Government benefits and their effects on citizens' demands: evidence from a natural experiment in Mumbai*

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Abstract

How do government benefits affect recipients' likelihood of demanding government resources and services? Citizens' interactions with governments in developing contexts are often described as driven by a need for resources, suggesting that receiving benefits may decrease one's need to participate. Yet research on "policy feedback" in the US shows that resources from benefits increase recipients' capacity for action and motivate them to protect these resources. I study the effects of a common welfare policy, subsidizing homeownership, on demand-making in Mumbai, India. A natural experiment of multiple housing lotteries shows that winning increases reported demand-making and knowledge about local politics, even among those who rent out the homes. Mechanisms may include changes in attitudes and an increased interest in improving communities. This study shows that rather than mitigating the need for resources from local governments, welfare policies can facilitate collective demand-making activity among groups of beneficiaries.

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1 Introduction

Governments in many low- and middle-income countries (LMICs) devote nontrivial portions of their budgets to social welfare spending. Between 2000 and 2005, for example, the median spending on health and education programs among countries eligible for concessional lending from the International Monetary Fund increased from 5.19 to 6.09 percent of gross domestic product, or by roughly 12%.[1] In India, central and state governments spend on numerous policies, including pensions, electrification, employment, financial inclusion, and subsidized housing programs. These programs 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. Yet activity beyond voting, such as demands placed with politicians, bureaucrats, and brokers for goods and services, 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?

There are reasons to expect that welfare policies may decrease beneficiaries' participation in making demands. If this political participation, like voting, is understood as a means of accessing state resources, then welfare benefits may preclude 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. Once citizens receive government benefits they may simply exit the political arena.

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 demands to protect this newfound wealth. 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. Making such demands can be thought of as *collective* action taken on behalf of a group of beneficiaries.

[1] These data are part of Clements *et al.* 2013 and publicly available.

In other words, benefits can affect behavior 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 for government goods and services. 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 housing 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 winners and non-winning applicants of multiple subsidized housing 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 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, any welfare program providing a sustained stream of benefits over time can be thought of as an asset that makes beneficiaries wealthier *and* whose value is affected by the government, and may thus also generate

demand-making among beneficiaries. Benefitting from other types of programs may thus also increase demand-making. 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).

To my knowledge, these findings are among the first sets of causally identified effects of any government welfare policy on non-electoral political behavior. In terms of theory, they contribute to and depart from existing scholarship in several different ways. The positive effects on local demand-making contradict the idea that individuals participate in local politics only for the clientelistic exchange of state resources; if this were the case, the increased access to state resources would decrease the need for participation. This study thus contributes 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 results on attitudes and reasons for vote choice also illuminate new mechanisms by which programmatic policies may change the the political fortunes of implementers. The aforementioned studies of the electoral effects of programmatic policies 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, while the welfare state has primarily been associated with OECD countries, 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 Welfare spending and its effects 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). These policies include "schemes" (programs) and subsidies implemented both at the central and state levels that target different groups. Such programs affect the lives of millions. Table 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 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).

Table 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

Given that these programs 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).[2] As described in this literature, an absence of baseline service

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

provision can create opportunities for rent-seeking among those who govern allocation.[3] As a result, a natural way to think about the political effects of welfare spending is to study the electoral returns to various 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. Much of the literature on distributive politics, or the allocation of state goods and services, particularly in India and other middle- and low-income countries (see Golden and Min 2013 for a review), focuses on the outcomes of citizens' everyday interactions with the state. Scholars describe efforts to access to goods and services ranging from cash or in-kind transfers (e.g. Stokes 2005; Nichter 2008) to jobs, roads, and lighting (Auyero 2001; Chhibber and Nooruddin 2004; Jha, Rao, and Woolcock 2007; Auerbach 2016; Kruks-Wisner 2018). Beyond simply voting for those who help them, individuals negotiate with intermediaries and collectively place pressure on bureaucrats and officials to get what they need. Many of these studies examine how different types of demand-making affect or predict the likelihood accessing benefits. I look at this relationship in the other direction: how might becoming a welfare beneficiary affect participation in this action?

2.1 Local demand-making in India

Much of this everyday demand-making that I study 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 local 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)[4], 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

[3]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).

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

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.[5] I scraped the website through which one makes and tracks complaints and found that 87,395 complaints were registered in 2017.[6] As shown in Figure 1, 89.5% of these complaints were resolved, with the resolution rate approaching 100% for several categories designated by the municipality.[7] This data is supported by qualitative interviews with lottery winners who said that the municipal government was responsive to their complaints.[8]

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.[9] 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 diverse urban neighborhood development societies.

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

If political action is simply an instrumental exchange for state-provided resources, then there are reasons to expect that becoming a welfare beneficiary may *decrease* participation in local demand-making. Several programs, first of all, themselves provide services to beneficiaries, thereby precluding the need for action. In Mexico, for example, Larreguy *et al.* 2015 find that

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

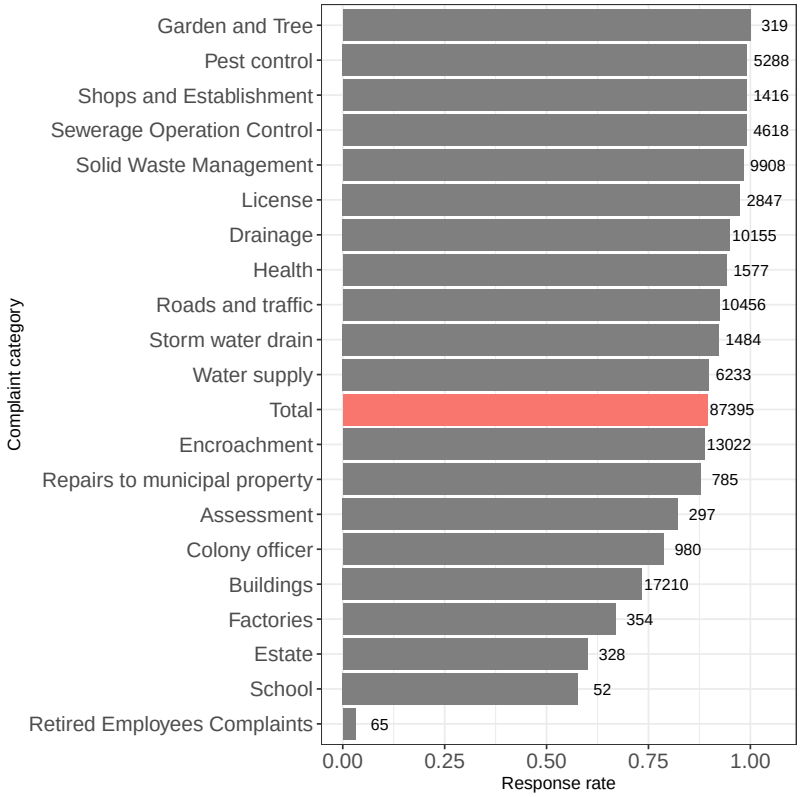
[6]In this website, one can look up a complaint by entering the ward, category, and date under which it was filed. If one enters all the possible combinations of these items, it is possible to download a complete set of complaints filed for a given time period. The website is here (https://portal.mcgm.gov.in/portal/)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

[7]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.

[8]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.

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

Figure 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.

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. Ramiréz-Álvarez (2019) similarly finds that the privatization of property rights in rural Mexico weakens the role of local *ejido* leaders. It would follow that such a program would also reduce demand-making as well. For example, a slum rehabilitation program providing water and electricity connections could eliminate the need to organize to demand these very same items. Welfare programs that work also make beneficiaries wealthier, through either in-kind or cash transfers. These wealth gains may decrease incentives to participate in local 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.

Yet if the likelihood of making demands is a function of other variables as well, such as attitudes and the existence of interest groups, then becoming a welfare beneficiary might *increase* participation in local 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 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, 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 local 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 local-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 pro-

gram 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. Such requests are for improvements in *collective* services in that they affect all beneficiaries of the program.

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.[10] Local officials are also likely more visible or accessible to ordinary citizens than officials at higher levels (Corbridge *et al.* 2005).[11] 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 government. In support of the hypothesis that benefits generally increase this type of local political participation, Table 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 patterns could simply be resulting from fundamental differences between program beneficiaries and non-beneficiaries, rather than any effect of the programs themselves. For example, it is highly plausible that program beneficiaries are simply more politically active than non-beneficiaries in the first place.

3 Case: subsidized housing in Mumbai

In this study, I present the effects of one type of government benefit, namely a program that subsidizes home purchase prices. This type of 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 housing programs are expen-

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

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

Table 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.

sive, 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.[12] 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.[13] 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).

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

[12]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 1991; Field 2005; Galiani and Schargrodsky 2010) and slum rehabilitation (e.g. Burra 2005), programs that are intended to resolve issues of informality and poor service delivery in slums.

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

for households to ask local officials for assistance with individual or group-level items.[14] 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.

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 allocation of subsidized housing is randomized among applicants in Mumbai, India to identify the effects of welfare programs on recipients' local demand-making.

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

[14] See AUTHOR 2019 for a study of the economic effects of this program.

[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.

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

while the LIG group could purchase a 320 square foot apartment for about Rs.2,000,000 (about 31,000 USD).

Table 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.

The homes were sold at a government "fair price" that was 30-60% of market prices. Table 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 A1 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.

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

As mentioned above, beneficiaries were selected through a lottery process. In fact, the winners were selected within caste and occupation groups (Table B1), 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 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.

4.1 Data collection

This study is based on both qualitative interviews and a large scale 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]

[18] 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.

[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

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 B2 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 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 B3 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 can 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 state and municipal ward membership are 0.35 and 0.46, respectively. These p-values do not allow me to reject the null hypothesis that members of any electoral constituency were equally likely to be in the mapped treatment group.

From 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.

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.

[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).

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.

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.

4.2 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 B4. 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 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

[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 B5) 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 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.

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 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 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]

4.3 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[26] and estimate the treatment effect on the pooled sample of lotteries, β, in the following equation where Y is the outcome (as measured through a survey), T is an indicator for treatment (winning the lottery), and $C_1...C_j$ is the group of fixed (or pre-treatment) covariates used for randomization checks, and ϵ is an error term. Given that randomization happened within blocks,

[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).
[26]Deviations from the pre-analysis plan are explained in appendix A.

Table 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.

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:

$$Y = \alpha + \beta T + \sum_{1}^{j} \gamma_j C_j + \sum_{1}^{l} \omega_l B_l + \sum_{1}^{l} \eta_l (T * B_i) + \epsilon \tag{1}$$

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.[27] 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. When an outcome is not binary or categorical, treatment effects are reported in standard deviations of the control group.

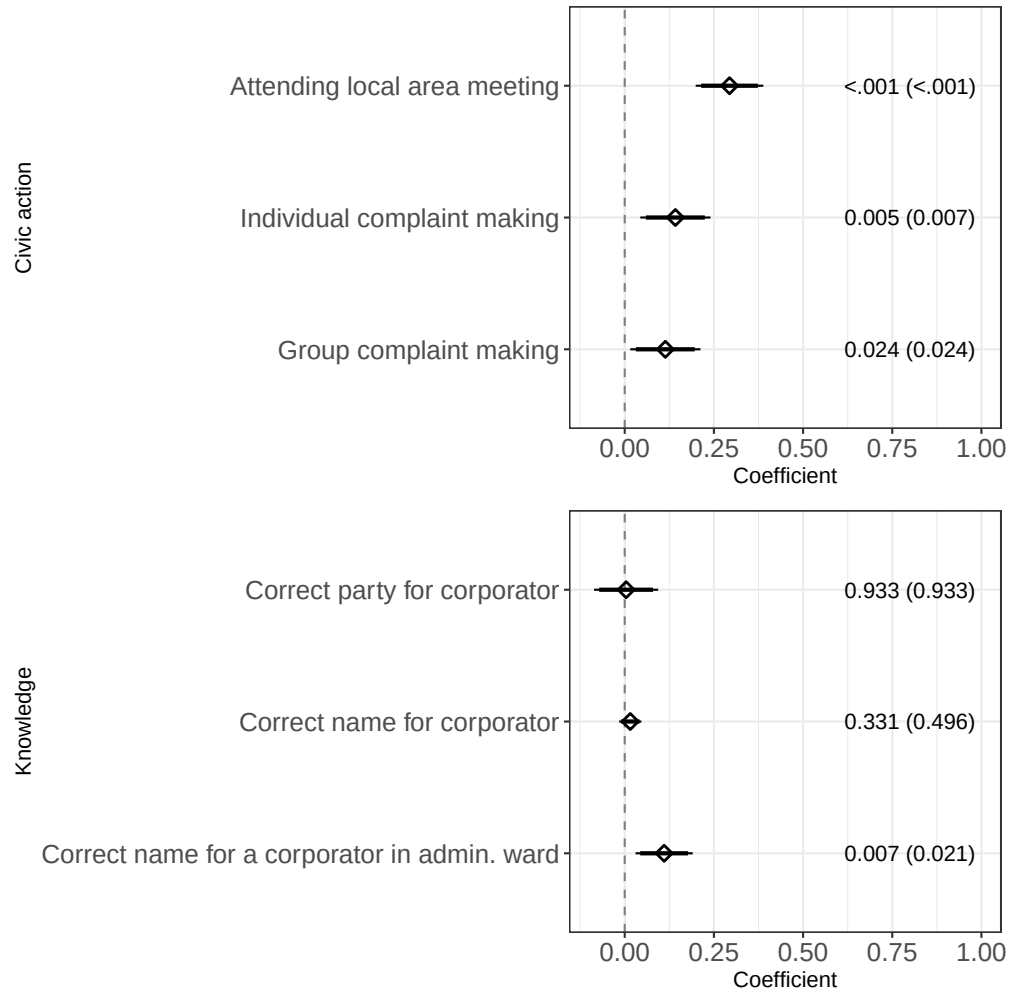
5 Results: demand-making and knowledge

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 their households 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 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

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

Figure 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 B6-B7. 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.

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 owner occupiers.[28] 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

[28]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).

low; only about 2% of the control group can name the relevant corporator correctly. As seen in Figure 2, I do not detect treatment effects for knowing the name or party of the corporator for the ward in which respondents live.

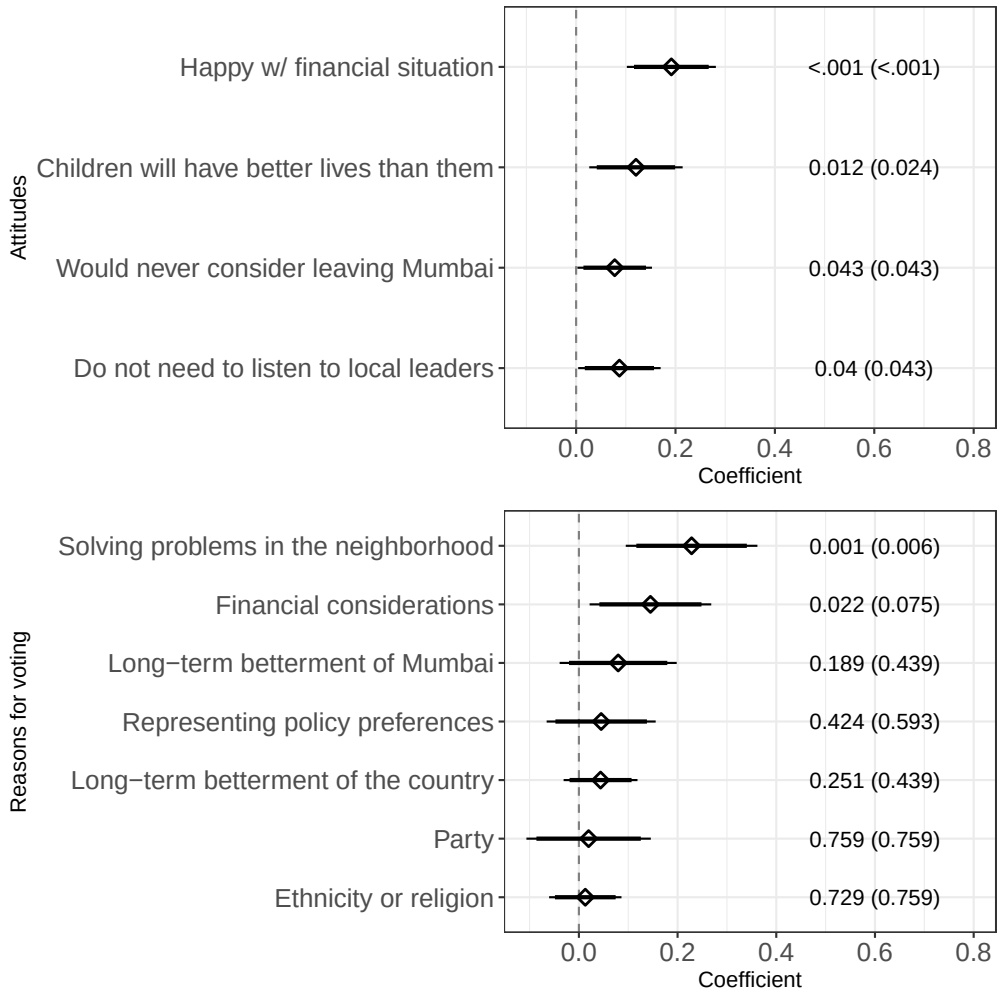
But in Mumbai, electoral wards are grouped into 24 larger administrative wards (Figure A2) It is the administrative ward office, not the electoral ward office, that is responsible for handling complaints. Mumbai residents therefore think in terms of administrative wards, not electoral wards.[29] As a 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 and Table B7). 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.

5.1 Mechanisms: attitudes, status, and motivations

I have argued that one channel through which government benefits might lead to increase demand-making is by making recipients feel wealthier and alter their time horizons (Figure 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

[29] 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 3: Treatment effects for proposed mechanisms



Bars show 90% and 95% confidence intervals. Full regression output with and without covariate adjustment available in Tables B8-B12. 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 AUTHOR 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?"

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 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 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 B13) 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.[30]

5.2 Alternative explanations

I argue that factors *aside* from relocation are responsible for the effects I see; nevertheless, it is also 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

[30]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.

itself, without additional modeling assumptions. Nevertheless, Table 6 shows that outcomes for landlords and owner occupiers are similar, especially when compared to the control group.

Table 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.[31] 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.[32] Furthermore, 88% of respondents claimed that 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

[31] 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.

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

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 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.

6 Similar effects for 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 welfare programs that are effective and 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 sus-

tained use of toilet or hospital facilities over time, respectively. All of these types of policies provide streams of in-kind benefits over time.[33] 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 housing in Mumbai, as we have seen, scores highly in each of these areas.

There is some evidence for the existence of similar policy feedback 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.

The extent to which such action surrounds services that affect the lives of non-beneficiaries may also vary. In the case of housing programs, improving communities clearly affects all non-beneficiaries living in the vicinity of lottery apartments. Yet it is not clear whether improving the timeliness of payments, for example, will affect those who are not members of a pension program. Even so, it is possible that deficiencies in welfare provision are often connected to

[33] 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.

broader problems with governance and service delivery in an area. Inadequate payments to laborers, for example, have alerted NREGA beneficiaries to patterns of corruption at village, block, and district levels that surely affect non-beneficiaries through other avenues (Jenkins and Manor 2017).

7 Conclusion

In this paper, I propose that welfare policies in India and other middle- and low- income countries potentially have important effects on beneficiaries' political behavior. Moving beyond studies of turnout and vote choice, I focus on beneficiaries' propensity for taking action to improve collective services at the local-level. I exploit a natural experiment in the form of a housing lottery in Mumbai to find that benefitting from subsidized housing leads individuals to increase their reported participation in collective demand-making and knowledge of local government. I suggest that these results 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 housing 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 agendas. If the subsidized housing 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 educa-

tion (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.

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Appendices

A Figures

Figure A1: Location of the addresses of households in the sample (pink) along with the location of apartment buildings (blue) at the time of application

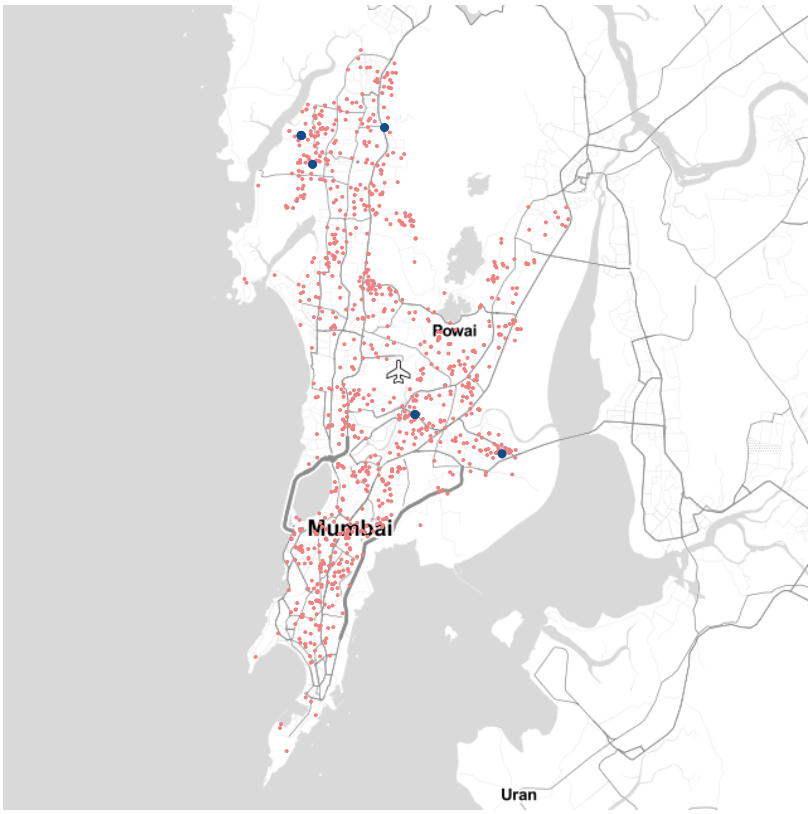


Figure A2: Map of electoral wards in Mumbai. Wards are filled to denote administrative ward membership.

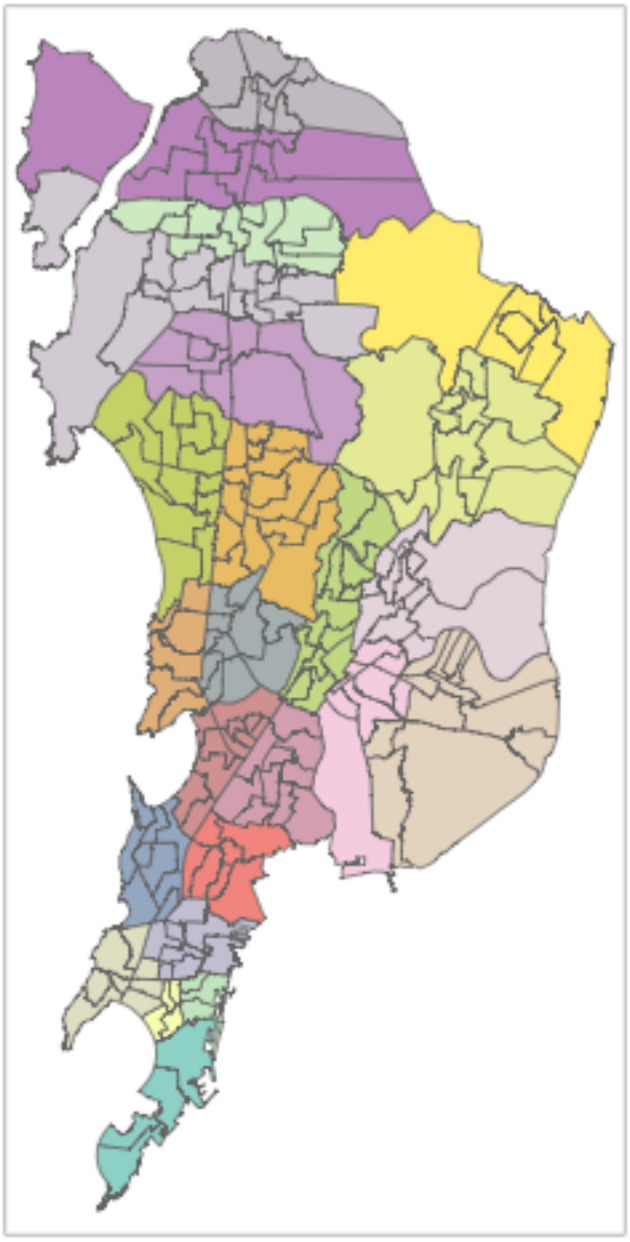
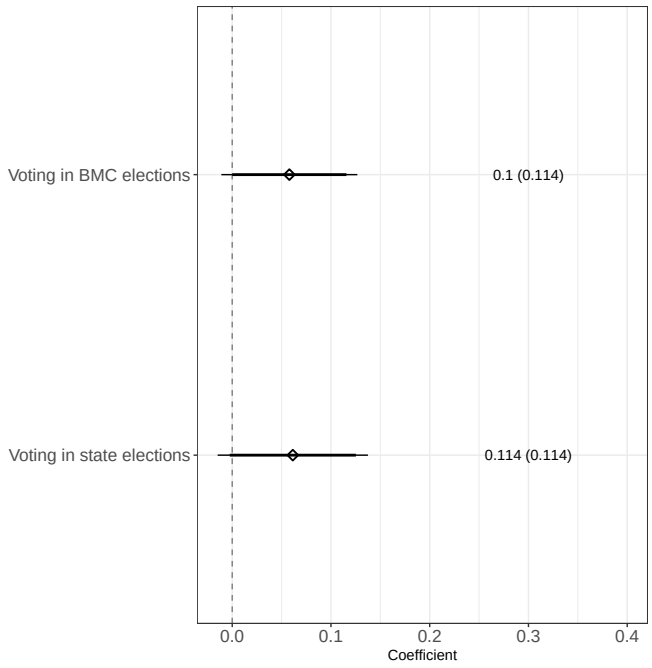


Figure A3: Treatment effects for responding "Yes" to "Did you vote in the last MCGM (municipal) or state elections?"



Bars show 90% and 95% confidence intervals. Full regression output with and without covariate adjustment available in Table B13. P-values (with p-values using Benjamini-Hochberg corrections for the false discovery rate in parentheses) are shown on the right.

B Tables

Table B1: 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 B2: 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 B3: 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 B4: 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 B5: 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 B6: Regression estimates for treatment effects reported participation in local demand-making. The first two outcomes show a binary indicator for respondents choosing "often" or "sometimes" (as opposed to "rarely" or "never") when asked "How often in your community do [you]/[a group of individuals jointly] petition government officials and political leaders for something benefitting your community?" The last outcome is a binary indicator for respondents reporting attending a local area development meeting in the past month 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***	0.115**	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 B7: Regression estimates for treatment effects on knowledge of local politics. Outcome is a binary indicator for whether or not respondents can correctly provide given names. 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 B8: Regression estimates for treatment effects on attitudes. To be "happy" with one's financial situation means to select the highest level on 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?" 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?"

	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 B9: Regression estimates for treatment effects for reported reasons for voting in the last municipal election (without covariates). Respondents were asked an open ended question, "How did you make your vote choice for the municipal elections?" Enumerators were instructed to select all responses that applied. Outcomes are binary indicators for choosing a response. All regressions include treatment indicator interactions with mean-centered block dummies.

	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 B10: Regression estimates for treatment effects for reported reasons for voting in the last municipal election (with covariates). Respondents were asked an open ended question, "How did you make your vote choice for the municipal elections?" Enumerators were instructed to select all responses that applied. Outcomes are binary indicators for choosing a response. All regressions include treatment indicator interactions with mean-centered block dummies.

	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 B11: Regression estimates for treatment effects on reported satisfaction with various outcomes (without covariates). Respondents were asked "How satisfied are you with the following services in your community?" Outcome is a binary indicator for the respondent saying "satisfied" rather than "neither satisfied nor dissatisfied" or "dissatisfied." 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	0.039	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 B12: Regression estimates for treatment effects on reported satisfaction with various outcomes (with covariates). Respondents were asked "How satisfied are you with the following services in your community?" Outcome is a binary indicator for the respondent saying "satisfied" rather than"neither satisfied nor dissatisfied" or "dissatisfied." 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	0.040	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 B13: 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