Welfare programs and local political participation: the effects of affordable housing in Mumbai*

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June 19, 2019

Abstract

How do welfare programs affect beneficiaries' local political activity in middle- and low-income countries? Many welfare programs are implemented by local governments and entail continued delivery to, or use by, beneficiaries over time. I argue that recipients are therefore motivated to participate in local politics to protect the value or quality of benefits. I support the argument with a natural experiment consisting of interviews of 834 applicants of subsidized home price lotteries in Mumbai, India. In this case, I predict that beneficiaries will protect their housing welfare benefits by demanding improvements to the neighborhoods in which lottery apartments are located. Winning an apartment increases both reported political participation to improve neighborhoods and knowledge about local politics. Winners who choose to rent out the apartments also report taking action to improve neighborhoods. The study highlights both the electoral and non-electoral political effects of programmatic policies and causes of civic participation among diverse groups.

*This project has been supported by the J-PAL Governance Initiative, the Weiss Family Program Fund for Development Economics at Harvard University, the Institute of International Studies at the University of California, Berkeley, and the American Political Science Association Centennial Center. Research has been approved by the Committee for Protection of Human Subjects at the University of California, Berkeley, protocol 2017-04-9808. A pre-analysis plan has been registered with EGAP (http://egap.org/registration/2810). I am extremely grateful for Partners for Urban Knowledge Action Research, its "barefoot researchers," and particularly Nilesh Kudupkar for assistance with data collection. I thank Pradeep Chhibber, Alison Post, Edward Miguel, and Joel Middleton for advice and mentorship. I also received valuable feedback from Claire Adida, Anustubh Agnihotri, Caroline Brandt, Graeme Blair, Christopher Carter, Aditya Dasgupta, Ritika Goyal, F. Daniel Hidalgo, Alisha Holland, Pranav Gupta, Nirvikar Jassal, Galen Murray, Agustina Paglayan, Bhumi Purohit, Matthew Stenberg, Valerie Wirtschafter, Laura Zimmerman, two anonymous students at Texas A&M University, and participants at CaliWEPS 2018. Finally and most importantly, I am indebted to the hundreds of survey and interview respondents who gave their time to this study.

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1 Introduction

Governments in many middle- and low-income countries 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, ambitious central and state governments spend on numerous policies, including pensions, electrification, employment, financial inclusion, and affordable housing programs. Do these policies affect political participation among beneficiaries?

Seeking to understand the political motivations for spending on such initiatives, several (e.g. Bechtel and Hainmueller 2011; Dasgupta 2015; De la O 2013; Imai *et al.* 2019; Manacorda *et al.* 2011; Zucco 2013) have investigated the electoral returns to specific welfare programs. The study of whether beneficiaries reward implementing politicians can be seen as part of a broader understanding of politics as an exchange of votes for resources, or clientelism (Kitschelt and Wilkinson 2007). But welfare benefits are rarely simple transfers of cash or favors. Instead, they are often policies that purportedly aim, either through sustained delivery or large transfers of benefits intended for sustained use, to substantially change the lives of beneficiaries. It is likely that their effects go beyond inducing reciprocal voting for implementers. Indeed, we know from an extensive literature on policy feedback from the United States and Europe (see Campbell [2012] for a review) that welfare policies have the potential to greatly change the interests, capacities, and beliefs of beneficiaries.

I argue that certain welfare policies can have an effect on the everyday political ac-

[1]These data are part of Clements *et al.* 2013 and can be found here (https://www.imf.org/external/pubs/ft/sdn/2011/data/sdn1115.zip).

tivity – such as negotiation with politicians, bureaucrats, and brokers for goods and services – that forms a cornerstone of political participation in many countries. As I show for the Mumbai case, this demand-making can have a real and positive effect on government service provision. Policies that entail sustained delivery or sustained use of benefits allow recipients to enjoy the benefits over time. As a result, recipients may seek to ensure that the value of these benefits remains the same or increases over the time that they remain beneficiaries of the program in question.

I further claim that monitoring the value of one's benefits entails local level political participation because while many welfare programs are crafted by national and state governments, in systems with decentralized government responsibilities they may be administered at the local government level, where there can be a great deal of variation in the effective value, or quality, of services and programs (Post *et al.* 2018). Value or quality can take on many dimensions, several of which local political participation may improve. For example, recipients of disability programs may demand more timely payments, or those participating in an employment guarantee program may wish to influence the types of projects on which they work.

In line with this framework, I predict that welfare programs that provide either the sustained delivery or sustained use of benefits over time should increase recipients' participation in local politics, change their reasons for participation, and increase their knowledge of local politics. The specific demands that beneficiaries make should vary with the welfare program in question.

To support my hypotheses, I use a natural experiment to study the effects of receiving a subsidized home for purchase in Mumbai, India. The program is implemented through a lottery system, allowing causal identification of its effects. Furthermore, these affordable housing programs entail large benefits that are likely to have a substantial impact on the lives of beneficiaries. Unlike ration cards or employment programs wherein

the government is clearly responsible for program quality, the lottery homes entail the one-time delivery of a benefit for *private* ownership, making beneficiaries' continued demands that the government improve the benefit particularly surprising. But as local level service provision to neighborhoods of even privately owned homes remains a public function, I argue that recipients will seek to improve the value and quality of their benefits by demanding that local governments better the communities in which homes are located.

This study is based on original interviews of 834 winners and non-winning applicants of multiple affordable housing lotteries conducted in Mumbai in 2012 and 2014. I find that on average, winners are roughly 14 percentage points more likely to report individually approaching bureaucrats and politicians to demand improvements to their local communities, and about 11 percentage points more likely to report doing so in groups. Winners are about 22 percentage points more likely to report voting for candidates on the basis of concerns about neighborhood improvements. I also estimate that winners are 11 percentage points more likely to correctly name a local elected official.

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.

In addition to shedding light on the political effects a large and understudied policy, these findings point to an avenue besides reciprocity through which programmatic policies can affect electoral behavior, namely by changing the motivations and beliefs of beneficiaries. They also have implications for our understanding of collective action

and public goods provision in middle- and low-income countries. Importantly, demand-making occurs among a diverse group of individuals who previously did not know each other, indicating that given the right incentives, collective action and civic participation can, in fact, occur among those that do not have an existing stock of social capital. Finally, the findings turn on its head a standard notion that taxation leads to demands for government services; I find instead that delivering government services can themselves spur demands for further delivery.

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", or programs, and subsidies implemented both at the central and state levels that target different groups. They may be inadequate or corrupt, and there has been considerable disagreement over whether expenditure on these items has increased, decreased, or remained constant since India's economic liberalization in 1991 (Dev and Mooij 2002; Joshi 2006; Nayar 2009). But the fact remains that 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 roughly two million citizens.

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 Wilkin-

Table 1: Fraction of survey respondents reporting benefiting 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	0.0023
Sanitary latrines	0.0509
Kisan credit card	0.0513
Indira Awas Yojana	0.0514
NREGA	0.2844
Ration cards	0.8626

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

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

son 2007).[2] As described in this literature, an absence of baseline service provision can create opportunities for rent-seeking among those who govern allocation. 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; Auerbach 2016; Bussell 2017). 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

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

other countries.

But of course, 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 citizens' everyday interactions with the state. Scholars describe a struggle for 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; Auerbach 2016). 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 (Scott 1969; Auyero 2001; Chandra 2004; Jha, Rao, and Woolcock 2007; Kitschelt and Wilkinson 2007; Stokes *et al.* 2013; Kruks-Wisner 2018).

Many of these studies examine how different types of participation 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 the "everyday" demand or claim-making (Kruks-Wisner 2018) described by those who study distributive politics?

2.1 Theory: welfare policies and local political participation

Many welfare policies can be understood as streams of benefits enjoyed by individuals or groups. I describe the relevant policies as those entailing *sustained use* or *sustained delivery* of benefits over time. Small one-time cash transfers do not fall in either category. In contrast, policies such as pensions or employment guarantees entail sustained delivery over time, while public hospitals or programs such as those that construct sanitary latrines allow the sustained use of toilet or hospital facilities over time, respectively. Such benefits quite obviously remain part of recipients' lives even after they first become beneficiaries. As a result, they should seek to ensure that the value of benefits increases or simply does not decrease over the lifetime of the benefit. In other words, welfare

benefits can be considered as wealth or asset shocks that recipients will seek to protect.

This line of reasoning is supported by a literature on policy feedback from the United States and Europe (see Campbell [2012] for a review) that finds that benefitting from government social welfare can encourage political participation to ensure either the continued or increased receipt of program benefits (e.g. Campbell 2012; Mettler and Soss 2004; Pierson 1993). Also, protests to improve 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.[3] In September 2018, senior citizens came together in Delhi to demand an increase to their old-age pension of Rs.200 (about 3 USD) per month.[4] In yet another example, in May 2018, beneficiaries of Kisan Credit Card loans in Rajasthan protested the mistakenly high interest rates charged by the local branch of the State Bank of India.[5] But perhaps these news-worthy protests are simply the tip of the iceberg when it comes to political participation among welfare beneficiaries.

Participation in local level politics is *not* necessarily an important way to protect or increase the nominal size of welfare benefits under programs created at the national or state level. For many of the programs listed in Table 1, eligible citizens are given a card that is used to track and receive beneficiaries. For others, such as the housing or latrine programs, benefits are given just once. It is likely that receiving an item entails some amount of political maneuvering for most citizens, but it is unlikely that this would occur during a public meeting. Once a card (or a home or a sanitary latrine) is given, revoking

[3]https://kashmirlife.net/baramulla-mg-nrega-employees-protest-against-continued-fund-shortage-in-srinagar-198463/

[4]https://www.ndtv.com/delhi-news/protest-for-universal-old-age-pension-scheme-in-delhi-tomorrow-1924355

[5]https://thewire.in/rights/sbi-rajasthan-farmers-extra-interest-kisan-credit-card

the benefit could be logistically extremely cumbersome, and the funds available for the program in question are not decided at the most local level.

Local governments *are*, however, often responsible for the implementation of welfare programs, particularly in places like India that have seen substantial devolution and decentralization of government responsibilities. 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. Local governments may thus be responsible for the *quality* of a benefit, where the term "quality" encompasses many dimensions affecting the real value of a benefit, from the timeliness of delivery to the cleanliness of certain facilities (Post *et al.* (2018). Gulzar and Pasquale (2017, 165) clearly display the huge variation in implementation quality of NREGA. In other words, a welfare benefit is no different from any other government provided good or service in that it may insufficient, of poor quality, or not reach those to whom it is promised. Participating in local politics can increase the quality, and therefore real value, of a welfare benefit. Table 2 shows that beneficiaries of various Indian welfare programs, with the exception of beneficiaries of certain programs intended for the elderly, report greater attendance of local public meetings wherein they make make complaints or demands of local government than non-beneficiaries.

In support of this argument, I test three specific hypotheses. First, welfare beneficiaries are more likely than non-beneficiaries to make demands of local politicians and bureaucrats. I investigate the likelihood of both individual and group-level behavior, but group-level action seems less likely to occur given the fact that beneficiaries do not know each other, thereby making organization costly. Also, beneficiaries are likely to report different motivations for local political participation than those reported by non-beneficiaries. Finally, because of their increased participation, welfare beneficiaries are likely to possess greater knowledge of local politics than non-beneficiaries. The demands

Table 2: Welfare beneficiaries and political participation

Program	Beneficiaries	Non-beneficiaries	p[1]
Old age pension	0.3450	0.2879	0.0000
Widows' pension	0.2922	0.2931	0.9248
Maternity scheme	0.3303	0.2920	0.0053
Disability scheme	0.3739	0.2920	0.0000
Annapurna (food security) scheme	0.2842	0.2931	0.8491
Sanitary latrines	0.4371	0.2849	0.0000
Kisan credit card	0.4336	0.2849	0.0000
Indira Awas Yojana	0.4376	0.2847	0.0000
NREGA	0.4398	0.2347	0.0000
Ration cards	0.3052	0.2173	0.0000

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 affordable housing program.
[4] Mahatma Gandhi National Rural Employment Guarantee Act.

that are made, the ways in which reasons for participation change, and the knowledge that beneficiaries gain should vary with the welfare program in question. These differing demands should in turn lead to differing motivations for political participation and knowledge of different aspects of local government.

Existing literature does provide some evidence to support these hypotheses. In a case of the sustained *delivery* of benefits, for example, Jenkins and Manor (2017, 166-181), find that NREGA increases political capacity and the "assertion of citizenship" among Indian villagers in order to demand the full and adequate delivery of benefits promised by the program. In a case of the sustained *use* of benefits, MacLean (2011) claims that citizens of African countries with some experience with public schools and clinics are more likely to engage in acts of everyday citizenship to improve the quality of schools and clinics. There is less evidence to support these hypotheses in a case of sustained use when the benefit is privately owned. This occurs when a welfare benefit, often an asset, is simply transferred to beneficiaries, effectively breaking the connection to local governments. Examples land titling, tractor subsidies, camel subsidies, and home subsidies. I next empirically examine the effects of this latter policy.

2.2 Applying the theory to affordable housing in Mumbai

The program I study is one in which home purchase prices are subsidized. It allows households to 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.[6] This type of program exists in many cities globally, including those is in middle, low-income,

[6]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 OECD countries, and is particularly common in urban India. 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.[7]. Part of this program entails central transfers to subsidize state-level housing programs. More generally, 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). Subsidized housing programs are expensive, extremely common, and virtually unstudied.[8]

This is a program that provides not sustained delivery, but sustained *use* of a benefit over time. Beneficiaries of such programs should be *less* likely to participate in local politics than those of one entailing sustained delivery because the local government may not always be directly responsible for the quality of the benefit. The housing program, for example, entails *ownership* of a home, responsibility for maintenance ostensibly lies with the beneficiary and not the government. As a result, it is not obvious why recipients should continuously participate in local government to improve the quality of the benefit. The program therefore provides a somewhat difficult test of the theory.

I argue that beneficiaries might seek to improve the quality of the benefit by improv-

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.

[7]This scheme is an extension of what used to be known as Indira Awas Yojana, which dealt mainly with rural homes.

[8]But there is growing interest in this policy type. Similar studies are currently underway in Montevideo and Rio de Janeiro. See pre-analysis plans here (http://egap.org/registration/255) and here (http://egap.org/registration/2912).

ing aspects of the *neighborhood* in which the home is located. To the extent that local governments provide neighborhood services like sanitation, safety, and electricity, they can be considered responsible for aspects of neighborhood quality. These improvements may increase the quality of life for those living in the apartments (owner-occupiers) and increase the resale value of the homes, benefitting even those who do not choose to live in them (landlords). I therefore predict that beneficiaries are more likely than non-beneficiaries to demand improvements to their communities. I further predict that they will be more likely to vote for candidates who prioritize improving local communities, and, finally, that they will possess greater knowledge about the local bodies tasked with improving communities.

Even while much of the literature on public goods provision highlight incentives and discretion in responsiveness, Mumbai, the site of this study, has a clear 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 Mumbai, all citizens place a complaint with their administrative ward governments 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 the progress of the complaint as it is passed to the appropriate department. Bureaucrats in the ward office mark the complaint as "closed" once it has been resolved or a reason has been given for why it cannot be resolved.[9] I scraped the website through which one makes and tracks complaints and found that 87,395 complaints were registered in 2017.[10]

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

[10]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,

As shown in Table 3, 89.5% of these complaints were resolved, with the resolution rate approaching 100% for several categories designated by the municipality.[11] This data is supported by qualitative interviews with lottery winners who said that the municipal government was responsive to their complaints.[12]

3 The natural experiment

Using observational evidence to test my hypotheses may generate misleading conclusions due to the fact that welfare 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 affordable housing is randomized among applicants in Mumbai, India to identify the effects of welfare programs on recipients' local political participation.

The Mumbai Housing and Area Development Authority (MHADA)[13] runs subsi-

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/) .

[11]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.

[12]Those working in the office are candid about the fact that the government is much less responsive to the complaints of those squatting illegally.

[13]The agency is a subsidiary of the Maharashtra Housing and Area Development Au-

Table 3: N complaints made to and resolved by the Municipal Corporation of Greater Mumbai in 2017.

Category[1]	Percentage resolved	N
Garden and tree	1.000	319
Pest control	0.992	5288
Shops and establishment	0.991	1416
Sewerage operation control	0.991	4618
Solid waste management	0.983	9908
License	0.975	2847
Drainage	0.950	10155
Health	0.942	1577
Roads and traffic	0.924	10456
Storm water drain	0.923	1484
Water supply	0.899	6233
Encroachment	0.888	13022
Repairs to municipal property	0.878	785
Assessment	0.822	297
Colony officer	0.787	980
Buildings	0.734	17210
Factories	0.669	354
Estate	0.601	328
School	0.577	52
Retired employees complaints	0.031	65
Total	**0.895**	**87395**

[1] Names of categories as they appear on the website.

Collected from https://portal.mcgm.gov.in/

dized housing lotteries for economically weaker section (EWS) and low-income group (LIG)[14] 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.1500000 (about 23500 USD at the time), while the LIG group could purchase a 320 square foot apartment for about Rs.2000000 (about 31000 USD).

The homes were sold at a government "fair price" that was 30-60% of market prices. 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). The subsidy estimates are based on neighborhood prices per square foot, but they do not account for the fact that government housing has a lower resale value than privately constructed housing (likely because of the mild social stigma and particular aesthetic associated with government housing). 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.[15] Winners had access to loans from a state owned bank and most took out 15 year mortgages. While the downpayment

thority 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.

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

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

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.

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. The homes are scattered throughout the city. At the time of application, households were permitted to choose the building for which they submitted an application. The MHADA apartment buildings are not in the outskirts of the metropolitan area; they are, instead, near major highways and transit lines. Each is within walking distance of the Mumbai suburban rail network, the main network that millions of city residents use to commute every day. The ability of households to choose their preferred building along with the proximity of the buildings to transit options suggests that these buildings, unlike those studied by Barnhardt *et al.* [2017], are not necessarily isolated or extremely disconnected from winners' neighborhoods at the time of application.

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 groupss, 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 the this process was fair, or truly randomized. First of all, the lottery was conducted using a protected computerized process.[16] Applicants

[16]While this lottery has been in place for at least twenty years, the computerization started in 2010, preventing me from studying earlier lottery cohorts. Interestingly, a handful of control group respondents complained about paying brokers who claimed to

also applied with their Permanent Account Numbers (PAN), which are linked to their bank accounts.[17] Before conducting the lottery, MHADA officials used the PAN numbers to check both whether individuals had applied multiple times for the same lottery round and whether or not they met the criteria for eligibility.[18] The single-application requirement was important in ensuring that the probability of winning remained the same among all applicants in the same group. Finally, I provide randomization checks by demonstrating balance on covariates across winners and non-winning applicants.

3.1 Data collection

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

In the case that households had applied for multiple lotteries included in the study,

be able to help "fix " the lottery and were subsequently never heard from again.

[17] 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.

[18] 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.

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

These addresses were mapped using Google Maps. Addresses that were incomplete (42), outside of Greater Mumbai (600), or could not be mapped (146) were removed from the sample. This left 531 and 532 control and treatment households, respectively. Table 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 the mapping exercise to systematically favor treatment or control units. Overall, however, I expect the 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.

Given the lack of availability of pretreatment covariates, I cannot test for non-random selection into winning the lottery among the 1,862 addresses provided by MHADA. Once mapped, however, I can place households into state and municipal electoral wards and test for evidence of selection into the mapped treatment group by electoral ward. Selection by ward would indicate that individuals from certain locations or with certain political representatives are more likely than others to win the lottery. Here, I 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.

Given that the study was budgeted for a sample of 1000, 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 the households and conduct interviews.[19] The addresses and phone numbers provided by MHADA constituted the contact information for households at the time of application. Non-winners were contacted at these addresses. In cases where they had moved away, neighbors were asked for updated contact information. Winners resided at either the old addresses or new lottery buildings, as

[19]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).

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 PAN card numbers; as a result, I assumed that the individual applying was most likely to be the head of the household. In the case a child had applied for the home (likely because the form could be completed online and older children may be better able to use computers and the internet than their parents), enumerators were instructed to speak to the family's primary earner. 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.

3.2 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 0.395. 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.[20] In other words, winners and non-winners appear to be similar based on a number of fixed observable covariates and there is no evidence of corruption in the lottery or differential selection into the sample.[21]

Although these households fall into the EWS and LIG income categories for the housing lottery, a summary of the assets, housing quality, education levels, and tenure status of the control group 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 lower-middle class and upwardly mobile.[22]

[20] *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.

[21] 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.

[22] This description is corroborated by an interview conducted with the commissioner of the Mumbai Metropolitan Regional Development Authority, who saw the main bene-

Table 4: Balance tests on household characteristics

Variable	Control	Treatment	sd	Pr(>|t|)
OBC[1]	0.150	-0.021	0.035	0.543
SC/ST[2]	0.080	-0.018	0.026	0.499
Maratha[3]	0.295	0.018	0.045	0.690
Muslim	0.090	0.006	0.029	0.852
Kutcha[4]floor	0.031	0.028	0.019	0.136
Kutcha[4]roof	0.039	0.001	0.018	0.945
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" column presents means for winning households. The "Treatment" column presents the difference between winning and non-winning households estimated through an OLS regression of each variable on indicators for winning the lottery. Each regression includes an interaction with the centered block-level indicator for randomization groups. All regressions include HC2 errors. N=834.
[1] Other backward class caste group members
[2] Scheduled caste or scheduled tribe groups, also known as Dalits.
[3] A dominant group in Mumbai and Maharashtra more generally.
[4] "*Kutcha*" means "rough" or "impermanent." Variable measured at time of application through recall.

Table 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)	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.

4 Results

In this section, I estimate effects of winning the lottery within the mapped sample on reported complaint-making to benefit a neighborhood, motivations for vote choice, and knowledge of local politics. I estimate the treatment effect, β, in the following equation where Y is the outcome, T is an indicator for treatment (winning the lottery), $C_1...C_j$ is the group of fixed (or pre-treatment) covariates used for randomization checks. Given that randomization happened within blocks, I treat each of the blocks as a separate lottery and include a set of dummies, $B_1...B_l$ for each:

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

It is likely that certain households apply for the lottery year after year, thereby increasing their probability of winning *any* lottery. I thus only label households as "treated" if they win the lottery in the specific year for which they appear in the sample. Following the pre-analysis plan and Lin (2013), I include an interaction between the treatment indicator and the mean-centered block indicators to account for varying probabilities of treatment assignment within each block.[23] Regression output with and without covariate adjustment can be found in Appendix B. Following Imbens and Kolesar (2015), I compute standard errors using the HC2 estimator (MacKinnon and White 1985). Also, I make Benjamini-Hochberg corrections for the false discovery rate within "families " of outcomes. While this study potentially suffers from two-sided noncompliance (8% of treated units did not purchase homes), I simply conduct an intent-to-treat (ITT) analysis. This choice should bias treatment effects to zero.

ficiaries the housing program to be lower-middle class households (Madan 2016).

[23]See this blog post for a discussion of a comparison between this estimator and the more commonly used inverse propensity score weighted estimator.

Treatment effects for all the main outcomes of interest are shown in Figure 1. I first measure effects on the extent to which respondents report actually taking action to improve their communities. I 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 participate in individual and group-level complaint-making, respectively.

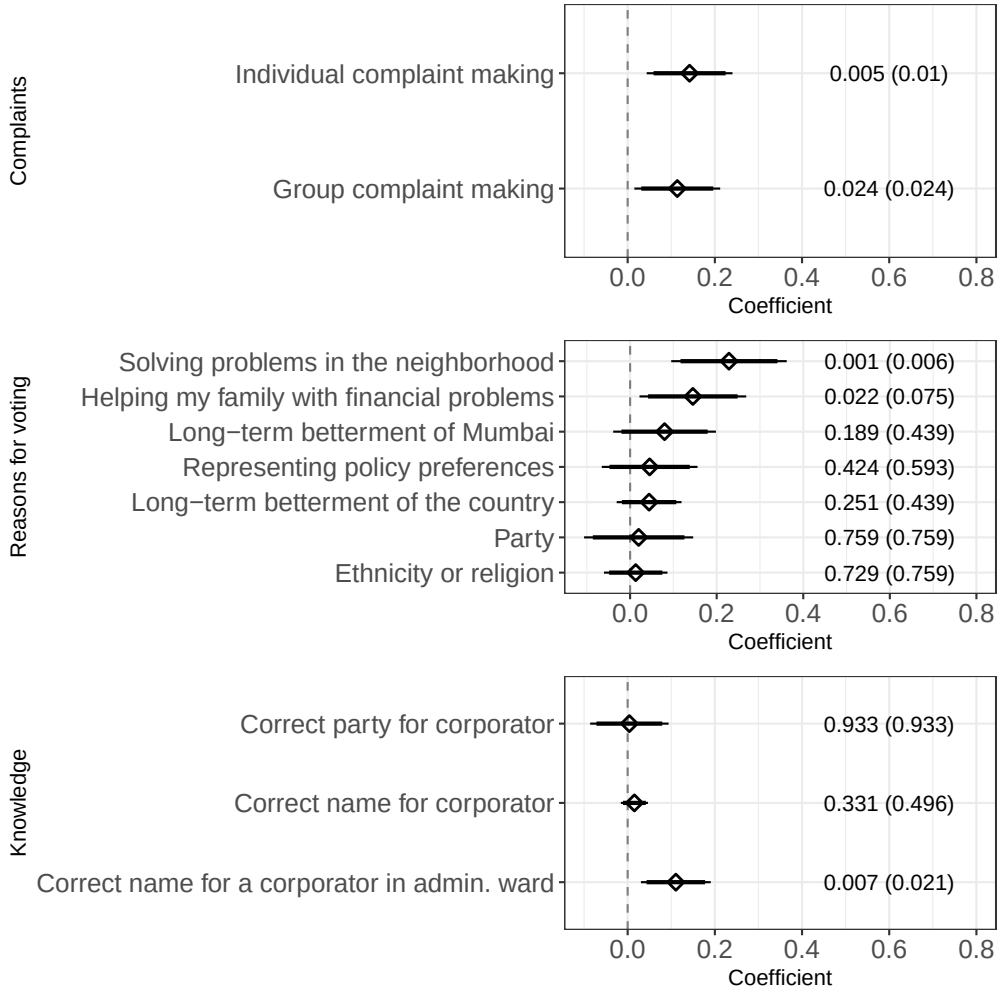
I also detect a change in stated motivations for another form of local political participation, namely voting in local elections.[24] The election of 227 ward corporators to the Municipal Corporation of Greater Mumbai (MCGM)[25] occurred in February 2017, roughly six months prior to the survey. I asked respondents how they made their choice in this election. Here, I use question in which respondents were not prompted with options and all of their responses were selected 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. Relative to non-winners, I estimate that winners are 22 percentage points more likely to state neighborhood problems as a reported reason for voting.

Of course, these treatment effects measure changes in reported behavior only. I also asked respondents questions to measure their knowledge of local politics, with the assumption that greater local political engagement leads to greater knowledge. An indi-

[24]I did 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 B10.) do show high rates of reported voting for the control group.

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

Figure 1: 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-B9. P-values (with p-values using Benjamini-Hochberg corrections for the false discovery rate in parentheses) are shown on the right. Treatment effects for complaints show the likelihood of 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?" 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.

vidual 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 3). I therefore asked respondents for the name and party for the corporator for the electoral ward in which they lived at the time of the survey. The ward was determined using the GPS coordinates for baseline addresses for non-winners and winning landlords, and using lottery apartment addresses for winning owner occupiers.[26] After determining the appropriate electoral ward for each household, I hand coded responses for corporator party and name as either "correct" or "incorrect." Baseline knowledge is low; only about 2% of the control group can name the relevant corporator correctly. As seen in Figure 1, I do not detect treatment effects for knowing the name or party of the corporator for the ward in which respondents live.

But in Mumbai, electoral wards are grouped into 24 larger administrative wards (Figure A2)[27] It is the administrative ward office, not the electoral ward office, that is responsible for handling complaints. Mumbai residents therefore think in terms of administrative wards, not electoral wards.[28] As a result, we might not expect complaint-making to increase knowledge of the names of the corporators at the electoral ward level, but we would expect complaint making to increase knowledge of the names of *any* of

[26]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).

[27]This portion of the analysis was not pre-registered and can be considered "exploratory."

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

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. 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 1 and Table B9). Increases in reported complaint-making to benefit neighborhoods are accompanied by real increases in knowledge of local politics.

4.1 Landlords and apartment society meetings

I also asked whether a member of the household had participated in a neighborhood development association (commonly know as a "society") in the past month. These associations are neighborhood-level meetings held to discuss common problems in the the community. They exist in all types of urban neighborhoods, from slums (see Auerbach 2017) to apartment complexes. 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. The associations are ubiquitous in the lottery apartments. Sixty-two percent of winners reported participating in these meetings.[29] Participation in neighborhood meetings in the lottery home communities is quite surprising as it is occurring among individuals from differ-

[29]I do not report treatment effects for this variable as they may be misleading because control group households may not have cooperative societies in their buildings or

ent neighborhoods and backgrounds; that they are a random assortment of households suggests that participation is reportedly occurring in spite of an absence of pre-existing social capital. These results are complementary to Barnhardt *et al.*'s (2017) finding that beneficiaries of subsidized *rental* programs often participated in building cooperative society meetings.

Furthermore, when I asked landlords whether they attend meetings in the neighborhood of the lottery home, and 55% reported that they did so "Often" or "Sometimes," a figure only slightly lower than the 65% attendance rate reported by owner-occupiers. The attendance of meetings in the lottery home neighborhoods is particularly surprising as going to these meetings can be very costly in terms of time; 68% of the landlords work 6 or more days a week, and the travel time (one way via transit) to the lottery building neighborhoods takes 1.1 hours on average.[30] Finally, the percentages of meeting attendance may actually be underestimates of participation because, according to interviews with development meeting leaders, some landlords also communicate their wishes through WhatsApp or by phone.

Why do we see participation among landlords? 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

neighborhoods even while they are mandatory in lottery buildings.

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

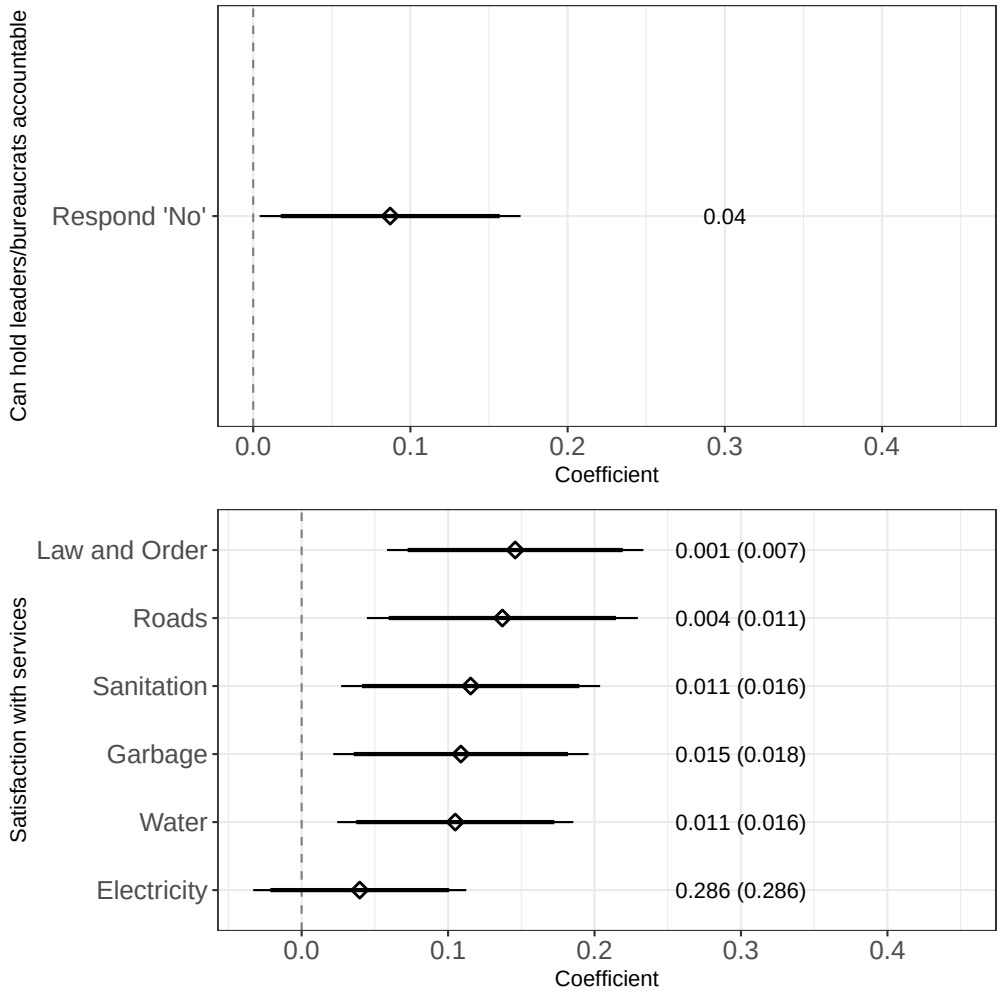
respondents were able to provide a figure for the value of the homes. About 16% of respondents were unsure about whether the value of the property had changed since the purchase, and about 80% claimed it had increased.[31] Furthermore, 88% of respondents claimed that they expected the values to increase in the future. Finally, when presented with an open-ended question about what they thought affected the values of their properties, about 83% of the responses were similar to "the property value of the surrounding areas," 25% included answers mentioning government policies and actions, 15% mentioned individual actions, and only 11% mentioned God or luck. About 9% claimed not to know. Winners are, in fact, aware of the property values and that they can change and even increase over time.

4.2 Alternative explanations

I have argued that the treatment effects observed arise as a result of increased resources and a desire to protect the value of the welfare benefit received here, a home. But participation in petitioning activity might also increase due to improved perceptions of one's ability to gain a response from a public official. This could be because of actually receiving a benefit from the government, or it could also be due a feeling of increased social status after becoming a homeowner. Interestingly, however, this does not appear to be the case. When asked "Do you think you/people like you can hold politicians and bureaucrats accountable for their actions," winners overall were actually *more* likely to say "No" than non-winners (Figure 2). One potential reason for this result is that winners may have, through the channel of increased demand-making, greater contact with government officials and thus have become disillusioned.

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

Figure 2: Treatment effects for outcomes related to alternate explanations.



Bars show 90% and 95% confidence intervals. Full regression output with and without covariate adjustment available in Tables B11-B13. P-values are shown on the right (with p-values using Benjamini-Hochberg corrections for the false discovery rate in parentheses). The top panel shows effects on a binary indicator for responses to "Do you think you/people like you can hold politicians and bureaucrats accountable for their actions?" The bottom panel shows responses to "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."

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. As a result, it would seem that not winning the lottery should have no effect on control group members' impressions of local government capacity and responsiveness.

Finally, it is possible that increased participation in demand-making to improve communities is the result of dissatisfaction with service delivery. Owner-occupiers experiencing worse services in the new buildings could organize to demand improvements in their new communities; landlords who have seen better services in the apartment buildings could be organizing to demand improvements in their baseline communities. To see whether increased participation is driven by dissatisfaction, I look at responses to questions that ask if individuals are satisfied with services in the neighborhoods in which they live (Figure 2). I see no evidence for this mechanism; in fact, I see greater satisfaction with the delivery of most services among lottery winners.

5 Discussion

In this paper, I have argued that welfare beneficiaries in India are more likely than non-beneficiaries to participate in local politics in order to improve the quality of their welfare benefits. This argument should apply in particular to programs that entail the sustained delivery or use of benefits over time. I have supported this argument by showing that an affordable housing program in Mumbai leads beneficiaries to claim to vote based on neighborhood interests, report greater participation in local politics to improve commu-

nities, and possess greater knowledge of local politics. The paper provides some of the first sets of findings about the political effects of a large and common welfare program; while other work does present suggestive evidence that these dynamics hold for NREGA and education policies, rigorously testing hypotheses about demand-making specific to other welfare programs entailing sustained use or delivery of benefits remains a goal for future work.

The results on motivations illuminate new mechanisms by which programmatic policies may change the the political fortunes of implementers. Those studying the electoral effects of programmatic policies (e.g. De La O 2013; Manacorda *et al.* 2011; Zucco 2013) find that such policies increase the electoral support for incumbents. The proposed mechanism (to which Imai *et al.* (2019) point out theoretical objections) is that beneficiaries reward implementers at the ballot box. This study shows that welfare programs might actually alter the motivations and knowledge of beneficiaries, in turn potentially affecting electoral behavior in ways that may (or may not) reward implementing parties and politicians at election time.

The results on complaint-making are particularly surprising given that expending effort to improve a program is a collective action problem. This is because individuals who want to improve a welfare benefit may free-ride on the demand-making activity of other beneficiaries. For example, a ration card recipient is disincentivized from complaining about poorly stocked ration shops because she may believe that another recipient or group of recipients will do so and she will benefit from improved shops without expending any effort. This phenomenon impedes both individual and group action to benefit a group as a whole. One way to increase the likelihood of action is to decrease the costs and thereby increase the return on cooperation. Interestingly, beneficiaries of welfare programs can face particularly high costs of political action as they do not know each other and are often from different ethnic groups, a problem highlighted by

Habyarimana *et al.* (2007). Furthermore, owner-occupiers are removed from their social networks, a phenomenon Gay [2012] finds leads to decrease political participation among beneficiaries of the Moving to Opportunity program in the United States.

Nevertheless, this study suggests that welfare programs may facilitate collective action among very diverse groups of individuals because they provide the time, money, and civic skills that Brady *et al.* (1995) find are essential to political activity. There are many reasons for why this may occur. First, the cash or in-kind transfers that form welfare benefits may generate the material *resources* necessary for action (Campbell 2012; Lowi 1964; Mettler and Soss 2004). For example, the resources may provide poorer citizens with the mental bandwidth (Mani at al. 2013) and time to engage in local politics. The resources may also allow them 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. They may also decrease the relative opportunity cost of participating in collective action by decreasing the value of wages relative to the individual's overall wealth. Second, the contact with the government generated through the receipt of welfare benefits can provide beneficiaries with *knowledge* about how governments work. Kruks-Wisner (2018), for example, similarly finds that encounters with governments increase the likelihood of "making a claim" on the state among rural citizens in India. Auerbach and Kruks-Wisner (2019) similarly argue that visible social welfare provision increases citizens' knowledge of how to make demands.

As demonstrated by the fact that affordable 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 positive spillovers for all citizens in general. In terms of the logic outlined by Olson (1965), welfare benefits create a group of individuals who might benefit *more* from an increase in levels of service provision than the average citizen, thereby giving them a greater incentive to organize around service

improvements. In this way, certain welfare policies can induce *civic* participation among beneficiaries. 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).

My findings are in line with research from OECD countries that finds that homeownership causes or is correlated with more active citizenship at the local level (DiPasquale and Glaeser 1999; Einstein 2017; Fischel 2009; Hall and Yoder 2018). It is perhaps not entirely surprising that giving people homes causes them to behave like homeowners. If the relationships between income and asset ownership and certain types of political behavior are causal, it is possible that welfare transfers cause individuals to behave like those who already possess the transferred item, income, or asset. Indeed, scholars (e.g. Chatterjee (2004) and Harriss (2006)) have claimed that the Indian wealthy and middle class are more likely to participate in civic action than the poor. The value of any income or asset, after all, is affected by government institutions and services. Even so, I would argue that wealth received through a welfare program may be more likely to yield political participation than those gained through private means, simply because welfare programs are implemented by government bodies and institutions.

More generally, a welfare program appears to induce beneficiaries to care about the development of local government capacity, behavior that has thus far been documented primarily among the wealthy and middle class. It is not just taxation that leads to demands for services (Tilly 1992). Government policies can themselves generate demands for development and change.

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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

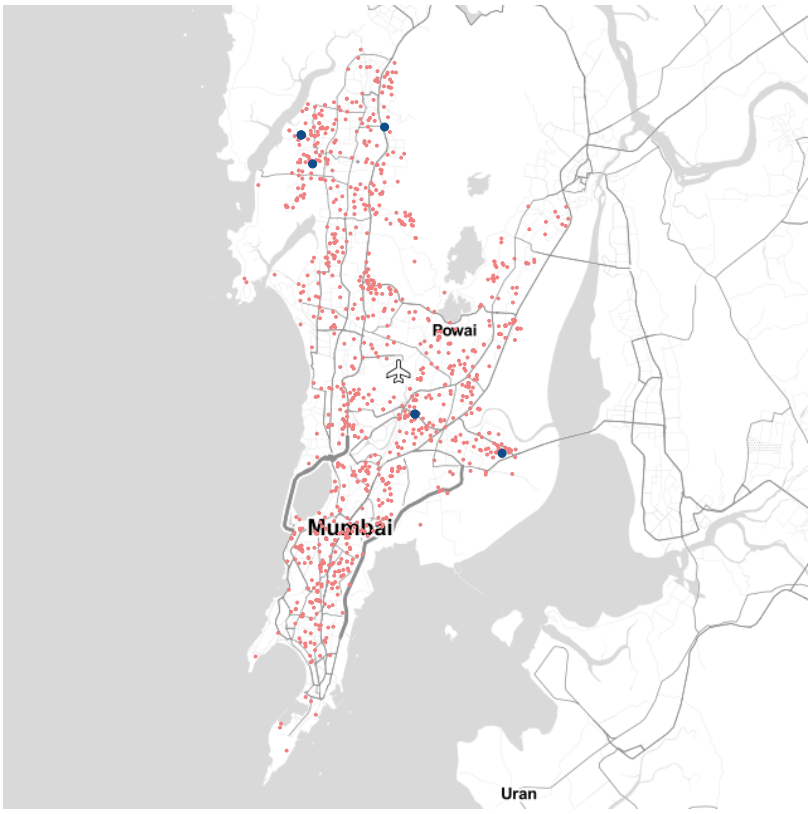


Figure A2: Map of electoral wards in Mumbai. Wards are filled to denote administrative ward membership.

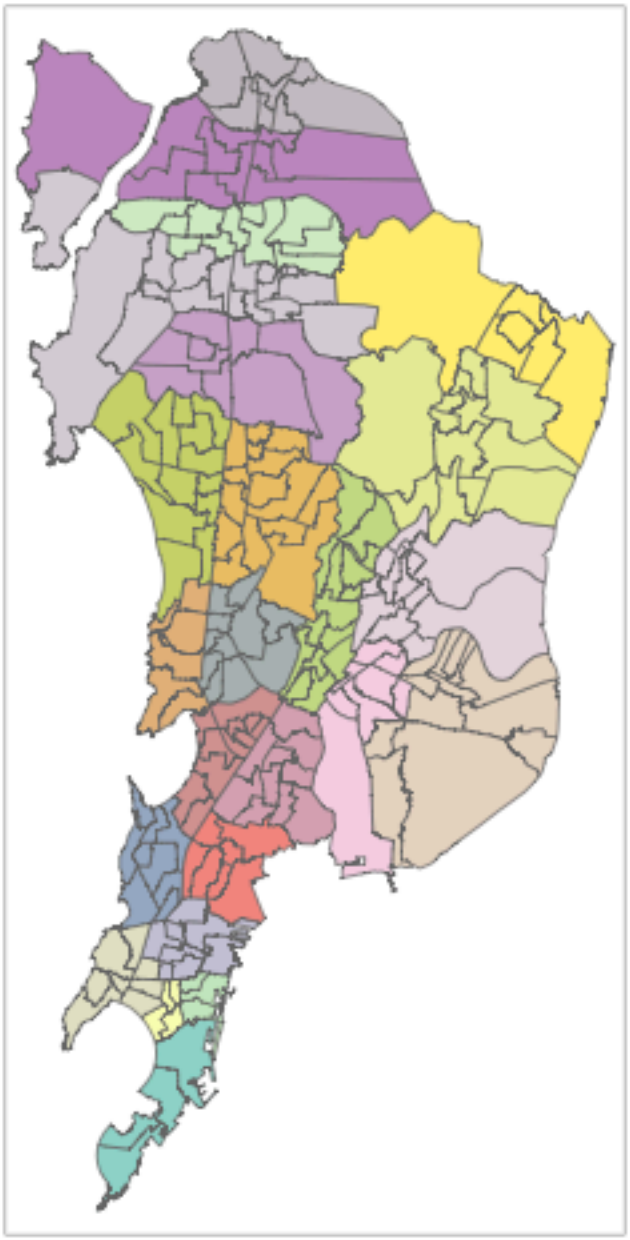
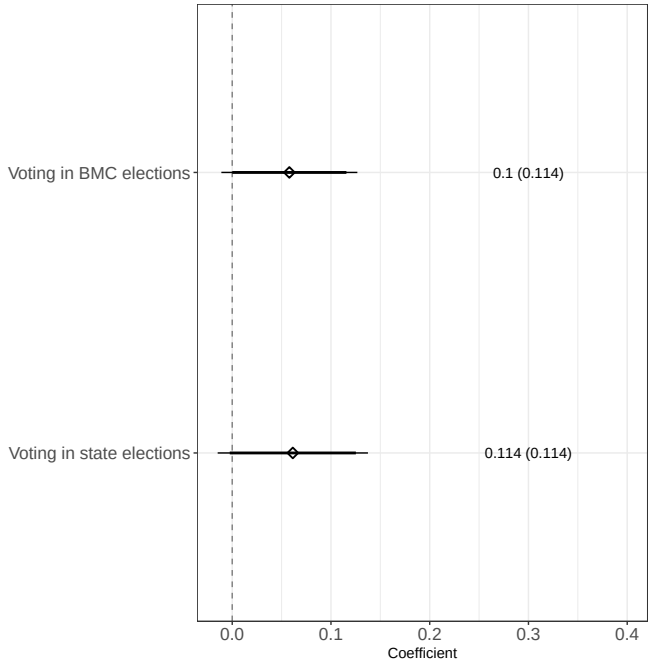


Figure A3: Treatment effects for responding "Yes" to "Did you vote in the last MCGM (municipal) or state elections?"



Bars show 90% and 95% confidence intervals. Full regression output with and without covariate adjustment available in Table B10. 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 two-tailed t-tests.

	Control	Treatment	p
Surveyed	413	421	0.373
Address not found	9	7	0.617
Home demolished	1	0	0.317
Home locked	5	11	0.131
Respondent deceased	1	0	0.373
Refused	14	20	0.294
Unable to locate household	19	10	0.090
Incomplete survey	37	31	0.453
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
Residual Std. Error	0.497 (df = 744)

*p < .1; **p < .05; ***p < .01

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

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

Table B6: Regression estimates for treatment effects reported complaint-making. Outcome is 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?" All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:			
	Individual complaint making		Group complaint making	
	(1)	(2)	(3)	(4)
T	0.144***	0.142***	0.115**	0.114**
	(0.050)	(0.050)	(0.050)	(0.050)
OBC		0.038		0.049
		(0.058)		(0.058)
SCST		0.077		0.065
		(0.075)		(0.075)
Maratha		0.015		0.017
		(0.047)		(0.047)
Muslim		0.034		0.023
		(0.068)		(0.068)
Kutcha floor		−0.036		−0.017
		(0.125)		(0.125)
Kutcha roof		−0.230*		−0.216*
		(0.130)		(0.130)
From Mumbai		0.096*		0.079
		(0.049)		(0.049)
From same ward as apt		−0.027		−0.067
		(0.063)		(0.063)
Constant	0.436***	0.351***	0.415***	0.346***
	(0.033)	(0.057)	(0.033)	(0.057)
Observations	834	834	834	834
R^2	0.169	0.185	0.168	0.182
Adjusted R^2	0.013	0.020	0.012	0.017
Residual Std. Error	0.497 (df = 701)	0.495 (df = 693)	0.497 (df = 701)	0.495 (df = 693)
Note:				*p<0.1; **p<0.05; ***p<0.01

Table B7: 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
Residual Std. Error (df = 588)	0.481	0.271	0.499	0.461	0.412	0.440	0.278

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

Table B8: 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/Religion	Neighborhood problems	Financial problems	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
Residual Std. Error (df = 580)	0.468	0.272	0.495	0.458	0.410	0.440	0.278

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

Table B9: Regression estimates for treatment effects for 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
Residual Std. Error	0.456 (df = 701)	0.452 (df = 693)	0.158 (df = 701)	0.157 (df = 693)	0.403 (df = 701)	0.403 (df = 693)

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

Table B10: 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
Residual Std. Error	0.349 (df = 701)	0.347 (df = 693)	0.386 (df = 701)	0.383 (df = 693)
Note:				*p<0.1; **p<0.05; ***p<0.01

Table B11: Regression estimates for treatment effects for "Do you think you/people like you can hold politicians and bureaucrats accountable for their actions? " All regressions include treatment indicator interactions with mean-centered block dummies.

	Dependent variable:	
	Respond "No "	
	(1)	(2)
T	0.100**	0.087**
	(0.043)	(0.042)
OBC		−0.019
		(0.049)
SCST		0.084
		(0.063)
Maratha		0.138***
		(0.040)
Muslim		0.056
		(0.058)
Kutcha floor		0.089
		(0.105)
Kutcha roof		−0.128
		(0.110)
From Mumbai		0.090**
		(0.041)
From same ward as apt		0.140***
		(0.053)
Constant	0.192***	0.063
	(0.028)	(0.048)
Observations	834	834
R^2	0.184	0.216
Adjusted R^2	0.030	0.057
Residual Std. Error	0.424 (df = 701)	0.418 (df = 693)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table B12: 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
Residual Std. Error (df = 701)	0.367	0.438	0.448	0.407	0.442	0.466
Note:						*p<0.1; **p<0.05; ***p<0.01

Table B13: 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
Residual Std. Error (df = 693)	0.366	0.439	0.445	0.406	0.440	0.465

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