Home-price subsidies increase local-level political participation in urban India *

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Abstract

Home-price subsidies are common in low- and middle-income countries. How do they affect an important input into local governance, namely citizens' propensity to make everyday demands or claims? I study the effects of a program in Mumbai, India through an original survey of winners and non-winners of program lotteries. Winning increases participants' reported claims to improve services, knowledge about municipal government, and changes policy preferences, even among those who rent out the homes. Transfers can thus generate active citizenship through many channels including increased political capacity, improved perceptions of self-efficacy, expanded expectations of government, and changed motivations of recipients. They also create interest groups at the local level, where their actions can have both positive and negative externalities. The findings are among the first causally identified effects of policy on claim-making, and add a new context and dependent variable to the literature on housing policy.

Keywords: affordable housing, India, claims, complaints, cities, participation

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Governments in low- and middle-income countries (LMICs) often deliver cash or in-kind transfers to many citizens through welfare programs aiming to mitigate poverty and inequality. Conditional cash transfers, for example, had spread to 18 countries in Latin America and the Caribbean and reached about 135 million beneficiaries by 2010 (Stampini and Tornarolli 2012). Table 1 further shows that in India, the site of this study, a large portion of the population of over 1 billion reports benefitting from various sets of transfers. While the stated aim of such programs is to provide economic assistance to households, transferring income or wealth might also alter recipients' *political* behavior, potentially leading to large shifts in the aggregate political landscape when such efforts are undertaken at scale. As a result, many have investigated the electoral returns to many different types of transfers in LMICs (e.g. Bobonis *et al.* 2017; De la O 2013; Diaz-Cayeros *et al.* 2016; Manacorda *et al.* 2011; Zucco 2013).

Table 1: Proportion of respondents to a nationally representative survey reporting that they benefit from a given program.

Program	Proportion
Old age pension	0.0908
Widows' pension	0.0511
Maternity scheme	0.0287
Disability scheme	0.0131
Food security scheme (elders)	0.0023
Sanitary latrines	0.0509
Subsidized loans (farmers)	0.0513
Rural housing subsidies	0.0514
Rural employment guarantee	0.2844

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

Yet political activity extends beyond voting. I study everyday demands, or claims, placed with politicians, bureaucrats, and brokers for state-provided goods and services. Claim-making forms a cornerstone of political participation in many countries and can occur even among those who engage in *quid pro quo* voting at election time (Bussell 2019 Kruks-Wisner 2018b). When the demand for publicly provided goods and services far outstrips supply, direct or mediated claims of politicians and bureaucrats can help generate access to the goods and services over which they have jurisdiction or discretion. Studying how transfers

shape this claim-making is essential to understanding how government policies can attenuate or exacerbate inequality in access to government resources, particularly if the likelihood of becoming a beneficiary varies across the population.

I predict that transfers increase claim-making through multiple channels. These predictions build upon and expand a literature on policy feedback from another context, namely the US and Europe (see Campbell 2012 for a review). First, welfare transfers can make beneficiaries wealthier, thereby increasing their time horizons and increasing their capacity for action. Transfers might also improve beneficiaries' perceptions of the efficacy of their actions through improved self-perceived status and repeated interactions with government. Furthermore, transfers can reshape citizens' expectations of what governments can and will provide to them. Finally, transfers might change the content of claim-making. They might motivate beneficiaries to make *new* claims to protect or increase the wealth transferred, even once other needs are fulfilled. I argue that when policy implementation is decentralized, protecting transfers also entails making claims for improvements at the *local* level, which has responsibility for program quality and where officials may be most visible to citizens.

These predictions run counter to an existing literature focusing on electoral politics, which has found that wealth shocks decrease citizens' dependence on clientelistic relationships (e.g. Bobonis *et al.* 2017; Hicken 2011; Larreguy *et al.* 2015; Nathan 2016). These studies on clientelism and the very poor, however, focus on strategies to access or provide basic goods and services that may offer little utility once citizens have escaped poverty. Due to its nature of being citizen-initiated and occurring even outside of election time, the domain of claim-making encompasses anything that citizens seek; it is not contingent on what patronage and clientelistic networks are able to provide (Kruks-Wisner 2018a, 199-200). Even wealthier citizens may participate in claim-making, and benefitting from welfare programs may induce *new* demands.

It is difficult to empirically assess the effects of welfare transfers on claim-making with existing data. A study of observational data would suffer from difficult problems of selection,

such as the high likelihood that being politically active actually helps one access transfers in the first place. Researchers can use the staggered or randomized rollout of programs to avoid such problems of endogeneity and identify causal effects on other outcomes, such as turnout and vote share (e.g. De la O 2013; Zimmerman 2020). Claim-making, however, is rarely measured in the administrative data upon which such studies rely.

I provide some of the first experimental evidence on the effects of welfare transfers on claim-making by studying a subsidized home-price program, a policy that is widespread in LMICs and high-income countries alike. Table 1 shows that at least 5% of the population has benefitted from such programs in rural India alone. The Indian government has spent up to 1.65% of its GDP on such home subsidy programs, indicating a deep financial commitment to distributing wealth through this vehicle (Nayar 2009, 99). I study a program implemented through a lottery system to causally identify the effects of receiving a highly subsidized apartment for purchase in Mumbai, India on claim-making. Winners can either move into the homes and enjoy a stream of housing benefits, or rent them out and use them as productive assets. To estimate the effect of home ownership on local claim-making, I located and surveyed 834 winning and non-winning applicants of multiple lotteries held between 2012 and 2014.

I estimate large and positive effects on reported claim-making. On average, winners are 14 percentage points (pp.) more likely than non-winners to report individually approaching bureaucrats and politicians to demand improvements to their communities, 16 pp. more likely to report doing so in groups, and 11 pp. more likely to be able to correctly name a local elected official. They are also 29 pp. more likely to report attending neighborhood meetings where claims for community improvements are made and discussed. They exhibit policy preferences that are distinct from the control group as well, as they are about 11 pp. more likely to report that the municipal government should prioritize issues of water, electricity, sanitation, and regulating street vendors. I measure knowledge and participation for the neighborhoods in which respondents *live*, regardless of whether or not they chose to

move into the homes. Outcome means are similar across those who did and did not relocate, suggesting that effects are not driven solely by relocation.

I next discuss possible mechanisms behind these effects. First, I illustrate reasons why the program may facilitate claim-making in general. Winners have longer time horizons and more hopeful attitudes about the future, suggesting that they have greater mental bandwidth and, therefore, capacity for action. They exhibit an increased sense of status relative to authority figures, suggesting greater self-efficacy. Increased knowledge about local government may further increase self-efficacy. Winners are also more satisfied with government services than non-winners, suggesting that claim-making may be the result of greater expectations of government as well. I show, however, that the specific changes in policy preferences suggest that there is not an effect on general claim-making, but rather action motivated by a desire to improve local communities and lottery home values. Beneficiaries further explicitly exhibit an increased interest in community-level issues as demonstrated by reported reasons for candidate choice in local elections.

Overall, the results show that subsidizing homeownership creates an interest group of beneficiaries able and motivated to protect their welfare transfer. I suggest that under certain conditions, other welfare programs providing a sustained stream of transfers 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 claim-making among beneficiaries. The relevant scope conditions are related to the ease of organizing around benefits, the size of the transfer and the ability of beneficiaries to actually change its value, and beneficiaries' beliefs about whether they will continue to have access to the transfer in the future.

The findings are among the first sets of causally identified effects of any government welfare policy on claim-making. This study contributes to a sparse literature on political behavior in LMICs by highlighting political capacity, efficacy, expectations, and motivations as causes for political participation beyond simply trying to meet one's immediate needs. I

also add to a nascent literature on policy feedback in LMICs (e.g. Holland 2018; Hern 2017; Hunter and Sugiyama 2014; MacLean 2011) by highlighting ways in which welfare policies can facilitate and motivate action beyond shaping expectations of government efficacy or will to provide resources to citizens. Furthermore, while US-based studies of policy feedback measure local-level political participation as evidence of increasing political capacity (e.g. Mettler and Welch 2004), I use the idea of decentralization to clarify *why* beneficiaries would be motivated to participate in local politics.

Finally, the study adds a new context and set of dependent variables to the "embryonic" (Ansell 2019, 166) literature on housing policy that has, until now, focused on effects on national-level political participation and preferences in the US and United Kingdom. It also demonstrates that by conferring wealth and power upon citizens, government policies to subsidize homeownership can create politically active interest groups of homeowners. The actions of these interest groups can have both positive and negative externalities. As such programs often reach the middle-class (rather than the poor), studying their effects is essential to understanding the perpetuation of power inequalities at the local level.

Welfare transfers and claim-making in India

To date, much of the research on the political effects of welfare in LMICs has focused on electoral behavior as observed through the lens of clientelism, wherein public goods and services are seen to be distributed in exchange for votes (Kitschelt and Wilkinson 2007). For example, representatives at India's municipal, state, and national levels receive "area development funds" to respond to requests made by constituents, and researchers have found that the use of these funds can be strategically targeted to win votes (Jensenius and Chhibber 2018). To better understand if citizens similarly trade votes for transfers, many study electoral returns to transfers made through various welfare programs. In the short term, research from Latin America has shown that voters often reward implementing parties or politicians for cash transfers (De la O 2013; Manacorda *et al.* 2011; Zucco 2013).

Programs that substantially increase recipients' well-being in the long-term, on the other hand, can lead to a decline in electoral participation. In Mexico, for example, Larreguy *et al.* (2015) 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 reduces clientelistic voting for the municipal government as households' need for political intermediation disappears. 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. These findings support the argument that the utility of clientelistic voting decreases with income (Hicken 2011; Nathan 2016).

But there exist important forms of political engagement that occur outside of the electoral arena. An emerging literature on India focuses on citizens' everyday interactions with local-level government, particularly efforts to access to goods and services such as improved public sanitation, better roads, and improved lighting (Auerbach 2016; Bussell 2019; Kruks-Wisner 2018b). Beyond simply voting for those who help them, individuals negotiate with intermediaries and place pressure on bureaucrats and officials to get what they want. This behavior is described as "claim-making." Following Kruks-Wisner (2018b, 124), I define claim-making as "action–direct or mediated–through which citizens pursue access to social...goods and [public] services, understood as publicly provided resources intended to protect and improve well-being and social security."

Defined in this way, claim-making can encompass any set of goods or services that a citizen chooses. As argued by Kruks-Wisner (2018b), a citizen's experiences shape what she believes is possible to ask for; the domain of claims thus is not determined by what clientelistic networks or even programmatic policies can provide. In contrast, a defining feature in the substantial literature on patronage and clientelism is that these networks specifically target the needs of the poor with items like cash, food, or land titles (see Hicken

2011 for a review). Claim-making can certainly address these needs and operate through clientelistic networks, but it does not have to. As a citizen-initiated action, claim-making can operate through whichever channel an individual believes is most effective to achieve her aims, including partisan networks or some other direct or mediated approach (Kruks-Wisner 2018a, 200). It may offer utility even as one's income increases, though the *content* of claims may vary with income.

From a governance perspective, claim-making alerts governments to deficiencies in service provision and helps citizens get what they need. When the demand for publicly provided goods and services is far greater than supply, politicians and bureaucrats can have discretion over how these resources are allocated, and can help individuals move up in a long queue. Even while much of the literature on local public goods provision highlights variables such as shared partisan identity or ethnicity as important predictors for when officials will exercise this discretion (see Golden and Min 2013 for a review), recent work has found that politicians in India may help those who approach them (Bussell 2019) and that participation in government meetings can support "deliberative democracy," thereby decreasing the scope of elite discretion (Sanyal and Rao 2018). The Municipal Corporation of Greater Mumbai (MCGM) has also digitized its process for making and receiving responses to claims for improvements to communities.[1] This is part of a larger trend wherein several state and municipal governments in India have developed a digital process to transparently handle complaints about government infrastructure and services. The government cannot, of course, be responsive to all requests; while making a claim is not sufficient, it is often necessary for getting a response. In democratic contexts with limited state capacity, officials may also have an incentive for responding to louder voices.

Yet there is variation in the extent to which individuals will participate in claim-making. Demands for collective services in particular require organization and entail the problem of freedridership; members of any group can defect from participation in such action but still

[1]MCGM is also known as the Brihanmumbai Municipal Corporation, or BMC.

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, or claim, to anybody about neighborhood sanitation conditions.[2] Furthermore, according to a nationally representative survey conducted in 2011-2012, only about 30% of households report ever having attended a ward or village level meetings where claims, service delivery, and the use of development funds are discussed (India Human Development Survey- II (IHDS-II) 2016).

How might becoming a welfare beneficiary affect participation in this cooperative behavior? Studying the determinants of claim-making can help shed light on local-level inequalities in service provision and quality. Given the redistributive aims of many welfare programs, it is further important to understand the effects of government transfers on participation in claim-making to uncover their role in attenuating or exacerbating such inequalities. Kruks-Wisner (2018b) finds that exposure to the state provides citizens with information and shapes their expectations of what government can provide, and is an important predictor of claim-making in rural areas. I further seek to understand how government transfers affect claim-making among those who already have exposure to the state. I turn to research on policy feedback from the United States to develop predictions.

Changes in capacity, expectations, and efficacy facilitate action

First, welfare transfers may increase beneficiaries' capacity for action through what the policy feedback literature calls "resource effects." Campbell (2003), for example, finds that the receipt of Social Security and Medicare allows Americans to retire and participate in politics more as they age. There are reasons to believe that this effect might be strong in India and other LMICs, where the average transfer recipient is likely to be even poorer than in the US. I have observed that non-winning applicants of the program I study appeared far too stressed to think about claim-making beyond their most immediate needs. Scholarship in development economics (see Haushofer and Fehr 2014) has found that poverty can create stress and

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

lead to short-sighted behavior. Increasing household wealth could decrease present bias and increase the mental bandwidth to participate in claim-making. Similarly, the 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.

Welfare transfers might also improve citizens' perceptions of their self-efficacy and thereby motivate action by increasing its perceived utility. Increases in income could change one's sense of status in a community, thereby increasing the perceived likelihood of success when making a claim. This mechanism is particularly important in extremely hierarchical societies; in India, for example, the state is frequently described as being indifferent to the needs of the poor or lower caste (Ahuja and Chhibber 2012). In the long-term, repeated interactions with government can increase real and perceived self-efficacy by building knowledge of how to effectively make future claims.

Additionally, welfare programs may generate "interpretive effects" or change beneficiaries' expectations of, and perceptions of their relationship to, government (Pierson 1993). It is this effect and mechanism that has received the most attention in LMICs. On the one hand, social welfare transfers signal the *ability* of an otherwise low capacity state to provide for its citizens. MacLean (2011), for example, discusses how past experiences with health and education services create a set of expectations about government programs that motivate action when the quality of these services declines. Kruks-Wisner (2018b) similarly finds that in rural Rajasthan, where service provision is uneven, greater exposure to the state and what it can provide is correlated with claim-making. On the other hand, research from LMICs has shown that when individuals receive transfers, they not only observe that the state has the capacity to deliver, but that it will deliver to *people like them* (Hern 2017). Holland (2018) finds, after all, that low support for welfare among the poor stems from a belief that the transfers will not reach them. Hunter and Sugiyama (2014) show that a program like Brazil's *Bolsa Familia* sends citizens important signals that the government will provide services to people like them. Overall, this research argues that one effect of

welfare programs, particularly in contexts with weak or uneven state capacity, is to provide citizens with information that shapes their expectations about what the state can and will provide. These interpretive or expectation effects are distinct from the increased capacity effect described above because they refer to citizens' expectations of state capacity, rather than beliefs about their *own* efficacy.

Changes in wealth motivate action

Given greater capacity, self-efficacy, and expectations of government, for what reasons will individuals make claims? Welfare beneficiaries may be motivated to protect their newfound wealth by improving levels of service provision. Those who study the US and Europe argue that benefitting from government social welfare can "create material incentives for mobilization" (Mettler and Soss 2004, 62). It can encourage political participation to ensure either the continued or increased receipt of program transfers (e.g. Campbell 2012; Mettler and Soss 2004; Pierson 1993). Welfare programs may thus induce new claims and changed policy preferences.

Why might increases in claim-making be visible at the *local* level, where policies are not made? Studies from the US cite changes in local-level political participation as evidence of increasing political capacity among welfare beneficiaries (e.g. Mettler and Welch 2004), but it is possible that transfers *motivate* local-level participation as well. While many welfare programs in India and elsewhere are crafted at the state or national levels, local governments are responsible for their implementation in places that have seen the devolution of administrative responsibilities to local government. India's National Rural Employment Guarantee Scheme (NREGS), for example, guarantees all rural households 100 days of wage labor on infrastructure projects. It lays out an important role for *gram sabhas*, or deliberative bodies of eligible voters in a village: they are the arenas for citizens to provide recommendations on priorities for the local public works and to conduct audits of completed and ongoing labor projects.

Furthermore, in places with political decentralization, individuals may be more likely to make transfer-related claims to local officials who are more visible or accessible to ordinary citizens than officials at higher levels (Corbridge *et al.* 2005).[3] 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 claims related to their welfare benefits.

The lottery

I use a policy implemented through a lottery to causally identify the effects of one of the policies presented in Table 1, namely housing subsidies. Housing subsidies have been implemented in many cities globally, including those in low-, middle-, and high-income countries, but their effects on recipients' local-level political behavior effects remain virtually unstudied. The specific program studied here provides households with a government-constructed home at a highly subsidized price. Households can enjoy transfers even without moving; they can rent out the homes and consume the asset as a stream of payments (rental income net of mortgage) instead.[4] Such programs can be found all over the world, including in cities in Ethiopia, Kenya, Brazil, and Uruguay. They have been spearheaded in all 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, to build 20 million affordable homes by 2022.

This is a wealth transfer made through a vehicle that has been found to be associated with high rates of local-level political participation. Those who study the effects of homeownership on political participation in the US, for example, find that homeowners are more likely than non-homeowners to make claims to improve communities and protect the value of the asset (e.g. Portney 1991; Dear 1992; Fischel 2001; Einstein *et al.* 2019). Even so, it is not clear

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

[4]As such, this program is distinct from one studied by Barnhardt *et al.* (2017), which is essentially a relocation program with subsidized rent. In this program, subletting was forbidden and "failure to pay monthly rent resulted in the occupant losing the legal right to remain in the property." This program has more in common with the United States' Moving to Opportunity than the asset transfer studied here.

whether the behavior exhibited by homeowners is a function of *other* variables correlated with homeownership, and if simply facilitating homeownership for a group of non-homeowners, particularly a low-income population otherwise unlikely to become homeowners, can generate this behavior.

This study examines the effects of a program in Mumbai. The Mumbai Housing and Area Development Authority (MHADA)[5] runs subsidized home-price lotteries 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). Recipients cover the costs of marketing and construction. A number of apartments are allocated to economically weaker section (EWS) and low-income group (LIG)[6] 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 269 square foot apartment for about Rs. 1,500,000 (about 23,500 USD at the time), while the LIG group could purchase a 403 square foot apartment for about Rs. 2,700,000 (about 42,000 USD). All applications required a refundable fee of Rs. 200 (about 3 USD).

Table 2: Lottery apartments included in the study.

Scheme	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 INR=1 USD.
[3] Average sale list price of a MHADA flat of the same square footage in the same community. Data collected from magicbricks.com in 2017 and verified with occupants during fieldwork.
[4] In INR with the cost stated in the lottery year. Includes application fee of Rs. 200.

The homes were sold at 30-60% of market prices; 3-5 years after the lottery, the difference between the apartment purchase price and list price for older MHADA apartments of

[5]The agency is a subsidiary of the Maharashtra Housing and Area Development Authority that uses the same acronym.

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

the same size in the same neighborhood lies between Rs. 661,700 (about 10,300 USD at 2017 conversion rates) to Rs. 3,535,500 (about 55,000 USD).[7] At the time of purchase, a downpayment of about 1-2% was required.[8] 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. Resale of the apartments was not permitted until 10 years after purchase, but households could put the apartments up for rent. Half of the households in my sample had done so. Households did not pay taxes on their dwelling for five years after they move in, or within the time period of this study.

Households were permitted to choose the building for which they submitted an application. [9] Each apartment building had quotas for caste and occupation groups within which randomization occurred (Table SI.1). The lottery was, therefore, stratified by apartment-groups. It is likely that the lottery was fair, or truly randomized. After facing scrutiny over allegations of corruption in the 1990s and early 2000s, MHADA implemented the lottery using a protected computerized process starting in 2010. I also check for corruption through a number of randomization checks, described below.

Data collection

I estimate treatment effects for outcomes measured through in-person household surveys of both winning (treatment) and non-winning (control) households. For the 2012 and 2014 lotteries, MHADA provided phone numbers and addresses for winners and a random sample of applicants. Because there were more than 300,000 economically weaker section applicants

[7] As I observed in my fieldwork, these prices represent lower bounds on the sale prices, as the list prices refer only to the official sale prices. Neighbors of those who had sold apartments reported that actual sale prices were about 10-20% higher than the official prices, with the difference paid in cash to avoid taxes. This is a common practice in real estate throughout India.

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

[9] The apartment buildings included in the lotteries are spread across the city. Figure SI.1 in the Online Appendix shows the location of the 2012 and 2014 EWS and LIG MHADA lottery apartments and households in the sample at the time of application.

for roughly 900 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 stratum, and I accessed a total of 1,862 addresses. The sampling procedure allowed for the possibility of the same household applying for multiple lotteries. If a household won lottery A but was also drawn in the sample of non-winners for lottery B, its data would have been included as a set of outcomes under treatment for lottery A and under control for lottery B. Ultimately, no household was drawn more than once, reflecting the fact that being sampled from the pool of applicants is a rare event.

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 for both treatment and control groups. This left 531 and 532 control and treatment households, respectively. Table SI.2 shows that even after this mapping procedure, the sample included 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 it to systematically favor treatment or control units.[10]

I randomly selected 500 of the mapped households from each treatment condition to interview. From September 2017-May 2018, I worked with a Mumbai-based organization to contact individuals in the households and conduct interviews.[11] We contacted non-winners at these addresses provided at the time of application. In cases where they had moved away, we asked neighbors for updated contact information. Winners resided at either the application-time addresses or new lottery buildings, as they were free to either inhabit their new property

[10]I expect this procedure to have favored wealthier applicants by dropping rural respondents and those living in slums. Table SI.3 does show that 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.

[11]More information about the organization can be found at http://www.pukar.org.in.

or rent it out. We thus first contacted lottery housing cooperative societies to determine which of the winners were living at the apartments. We approached owner-occupiers at the lottery apartments, and approached landlords at the application-time addresses using the procedure developed for non-winners.

In all cases, we attempted to speak to the individual who had filled out the application for the lottery home. In the case that a child had applied for the home, enumerators were instructed to speak to the family's primary earner. In my sample, 78% of respondents had filled out the application themselves. About 76% of respondents were men (77% in the treatment group, 76% in the control group).[12]

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 reasons for attrition can be found in Table SI.4. I do not see evidence of differential rates of contact for control and treated units; the p-value for the difference in proportion contacted is 0.55. I show balance on fixed or baseline characteristics among the contacted sample in Table 3. Each treatment condition includes a roughly equal proportion of those belonging to the Maratha caste group, a politically dominant group in Mumbai and Maharashtra more generally, attenuating concerns that politically favored groups may have been more likely to win. I further conduct an omnibus test by regressing the treatment indicator on the covariates (Table SI.5) and calculate a heteroscedasticity-robust Wald statistic for the hypothesis that the coefficients on all of the covariates (other than stratum dummies) are zero. The p-value for this ombnibus balance test is 0.39.[13]

[12] I do not report balance tests on the gender of the respondent because the characteristics of whoever is available to answer questions are post-treatment outcomes.

[13] I also 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. 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 stratum randomization dummies) are zero. The p-values for regressions on state and municipal ward membership are 0.35 and 0.46, respectively.

Table 3: Balance tests on household characteristics

Variable	Control mean[1]	Treatment effect[2]	se	Pr(>|t|)
Other backwards class[3]	0.157	-0.021	0.035	0.543
Scheduled caste/tribe[4]	0.106	-0.018	0.026	0.499
Maratha[5]	0.276	0.018	0.045	0.690
Muslim	0.090	0.006	0.029	0.852
Makeshift floor[6]	0.019	0.028	0.019	0.136
Makeshift roof[6]	0.027	0.001	0.018	0.945
From Mumbai	0.810	0.062	0.039	0.114
From the same ward as the apartment	0.094	0.023	0.030	0.454

[1] Means for control households.
[2] Coefficient in an OLS regression of each variable on an indicator for winning the lottery, including interactions with centered stratum indicators and HC2 errors.
[3] Non-scheduled caste groups for which there are quotas.
[4] Historically disadvantaged members of society.
[5] A dominant caste group in Mumbai and Maharashtra.
[6] Measured at time of application through recall.

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 group reveals that they should not be considered among the lowest income groups in the city (Table SI.6). They have an average of 10 years of education, and on average have roughly 50% of the household employed and earning. Most live in dwellings with permanent floors and roofs. As none of the applicants, by rule, owns housing in the state of Maharashtra, they are all living in rental housing, homes with large, or joint, 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, which was corroborated by an interview conducted with the commissioner of the Mumbai Metropolitan Regional Development Authority (Madan 2016).

Estimation

I estimate effects of winning the lottery among the contacted sample on a number of survey-based outcomes. I follow my pre-analysis plan and estimate the treatment effect β, on i respondents.[14] In the following equation, Y_i is the outcome, 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

[14]Control and treatment means for all dependent variables along with p-values from a two-tailed t-test are presented in Table SI.8.

randomization checks, and ϵ_i is an error term. Given that randomization happened within strata, I include a set of centered dummies, $S_1 ... S_l$ for each. Following Lin (2013), I allow for heterogeneous effects within the strata by interacting the centered stratum dummies with the treatment indicator:

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

I label households as "treated" if they win the lottery in the specific year for which they appear in the sample. While 8% of treated units did not purchase homes, I simply conduct an intent-to-treat (ITT) analysis. β can thus be interpreted as a weighted average of stratum-specific ITT effects. I compute heteroskedasticity-consistent standard errors using the HC2 estimator. Given the large number of hypotheses being tested, I make Benjamini-Hochberg corrections for the false discovery rate within "families" of outcomes.

Note that this paper estimates ITT effects across both owner-occupiers and landlords. It is not possible to estimate effects for owner-occupier or landlord subgroups, as we do not know which members of the control group would have chosen to move if they had won. I do, however, present outcome means for owner-occupiers and landlords to clarify mechanisms.

Results: claim-making, knowledge, and preferences

I use the household surveys to measure reported behavior and attitudes 3-5 years after the lottery was held. Variable definitions are available in Table SI.7. All of the 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.

Figure 1 displays the treatment effect estimates. I first asked how often respondents participate in both individual and group petitioning of politicians and bureaucrats for something benefitting the community. An effect on claims made individually would indicate an effect on one's willingness to approach officials alone, regardless of the behavior of others in the community. An effect on claims made as part of a group would indicate an effect on

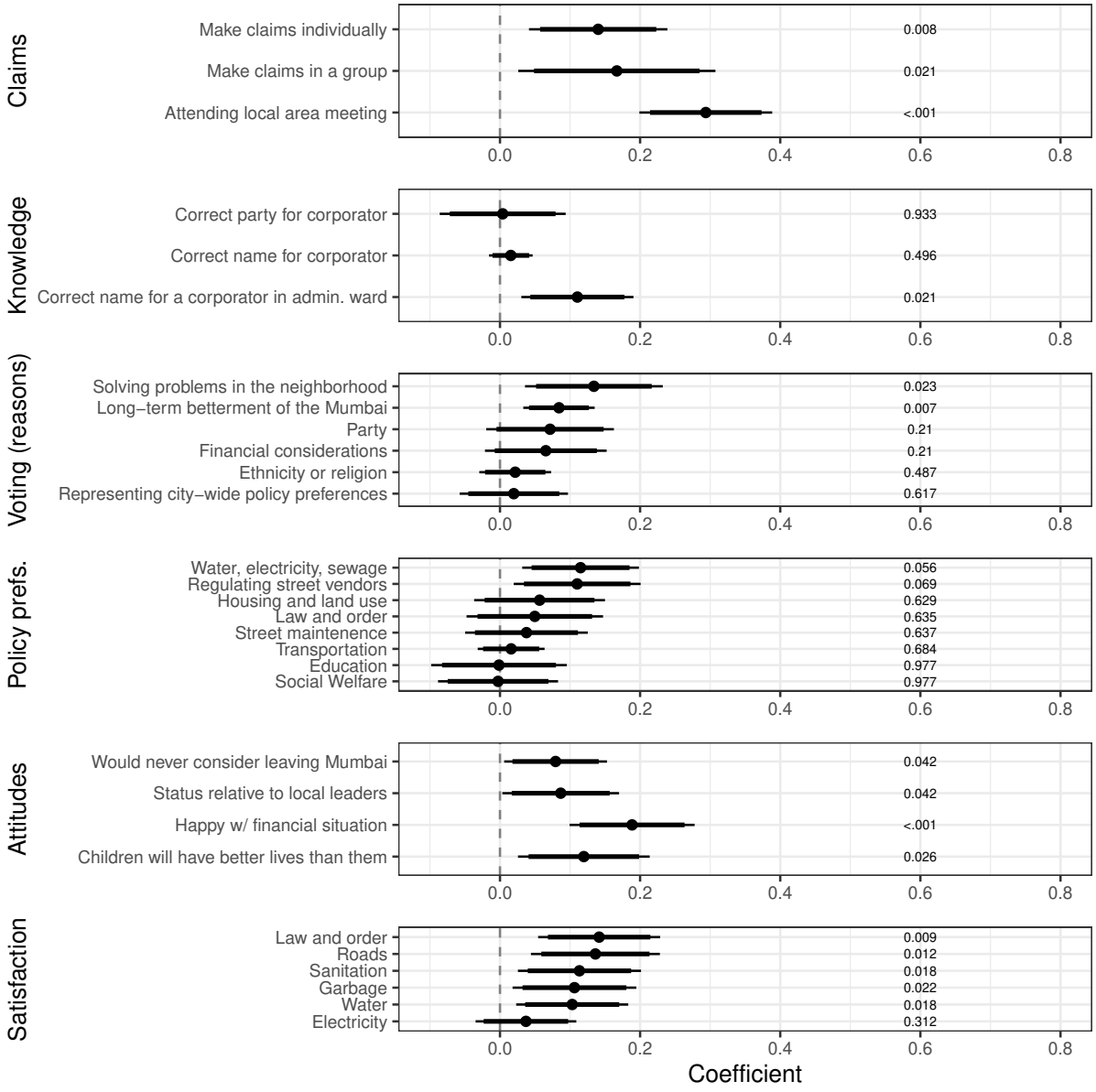
one's interest in participating in community-based collective action. I find that winning increases both. Lottery winners are 14 and 16 pp. (over control group means of 45% and 41%) more likely to report making claims individually and in groups, respectively, for "something" benefitting their communities. During qualitative interviews, I found that these claims were often related to problems with water scarcity and encroachment by street vendors.

To better learn how respondents come together as groups, I also measure the extent to which they report attending local area meetings to discuss improving their communities with their neighbors. These meetings are similar to the local development meetings described by Auerbach (2016). The range of issues being discussed is enormous and includes water supply, sidewalk construction, water leakages in apartment buildings, local safety, and the occasional birthday party. During the time of the survey, these meetings were much preoccupied with discussions surrounding the Mumbai Draft Development Plan, or a document outlining MCGM's plan for land use in the city. Winners are about 29 pp. (over a control group mean of 36%) more likely than non-winners to report that someone in the household has attended a local area improvement meeting in the last month. This effect is substantively large, but the smaller effect on group-based claim-making suggests that local meeting attendance only sometimes translates into claims.

In addition to measuring changes in reported behavior, I also test respondents' knowledge of local politics, with the assumption that greater local political engagement leads to greater knowledge. An individual who has asked a politician for community improvements is more likely to know the name of the politician than one who has not. 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 of the corporator for the electoral ward in which they lived at the time of the survey. I placed households in wards using baseline addresses for non-winners and winning landlords and using lottery apartment addresses for winning owner occupiers.[15]

[15]Ward maps were provided by the Urban Design Research Institute (http://www.udri.org).

Figure 1: The effects of EWS and LIG groups winning the MHADA housing lottery in Mumbai in 2012 and 2014.



Bars show 90% and 95% confidence intervals. P-values using Benjamini-Hochberg corrections for the false discovery rate are shown on the right. Variable definitions and control means for the main outcomes of interest are reported in Table SI.7. Full regression output with and without covariate adjustment available in Tables SI.9-SI.17.

In Mumbai, electoral wards are grouped into 24 larger administrative wards of about 6-14 adjacent electoral wards. Knowledge about electoral wards is low; only about 2% of the control group can name the relevant corporator correctly. As seen in Figure 1, I do not detect treatment effects for knowing the name or party of the corporator for the ward in

which respondents live.

Yet is the administrative ward office, not the electoral ward office, that is responsible for handling claims. For most Mumbai residents, the term "ward" refers to administrative wards, not electoral wards.[16] 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. Winners are 11 pp., or almost 80% more likely than the control group, to be able to provide a correct response. These effects are striking as outcomes were measured a mere six months after the municipal elections in February 2017, suggesting that beneficiaries actively seek up-to-date information about local government.

Finally, I measure changes in citizens' policy preferences with respect to local government. I asked respondents what they believed to be the MCGM's most important duties. I did not directly ask about the content of claims in order to be able to measure the opinions of those who did not make claims as well.[17] I find an 11.5 pp. increase in responses that the MCGM should prioritize improvements to water, sanitation, or electricity services, and an 11 pp. increase in responses mentioning the importance of regulating street vendors. These effects are in line with my observations that most claims by winners were about either intermittent public services or attempts to remove street vendors from outside their residences. Throughout Mumbai, I found that many complained that local fruit and vegetable vendors would crowd streets and irritate passersby. I measure no effects on responses mentioning social welfare or housing and land use (a category including slum clearance).[18]

Because this program allows households to rent out homes and benefit from the program without relocating, it is possible to observe whether those who do and do not relocate exhibit

[16] I asked 15 individuals on the street in different administrative wards about their ward membership. Four did not know which ward they belonged to, and eleven gave the names of their administrative wards.

[17] Respondents were not prompted with options and all of their responses were selected by enumerators from a multiple-choice list that was created during piloting.

[18] The knowledge and policy preferences outcomes, unlike the claim-making outcomes, ask about the respondent as opposed to anyone in the household. Characteristics of the respondent, particularly gender, may affect results here. As noted earlier, however, both treatment and control groups have equal proportions of male and female respondents. Tables SI.18 and SI.19 further show that I do not measure different treatment effects conditional on gender for these outcomes.

gains to participation in claim-making when compared to the control group. While this comparison suffers from the problem of selection into landlord or owner-occupier groups, it does provide an idea of how likely it is that effects are driven solely by either groups. Table 4 shows that outcomes for landlords and owner occupiers are similar, especially when compared to the control group. The behavior among owner-occupiers is surprising because they have relocated and may have been removed from their social networks, a phenomenon Gay (2012) finds leads to decreased political participation among beneficiaries of the Moving to Opportunity program in the US. Furthermore, means for owner-occupiers and landlords are fairly similar for reported policy preferences.

Table 4: Mean outcomes for landlords, owner-occupiers, and the control group.

	Landlords	Owner-occupiers	Control group
Individual claims	0.61	0.52	0.45
Group claims	0.54	0.53	0.41
Meeting attendance	0.59	0.64	0.36
Can name corporator in admin. ward	0.29	0.25	0.15
Policy preferences: water/electricity/sanitation	0.81	0.82	0.72
Policy preferences: regulating street vendors	0.39	0.31	0.26

Mechanisms: capacity, efficacy, expectations, and motivations

Government transfers might lead to increased claim-making by increasing recipients' political capacity. They may do so by making recipients feel wealthier and extending their time horizons. I estimate that winners are 19 pp. (over a base of 63%) 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 pp. more likely than non-winners to say "yes" when asked if their children will have better lives than them. They are about 8 pp. more likely than non-winners to respond that they "would never leave" when asked if would ever consider relocating from Mumbai, suggesting longer time horizons. These findings are complementary to research (see Haushofer and Fehr 2014 for a review) that has found that income shocks can increase psychological well-being, happiness, and time horizons. These effects may reduce the cognitive cost of action. 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."

The program may also improve a beneficiary's sense of her own political efficacy. Gains to wealth might improve a beneficiary's perception of her own status. I estimate an 8.7 percentage point (over a base of 19%) 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 status relative to local officials, and the potential effectiveness of their claim-making.[19] These effects are complementary to beneficiaries' near universal claim in qualitative interviews that they "now have some standing in society."

It is likely that over time, past experiences with local government further increase one's knowledge of how to make claims. I observed this when a winner, a young mother, walked me through exactly how she lodged a complaint with her local ward representative. "It's very simple," she said. "You write a letter, and I know how to word them now." She also noted the importance of using some kind of letterhead. "It needs to look official, no matter who it's coming from. It's what makes them listen." This anecdote illustrates how one's status can justify making a claim, and repeated action over time increases knowledge of how to make claims. Together, these two factors can create a sense of self-efficacy that increases the perceived utility of action. Effects on knowledge reported as results are thus also evidence for a mechanism facilitating future action.

The housing program may also generate new expectations of what government can or will provide, thereby further increasing the expected utility of action. Figure 1 shows no change in policy preferences surrounding housing or social welfare, both of which the intervention

[19]This survey question was originally phrased as "Do leaders in the area need to listen to what people like you say," but I inverted the phrasing because during survey piloting, non-winners frequently described being bullied or intimidated at ward offices. 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.

provide. Given that this is a long-running and highly visible program for which many in both the control and treatment group have applied many times, it is unlikely that winning should increase expectations of government capacity. Furthermore, the eligibility of both control and treatment group members for the program alongside its randomized allocation leads me to expect treatment group members not to have updated beliefs about the universality of welfare provision. During my qualitative interviews, I found that both the control and treatment group respondents felt that the program was distributed equitably and fairly, if scarcely.

After winning, however, beneficiaries receive not only subsidized housing but also improvements to other services adjacent to housing, such as water provision or street cleaning, that may shape expectations and motivate claim-making. When comparing these responses to those about policy preferences, I do see that winners are more likely to prefer that the MCGM focus on improving water, electricity, and sanitation, a subset of the services with which their satisfaction has increased. Yet I do not see evidence for changing policy preferences around roads or law and order, even though winners are more satisfied with these services as well. This selective preference for certain services among those with which winners are satisfied suggests that it is not just improved expectations, but some specific motivation, that is driving reported policy preferences and, therefore, claim-making.

I find that program beneficiaries are motivated to make claims to improve the services around their immediate communities. Most obviously, the changes in policy preferences surround local neighborhood-specific services. I also show effects on stated motivations for another form of local political participation, namely voting in local elections (Figure 1).[20] I asked respondents how they made their choices in the most recent municipal election. Respondents were not prompted with options and all of their responses were selected by enumerators from a multiple-choice list. Relative to non-winners, I estimate that winners are about 13 pp. more likely to state neighborhood problems as a reported reason for voting,

[20] I do not detect a treatment effect for reported voting in past municipal or state elections (Figure SI.2).

and 8 pp. more likely to report improving Mumbai as a reason for voting, thus supporting increased interest in local problems as a mechanism for my findings.[21]

The behavior of landlords further illustrates their motivation to protect the value of the lottery apartments. 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.[22]

I asked landlords if they had attended local area improvement meetings in the neighborhood of the lottery home (as opposed to where the live) in the past month. Fifty-five report that they did so "Often" or "Sometimes," a figure similar to their rates of reported attendance at voluntary meetings in the neighborhoods in which they live (59%, see Table 4), and only slightly lower than the 65% attendance rate reported by owner-occupiers. Meeting attendance in the lottery home neighborhoods is surprising, as going to these meetings can be costly in terms of time; 68% of the landlords work six or more days a week, and the travel time (one way via transit) to the lottery building neighborhoods takes 1.1 hours on average.[23] These figures may actually underestimate participation because some landlords also communicate their wishes through WhatsApp or by phone.

In addition to increased political capacities, evidence from qualitative interviews suggests that landlords' participation in claim-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 explained 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 explained.

[21]Those who did not vote are simply assumed to have found none of the listed reasons important enough.

[22]Winners are aware of their property values and potential for increases: 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.

[23]Travel times are calculated using for Sunday mornings (when meetings usually occur) using the Google Maps API and households' addresses at the time of application.

Alternative explanations

Here, I consider alternative explanations for effects that are unique to this specific implementation of the program or data collection. But given the size of transfer, it is possible that there are other mechanisms through which home price subsidies affect claim-making more generally, and it is beyond the scope of this paper to provide evidence for each and every one.

The claim-making here could refer not to new demands, but rather actions winners need to take to obtain the apartments. These outcomes, however, are measured 3-5 years after individuals win the lottery. They do not reflect one-time actions, but rather longer-term changes to behavior. It is possible that these initial interactions with government themselves generated a sense of self-efficacy and government efficacy. This is a mechanism that will apply to all types of government transfers and is worthy of further exploration in future studies.

It is unlikely that effects are driven by a demand for services in exchange for a property tax, as the program does not require winners to pay property taxes within the time period of the study. Increased spending could increase expenditure on consumption taxes. Demands for better services as a result of greater exposure to indirect consumption taxes is an additional mechanism to explore for all welfare programs more generally. Unlike a property tax, an increase in consumption taxes would theoretically induced demands for better services across the board. In this case, the selective change in policy preferences makes it unlikely that general consumption taxes are the sole motivation for action. The intervention, unlike land-titling programs for slum-dwellers, also does not generate a shock to property rights, as the target group itself is lower-middle class and tends to have secure property rights.

It is 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. Yet the program is not seen as politically allocated but rather truly a lottery; about 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 participate in the lottery repeatedly, much like someone in the US can repeatedly buy lottery tickets or put quarters into a slot machine. For this reason, I found that non-winners were generally accustomed to not winning the lottery; it was the winners who were surprised by their good fortune. "I never expect anything when I fill out the form," reported an elderly control group respondent who had participated for years. Indian citizens are disappointed by the state in so many ways each week; not succeeding at an extremely low probability event is unlikely to suddenly change their beliefs about the state, and, therefore, their behavior. Haushofer *et al.* (2015) do find that Kenyan cash transfer beneficiaries' neighbors exhibited decreased life satisfaction, an effect they attribute to the idea that individual utility is negatively affected by the income and consumption of others. This phenomenon is unlikely to be driving effects in this study, however, as only 5% of control group respondents reported knowing, even indirectly through someone else, anyone who had ever won the housing lottery.

Increased participation may be driven by relocation and the presence of informal institutions for, or norms surrounding, claim-making in the lottery apartments. Recall, however, that outcomes are measured for the areas in which respondents live; similar rates of claim-making among landlords and owners (Table 4) make this alternative explanation less likely. Meeting participation among owner-occupiers is slightly higher than landlords, but the difference in meeting attendance between landlords and the control group is much higher still, indicating that that the availability of meetings at lottery apartments is not solely responsible for the results. I actually observed that local area improvement meetings were ubiquitous in the control group neighborhoods I visited. In fact, the potential to separate out these mechanisms is an important feature of the study design; in rent subsidy programs, such as those studied by Barnhardt *et al.* (2017), relocation is bundled with the program's financial benefit, making it difficult to separate effects of the apartment locations, which are specific to the instance of the intervention studied, from any other more generalizable effects of the intervention.

The results may also arise from dissatisfaction with service delivery in the new apartment locations. Owner-occupiers experiencing worse services in the new buildings could organize to demand improvements in their new communities. But as discussed above, I generally observe that winners are more satisfied with service delivery than non-winners.

The effects of other policies

To what extent should there exist similar effects for other types of policies? The strength of different mechanisms will vary by context, recipient type, and transfer. It is likely that receiving *any* type of welfare transfer will change one's expectations of the state. Hern (2017), for example, argues that welfare may shape expectations of government efficacy and will to provide even when it is poorly or barely delivered. This mechanism will be important in areas with low levels of state capacity and among underserved citizens. It is possibly for this reason that changing expectations of the state have been explored in other LMIC-based studies of policy feedback. In contrast, the capacity, efficacy, and motivation mechanisms might affect a broader set of recipients, but would only arise in the case of effectively delivered transfers of a certain size.

We might see changes in motivations and policy preferences particularly in the case of programs entailing the sustained use or sustained delivery of benefits that recipients may aim to protect. The motivation mechanism would be particularly relevant to policies entailing the sustained use or sustained delivery of benefits that recipients may aim to protect. 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.

Scope conditions will apply to even these programs that entail the sustained use or delivery of benefits. As described by Olson (1965), the extent to which participation in local collective demand-making is inhibited by free-riding may 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. Finally, beneficiaries must believe that benefits cannot be easily taken away. This belief could arise from property rights, as in the case of the lottery homes, or through government rhetoric surrounding the permanence of a program. The manner of program implementation (whether it is programmatic or clientelistic) is likely to affect these expectations. As shown by Field's (2005) study in which land titling incentivizes informal dwellers to improve their homes, individuals are more likely to improve the value of something they believe they "own." Furthermore, these rights can provide the moral and legal basis for making claims (O'Brien 1996; Jenkins and Manor 2017).

Many welfare transfers including, but not limited to, home price subsidies can thus be considered to be asset shocks that recipients will seek to protect. 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). In line with the capacity and motivation mechanisms I propose, Jenkins and Manor (2017, 166-181) 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.

Conclusion

I exploit subsidized home-price lotteries in Mumbai and show that benefitting from this large intervention changes policy preferences and leads individuals to increase their reported participation in claim-making and knowledge of local government. I illustrate multiple possible mechanisms for these effects, including increased political capacity due to beneficiaries' newfound wealth, an increased sense of self-efficacy, and greater expectations of government. I further argue that this claim-making is motivated by a specific desire to protect the new wealth. Other wealth and income transfers that effectively boost beneficiaries' political capacities and change their motivations possibly have similar effects.

The study suggests that it may not always be politically attractive to implement programs to subsidize homeownership. Alan and Ward (1985, 5-6) argue that subsidized housing is appealing to officials for three main reasons: it provides visual evidence that the government is providing for the poor, construction creates jobs, and it provides homes for government supporters and officials. But if such policies can also increase citizen engagement, then they may also increase officials' administrative burden at the local level and be seen as undesirable. Nevertheless, politicians do implement such programs at scale, suggesting that the political benefits often outweigh the costs. The conditions under which program implementation becomes more or less attractive to officials, such as electoral competitiveness, state capacity, or party control, is an important avenue for future research.

The behavior observed among lottery winners is similar to that described by research on homeowners in the US who participate in local to politics to defend their property values (e.g. Portney 1991; Dear 1992; Fischel 2001; Einstein *et al.* 2019). This US-based literature focuses on a "not-in-my-backyard" (NIMBY) behavior, or the negative externalities of homeownership. Owners defect from land use policies that are of general benefit to a municipality because they impose costs on the very local communities in which individuals own homes. In the context described in this study, homeownership and a desire to protect

property values potentially has *positive* externalities in communities with mixed housing tenure and low levels of baseline service provision.

Nevertheless, homeownership can have negative NIMBY-type externalities in urban India as well. While the beneficiaries in this study did not exhibit changes in policy preferences surrounding housing, slums, and land use, they did report an increased preference for "regulating" (i.e. removing) neighborhood street vendors for want of space and quiet on the streets. Many have documented the urban middle class's attempts to "beautify" cities; such actions likely share the same underpinnings as NIMBYism in that they benefit homeowners at the expense of others in the city (Fernandes 2006). Understanding NIMBYism in LMICs and, given the stratification of society in India, its intersection with the politics of class and religion, is a fruitful area for future research.

The relative power of homeowners in local-level politics in the US suggests that in the long term, government transfers to subsidize homeownership may have political distributional effects by amplifying the voices of beneficiaries. Furthermore, because households must be able to purchase the unsubsidized portion of the apartment, home-price subsidies may benefit middle-class households over their poorer counterparts, a pattern visible in the policy studied here and mortgage subsidies in the US (Glaeser and Shapiro 2003). Program targeting can thus exacerbate patterns of political inequality due to the transfers' effects on claim-making. This is likely a problem with other welfare programs and may be even worse for transfers not distributed through programmatic rules.

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Appendices

For online publication

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

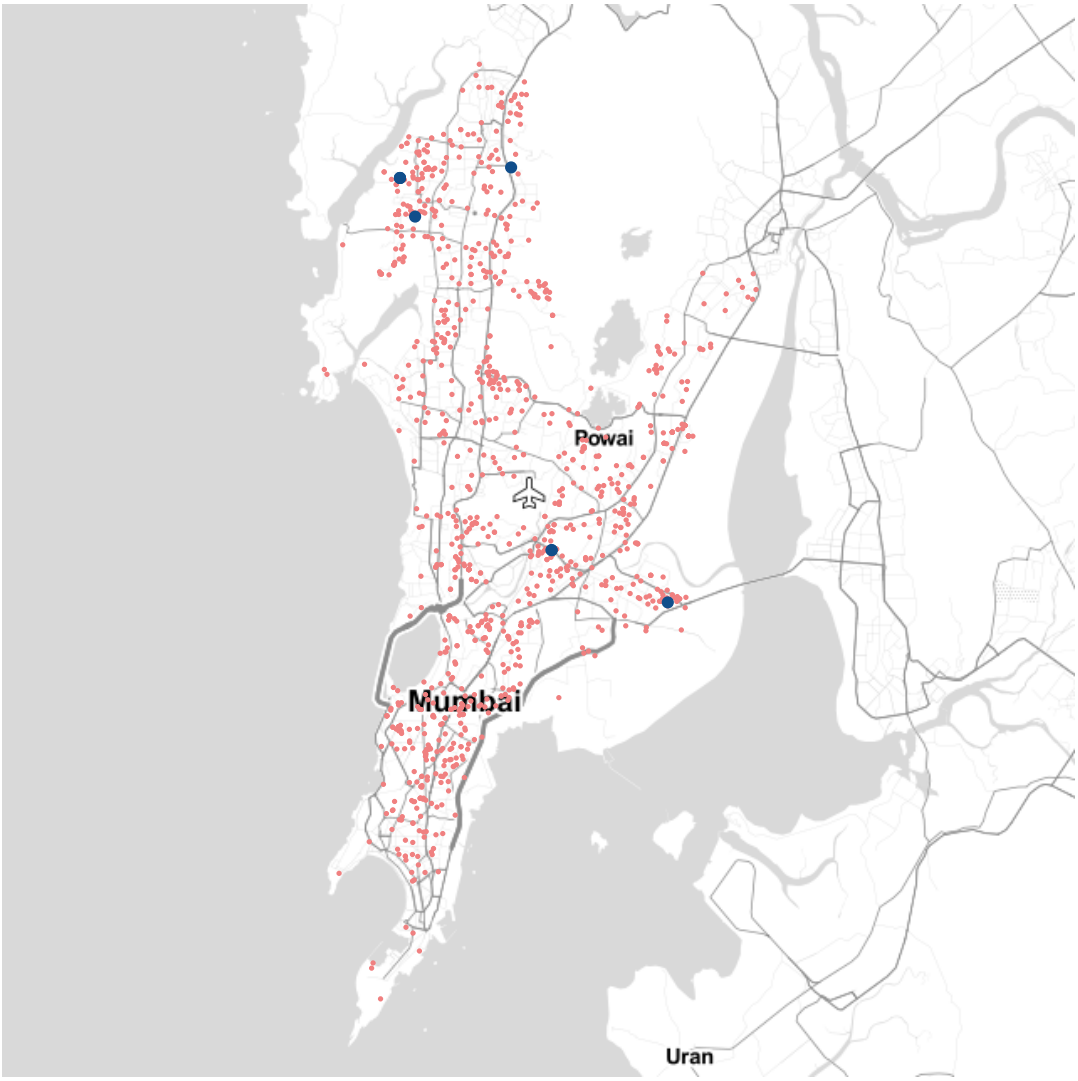
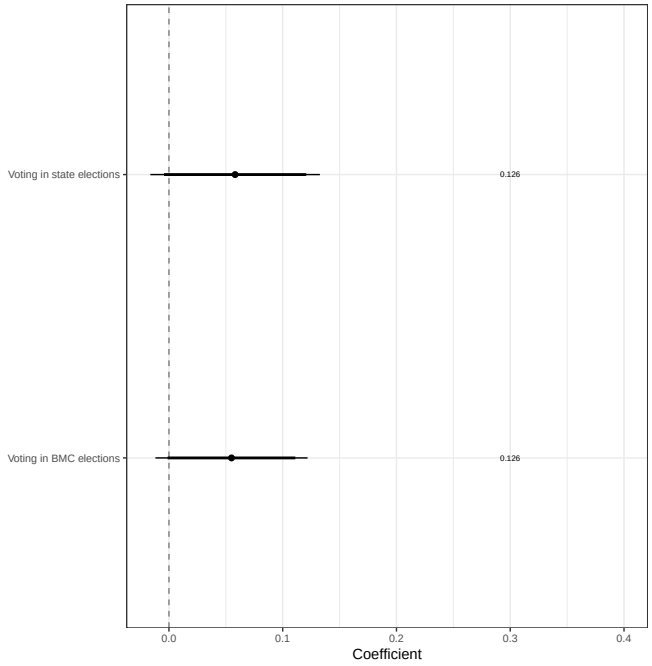


Figure SI.2: Treatment effects for responding "Yes" to "Did you vote in the last MCGM (municipal) or state elections?"



Bars show 90% and 95% confidence intervals. P-values using the Benjamini-Hochberg corrections for the false discovery rate are shown on the right.

Table SI.1: Caste/occupation category codes

Code	Category
AR	Artist
CG	Central govt. servant occupying staff qrts.
DF	Families of defense personall
DT	Denotified tribes
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 SI.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 SI.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	0.726
CG	0.020	0.020	0.900
DF	0.022	0.012	0.052
DT	0.014	0.009	0.256
EX	0.052	0.023	0
FF	0.028	0.010	0
GP	0.523	0.596	0
JR	0.028	0.026	0.795
ME	0.017	0.015	0.735
MP/MLA/MLC	0.004	0.005	0.877
NT	0.014	0.015	0.816
PH	0.026	0.026	0.931
SC	0.116	0.130	0.282
SG	0.053	0.055	0.878
ST	0.062	0.034	0
	1.00	**1.00**	
Lottery income category			
EWS	0.309	0.306	0.839
LIG	0.691	0.694	0.839
	1.00	**1.00**	
Apartment building #			
274	0.015	0.014	0.840
275	0.015	0.017	0.697
276	0.015	0.017	0.697
283	0.290	0.299	0.602
284	0.140	0.139	0.976
302	0.244	0.241	0.856
303	0.216	0.208	0.610
305	0.066	0.065	0.949
	1.00	**1.00**	

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

	Control	Treatment	p
Surveyed	413	421	0.55
Address not found	9	7	0.80
Home demolished	1	0	1.0
Home locked	5	11	0.21
Respondent deceased	1	0	.01
Refused	14	20	0.38
Unable to locate household that has moved	19	10	0.13
Incomplete survey	37	31	0.53
Total	**500**	**500**	-

Table SI.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] All covariates are dummy variables.

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

Table SI.6: Summary of control group characteristics.

Variable	Control mean (SD)
Household Assets	
TV	0.91 (0.29)
Computer	0.39 (0.49)
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)
Permanent roof	0.79 (0.41)
Private tap	0.74 (0.44)
Private latrine	0.63 (0.49)
Education and labor	
Percentage of the household employed	0.48 (0.25)
Years of education (HH mean)	10.35 (2.87)
Tenure status	
Migrants	0.20 (0.40)
Renting	0.75 (0.43)
Sharing/live in a joint family	0.78 (0.41)

Table SI.7: Variable definitions.

Label	Survey question	Response options	Coding
Attending local area meeting	In the last month, has anybody in the HH attended a local area meeting in the neighborhood in which you live?	Yes, No, Don't Know	1 if Yes, else 0
Make claims individually	How often do you individually petition government officials and political leaders for something benefitting the community in which you live?	Often, Sometimes, Rarely, Never	1 if Often or Sometimes, else 0
Make claims in group	How often do you participate in group petitions of government officials and political leaders for something benefitting the community in which you live?	Often, Sometimes, Rarely, Never	1 if Often or Sometimes, else 0
Correct party for corporator	What is the party of your *nagar sevak*?	Open ended	1 if correct, else 0
Correct name for corporator	What is the name of your *nagar sevak*?	Open ended	1 if correct, else 0
Correct name for corporator in admin. ward	What is the name of your *nagar sevak*?	Open ended	1 if named individual is a corporator in admin ward, else 0
Policy preferences	What do you think are the BMC/MCGM's most important duties?	Open ended, enumerators selected all applicable from list	1 if [duty] chosen, else 0
Would never consider leaving Mumbai	Do you think you will leave Mumbai in the future?	Would never leave, Might leave in future, Will definitely leave	1 if Would never leave , else 0
Happy with financial situation	How happy are you with the financial situation of your household?	Happy, Neither happy nor unhappy, Unhappy	1 if Happy, else 0
Status relative to local leaders	Do you/people like you need to listen to what leaders in the area say?	Yes, No, Don't know	1 if No, else 0
Children will have better lives than them	Do you expect your children to have better lives than you?	Yes, No, Don't Know	1 if Yes, else 0
Reasons for voting	What factors did you consider when you voted in the last (2017) municipal elections?	Open ended, enumerators selected all applicable from list	1 if [reason] chosen, else 0
Satisfaction with services	How satisfied are you with the [service type] in the neighborhood in which you live?	Satisfied, Neither satisfied nor dissatisfied, Dissatisfied	1 if Satisfied, else 0

Table SI.8: Control and treatment means for all dependent variables with p-value for two-tailed t-test.

Variable	Control	Treatment	p
Make claims individually	0.448	0.565	0.001
Make claims in a group	0.412	0.537	0.000
Attending local area meeting	0.360	0.618	0.00
Correct party for corporator	0.305	0.276	0.348
Correct name for corporator	0.019	0.036	0.151
Correct name for corporator in admin. ward	0.145	0.271	0.00
Policy prefs: social welfare	0.738	0.753	0.632
Policy prefs: street maintenence	0.676	0.760	0.007
Policy prefs: water/electricity/sanitation	0.722	0.817	0.001
Policy prefs: law/order	0.419	0.501	0.017
Policy prefs: street vendors	0.262	0.352	0.005
Policy prefs: housing/land use	0.344	0.337	0.842
Policy prefs: education	0.426	0.451	0.465
Policy prefs: transportation	0.102	0.067	0.515
Happy w/ financial situation	0.630	0.770	0.00
Children have better lives	0.559	0.686	0.00
Would never leave	0.772	0.862	0.001
Under control of local leaders	0.194	0.295	0.001
Reasons for voting: party	0.303	0.342	0.224
Reasons for voting: ethnicity	0.063	0.083	0.263
Reasons for voting: neighborhood	0.414	0.492	0.024
Reasons for voting: financial issues	0.257	0.268	0.700
Reasons for voting: city-wide policy preferences	0.203	0.166	0.168
Reasons for voting: generally improving city	0.051	0.090	0.026
Satisfaction: electricity	0.826	0.864	0.197
Satisfaction: garbage	0.709	0.770	0.076
Satisfaction: sanitation	0.676	0.761	0.009
Satisfaction: water	0.772	0.818	0.146
Satisfaction: law/order	0.697	0.775	0.019
Satisfaction: roads	0.639	0.727	0.011

Table SI.9: Regression estimates for treatment effects reported participation in local claim-making. All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:					
	Individual complaint making		Group complaint making		Attending local area meetings	
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.144***	0.142***	0.160***	0.158***	0.300***	0.293***
	(0.050)	(0.050)	(0.050)	(0.050)	(0.048)	(0.048)
OBC		0.038		0.066		0.049
		(0.058)		(0.058)		(0.056)
SCST		0.077		0.092		0.067
		(0.075)		(0.074)		(0.072)
Maratha		0.015		0.045		0.037
		(0.047)		(0.047)		(0.045)
Muslim		0.034		−0.033		0.037
		(0.068)		(0.068)		(0.066)
Kutcha floor		−0.036		−0.001		0.055
		(0.125)		(0.124)		(0.120)
Kutcha roof		−0.230*		−0.249*		−0.257**
		(0.130)		(0.129)		(0.125)
From Mumbai		0.096*		0.091*		0.093**
		(0.049)		(0.049)		(0.047)
From same ward as apt		−0.027		−0.019		0.066
		(0.063)		(0.062)		(0.060)
Constant	0.436***	0.351***	0.396***	0.305***	0.338***	0.238***
	(0.033)	(0.057)	(0.033)	(0.056)	(0.032)	(0.054)
Observations	834	834	834	834	834	834
R^2	0.169	0.185	0.173	0.191	0.233	0.246
Adjusted R^2	0.013	0.020	0.017	0.028	0.088	0.094
Note:						*p<0.1; **p<0.05; ***p<0.01

Table SI.10: Regression estimates for treatment effects on knowledge of local politics. All regressions include treatment indicator interactions with mean-centered block dummies.

	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 SI.11: Regression estimates for treatment effects on beliefs about MCGM's most important duties (no covariates). All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:							
	Welfare	Streets	Water/Elec/San	Law	Vendors	Housing	Education	Transportation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	−0.012	0.045	0.111***	0.058	0.112**	0.048	−0.001	0.015
	(0.044)	(0.045)	(0.042)	(0.050)	(0.046)	(0.048)	(0.049)	(0.024)
Constant	0.758***	0.700***	0.715***	0.433***	0.241***	0.306***	0.443***	0.051***
	(0.029)	(0.030)	(0.028)	(0.033)	(0.030)	(0.031)	(0.033)	(0.016)
Observations	834	834	834	834	834	834	834	834
R^2	0.169	0.167	0.170	0.176	0.175	0.169	0.181	0.146
Adjusted R^2	0.013	0.010	0.013	0.020	0.019	0.013	0.027	−0.015
Note:								*p<0.1; **p<0.05; ***p<0.01

Table SI.12: Regression estimates for treatment effects on beliefs about MCGM's most important duties (with covariates). All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:							
	Welfare	Streets	Water/Elec/San	Law	Vendors	Housing	Education	Transportation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	−0.003	0.038	0.115***	0.050	0.110**	0.056	−0.001	0.016
	(0.044)	(0.045)	(0.042)	(0.050)	(0.046)	(0.048)	(0.049)	(0.024)
OBC	0.074	−0.009	0.018	−0.046	0.006	−0.040	0.091	0.031
	(0.051)	(0.052)	(0.049)	(0.058)	(0.054)	(0.055)	(0.057)	(0.028)
SCST	0.052	0.103	0.034	−0.028	−0.072	−0.085	0.061	−0.088**
	(0.065)	(0.067)	(0.063)	(0.074)	(0.069)	(0.071)	(0.074)	(0.036)
Maratha	−0.016	0.017	0.017	0.082*	0.082*	−0.030	0.117**	0.025
	(0.041)	(0.042)	(0.040)	(0.047)	(0.043)	(0.045)	(0.046)	(0.023)
Muslim	−0.021	0.158***	−0.032	−0.009	−0.021	−0.009	0.115*	−0.012
	(0.059)	(0.061)	(0.058)	(0.068)	(0.063)	(0.065)	(0.067)	(0.033)
Kutcha floor	−0.030	−0.045	−0.047	−0.051	0.017	−0.189	0.179	0.015
	(0.108)	(0.111)	(0.105)	(0.123)	(0.115)	(0.118)	(0.123)	(0.060)
Kutcha roof	−0.002	−0.138	−0.151	−0.011	−0.108	−0.136	−0.272**	−0.056
	(0.113)	(0.116)	(0.110)	(0.129)	(0.119)	(0.123)	(0.128)	(0.063)
From Mumbai	−0.118***	0.171***	−0.013	0.137***	0.024	−0.059	−0.069	−0.045*
	(0.043)	(0.044)	(0.041)	(0.049)	(0.045)	(0.047)	(0.048)	(0.024)
From same ward as apt	0.078	−0.078	−0.018	−0.078	−0.090	−0.033	−0.003	−0.001
	(0.055)	(0.056)	(0.053)	(0.062)	(0.058)	(0.060)	(0.062)	(0.030)
Constant	0.831***	0.553***	0.724***	0.326***	0.220***	0.386***	0.437***	0.085***
	(0.049)	(0.050)	(0.048)	(0.056)	(0.052)	(0.054)	(0.056)	(0.027)
Observations	834	834	834	834	834	834	834	834
R^2	0.186	0.202	0.177	0.196	0.189	0.183	0.198	0.164
Adjusted R^2	0.021	0.041	0.010	0.034	0.025	0.018	0.037	−0.005
Note:								*p<0.1; **p<0.05; ***p<0.01

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

	Dependent variable:							
	Happy w/ finances		Children have better lives		Never leave Mumbai		Don't listen to 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 SI.14: Regression estimates for treatment effects for reported reasons for voting in the last municipal election (without covariates). All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:					
	Party	Ethnicity/Religion	Neighborhood problems	Financial problems	Policy prefs	Improving Mumbai
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.067	0.024	0.134***	0.068	0.019	0.081***
	(0.047)	(0.026)	(0.050)	(0.044)	(0.039)	(0.026)
Constant	0.288***	0.063***	0.392***	0.227***	0.166***	0.028
	(0.031)	(0.017)	(0.033)	(0.029)	(0.026)	(0.017)
Observations	834	834	834	834	834	834
R^2	0.173	0.175	0.156	0.161	0.150	0.161
Adjusted R^2	0.017	0.020	−0.003	0.002	−0.010	0.002
Note:						*p<0.1; **p<0.05; ***p<0.01

Table SI.15: Regression estimates for treatment effects for reported reasons for voting in the last municipal election (with covariates). All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:					
	Party	Ethnicity/Religion	Neighborhood problems	Financial problems	Policy prefs	Improving Mumbai
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.071	0.022	0.134***	0.065	0.020	0.084***
	(0.046)	(0.026)	(0.050)	(0.044)	(0.039)	(0.026)
OBC	−0.015	−0.023	0.047	−0.079	−0.018	−0.0002
	(0.054)	(0.030)	(0.058)	(0.051)	(0.046)	(0.030)
SCST	0.036	0.040	0.104	−0.056	−0.172***	−0.045
	(0.069)	(0.039)	(0.075)	(0.066)	(0.059)	(0.039)
Maratha	−0.068	−0.013	0.101**	0.044	−0.018	−0.025
	(0.044)	(0.024)	(0.047)	(0.041)	(0.037)	(0.024)
Muslim	0.025	−0.012	0.176***	−0.066	0.038	−0.008
	(0.063)	(0.036)	(0.068)	(0.060)	(0.054)	(0.035)
Kutcha floor	0.133	−0.005	−0.097	−0.146	−0.096	−0.085
	(0.115)	(0.065)	(0.124)	(0.110)	(0.098)	(0.064)
Kutcha roof	0.026	−0.049	−0.060	0.034	0.006	−0.039
	(0.120)	(0.068)	(0.130)	(0.115)	(0.102)	(0.067)
From Mumbai	−0.144***	0.045*	0.088*	0.088**	−0.014	−0.028
	(0.045)	(0.026)	(0.049)	(0.043)	(0.039)	(0.025)
From same ward as apt	0.106*	0.022	−0.123**	−0.073	−0.016	0.026
	(0.058)	(0.033)	(0.063)	(0.055)	(0.049)	(0.032)
Constant	0.400***	0.032	0.277***	0.180***	0.203***	0.062**
	(0.052)	(0.029)	(0.057)	(0.050)	(0.044)	(0.029)
Observations	834	834	834	834	834	834
R^2	0.200	0.184	0.179	0.181	0.164	0.170
Adjusted R^2	0.039	0.019	0.013	0.015	−0.005	0.002
Residual Std. Error (df = 693)	0.459	0.258	0.495	0.437	0.389	0.256
Note:						*p<0.1; **p<0.05; ***p<0.01

Table SI.16: Regression estimates for treatment effects on reported satisfaction with various outcomes (without covariates). All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:					
	Electricity	Garbage	Sanitation	Water	Law and Order	Roads
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.038	0.106**	0.115**	0.103**	0.144***	0.144***
	(0.036)	(0.045)	(0.045)	(0.041)	(0.044)	(0.047)
Constant	0.828***	0.686***	0.664***	0.743***	0.661***	0.610***
	(0.024)	(0.029)	(0.030)	(0.027)	(0.029)	(0.031)
Observations	829	827	829	829	828	827
R^2	0.149	0.166	0.171	0.150	0.160	0.163
Adjusted R^2	−0.012	0.009	0.014	−0.011	0.0005	0.004
Note:						*p<0.1; **p<0.05; ***p<0.01

Table SI.17: Regression estimates for treatment effects on reported satisfaction with various outcomes (with covariates). All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:					
	Electricity	Garbage	Sanitation	Water	Law and Order	Roads
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.037	0.106**	0.113**	0.103**	0.142***	0.136***
	(0.037)	(0.045)	(0.045)	(0.041)	(0.044)	(0.047)
OBC	−0.020	−0.020	−0.050	−0.011	−0.047	−0.026
	(0.043)	(0.051)	(0.052)	(0.047)	(0.052)	(0.054)
SCST	−0.098*	−0.156**	−0.264***	−0.127**	−0.150**	−0.187***
	(0.055)	(0.066)	(0.067)	(0.061)	(0.066)	(0.070)
Maratha	0.026	−0.023	−0.046	0.053	−0.051	0.008
	(0.034)	(0.042)	(0.042)	(0.038)	(0.042)	(0.044)
Muslim	−0.034	−0.052	−0.129**	−0.084	−0.056	−0.064
	(0.050)	(0.060)	(0.061)	(0.056)	(0.060)	(0.064)
Kutcha floor	−0.114	−0.126	−0.154	−0.010	−0.164	−0.022
	(0.092)	(0.111)	(0.113)	(0.102)	(0.112)	(0.118)
Kutcha roof	−0.020	0.048	0.142	−0.065	0.120	0.063
	(0.096)	(0.116)	(0.117)	(0.107)	(0.117)	(0.123)
From Mumbai	0.015	−0.004	0.008	−0.040	0.078*	0.051
	(0.036)	(0.043)	(0.044)	(0.040)	(0.044)	(0.046)
From same ward as apt	0.024	0.020	0.032	−0.005	−0.041	0.058
	(0.046)	(0.056)	(0.056)	(0.051)	(0.056)	(0.059)
Constant	0.826***	0.720***	0.714***	0.784***	0.647***	0.592***
	(0.041)	(0.050)	(0.051)	(0.046)	(0.050)	(0.053)
Observations	829	827	829	829	828	827
R^2	0.159	0.175	0.195	0.166	0.174	0.176
Adjusted R^2	−0.012	0.008	0.031	−0.003	0.005	0.008
Note:						*p<0.1; **p<0.05; ***p<0.01

Table SI.18: Regression estimates for treatment effects on knowledge of local politics conditional on gender. All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:		
	Party for corporator	Name for corporator	Name for a corporator in admin. ward
	(1)	(2)	(3)
T	0.012	0.015	0.099**
	(0.049)	(0.017)	(0.044)
Female	0.075	0.040**	−0.007
	(0.058)	(0.020)	(0.051)
T × Female	−0.036	−0.006	0.063
	(0.081)	(0.028)	(0.072)
Constant	0.277***	0.011	0.149***
	(0.033)	(0.011)	(0.029)
Observations	834	834	834
R^2	0.153	0.230	0.176
Adjusted R^2	−0.010	0.082	0.018
Note:			*p<0.1; **p<0.05; ***p<0.01

Table SI.19: Regression estimates for treatment effects on beliefs about MCGM's most important duties conditional on gender. All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:							
	Welfare	Streets	Water/Elec/San	Law	Vendors	Housing	Education	Transportation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T	−0.030	0.045	0.088*	0.036	0.100**	0.047	0.015	0.013
	(0.047)	(0.048)	(0.045)	(0.053)	(0.049)	(0.051)	(0.053)	(0.026)
Female	0.013	0.067	−0.078	−0.036	0.057	0.097	0.118*	−0.004
	(0.054)	(0.056)	(0.053)	(0.062)	(0.057)	(0.059)	(0.062)	(0.030)
T × Female	0.085	−0.0001	0.107	0.097	0.058	0.008	−0.074	0.009
	(0.077)	(0.079)	(0.074)	(0.088)	(0.081)	(0.083)	(0.087)	(0.043)
Constant	0.754***	0.683***	0.733***	0.440***	0.227***	0.282***	0.415***	0.052***
	(0.031)	(0.032)	(0.030)	(0.036)	(0.033)	(0.034)	(0.035)	(0.018)
Observations	834	834	834	834	834	834	834	834
R^2	0.173	0.170	0.173	0.177	0.181	0.176	0.186	0.146
Adjusted R^2	0.015	0.011	0.014	0.019	0.024	0.018	0.030	−0.018
Note:								*p<0.1; **p<0.05; ***p<0.01