(0.045)	(0.045)	(0.039)	(0.039)	(0.040)	(0.040)
OBC		−0.006		0.017		−0.029		−0.046		0.034		−0.022
		(0.023)		(0.021)		(0.021)		(0.052)		(0.046)		(0.046)
SCST		−0.011		0.048		−0.101		−0.229		0.009		−0.087
		(0.030)		(0.027)		(0.027)		(0.067)		(0.059)		(0.059)
Maratha		0.022		0.005		−0.095		−0.072		0.014		−0.022
		(0.019)		(0.017)		(0.017)		(0.042)		(0.037)		(0.037)
Muslim		0.047		0.028		0.021		−0.103		0.079		−0.101
		(0.027)		(0.025)		(0.025)		(0.061)		(0.054)		(0.054)
Kutcha floor		−0.032		−0.024		−0.188		−0.054		−0.007		−0.041
		(0.050)		(0.045)		(0.045)		(0.112)		(0.098)		(0.098)
Kutcha roof		0.072		0.064		0.145		−0.189		−0.033		−0.074
		(0.052)		(0.047)		(0.047)		(0.117)		(0.102)		(0.103)
From Mumbai		−0.041		−0.052		−0.058		−0.107		−0.148		−0.159
		(0.020)		(0.018)		(0.018)		(0.044)		(0.038)		(0.039)
From same ward as apt		0.042		0.039		0.028		0.027		−0.017		0.032
		(0.025)		(0.023)		(0.023)		(0.057)		(0.049)		(0.050)
Constant	0.095	0.113	0.088	0.111	0.277	0.359	0.273	0.420	0.219	0.318	0.217	0.366
	(0.013)	(0.023)	(0.012)	(0.021)	(0.017)	(0.021)	(0.030)	(0.051)	(0.026)	(0.044)	(0.026)	(0.045)
Observations	823	823	822	822	823	823	822	822	834	834	834	834
R^2	0.203	0.222	0.237	0.260	0.172	0.187	0.175	0.203	0.181	0.203	0.166	0.192
Adjusted R^2	0.050	0.062	0.090	0.107	0.013	0.020	0.016	0.039	0.027	0.042	0.008	0.028

All regressions include HC2 errors and treatment indicator interactions with mean-centered block dummies.

Table A.12: Regressions estimates of household-level employment effects.

	Dependent variable:					
	Main earner salaried		Main earner govt job		Main earner formal job	
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.079	0.080	0.038	0.039	0.053	0.056
	(0.039)	(0.039)	(0.039)	(0.039)	(0.034)	(0.034)
OBC		0.034		−0.017		−0.045
		(0.045)		(0.045)		(0.039)
SCST		0.165		−0.002		0.076
		(0.057)		(0.058)		(0.051)
Maratha		0.121		0.082		0.026
		(0.036)		(0.037)		(0.032)
Muslim		−0.130		−0.136		−0.047
		(0.052)		(0.053)		(0.046)
Kutcha floor		0.028		−0.114		0.003
		(0.096)		(0.097)		(0.084)
Kutcha roof		−0.016		0.070		−0.064
		(0.100)		(0.101)		(0.088)
From Mumbai		−0.017		−0.014		−0.050
		(0.038)		(0.038)		(0.033)
From same ward as apt		0.045		0.053		0.048
		(0.048)		(0.049)		(0.042)
Constant	0.782	0.746	0.181	0.180	0.096	0.127
	(0.026)	(0.043)	(0.026)	(0.044)	(0.022)	(0.038)
Observations	834	834	834	834	834	834
R^2	0.139	0.174	0.206	0.227	0.139	0.152
Adjusted R^2	−0.024	0.008	0.056	0.071	−0.023	−0.019

All regressions include HC2 errors and treatment indicator interactions with mean-centered block dummies. Having a formal sector job here means having received a letter or contract at the start of employment.

Table A.13: Regression estimates for treatment effects of standardized characteristics of wards in which households live (no covariates).

	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
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

All regressions include HC2 errors and treatment indicator interactions with mean-centered block dummies.

Table A.14: Regression estimates for treatment effects of standardized characteristics of wards in which households live (with covariate adjustment).

	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

All regressions include HC2 errors and treatment indicator interactions with mean-centered block dummies.

Table A.15: Regression estimates for treatment effects on standardized school quality variables measured by postal code of where interviewed households are living (no covariates).

	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

Table A.16: Regression estimates for treatment effects on standardized school quality variables measured by postal code of where interviewed households are living (with covariate adjustment).

	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
R^2	0.158	0.164	0.165	0.209	0.163	0.225	0.386	0.236
Adjusted R^2	−0.011	−0.003	−0.002	0.051	−0.005	0.070	0.263	0.083

All regressions include HC2 errors and treatment indicator interactions with mean-centered block dummies.

Table A.17: 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.

	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

All regressions include HC2 errors and treatment indicator interactions with mean-centered block dummies.

Table A.18: Regression estimates for reported individualistic attitudes.

	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

All regressions include HC2 errors and treatment indicator interactions with mean-centered block dummies.

Table A.19: 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).

	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

All regressions include HC2 errors and treatment indicator interactions with mean-centered block dummies.

Table A.20: 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).

	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

All regressions include HC2 errors and treatment indicator interactions with mean-centered block dummies.

Table A.21: Regression estimates of treatment effects on asset ownership (no covariates).

	Dependent variable:												
	Almirah	Dining tbl	TV	Fridge	Gas	Computer	Internet	Sewing machine	Mobile	Smartphone	Car	2 whlr	Bicycle
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
T	−0.098	−0.021	0.034	0.047	0.037	0.024	−0.110	0.022	−0.028	0.037	0.001	0.001	−0.079
	(0.049)	(0.039)	(0.026)	(0.031)	(0.029)	(0.049)	(0.050)	(0.035)	(0.047)	(0.042)	(0.025)	(0.048)	(0.018)
Constant	0.711	0.206	0.914	0.879	0.886	0.379	0.513	0.127	0.696	0.751	0.064	0.357	0.078
	(0.032)	(0.026)	(0.017)	(0.020)	(0.019)	(0.032)	(0.033)	(0.023)	(0.031)	(0.028)	(0.016)	(0.032)	(0.012)
Observations	834	834	834	834	834	834	834	834	834	834	834	834	834
R^2	0.140	0.188	0.167	0.132	0.188	0.171	0.166	0.155	0.166	0.179	0.171	0.158	0.191
Adjusted R^2	−0.022	0.035	0.010	−0.032	0.035	0.015	0.009	−0.005	0.008	0.025	0.015	−0.0004	0.039

All regressions include HC2 errors and treatment indicator interactions with mean-centered block dummies.

Table A.22: Regression estimates of treatment effects on asset ownership (with covariate adjustment).

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

All regressions include HC2 errors and treatment indicator interactions with mean-centered block dummies.

Table A.23: 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?"

	Dependent variable:									
	Savings		Family,friends,neighbors		Informal lender		Commerical bank		DK	
	(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
R^2	0.172	0.190	0.151	0.164	0.205	0.211	0.124	0.136	0.211	0.225
Adjusted R^2	0.013	0.024	−0.011	−0.008	0.053	0.049	−0.043	−0.041	0.060	0.066

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 HC2 errors and treatment indicator interactions with mean-centered block dummies.

A.6 Predictors of moving among winners

Table A.24: OLS estimates of predictors of moving among winning applicants.

	Dependent variable:					
	Moving					
	(1)	(2)	(3)	(4)	(5)	(6)
OBC	−0.150	−0.119	−0.155	−0.119	−0.150	−0.119
	(0.073)	(0.081)	(0.074)	(0.081)	(0.073)	(0.081)
SCST	−0.214	−0.195	−0.217	−0.195	−0.215	−0.195
	(0.081)	(0.098)	(0.082)	(0.098)	(0.081)	(0.098)
Maratha	−0.138	−0.146	−0.142	−0.146	−0.140	−0.146
	(0.059)	(0.066)	(0.060)	(0.066)	(0.059)	(0.066)
Muslim	−0.022	−0.004	−0.032	−0.004	−0.023	−0.004
	(0.085)	(0.093)	(0.086)	(0.093)	(0.085)	(0.093)
Kutcha floor	0.378	0.332	0.365	0.332	0.377	0.332
	(0.150)	(0.167)	(0.151)	(0.167)	(0.150)	(0.167)
Kutcha roof	0.077	0.092	0.062	0.092	0.076	0.092
	(0.196)	(0.209)	(0.197)	(0.209)	(0.196)	(0.209)
From Mumbai	−0.092	−0.117	−0.092	−0.117	−0.093	−0.117
	(0.061)	(0.070)	(0.061)	(0.070)	(0.061)	(0.070)
From same ward as apt	0.277	0.274	0.283	0.274	0.278	0.274
	(0.076)	(0.085)	(0.077)	(0.085)	(0.076)	(0.085)
LIG	0.003	0.087				
	(0.050)	(0.455)				
Scheme 275			−0.012	1.115		
			(0.269)	(0.699)		
Scheme 276			−0.155	0.456		
			(0.258)	(0.608)		
Scheme 283			−0.100	0.361		
			(0.189)	(0.602)		
Scheme 284			0.017	0.996		
			(0.192)	(0.697)		
Scheme 302			−0.062	0.480		
			(0.188)	(0.546)		
Scheme 303			−0.032	0.438		
			(0.189)	(0.606)		
Scheme 305			0.005	0.350		
			(0.204)	(0.575)		
2014 lottery					0.010	−0.646
					(0.048)	(0.570)
Constant	0.611	0.570	0.664	0.126	0.609	0.987
	(0.072)	(0.318)	(0.190)	(0.518)	(0.066)	(0.319)
Block dummies?	No	Yes	No	Yes		
Observations	421	421	421	421	421	421
R^2	0.100	0.221	0.107	0.221	0.100	0.221
Adjusted R^2	0.080	0.049	0.074	0.049	0.080	0.049

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

Appendix B

Supplemental information for paper 2

B.1 Figures

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

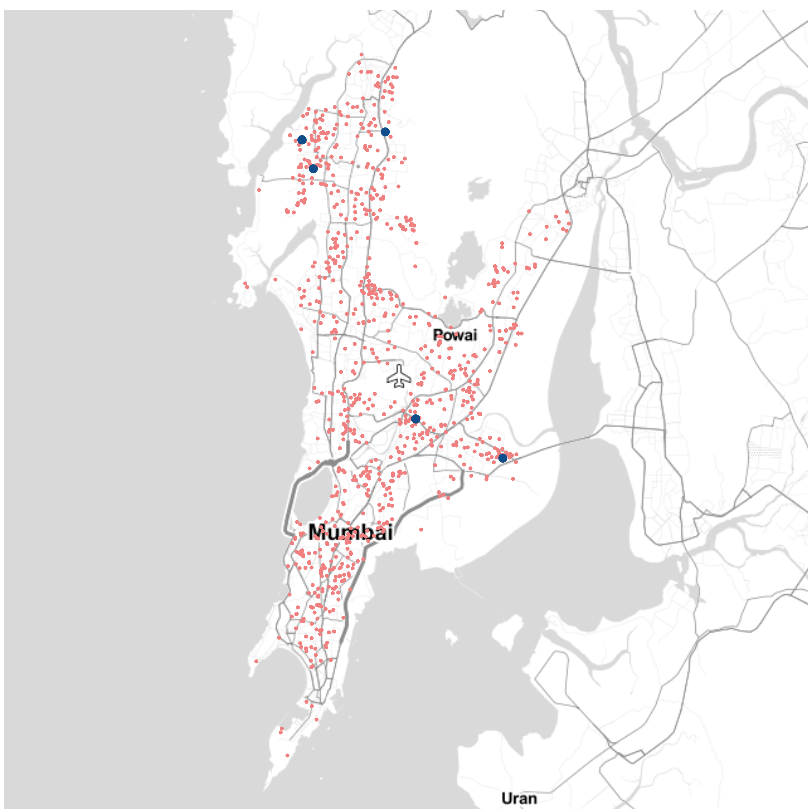


Figure B.2: Map of electoral wards in Mumbai. Wards are filled to denote administrative ward membership.

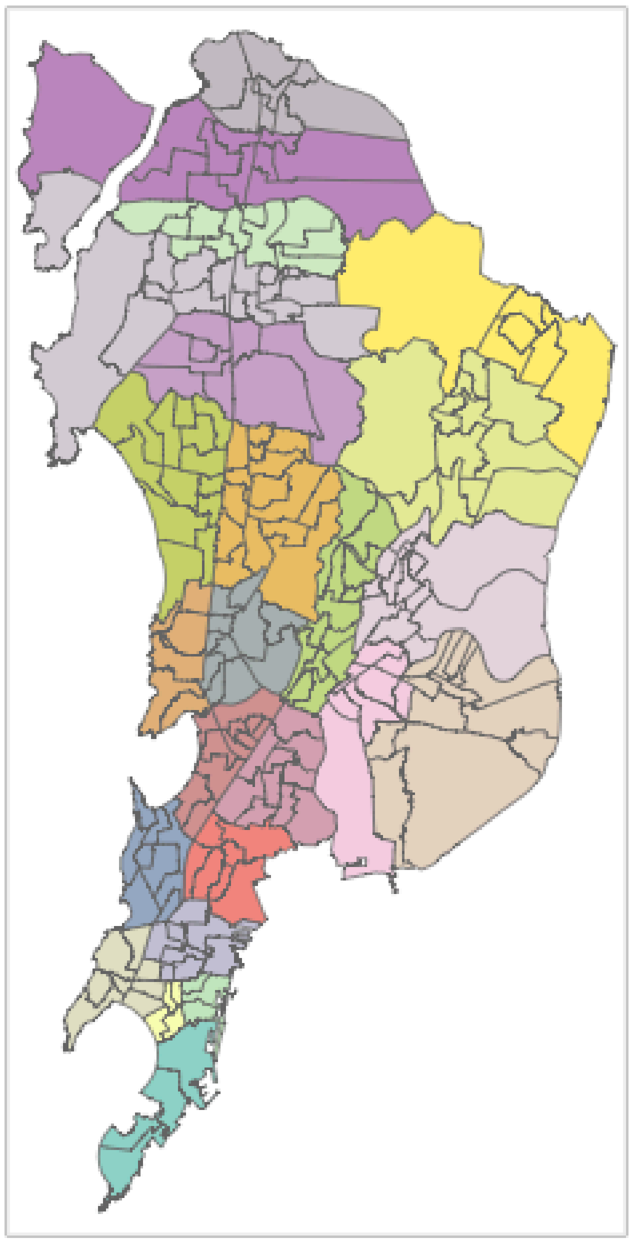
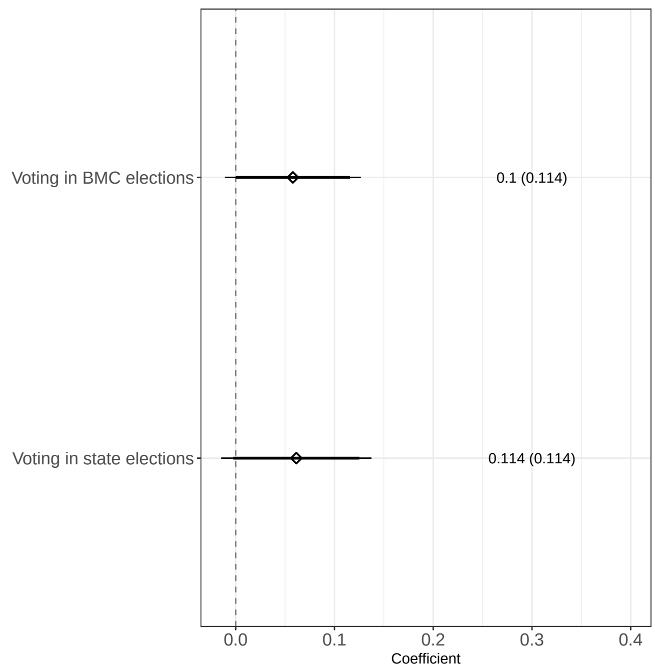


Figure B.3: Treatment effects for responding "Yes" to "Did you vote in the last MCGM (municipal) or state elections?"



Bars show 90% and 95% confidence intervals. Full regression output with and without covariate adjustment available in Table B.13. P-values (with p-values using Benjamini-Hochberg corrections for the false discovery rate in parentheses) are shown on the right.

B.2 Tables

Table B.1: 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 B.2: Proportion of members of each category in treatment and control groups after mapping with p-values for two-tailed t-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 B.3: Proportion of members of each category in full and mapped samples after mapping with p-values for two-tailed t-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 B.4: Reasons for attrition with p-values for difference in proportions tests.

	Control	Treatment	p
Surveyed	413	421	0.6
Address not found	9	7	0.8
Home demolished	1	0	1
Home locked	5	11	0.2
Respondent deceased	1	0	1
Refused	14	20	0.4
Unable to locate household that has moved	19	10	0.1
Incomplete survey	37	31	0.5
Total	**500**	**500**	-

Table B.5: 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

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

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

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

Table B.6: Regression estimates for treatment effects reported participation in local demand-making.

	Dependent variable:					
	Individual complaint making		Group complaint making		Attending local area meetings	
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.144***	0.142***	0.115**	0.114**	0.303***	0.294***
	(0.050)	(0.050)	(0.050)	(0.050)	(0.048)	(0.048)
OBC		0.038		0.049		0.045
		(0.058)		(0.058)		(0.056)
SCST		0.077		0.065		0.061
		(0.075)		(0.075)		(0.072)
Maratha		0.015		0.017		0.032
		(0.047)		(0.047)		(0.045)
Muslim		0.034		0.023		0.042
		(0.068)		(0.068)		(0.066)
Kutcha floor		−0.036		−0.017		0.070
		(0.125)		(0.125)		(0.121)
Kutcha roof		−0.230*		−0.216*		−0.250**
		(0.130)		(0.130)		(0.127)
From Mumbai		0.096*		0.079		0.095**
		(0.049)		(0.049)		(0.047)
From same ward as apt		−0.027		−0.067		0.079
		(0.063)		(0.063)		(0.061)
Constant	0.436***	0.351***	0.415***	0.346***	0.339***	0.239***
	(0.033)	(0.057)	(0.033)	(0.057)	(0.032)	(0.055)
Observations	834	834	834	834	828	828
R^2	0.169	0.185	0.168	0.182	0.234	0.247
Adjusted R^2	0.013	0.020	0.012	0.017	0.089	0.093
Note:						*p<0.1; **p<0.05; ***p<0.01

Table B.7: Regression estimates for treatment effects on knowledge of local politics.

	Dependent variable:					
	Party for corporator		Name for corporator		Name for a corporator in admin. ward	
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.003	0.004	0.014	0.015	0.113^{***}	0.110^{***}
	(0.046)	(0.046)	(0.016)	(0.016)	(0.041)	(0.041)
OBC		0.148^{***}		0.042^{**}		0.076
		(0.053)		(0.018)		(0.047)
SCST		0.099		0.035		0.005
		(0.068)		(0.024)		(0.061)
Maratha		0.092^{**}		0.039^{***}		−0.001
		(0.043)		(0.015)		(0.038)
Muslim		−0.064		0.066^{***}		−0.022
		(0.062)		(0.022)		(0.055)
Kutcha floor		−0.065		−0.025		0.075
		(0.114)		(0.039)		(0.101)
Kutcha roof		0.154		−0.009		−0.146
		(0.119)		(0.041)		(0.106)
From Mumbai		0.087^{*}		−0.012		0.011
		(0.045)		(0.016)		(0.040)
From same ward as apt		−0.030		0.0003		0.086^{*}
		(0.057)		(0.020)		(0.051)
Constant	0.295^{***}	0.175^{***}	0.021^{**}	0.004	0.148^{***}	0.124^{***}
	(0.030)	(0.052)	(0.010)	(0.018)	(0.027)	(0.046)
Observations	834	834	834	834	834	834
R^2	0.150	0.174	0.221	0.239	0.174	0.184
Adjusted R^2	−0.010	0.007	0.075	0.086	0.019	0.019

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

Table B.8: Regression estimates for treatment effects on attitudes.

	Dependent variable:							
	Happy w/ finances		Think children will have better lives		Would never leave Mumbai		Don't listen to local leaders	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	0.200***	0.192***	0.122**	0.120**	0.087**	0.078**	0.100**	0.087**
	(0.046)	(0.046)	(0.048)	(0.048)	(0.039)	(0.038)	(0.043)	(0.042)
OBC		−0.066		0.030		−0.015		−0.019
		(0.053)		(0.056)		(0.044)		(0.049)
SCST		−0.048		−0.141**		−0.048		0.084
		(0.068)		(0.071)		(0.057)		(0.063)
maratha		0.036		0.087*		0.067*		0.138***
		(0.043)		(0.045)		(0.036)		(0.040)
Muslim		0.062		0.005		−0.049		0.056
		(0.062)		(0.065)		(0.052)		(0.058)
Kutcha floor		−0.124		0.035		−0.136		0.089
		(0.113)		(0.119)		(0.095)		(0.105)
Kutcha roof		−0.129		−0.080		0.132		−0.128
		(0.118)		(0.124)		(0.099)		(0.110)
From Mumbai		0.160***		−0.011		0.172***		0.090**
		(0.045)		(0.047)		(0.037)		(0.041)
From same ward as apt		−0.037		−0.071		0.031		0.140***
		(0.057)		(0.060)		(0.048)		(0.053)
Constant	0.596***	0.483***	0.561***	0.563***	0.774***	0.632***	0.192***	0.063
	(0.030)	(0.052)	(0.032)	(0.054)	(0.025)	(0.043)	(0.028)	(0.048)
Observations	834	834	834	834	834	834	834	834
R^2	0.165	0.195	0.193	0.209	0.168	0.205	0.184	0.216
Adjusted R^2	0.008	0.033	0.041	0.049	0.011	0.045	0.030	0.057

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

Table B.9: Regression estimates for treatment effects for reported reasons for voting in the last municipal election (without covariates).

	Dependent variable:						
	Party	Ethnicity/Religion	Neighborhood problems	Financial problems	Policy prefs	Improving Mumbai	Improving country
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
T	0.052	0.023	0.218***	0.120*	0.019	0.065	0.043
	(0.065)	(0.037)	(0.067)	(0.062)	(0.056)	(0.059)	(0.037)
Constant	0.351***	0.081***	0.414***	0.239***	0.199***	0.222***	0.063**
	(0.043)	(0.024)	(0.044)	(0.041)	(0.037)	(0.039)	(0.025)
Observations	710	710	710	710	710	710	710
R^2	0.187	0.224	0.172	0.175	0.173	0.160	0.162
Adjusted R^2	0.020	0.064	0.002	0.005	0.003	−0.013	−0.011

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

Table B.10: Regression estimates for treatment effects for reported reasons for voting in the last municipal election (with covariates).

	Dependent variable:						
	Party	Ethnicity	Neighborhood problems	Finances	Policy prefs	Improving Mumbai	Improving country
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
T	0.020	0.013	0.228***	0.145**	0.045	0.080	0.044
	(0.064)	(0.037)	(0.068)	(0.063)	(0.056)	(0.061)	(0.038)
OBC	−0.029	−0.005	0.052	−0.099*	−0.022	0.042	−0.003
	(0.060)	(0.035)	(0.063)	(0.059)	(0.053)	(0.056)	(0.036)
SCST	0.070	0.049	0.087	−0.108	−0.212***	−0.085	−0.052
	(0.079)	(0.046)	(0.083)	(0.077)	(0.069)	(0.074)	(0.047)
Maratha	−0.064	−0.013	0.134***	0.050	−0.002	−0.014	−0.027
	(0.048)	(0.028)	(0.051)	(0.047)	(0.042)	(0.045)	(0.029)
Muslim	−0.027	−0.021	0.153**	−0.090	0.034	0.021	−0.015
	(0.068)	(0.040)	(0.072)	(0.067)	(0.060)	(0.064)	(0.041)
Kutcha floor	0.343**	0.021	−0.019	−0.101	−0.077	−0.123	−0.099
	(0.140)	(0.082)	(0.149)	(0.137)	(0.123)	(0.132)	(0.083)
Kutcha roof	−0.031	−0.078	−0.100	0.019	0.022	−0.042	−0.036
	(0.136)	(0.079)	(0.144)	(0.133)	(0.119)	(0.128)	(0.081)
From Mumbai	−0.247***	0.029	0.052	0.073	−0.041	0.068	−0.039
	(0.053)	(0.031)	(0.056)	(0.052)	(0.046)	(0.050)	(0.031)
From same ward as apt	0.142**	0.021	−0.142**	−0.100	−0.021	−0.032	0.026
	(0.066)	(0.038)	(0.070)	(0.064)	(0.058)	(0.062)	(0.039)
Constant	0.567***	0.064*	0.315***	0.197***	0.242***	0.169***	0.111***
	(0.066)	(0.038)	(0.070)	(0.064)	(0.058)	(0.062)	(0.039)
Observations	710	710	710	710	710	710	710
R^2	0.240	0.229	0.195	0.198	0.191	0.169	0.172
Adjusted R^2	0.071	0.058	0.016	0.020	0.011	−0.016	−0.012
Note:							*p<0.1; **p<0.05; ***p<0.01

Table B.11: Regression estimates for treatment effects on reported satisfaction with various outcomes (without covariates).

	Dependent variable:					
	Electricity	Garbage	Sanitation	Water	Law and Order	Roads
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.039	0.107**	0.116**	0.104**	0.146***	0.144***
	(0.037)	(0.044)	(0.045)	(0.041)	(0.045)	(0.047)
Constant	0.823***	0.680***	0.660***	0.739***	0.655***	0.605***
	(0.024)	(0.029)	(0.030)	(0.027)	(0.029)	(0.031)
Observations	834	834	834	834	834	834
R^2	0.146	0.166	0.168	0.148	0.158	0.160
Adjusted R^2	−0.015	0.009	0.011	−0.012	−0.0004	0.002
Note:						*p<0.1; **p<0.05; ***p<0.01

Table B.12: Regression estimates for treatment effects on reported satisfaction with various outcomes (with covariates).

	Dependent variable:					
	Electricity	Garbage	Sanitation	Water	Law and Order	Roads
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.040	0.109^{**}	0.115^{**}	0.105^{**}	0.146^{***}	0.137^{***}
	(0.037)	(0.044)	(0.045)	(0.041)	(0.045)	(0.047)
OBC	−0.007	−0.008	−0.037	0.002	−0.033	−0.015
	(0.043)	(0.052)	(0.052)	(0.048)	(0.052)	(0.055)
SCST	−0.079	-0.139^{**}	-0.245^{***}	-0.109^{*}	-0.132^{**}	-0.170^{**}
	(0.055)	(0.066)	(0.067)	(0.061)	(0.067)	(0.070)
Maratha	0.041	−0.014	−0.031	0.067^{*}	−0.036	0.017
	(0.035)	(0.042)	(0.042)	(0.039)	(0.042)	(0.044)
Muslim	−0.017	−0.036	-0.112^{*}	−0.068	−0.037	−0.047
	(0.050)	(0.060)	(0.061)	(0.056)	(0.061)	(0.064)
Kutcha floor	−0.140	−0.154	−0.182	−0.040	-0.208^{*}	−0.052
	(0.092)	(0.110)	(0.112)	(0.102)	(0.111)	(0.117)
Kutcha roof	−0.052	0.012	0.104	−0.101	0.064	0.025
	(0.096)	(0.115)	(0.117)	(0.106)	(0.116)	(0.122)
From Mumbai	0.018	−0.001	0.013	−0.035	0.080^{*}	0.055
	(0.036)	(0.043)	(0.044)	(0.040)	(0.044)	(0.046)
From same ward as apt	0.019	0.017	0.029	−0.008	−0.041	0.056
	(0.046)	(0.056)	(0.056)	(0.051)	(0.056)	(0.059)
Constant	0.811^{***}	0.705^{***}	0.699^{***}	0.769^{***}	0.633^{***}	0.578^{***}
	(0.042)	(0.050)	(0.051)	(0.046)	(0.050)	(0.053)
Observations	834	834	834	834	834	834
R^2	0.159	0.174	0.189	0.165	0.172	0.171
Adjusted R^2	−0.011	0.008	0.025	−0.004	0.005	0.004
Note:						$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table B.13: Regression estimates for treatment effects on reported voting. All regressions include treatment indicator interactions with mean-centered block dummies.

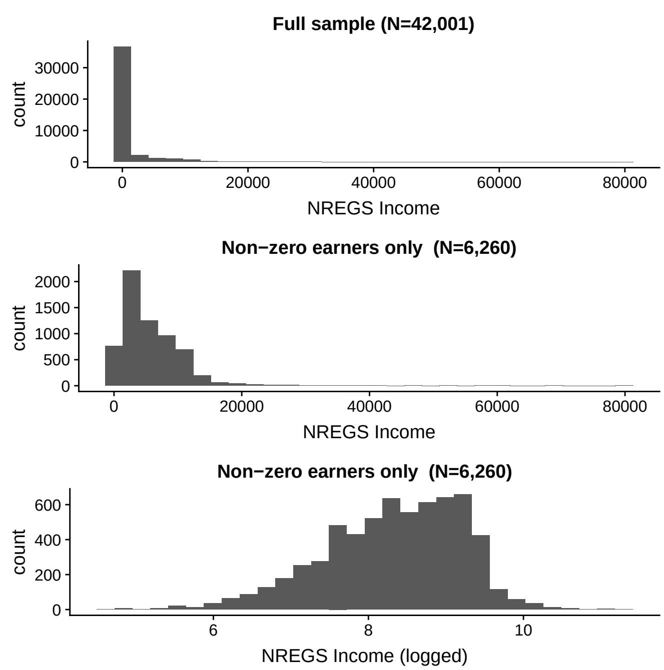
	Dependent variable:			
	Voting in BMC elections		Voting in state elections	
	(1)	(2)	(3)	(4)
T	0.060*	0.058*	0.069*	0.061
	(0.035)	(0.035)	(0.039)	(0.039)
OBC		0.009		−0.004
		(0.041)		(0.045)
SCST		0.004		0.002
		(0.052)		(0.058)
Maratha		−0.030		0.002
		(0.033)		(0.036)
Muslim		0.072		0.141***
		(0.048)		(0.053)
Kutcha floor		−0.168*		−0.085
		(0.087)		(0.096)
Kutcha roof		0.046		−0.029
		(0.091)		(0.100)
From Mumbai		0.114***		0.131***
		(0.034)		(0.038)
From same ward as apt		−0.012		0.028
		(0.044)		(0.049)
Constant	0.819***	0.735***	0.772***	0.658***
	(0.023)	(0.040)	(0.026)	(0.044)
Observations	834	834	834	834
R^2	0.185	0.206	0.179	0.202
Adjusted R^2	0.031	0.046	0.024	0.041
Note:				*p<0.1; **p<0.05; ***p<0.01

Appendix C

Supplemental information for paper 3

C.1 Figures and Tables

Figure C.1: The distribution of yearly NREGS earnings reported across India in 2011-2012.



Source: IHDS-II (2011-2012)

Table C.1: Difference-in-difference estimates for reported voting and confirmed voting in Uttar Pradesh in 2002 and 2007 (alternate measure of poverty).

	Dependent variable:	
	Voted	Voted (mark found)
	(1)	(2)
$\mathbb{I}_{t=2}$[1]	-0.072	0.103
	(0.054)	(0.071)
NREGA[2]	0.400^{***}	0.270^{***}
	(0.023)	(0.081)
$\mathbb{I}_{t=2} \times$ NREGA	-0.228^{**}	-0.273^{**}
	(0.059)	(0.108)
Constant	0.900^{***}	0.900^{***}
	(0.000)	(0.000)
Observations	1,019	1,019
R^2	0.180	0.216
Adjusted R^2	0.056	0.097

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

Coefficients on difference in difference estimates for turnouts in two elections. All models include district, year, and assembly constituency dummies. All standard errors are clustered at the district level.

Only poor respondents living in villages are included here. "Poor" is defined as those living in single room homes.

[1] Indicator for whether or not the observation occurred in 2007 (as opposed to 2002).

[2] Indicator for whether the assembly constituency belongs to a Phase I district.

Table C.2: Difference-in-difference estimates for reasons for not voting and reported interest in campaign interest in Uttar Pradesh in 2002 and 2007 (alternate measure of poverty).

	Reasons for not voting:					
	Out of station	Unwell	Did not know I was a voter	No Interest	Lack of ID	Interest in campaign[1]
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{I}_{t=2}$[2]	0.030	-0.020	0.025	-0.032	0.014	0.305^{*}
	(0.033)	(0.071)	(0.023)	(0.035)	(0.013)	(0.167)
NREGA[3]	-0.190^{***}	-0.000	-0.050	-0.093	-0.000	0.060
	(0.036)	(0.081)	(0.053)	(0.068)	(0.000)	(0.183)
$\mathbb{I}_{t=2} \times$ NREGA	0.060	0.020	0.026	0.125^{*}	-0.014	-0.556^{**}
	(0.049)	(0.108)	(0.058)	(0.076)	(0.013)	(0.247)
Constant	0.100^{***}	0.000	0.000	0.000	-0.000	0.333^{***}
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,019	1,019	1,019	1,019	1,019	1,019
R^2	0.139	0.070	0.125	0.142	0.190	0.316

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

$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$
Coefficients on difference in difference estimates for turnouts in two elections. All models include district and assembly constituency dummies. All standard errors are clustered at the district level.
Only poor respondents living in villages are included here. "Poor" is defined as those demarcated as "very poor" or "poor" by Lokniti-CSDS based on an index of asset ownership.
[1] Dependent variable is whether or not respondent says "somewhat" or "a great deal" when asked "Now I would like to talk with you about the election campaigning during this elections. How much interest did you have this time?" much interest did you have this time - a great deal or somewhat or not at all?
[2] Indicator for whether or not the observation occurred in 2007 (as opposed to 2002).
[3] Indicator for whether the assembly constituency belongs to a Phase I district.

Appendix D

Pre-analysis plan filed on September 10, 2017

Subsidized low-income housing programs can found in urban centers in Brazil, Mexico, Chile, Argentina, Kenya, Ethiopia, and every major city in India. Yet there seems to be no rigorous evidence of whether and how such programs affect the lives of beneficiaries. I will evaluate one such program in Mumbai, Maharashtra. Faced with overwhelming demand, this particular program is implemented through a lottery system, thereby allowing causal identification of its effect among applicants. This study will survey 1000 winners and non-winners of this program to identify the impact of property ownership on political and economic behavior.

D.1 The intervention

I will evaluate Maharashtra Housing and Development Authority's (MHADA) housing program for economically weaker section (EWS) and low-income group (LIG)[1] urban residents. While the application fee leaves this program out of reach of many slumdwellers, eligible low-income families without property have the opportunity to purchase subsidized apartments. Based on qualitative surveys of program applicants that I conducted in 2016, this segment of the urban population is comprised mainly of poor renters and large extended families cramped in small ancestral homes. On average, the price per square foot of a MHADA apartment is 60% of the median price per square foot of other apartments in the same neighborhood. Provision of housing at such prices is possible because housing is constructed on land obtained for free from the city's dismantled textile industry - this land is earmarked specifically for social projects and cannot be used for other purposes Madan (2016).

[1]EWS earn up to 3200 USD/year. LIG earn up to 7400 USD/year

D.2 Theory and outcomes

I will estimate the impact of property ownership on an array of outcomes. I predict that these many outcomes are linked to the treatment through a smaller set of mechanisms, each of which I discuss below.

Permanent income mechanism

The intervention is an asset transfer combined with the option to relocate to the new apartments. Subsidized prices for the asset lead to an increase in a households permanent income; while households do not immediately realize the income effect of the subsidy as they cannot sell their apartments for until 10 years after possession, I assume that they smooth their lifetime consumption across time periods with this subsidy in mind Friedman (1957). I will measure changes in consumption of items with long-term payout ratios such as education, the education of women in particular, and medical expenditure. As income is also correlated with psychological well-being (Haushofer & Fehr (2014)), I will also measure changes in indicators such as stress, life satisfaction and an individuals locus of control. Additionally, I will investigate the claim made by DiTella et al (2007) that property ownership leads to an increased belief in "market values."I refer to this mechanism as the **permanent income mechanism**. I will assess the impact of property ownership on savings, psychological well-being, belief in market values, and expenditure on education through this mechanism.

Collateral mechanism

Moreover, as assets are collateral for loans, a household's ability to borrow formally may increase (Soto (2003)). Voting citizens can be thought of as principals who elect politicians, their agents, to represent their policy preferences. Yet poor voters may place a higher utility on immediate receipt of cash or in-kind services than affecting the policy making process. One way to reduce the incidence of vote-buying is to reduce the voters utility from participating in vote buying. Larreguy et al. (2015b) find that a land titling program in Mexico weakens patterns of vote buying by reducing voters reliance on politicians for tenure security. Frey (2015) also claims that Brazil's Bolsa Familia conditional cash transfer program reduces the marginal utility of cash payments to voters, thereby reducing support for clientelistic parties. Similarly, property ownership may decrease the marginal utility from cash or in-kind payments by making an asset available as collateral for loans, thereby generating another source of cash for voters (Field & Torero (2006); Soto (2003)). I refer to this mechanism as the **collateral mechanism**. I will assess the impact of property ownership on access to formal loans and turning to local leaders and informal loans when faced with shocks this mechanism.

Property values mechanism

Asset ownership should also affect the extent to which an individual feels invested in his or her immediate political context. First, as concerns about property value should increase the marginal utility from participating in the policy-making process, I will estimate treatment effects on voting with the aim of having ones policy preferences represented (as opposed to identity based or clientelistic voting), and participation in civic associations and town hall meetings. Also, as it would seem that ownership of a fixed asset would decrease a households propensity to move from the area, I will measure changes in a households predicted likelihood of migrating from the city. I refer to this mechanism as the **property values** mechanism.

Property tax mechanism

Moreover, in India and other countries with a large informal sector, property ownership entails the incidence of a property tax, the first and only direct tax most citizens pay. The prevalence of informal labor means that income tax is rarely collected; in fact, in India, only 1% of the country's population paid income tax in 2013. During qualitative interviews, most low-income individuals were unaware that they paid sales or value-added taxes and claimed that they paid only property tax to the local municipal government. As a vast literature on state-formation suggests individuals demand services from the state in exchange for tax payment, I predict that property ownership will also lead individuals to demand higher quality service provision from the local government in sectors such as road quality, maintenance of law and order, and public waste management. I refer to this as the **property tax** mechanism.

Reciprocity mechanism

Finally, the project will investigate the claim made by De La O (2013) and others that receipt of a government benefit should increase an individual's support for the incumbent party/administration. I refer to this as the **reciprocity** mechanism.

D.3 Design

I will survey winners and applicants from both 2014 and 2012 to see if effects attenuate or increase over time. This study will be completed for both EWS and LIG income groups.

Estimation

For all outcomes, I will calculate both average treatments and treatment effects conditional on income group (LIG or EWS), whether a household actually moves, and the number of years that have elapsed since the lottery was conducted. These variables should moderate the intensity or level of treatment. For outcomes related to **property values** and **property tax** mechanisms, I will also calculate treatment effects conditional on whether the property is in the same ward as that in which the household lived when applying. Additionally, the **property values** and **property tax** mechanisms will be investigated through embedded survey experiments. Respondents will randomly receive a series of questions that prompt them to think about property taxes, property values, both, or neither. See modules 3 and 4 in the attached questionnaire to see the exact wording of the questions. Finally, I will also estimate a local average treatment for households that actually comply, or purchase lottery housing.

Treatment effects will be estimated using regression with covariate adjustment. Rules for hypothesis testing are specified in the section titled "Multiple testing and split-sample strategies." As randomization occurs within groups applying for ownership within individual buildings *and* within caste and profession groups (by building), regressions will be adjusted for this "nested blocking" design. This will be done as follows: A group-building dummy will be created by multiplying the indicator for membership in individual building lotteries and the indicator for membership in social groups. Then, two regressions will be run. The first will be a regression of the outcome on the treatment indicator, covariates, and group level dummies. The second will be a regression of the outcome on the treatment indicator, covariates, and the treatment indicator interacted with the building dummy minus the the percentage of the sample in that building. This second equation is suggested for block randomized design by Green et al. (2016).

If there are $C_1...C_j$ covariates, $G_1...G_k$ social groups, $B_1...B_l$ buildings, and $Q_1...Q_m$ building groups, the following equations (written in vector format) will be estimated for each outcome Y:

$$Y = T + \sum_1^j C_i + \sum_1^l Q_i \tag{D.1}$$

$$Y = T + \sum_1^j C_i + \sum_1^l (B_i - \bar{B}_i) \tag{D.2}$$

Bell-McCaffrey adjustments will be used for reporting standard errors, degrees of freedom, and confidence intervals.

Variables and measurement

I will estimate the effect of the treatment on several outcomes, enumerated below. For each of the dependent variables, the relevant questions from the survey instrument (see Appendix) are provided in parentheses. For outcomes with multiple measurements, an index will be created using principal component analysis.

The mechanisms, dependent variables, and moderating variables are summarized in Table D.1.

Table D.1: Mechanisms, dependent variables, and moderating variables

Mechanism	Outcomes	Moderating Variables	Survey experiment?	Primary?
Collateral	1,3	Income group, lottery year, moving	No	No
Permanent Income hypothesis	2,12,13,14,15	Income group, lottery year, moving	No	Yes
Property values	1,6,7,8,9,11	Income group, lottery year, moving , ward	Yes	Yes
Property tax	10	Income group, lottery year, moving , ward	Yes	Yes
Reciprocity	16	Income group, lottery year, moving	No	No

Dependent variables

1. Access to formal loans (Q7.7, Q7.8, Q7.9, Q7.10, Q7.11)
2. Savings (Q6.34)
3. Turning to local leaders and informal loans when faced with shocks (Q7.7)
4. Supporting candidates who can help with day-to-day financial assistance (Q5.3, Q5.5)
5. Supporting candidates who can represent specific policy preferences (Q5.3, Q5.5)
6. Supporting candidates who support long-term improvement of economy and infrastructure (Q5.3, Q5.5)
7. Knowledge about different the role of local government (Q5.9, Q5.10)
8. Participation in civic associations (Q5.11, Q5.12, Q5.13, Q5.14)
9. Running for office (Q5.15)
10. Demand for better local services (Q5.17, Q5.18, Q5.19, Q5.20, Q5.21, Q5.22, Q5.23)
11. Propensity to leave Mumbai (Q9.7, Q9.8)
12. Psychological well-being (Q9.1, Q9.2)
13. Independence (Q9.5, Q9.6)
14. Belief in market values(Q9.3, Q9.4)
15. Expenditure on education (Q6.11.1-Q6.11.x (x denotes number of children) Q6.13, Q6.14, Q6.15, Q6.16)
16. Assigning responsibility for lottery winnings (Q2.9, Q2.10)

Moderating variables (all information known prior to survey)

Heteregeneous treatment effects will be calculated conditional on the following variables in accordance with Table D.1.

1. Income group
2. Lottery year
3. Treatment condition for embedded survey experiment (individuals will be randomly assigned to receive a prompt about property taxes, property values, both, or neither)
4. Whether the lottery building is in the same ward as the original home

Covariates and variables for randomization checks

As I am collecting post-treatment data only, these variables will be either fixed characteristics or variables measured through recall. I will perform a statistical test to judge whether observed covariate imbalances are larger than would normally be expected from chance alone. This test involves a regression of the treatment indicator on the covariates and calculation of a heteroscedasticity-robust Wald statistic for the hypothesis that all the coefficients on the covariates are zero. As the experiment is block-randomized with treatment probabilities that vary by block, I will also include dummy variables for the varying treatment probabilities in the regression, and I will test the hypothesis that all coefficients on the covariates, excluding the treatment probability dummies, are zero.

I will use a permutation test (randomization inference) to calculate the p-value associated with the Wald statistic.

1. Jati (Q6.30)
2. Religion (Q6.29)
3. N household members over the age X, where X is the number of years since the lottery has occurred (Q6.11.1-Q6.11.x (x denotes number of children))
4. Whether or not the lottery home and original address are in the same ward (known prior to survey)
5. Years in Mumbai (Q6.5)
6. Roof construction in home at time of lottery application (permanent or semi-permanent) (Q6.4)
7. Floor construction in home at time of lottery application (permanent or semi-permanent) (Q6.2)

Multiple testing and split-sample strategies

Given the sheer number of hypotheses being tested, it is important to adopt a strategy to avoid false discoveries while preserving power at the same time. As a result, I will adopt a hybrid split-sample strategy as recommended by Anderson and Magruder (2017) and Olken (2015). In other words, a plan for analysis and multiple testing adjustments for certain outcomes will be pre-specified. Hypotheses related to other outcomes will be tested on an exploratory sample to be passed on to a confirmatory sample based on rules outlined below.

Prespecifying an analysis plan for primary outcomes

This section refers to outcomes related to mechanisms identified as "Primary" in Table D.1. Treatment effects conditional on moderating variables are *not* included in the group of primary outcomes, but the survey experiments are included in this group. Furthermore, all local average treatments for compliers (purchasers) are included. The impact of property ownership on these outcomes are hypotheses I intend to test with the full survey sample. A null hypothesis will be rejected at the 5% level under a two tailed test.

I will make multiple testing adjustments to correct for the family-wise error rate (using simulation) for the hypotheses related to the multiple outcomes *within* each family of mechanisms. For example, all of the outcomes related to the **permanent income hypothesis** mechanism will be considered a family within which multiple testing corrections will be made. Treatment effects for the embedded survey experiments are considered distinct families. Local average treatment effects for compliers are also considered distinct families.

Employing a split sample strategy for secondary outcomes

This section refers to outcomes related to mechanisms *not* identified as "Primary" in Table D.1. Treatment effects conditional on moderating variables included (but not limited to) those in Table D.1 are included in the group of secondary outcomes. This approach is derived from suggestions made by Anderson and Magruder (2017). I will first test these hypotheses on an exploratory sample of 30% of the final survey sample.[2]. Null hypotheses rejected with a t-value of 1.6 or higher will be tested in the remaining 70% confirmatory sample. Tests in the confirmatory sample will be rejected at the 5% level under a one-tailed test, with the direction of the treatment effect obtained from tests on the exploratory sample. Before tests are run in the confirmatory sample, a simple pre-analysis plan with the results of tests run on the exploratory sample will be filed as an amendment to this analysis plan.

[2]Other hypotheses not specified in this plan may be tested in the exploratory sample

Power

The sample size needed to detect an effect of a given treatment 80% of the time at the 5% level under a two-tailed test (size and SD taken from sources) is presented in Table D.2. The N required here is the total for both treatment and control arms. Power estimates are conservative, as they do not account for efficiency gains from stratification or covariate adjustment.

Table D.2: Range of sample sizes required to detect given an ATE and SD from various sources

Source	**Treatment**	**Outcome**	**ATE**	**SD**	**N**
Pilot survey	Winning 2011 lottery	Knowledge of Mumbai's Open Spaces Policy	0.20	0.40	197
DiPasquale & Glaeser (1999)	Homeownership	Knowledge of US Representative	0.25	0.19	20
DiPasquale & Glaeser (1999)	Homeownership	N of Non-professional orgs. to which respondent belongs (count)	0.59	0.23	5
Pilot survey	Winning 2011 lottery	Access to credit	0.12	0.20	88
Field & Torero (2006)	Land title	Approval of loan application	0.09	0.03	4
Pilot survey	Winning 2011 lottery	Expenditure on education per term	1200	2500	137
Haushofer & Shapiro (2016)	Large unconditional cash transfer	Expenditure on education per term	0.92	0.67	17

Data collection and estimated sample size

Estimation of treatment effects requires that I observe outcomes for both treated and control units. The sample size is not fixed beforehand, but will be determined by a process of contacting. For the 2012 and 2014 MHADA lotteries, I have procured from MHADA phone numbers and addresses for winners and a random sample of applicants.[3] In particular, 1,849 addresses (921 non-winners, and 928 winners, respectively) were provided by the agency. These addresses were mapped using Google Maps. Addresses that were incomplete, outside of Greater Mumbai, or could not be found were removed from the sample. This left 531 and 532 control and treatment households, respectively. The final breakdown of households in the sample is provided in Table D.3. Note that there was no housing lottery for EWS residents in 2012.

Table D.3: N Households to be contacted in each category

	EWS(Control)	EWS (Treatment)	LIG(Control)	LIG (Treatment)	**Total**
2012	0	0	253	264	**517**
2014	167	158	112	109	**546**
Total	**167**	**158**	**365**	**373**	

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

These addresses will be given to a Mumbai based survey firm that will then contact the households and conduct interviews.[4] The process for contact will be as follows: The addresses and phone numbers provided by MHADA constitute the contact information for households at the time of application. Non-winners will be attempted at these addresses. In case they have moved away, neighbors will be asked for updated contact information, with which the enumerators will once again attempt to contact non-winners. Winners may reside at either the old addresses or new lottery buildings, as they are free to either inhabit their new property or rent it out. Lottery housing societies will first be contacted to ascertain which of the winners are living at the apartments. These individuals will be approached at the lottery apartments; the others will be approached at the addresses listed on the application using the procedure developed for non-winners. Care will be taken to ensure that the same team and identical protocols are used to approach both winners and non-winners.

In an exercise conducted in January 2017, 72.5% and 80% (p-value of the difference is 0.430) of samples of applicants and winners, respectively, from 2014 could be re-contacted. Given a 70% rate of contacting, the expected number of surveys completed is 745. The expected number of interviews completed per category is given in Table D.4. Given the power calculations conducted earlier, this sample size should be sufficient not only to calculate treatment effects on outcomes of interest for the whole sample, but for some effect sizes for the main subgroups of interest as well. Final results will be accompanied by an illustration of the minimum detectable effect for each outcome, given the final rate of contact.

Table D.4: N estimated households surveyed in each category

	EWS(Control)	EWS (Treatment)	LIG(Control)	LIG (Treatment)	**Total**
2012	0	0	177	185	**362**
2014	117	111	79	76	**383**
Total	**117**	**111**	**256**	**261**	

D.4 Conclusion

This document has prespecified the analysis plan for the evaluation of a low-income housing program conducted by lottery in Mumbai in 2012 and 2014. As discussed in the section titled "Multiple testing and split-sample strategies," secondary outcomes (as defined in Table D.1) to be included in confirmatory analysis will be tabulated in an amendment to this analysis plan to be filed before I see the entire data set. While all

[4]More information about the firm can be found [here].

results may not be included in a single paper or publication, all prespecified results will be made available in a single document available on request and to be made public following the publication of all papers resulting from this study. Adjustments to models or analyses will be explained and filed prior to data analysis if possible. Any additional specifications or analyses will clearly described as “exploratory analysis" in papers and publications.