

Home-price subsidies increase claim-making in urban India*

July 7, 2020

Abstract

Government transfers are common in low- and middle-income countries (LMICs). How does receiving transfers affect an important input into local governance, namely citizens' propensity to make everyday demands, or claims, of local government? Existing research on LMICs and the US suggests that this relationship could be positive or negative. I study the effects of subsidized home-prices in Mumbai, India through an original survey of winners and non-winners of program lotteries. Winning increases participants' reported claims to improve services and knowledge about municipal government, even among those who rent out the homes. Even where politics is described as a set of exchange-based relationships between officials and citizens, transfers can generate active citizenship by increasing the political capacity and changing the motivations of recipients. They also create interest groups at the local level, where such policies are not made, but implemented and experienced.

*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 Nilesch Kudupkar for assistance with data collection. I thank Pradeep Chhibber, Alison Post, Edward Miguel, and Joel Middleton for advice and mentorship. Finally and most importantly, I am indebted to the hundreds of survey and interview respondents who gave their time to this study.

Governments in low- and middle-income countries (LMICs) often deliver cash or in-kind transfers to a large number of citizens through welfare programs aiming to mitigate poverty and inequality. Well-known examples include Mexico’s *Progresa* conditional cash transfer and home price subsidies in Brazil and India. These programs reach millions. Conditional cash transfers, for example, had spread to 18 countries in Latin America and the Caribbean and reached about 135 million beneficiaries by 2010 (Stampini and Tornarolli 2012). The Indian central government reports, furthermore, that it gave away or sold at a highly subsidized price 10.5 million houses to rural citizens under *Pradhan Mantri Awas Yojana* (“The Prime Minister’s Dwelling Scheme”) from 2015-2020.¹ While the stated aim of such programs is to provide economic assistance to households, transferring income or wealth might also alter recipients’ *political* behavior, potentially leading to large shifts in the aggregate political landscape when such efforts are undertaken at scale. As a result, many have investigated the electoral returns to such transfers in LMICs (e.g. De la O 2013; Diaz-Cayeros *et al.* 2016; Imai *et al.* 2019; Manacorda *et al.* 2011; Zucco 2013).

Yet political activity extends beyond voting. I study the everyday demands, or claims, placed with politicians, bureaucrats, and brokers for state-provided goods and services. Claim-making forms a cornerstone of political participation in many countries and can occur even among those who engage in *quid pro quo* voting at election time (Auyero 2001; Jha, Rao, and Woolcock 2007; MacLean 2011; Kruks-Wisner 2018; Bussell 2019). When the demand for publicly provided goods and services far outstrips supply, direct or mediated claims of the politicians and bureaucrats can help generate access to the goods and services over which they have discretion. Understanding how transfers shape this claim-making is essential to understanding how government policies can attenuate or exacerbate inequality in access to government resources, particularly if the likelihood of becoming a beneficiary varies across the population.

Theoretical expectations generated by existing literature suggest that receiving transfers

¹These figures are found on the website of the Indian Ministry of Rural Development.

could either increase or decrease claim-making. Research from LMICs suggests that transfers may decrease the need to take part in such activity either by providing services themselves or increasing one’s capacity to procure private alternatives. Given that citizens’ dependence on state-provided resources decreases with wealth, citizens may simply exit the claim-making arena once they receive substantial government transfers (e.g. Bobonis *et al.* 2017 Brusco, Nazareno, and Stokes 2004; Calvo and Murillo 2004; Dixit and Londregan 1996; Hicken 2011; Larreguy *et al.* 2015; Nathan 2016; Ramìrez-Àlvarez 2019). In this case, local officials might be less accountable to citizens who cease to make claims, but they might also be better able to hear the voices of those who still need assistance.

On the other hand, transfers might *increase* claim-making by simultaneously affecting citizens’ capacity and motivations to participate in the political process. A literature on policy feedback from another context, namely the US and Europe (see Campbell 2012 for a review), finds that welfare policies have the potential to increase political participation among beneficiaries through many channels. They can make beneficiaries wealthier, thereby improving their self-perceived status and increasing their time horizons, both of which may facilitate political participation. They might also motivate beneficiaries to make *new* claims to protect or increase this wealth, even once other needs are fulfilled. I argue that when policy implementation is decentralized, protecting transfers also entails making claims for improvements at the *local* level, which has responsibility for program quality and where officials may be most visible to citizens.

It is difficult to empirically assess whether becoming a welfare recipient increases or decreases claim-making with existing data. While simple descriptive data show that welfare beneficiaries participate in local meetings where claims are made at a higher rate than non-beneficiaries, it is likely that being politically active actually helps one access transfers. Researchers have used the staggered or uneven rollout of programs to avoid such problems of endogeneity and identify causal effects on other outcomes, such as turnout and vote share. Yet claim-making is rarely measured in the administrative data upon which such studies rely.

I provide some of the first experimental evidence on the effects of welfare transfers on claim-making by studying a subsidized home-price program, a policy that is widespread in LMICs and high-income countries alike. The Indian government in particular has spent up to 1.65% of its GDP on such home subsidy programs, indicating a deep financial commitment to distributing wealth through this vehicle (Nayar 2009, 99). I use a policy experiment, or an experiment that occurs as a matter of government policy, to study the effects of receiving a highly subsidized apartment for purchase in Mumbai, India on claim-making.² The program is implemented through a lottery system, allowing causal identification of its effects among applicants, who tend to be middle class households. Winners can either move into the homes and enjoy a stream of housing benefits, or rent them out and use them as productive asset. To estimate the effect of home ownership on local claim-making, I located and surveyed 834 winning and non-winning applicants of multiple lotteries held between 2012 and 2014.

I estimate large and positive effects on reported claim-making. On average, winners are 14 percentage points more likely than non-winners 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. They are also 29 percentage points more likely to report attending neighborhood meetings where claims for community improvements are made and discussed. I measure knowledge and participation for the neighborhoods in which respondents *live*, regardless of whether or not they chose to move into the homes. Outcome means are similar across those who did and did not relocate.

The effects are accompanied by more hopeful attitudes about the future and an increased sense of status relative to authority figures, suggesting greater political capacity. Beneficiaries also exhibit an increased interest in local-level issues as demonstrated by reported reasons

²I use the term “policy experiment” here to distinguish the design from an experiment in which the researcher conducts randomization, and a natural experiment, which Titunik (2020) describes as “a research study where the treatment assignment mechanism (i) is neither designed nor implemented by the researcher, (ii) is unknown to the researcher, and (iii) is probabilistic by virtue of depending on an external factor.” The present study design fails to meet criteria (ii) and (iii).

for candidate choice in local elections, indicating new motivations for political participation. The effects occur in spite of increased satisfaction with local services among beneficiaries, making it less likely that they are driven mainly by dissatisfaction with the apartments and neighborhoods. The homes are also not subject to property tax during the time period of the study, leading a demand for services in exchange for taxes to be an unlikely mechanism for the effects.

The findings are among the first sets of causally identified effects of any government welfare policy on claim-making, a relationship for which research on political behavior in LMICs and the US present opposing predictions. Theoretically, the findings contribute to a sparse literature on political behavior in LMICs by highlighting political capacity and motivations to protect wealth as reasons for which citizens might make claims beyond demanding resources to meet their immediate needs. The findings also bolster theoretical expectations generated by US-based studies of policy feedback. While studies in this literature measure local-level political participation as evidence of increasing political capacity (e.g. Mettler and Welch 2004), I use the idea of decentralization to clarify *why* beneficiaries would be motivated to participate in local politics.

In one of the few LMIC-based studies of policy feedback, Maclean (2011) argues that the decreased provision of health and education services in African countries mobilized beneficiaries to voice concerns about poor levels of service provision on the ground. The present study further shows that transfer recipients may make *new* claims to further increase the value of their transfers even when they are delivered as promised.

Finally, the study adds a new context and set of dependent variables to the “embryonic” (Ansell 2019, 166) literature on housing policy that has, until now, generally focused on effects on national-level political participation and preferences in the US and United Kingdom. It also demonstrates that by conferring wealth and power upon citizens, government policies to subsidize homeownership can create politically active interest groups of homeowners. The actions of these interest groups can have both positive and negative externalities. As

such programs often reach the middle-class (rather than the poor), studying their effects is essential to understanding the perpetuation of power inequalities at the local level.

Welfare transfers and claim-making in India

Since its independence, the Indian government has enacted numerous policies dedicated to supporting its founders’ stated goals of poverty alleviation (Varshney 2014, 7). Table 1 first shows that a large portion of India’s population benefits from various sets of transfers. As its population is over one billion, even a food security program for the elderly from which only 0.2% of the population reportedly benefits 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, the Indian National Congress focused on Universal Basic Income programs as a key component of their platform (Safi 2019).

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

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

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

Given that these transfers reach so many citizens, learning about their effects is fundamental to understanding long-term political trends. How do they 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). As described in this literature, scarce resources 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 researchers have found that the use of these funds can be strategically targeted to win votes (Jensenius and Chhibber 2018). To better understand if citizens similarly trade votes for transfers, many study electoral returns to transfers made through various welfare programs (De la O 2013; Imai *et al.* 2019; Manacorda *et al.* 2011; Zucco 2013) in other countries.

Yet there exist important forms of political engagement that occur outside of election time. A literature on India and other LMICs focuses on citizens’ everyday interactions with local-level government, particularly efforts to access to goods and services such as jobs, roads, and lighting (Auyero 2001; Jha, Rao, and Woolcock 2007; Auerbach 2016; Bussell 2019; Kruks-Wisner 2018). Beyond simply voting for those who help them, individuals negotiate with intermediaries and place pressure on bureaucrats and officials to get what they need. This behavior can simply be described as “claim-making.” Following Kruks-Wisner (2018, 124), I define claim-making as “action—direct or mediated—through which citizens pursue access to social welfare goods and [public] services, understood as publicly provided resources intended to protect and improve well-being and social security.”

Claim-making alerts governments to deficiencies in service provision and helps citizens get what they need. When the demand for publicly provided goods and services is far greater than supply, politicians and bureaucrats can have a great deal of discretion over how these resources are allocated, and can help individuals move up in a very long queue. Even while much of the literature on local public goods provision highlights variables such as shared partisan identity or ethnicity as important predictors for when officials will exercise this discretion (see Golden and Min 2013 for a review), recent work has found that politicians in India may simply effectively deliver constituency service to those who approach them (Bussell 2019) and that participation in government meetings can support “deliberative democracy” and decrease the scope of elite discretion (Sanyal and Rao 2018). Moreover, the Municipal Corporation of Greater Mumbai (MCGM) has digitized its process for making and receiving

responses to claims for improvements to communities.³ This is part of a larger trend wherein several state and municipal governments in India have developed a transparent bureaucratic process to handle complaints about government infrastructure and services.⁴

Yet there is variation in the extent to which individuals will participate in claim-making. Demands for collective services in particular require organization and entail the problem of freeridership; 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, or claim, to anybody about neighborhood sanitation conditions.⁵ Moreover, according to a nationally representative survey conducted in 2011-2012, only about 30% of households report ever having attended a ward or village level meetings where claims, service delivery, and the use of development funds are discussed (India Human Development Survey- II (IHDS-II) 2016).

Studying the determinants of claim-making can help shed light on local-level inequalities in service provision and quality. 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 (Auerbach 2017).

How might becoming a welfare beneficiary affect participation in this cooperative behavior? Given the redistributive aims of many welfare programs, it is further important

³MCGM is also known as the Brihanmumbai Municipal Corporation, or BMC.

⁴Citizens can file claims with their local administrative units (wards) over the phone, in person, through an app, or online. The local administrative ward then assigns each claim with a number that one can use to track its progress as it is passed to the appropriate department.

⁵This survey was conducted by Lokniti CSDS in Delhi in 2012.

to understand the effects of government transfers on participation in claim-making to uncover their role in attenuating or exacerbating such inequalities. Kruks-Wisner (2018) finds that exposure to the state provides citizens with information and shapes their expectations of what government can provide, and is an important predictor of claim-making in rural areas. I further seek to understand how actual monetary transfers affect claim-making or claim-making among those who already have exposure to the state.

Hypothesis 1: Transfers may decrease claim-making

Existing literature on political behavior in LMICs suggests that receiving transfers may *decrease* participation in claim-making. Many argue that the utility of public resources decreases with income (e.g. Brusco et al. 2004; Calvo and Murillo 2004; Dixit and Londregan 1996; Hicken 2011; Nathan 2016). This implies that participation in clientelistic politics would decrease with income or benefitting from a program that increases one's resources. Indeed, in Mexico, Larreguy *et al.* (2015) find that insecure property rights create opportunities for political intermediation by municipal agents as residents seek access to titles, ways to provide proof of residence, or protection from eviction. They further find that a program issuing land titles to squatters reduces clientelistic voting for the municipal government as households' need for political intermediation disappears. Bobonis *et al.* (2017) similarly find that building water cisterns in drought-prone areas of Brazil decreases requests of politicians, especially among citizens likely to be in what they define as clientelistic relationships.

It follows that welfare transfers decrease the utility of claim-making among beneficiaries as well. Several programs themselves provide services to beneficiaries, thereby precluding the need for making claims for goods and services. For example, a slum rehabilitation program providing water and electricity connections could eliminate the need to organize to demand these same items. Wealth gains from government programs may also decrease incentives to participate in claim-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 claim-making truly is “poor people’s politics” (Auyero 2001) and welfare programs cause beneficiaries to exit this political arena. This would be in line with arguments made by Chatterjee (2004) and Harris (2006) that it is urban India’s poorest citizens who make claims on the state.

Hypothesis 2: Transfers may increase claim-making

Claim-making may be a function not only of need, but of other variables as well. 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 *motivations* to make new types of claims.

First, welfare transfers may increase beneficiaries’ *capacity* for action through what the policy feedback literature from the US calls “resource effects.” Campbell (2003), for example, finds that the receipt of Social Security and Medicare allows Americans to retire and participate in politics more as they age. There are reasons to believe that this effect might be particularly strong in India and other LMICs, where the average transfer recipient is likely to be even poorer than in the US. I have observed that non-winning applicants of the program I study appeared far too stressed to think about claim-making beyond their most immediate needs. Scholarship in development economics (see Haushofer and Fehr 2014) has found that poverty can create stress and lead to short-sighted behavior. Increasing household wealth could decrease present bias and increase the mental bandwidth to participate in claim-making. Similarly, the resources may also allow households to prioritize other “higher” items on Maslow’s (1943) hierarchy of needs such as belonging and self-esteem, both of which may be fulfilled by political participation. 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 claim. This mechanism is particularly important in extremely hierarchical societies; in India, for example, the state is frequently described as being indifferent to the needs of the poor or lower caste (Ahuja and Chhibber 2012).

Second, welfare beneficiaries may be particularly *motivated* to protect their newfound wealth by improving levels of service provision. Those who study the US and Europe argue that benefitting from government social welfare can “create material incentives for mobilization” (Mettler and Soss 2004, 62). It can encourage political participation to ensure either the continued or increased receipt of program transfers (e.g. Campbell 2012; Mettler and Soss 2004; Pierson 1993). Welfare programs may thus induce new claims even if they suppress others.

Why might increases in claim-making be particularly visible at the *local* level, where policies are not made? Studies from the US measure increases in local-level political participation as evidence of increasing political capacity among welfare beneficiaries (e.g. Mettler and Welch 2004), but it is possible that transfers *motivate* local-level participation as well. While many welfare programs in India are crafted at the state or national levels, local governments are responsible for their implementation in places that have seen the devolution of administrative responsibilities to local government. India’s National Rural Employment Guarantee Scheme (NREGS), for example, guarantees all rural households 100 days of wage labor on infrastructure projects. It lays out an important role for *gram sabhas*, or deliberative bodies of eligible voters in a village: they are the arenas for citizens to provide recommendations on priorities for the local public works and to conduct audits of completed and ongoing labor projects. Jenkins and Manor (2017, 166-181) thus argue that NREGS increases political capacity and the “assertion of citizenship” among Indian villagers as they participate in village-level meetings in order to shape the nature of projects and determine who gets access to programs. The precise role of local government in program implementation, of course, will vary with the program in question.

Furthermore, in places with political decentralization, individuals may be more likely to make transfer-related claims on the local officials who are more visible or accessible to ordinary citizens than officials at higher levels (Corbridge *et al.* 2005).⁶ As a result, local

⁶See Bussell 2019 for an explanation of why motivated members of minority groups may, however, seek out higher level officials.

officials may both appear responsible for the implementation of welfare benefits and naturally be the first individuals to whom individuals make claims related to their welfare benefits.

Preliminary survey evidence

A preliminary test of these opposing hypotheses can be conducted using data from IHDS-II. Table 2 shows that beneficiaries of various Indian welfare programs report greater attendance of local public meetings wherein they make claims on local government than non-beneficiaries. But of course, this pattern could simply be a result of fundamental differences between program beneficiaries and non-beneficiaries, rather than any effect of the programs themselves. It is highly plausible that program beneficiaries are simply more politically active than non-beneficiaries in the first place. I thus use a policy experiment to causally identify the effects of one of the policies presented in Table 2, namely housing subsidies.

Table 2: 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.

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
Food security scheme (elders)	0.28	0.29	0.84
Sanitary latrines	0.44	0.28	0.00
Subsidized loans (farmers)	0.43	0.28	0.00
Rural housing subsidies	0.44	0.28	0.00
Rural employment guarantee	0.44	0.23	0.00

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

¹ P-value from a two-tailed t-test.

The policy experiment

Housing subsidies have been implemented in many cities globally, including those in low-, middle-, and high-income countries, yet their effects on recipients' local-level political behavior effects remain virtually unstudied. The specific program studied here provides households with a government-constructed home at a highly subsidized price. Households can enjoy transfers even without moving; they can rent out the homes and consume the asset as

a stream of payments (rental income net of mortgage) instead. Such programs can be found all over the world, including in cities in Ethiopia, Kenya, Brazil, and Uruguay. They have been spearheaded in all major Indian cities by state level development boards to build low-income housing. Moreover, in 2015, India’s federal government announced a plan, Pradhan Mantri Awas Yojana, to build 20 million affordable homes by 2022.⁷

As discussed, these wealth gains might increase or decrease claim-making in urban India. Furthermore, this wealth transfer is made through a vehicle that has been found to be associated with high rates of local-level political participation. Those who study the effects of homeownership on political participation in the US, for example, find that homeowners are more likely than non-homeowners to make claims to improve communities and protect the value of the asset (e.g. Portney 1991; Dear 1992; Fischel 2001; Schively 2007; Hankinson 2018; Marble and Nall 2018; Einstein *et al.* 2019). Yet it is not clear whether the behavior exhibited by homeowners is a function of *other* variables correlated with homeownership, and if simply facilitating homeownership for a group of non-homeowners can generate this behavior. Furthermore, this work focuses mainly on the US, where local political participation has generally been described as increasing, rather than decreasing, with wealth (Verba *et al.* 1995), and it is unclear whether the findings will extend to India.

This study examines the effects of a program in Mumbai. The Mumbai Housing and Area Development Authority (MHADA)⁸ runs subsidized home-price lotteries on land obtained for free from the city’s dismantled textile industry. This land has been earmarked specifically for “social” projects and cannot be used for other purposes (Madan 2016). Recipients cover the costs of marketing and construction. A number of apartments are allocated to economically weaker section (EWS) and low-income group (LIG)⁹ urban residents who 1) do not own housing, and 2) who have lived in the state of Maharashtra for at least 15 continuous years

⁷This program is an extension of what used to be known as Indira Awas Yojana, which provided mostly rural homes.

⁸The agency is a subsidiary of the Maharashtra Housing and Area Development Authority that uses the same acronym.

⁹Members of the EWS earn up to 3,200 USD/year. Members of the LIG earn up to 7400 USD/year.

within the 20 years prior to the sale. In 2012 and 2014, the EWS group could purchase a 180 square foot apartment for about Rs. 1,500,000 (about 23,500 USD at the time), while the LIG group could purchase a 320 square foot apartment for about Rs. 2,000,000 (about 31,000 USD).

Table 3: Lottery apartments included in the study.

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

¹ In square feet. Refers to “carpet area”, or the actual apartment area and excludes common space.

² Price at which winners purchased the home in INR with the cost stated in the lottery year. In 2017, about 64 INR=1 USD.

³ Average sale list price of a MHADA flat of the same square footage in the same community. Data collected from magicbricks.com in 2017.

⁴ In INR with the cost stated in the lottery year. Includes application fee of Rs. 200.

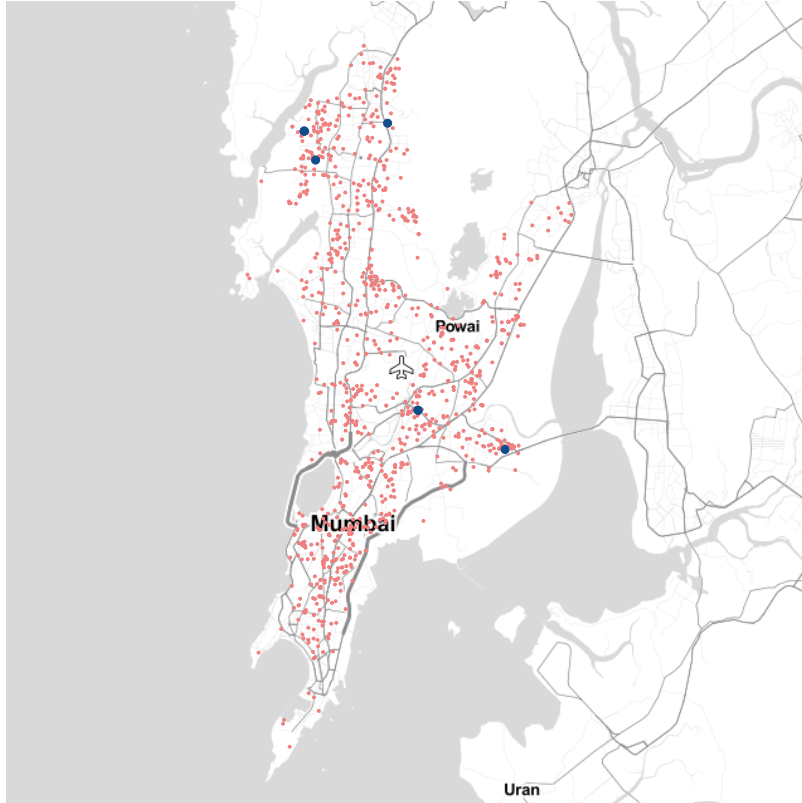
The homes were sold at a “fair price” that was 30-60% of market prices. Table 3 shows winners could eventually hope for large gains; 3-5 years after the lottery, the difference between the apartment purchase price and list price for older MHADA apartments of the same size in the same neighborhood appears to lie anywhere between Rs. 661,700 (about 10,300 USD at 2017 conversion rates) to Rs. 2,869,015 (about 45,000 USD). 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.¹⁰ Winners had access to loans from a state owned bank and most took out 15 year mortgages. While the downpayment and mortgage left this program out of the reach of many of the city’s poorest residents, it gave eligible lower middle-class families without property the opportunity to purchase heavily subsidized apartments. Resale of the apartments is not permitted until 10 years after purchase, but households can put the apartments up for rent. Half of the households in my sample have done so. Households do not pay taxes on their dwelling for five years after they move in, or within the time period

¹⁰Prices and downpayments vary by year and apartment location.

of this study.

Figure 1 shows the location of the 2012 and 2014 EWS and LIG MHADA apartment buildings and households in the sample at the time of application. Households were permitted to choose the building for which they submitted an application.

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



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

Data collection

I estimate treatment effects for outcomes measured through in-person household surveys of both winning (treatment) and non-winning (control) households. I aimed to interview 500 treatment and 500 control households. For the 2012 and 2014 lotteries, MHADA provided 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 stratum, and I accessed a total of 1,862 addresses.

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

I next located the addresses of these households on Google Maps. Addresses that were incomplete (42), outside of Greater Mumbai (611), or could not be mapped (146) were removed from the sample. This left 531 and 532 control and treatment households, respectively. Table SI.2 shows that even after this mapping procedure, the sample included 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.¹¹

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.¹² The addresses and phone numbers provided by MHADA constituted the contact information for households at the time of application. We contacted non-winners at these addresses. In cases where they had moved away, we asked neighbors 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. We thus first contacted lottery housing cooperative societies to determine which of the winners were living at the apartments. We approached owner-occupiers at the lottery apartments, and approached landlords at the addresses listed on the application using the procedure developed for non-winners.

In all cases, we attempted to speak to the individual who had filled out the application for the lottery home. In the case that a child had applied for the home, enumerators were instructed to speak to the family’s primary earner. In my sample, 78% of respondents had filled out the application themselves.

Balance

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 the reasons for attrition can be found in Table SI.4. I do not see evidence of differential rates of contact for control and treated units; the p-value for the difference in proportion contacted is 0.8. Balance tests for fixed or baseline characteristics among the contacted

¹¹Overall, 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 SI.3 indeed shows that there are relatively fewer Scheduled Tribe members and more General Population (e.g. Forward Castes) members in the mapped sample than in the full sample provided by MHADA.

¹²More information about the organization can be found here (<http://www.pukar.org.in>).

sample can be found in Table 4. I generally see balance on all covariates. Importantly, each treatment condition includes a fairly equal proportion of those belonging to the *Maratha* caste group, a politically dominant group in Mumbai and Maharashtra more generally, attenuating concerns that politically favored groups may have been more likely to win. I further conduct an omnibus test by regressing the treatment indicator on the covariates (Table SI.5) and calculate a heteroscedasticity-robust Wald statistic for the hypothesis that the coefficients on all of the covariates (other than stratum dummies) are zero. The p-value for this omnibus balance test is 0.39.

Table 4: Balance tests on household characteristics

Variable	Control mean ¹	Treatment effect ²	sd	Pr(> t)
OBC ³	0.150	-0.021	0.035	0.543
Scheduled caste/tribe ⁴	0.080	-0.018	0.026	0.499
Maratha ⁵	0.295	0.018	0.045	0.690
Muslim	0.090	0.006	0.029	0.852
Rough floor ⁶	0.031	0.028	0.019	0.136
Rough roof ⁶	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

¹ Means for non-winning households.

² Coefficient in an OLS regression of each variable on an indicator for winning the lottery. Regressions include an interaction with centered stratum indicators for randomization groups and HC2 errors.

³ Other backward class caste group members, or non-scheduled caste groups for which there are quotas.

⁴ Historically disadvantaged members of society.

⁵ A dominant group in Mumbai and Maharashtra more generally.

⁶ “Rough” here is a translation of the word “*Kutcha*.” Measured at time of application through recall.

I also test for evidence of selection into the mapped treatment group by electoral ward. A higher likelihood of certain ward members to be treated would indicate that individuals from certain locations or with certain political representatives are more likely than others to win the lottery. I conduct regressions of the treatment indicator on the state and municipal ward membership indicators and calculate a heteroscedasticity-robust Wald statistic for the hypothesis that the coefficients on all of the indicators (other than stratum randomization dummies) are zero. The p-values for regressions on state and municipal ward membership are 0.35 and 0.46, respectively.

Descriptive statistics

Although these households fall into the EWS and LIG income categories for the housing lottery, a summary of the assets, housing quality, education levels, and tenure status of the control group reveals that they should not be considered among the lowest income groups in the city (Table 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 in rental housing, homes with large, or joint, families, or self-constructed homes to which they have no title. Many live in Mumbai chawls, or large buildings with shared taps and cheap, single room apartments. I thus describe the sample as middle class.¹³

Estimation

I estimate effects of winning the lottery within the contacted sample on a number of survey-based outcomes. I follow my pre-analysis plan and estimate the treatment effect β , on i respondents. In the following equation, Y_i is the outcome, T_i is an indicator for treatment (winning the lottery), and $C_1...C_j$ is the group of fixed (or pre-treatment) covariates used for randomization checks, and ϵ_i is an error term. Given that randomization happened within strata, I include a set of centered dummies, $S_1...S_l$ for each. Following Lin (2013), I allow for heterogeneous effects within the strata by interacting the centered stratum dummies with the treatment indicator:

$$Y_i = \alpha + \beta T_i + \sum_1^j \gamma_j C_j + \sum_1^l \omega_l S_l + \sum_1^l \eta_l (T \times S_l) + \epsilon_i \quad (1)$$

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

¹³This description is corroborated by an interview conducted with the commissioner of the Mumbai Metropolitan Regional Development Authority, who saw the main beneficiaries the housing program to be lower-middle class households (Madan 2016).

Table 5: Summary of control group characteristics.

Variable	Control mean ¹ (SD)
<i>Household Assets</i>	
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)
<i>Housing quality</i>	
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)
<i>Education and labor</i> 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)
<i>Tenure status</i>	
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 questions.

² Labor outcomes measured for the family's principal earner.

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.

(8% of treated units did not purchase homes), I simply conduct an intent-to-treat (ITT) analysis.¹⁴ β can thus be interpreted as a weighted average of stratum-specific intent-to-treat effects. Following Imbens and Kolesar (2015), I compute standard errors using the HC2 estimator (MacKinnon and White 1985). Given the large number of hypotheses being tested, I make Benjamini-Hochberg (1995) corrections for the false discovery rate within “families” of outcomes.

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

Results: claim-making and knowledge

I use the household surveys to measure reported behavior and attitudes 3-5 years after the lottery was held. Variable definitions and control means for the outcomes of interest are reported in Table 6. All of the questions for the main results were phrased to understand winners’ actions in the places in which they *live*, whether or not it is in the lottery apartments.

Treatment effects are summarized in Figure 2. I first asked how often respondents participate in both individual and group petitioning of politicians and bureaucrats for something benefitting the community. An effect on claims made individually would indicate an effect on one’s willingness to approach officials alone, regardless of the behavior of others in the community. An effect on claims made as part of a group would indicate an effect on one’s interest in participating in community-based collective action. I find that winning increases both. Lottery winners are 14 and 11 percentage points more likely to report making claims individually and in groups, respectively, for “something” benefitting their communities. During qualitative interviews, I found that these claims were often related to problems with water

¹⁴This choice should typically bias treatment effects to zero.

Table 6: Variable definitions and control means.

Label	Survey question	Response options	Coding	Control mean
Attending local area meeting	In the last month, has anybody in the HH attended a local area meeting in the neighborhood in which you live?	Yes, No, Don't Know	1 if Yes, else 0	0.36
Make claims individually	How often do you individually petition government officials and political leaders for something benefitting the community in which you live?	Often, Sometimes, Rarely, Never	1 if Often or Sometimes, else 0	0.45
Make claims in group	How often do you participate in group petitions of government officials and political leaders for something benefitting the community in which you live?	Often, Sometimes, Rarely, Never	1 if Often or Sometimes, else 0	0.41
Correct party for corporator	What is the party of your <i>nagar sevak</i> ?	Open ended	1 if correct, else 0	0.31
Correct name for corporator	What is the name of your <i>nagar sevak</i> ?	Open ended	1 if correct, else 0	0.02
Correct name for corporator in admin. ward	What is the name of your <i>nagar sevak</i> ?	Open ended	1 if named individual is a corporator in admin ward, else 0	0.14
Would never consider leaving Mumbai	Do you think you will leave Mumbai in the future?	Would never leave, Might leave in future, Will definitely leave	1 if Would never leave, else 0	0.77
Happy with financial situation	How happy are you with the financial situation of your household?	Happy, Neither happy nor unhappy, Unhappy	1 if Happy, else 0	0.31
Under control of local leaders	Do you/people like you need to listen to what leaders in the area say?	Yes, No, Don't know	1 if No, else 0	0.19
Children will have better lives than them	Do you expect your children to have better lives than you?	Yes, No, Don't Know	1 if Yes, else 0	0.56
Reasons for voting	What factors did you consider when you voted in the last (2017) municipal elections?	Open ended, enumerators selected all applicable from list	1 if [reason] chosen, else 0	Party, 0.30; Ethnicity, 0.06; Neighborhood, 0.42; Financial, 0.26; Policy preferences, 0.20; Country 0.20; Mumbai, 0.05
Satisfaction with services	How satisfied are you with the [service type] in the neighborhood in which you live?	Satisfied, Neither satisfied nor dissatisfied, Dissatisfied	1 if satisfied, else 0	Electricity, 0.83; Garbage, 0.71; Sanitation, 0.68; Water, 0.77; Law, 0.70; Roads, 0.64

scarcity and encroachment by hawkers and squatters.

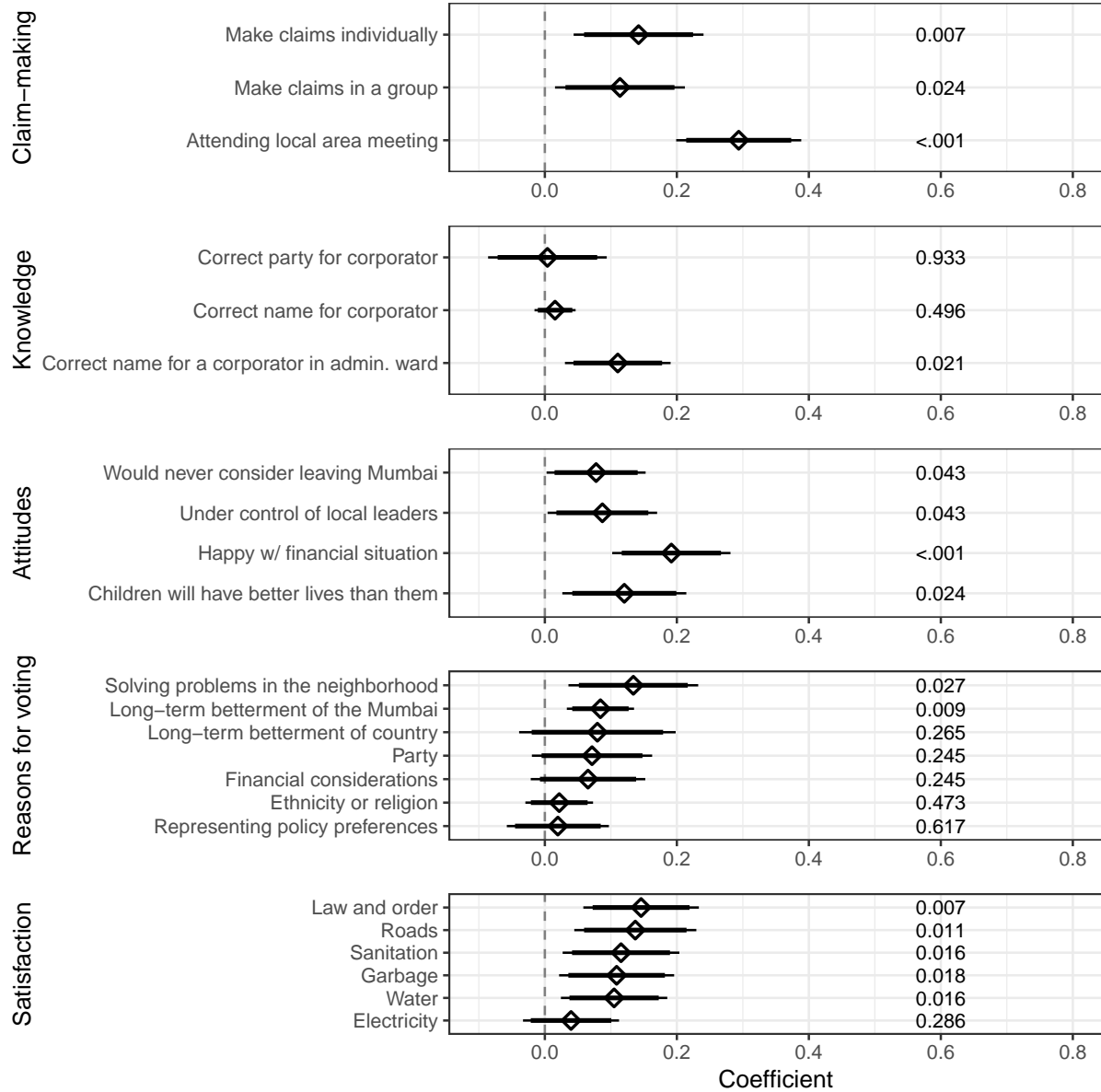
To better learn how respondents come together as groups, I also measure effects on the extent to which respondents report attending local area meetings to discuss improving their communities with their neighbors. These meetings are similar to the local development meetings described by Auerbach (2016). The range of issues being discussed is enormous and includes water supply, sidewalk construction, water leakages in apartment buildings, local safety, and, of course, the occasional birthday party. 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. Winners are about 29 percentage points (over a base of 36%) more likely than non-winners to report that someone in the household has attended a local area improvement meeting in the last month. This effect is substantively large, but the smaller effect on group-based claim-making suggests that local meeting attendance only sometimes translates into claims.

In addition to measuring changes in reported behavior, I also tested respondents' knowledge of local politics, with the assumption that greater local political engagement leads to greater knowledge. An individual who has asked a politician for community improvements is more likely to know the name of the politician than one who has not. The election of 227 ward representatives, or corporators, to the MCGM occurred in February 2017, roughly six months prior to the survey. Four parties emerged with a plurality of seats. 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. I placed households in wards using baseline addresses for non-winners and winning landlords, and using lottery apartment addresses for winning owner occupiers.¹⁵

In Mumbai, electoral wards are grouped into 24 larger administrative wards (Figure SI.1). Knowledge about electoral wards is 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

¹⁵GIS maps for Mumbai's electoral wards were provided by the Urban Design Research Institute of Mumbai, India(<http://www.udri.org>).

Figure 2: Treatment effects for main outcomes and mechanisms.



Bars show 90% and 95% confidence intervals. P-values using Benjamini-Hochberg corrections for the false discovery rate are shown on the right. Variable definitions and control means for the main outcomes of interest are reported in Table 6. Full regression output with and without covariate adjustment available in Tables SI.6-SI.12.

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

Yet is the administrative ward office, not the electoral ward office, that is responsible for handling claims. For most Mumbai residents, the term “ward” refers to administrative wards, not electoral wards.¹⁶ Control group members are over seven times more likely to

¹⁶As a quick check of this claim, I asked 15 individuals on the street in different administrative wards about

correctly name a corporator from their administrative wards than give the correct name of the corporator for their electoral wards. Winners are 11 percentage points, or almost 80% more likely than the control group, to be able to provide a correct response. 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.

Table 7 shows that outcomes for landlords and owner occupiers are similar, especially when compared to the control group. The behavior among owner-occupiers is particularly surprising because they have relocated and may have been removed from their social networks, a phenomenon Gay (2012) finds leads to decreased political participation among beneficiaries of the Moving to Opportunity program in the US.

Table 7: Mean outcomes for landlords, owner-occupiers, and the control group.

	Landlords	Owner-occupiers	Control group
Individual claims	0.52	0.61	0.45
Group claims	0.52	0.54	0.41
Meeting attendance	0.59	0.65	0.36
Can name corporator in admin. ward	0.25	0.29	0.14

Mechanisms: attitudes, status, and motivations

One channel through which government transfers might lead to increased claim-making is by making recipients feel wealthier and extending their time horizons. I estimate that winners are 19 percentage points (over a base of 31%) more likely than non-winners to claim to be “happy” with the financial situation of the household (Figure 2). 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 longer time horizons. These findings are complementary to research (e.g. Fernald *et al.* 2008; Haushofer and Fehr 2014; Haushofer and Shapiro 2016)

their ward membership. Four respondents did not know which ward they belonged to, and the remaining 11 gave the names of their administrative wards.

that has found that income shocks can increase psychological well-being, happiness, and time horizons. These effects may reduce the cognitive cost of action. Indeed, a winning respondent in his fifties claimed he felt less stressed about his childrens’ 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 transfers may also alter a beneficiary’s 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 status relative to local officials, or the potential effectiveness of their claim-making. This survey question was originally phrased as “Do leaders in the area need to listen to what people like you say,” but I inverted the phrasing because during survey piloting, non-winners frequently described being bullied or intimidated at ward offices. I observed that respondents usually fell into two categories: those who appeared to be afraid of authority figures, and those who did not. The intervention appears to have shifted winners into the latter category. These effects are complementary to beneficiaries’ near universal claim in qualitative interviews that they “now have some standing in society.” The intervention may enable citizens to actually make claims on elected officials they had once feared.

Finally, welfare programs can create interest groups of beneficiaries who are particularly motivated to work together to protect their transfers. To illustrate this mechanism, I also show effects on stated motivations for another form of local political participation, namely voting in local elections (Figure 2).¹⁷ I asked respondents how they made their choices in the most recent municipal election. Respondents were not prompted with options and all of their responses were selected by enumerators from a multiple-choice list. I attempted to

¹⁷ I do not detect a treatment effect for reported voting in past municipal or state elections (Figure SI.2). 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 SI.13) do show high rates of reported voting for the control group.

make an exhaustive list of multiple-choice options based on responses to a pilot survey I conducted in March 2017. Relative to non-winners, I estimate that winners are about 13 percentage points more likely to state neighborhood problems as a reported reason for voting, and 8 percentage points more likely to report improving Mumbai as a reason for voting, thus supporting increased interest in local problems as a mechanism for my findings.¹⁸

The behavior of landlords further illustrates their motivation to protect the value of the lottery apartments. Even though landlords do not benefit from the quality of life improvements that may result from changes in the community, they will benefit from home value appreciation that may occur as a result of improved neighborhoods.¹⁹ I asked landlords if they had attended local area improvement meetings in the neighborhood of the lottery home (as opposed to where they live) in the past month.

Fifty-five reported that they did so “Often” or “Sometimes,” a figure very similar to their rates of reported attendance at meetings in the neighborhoods in which they live, and 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 six or more days a week, and the travel time (one way via transit) to the lottery building neighborhoods takes 1.1 hours on average.²⁰ The percentages of meeting attendance may actually underestimate participation because some landlords also communicate their wishes through WhatsApp or by phone.

In addition to increased political capacities, evidence from qualitative interviews suggests that landlords’ participation in claim-making in their *own* communities arises from develop-

¹⁸Those 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.

¹⁹According to the survey data, winners are, in fact, aware of the property values and that they can change and even increase over time: 91% of winning respondents are aware that the value of their properties had increased since purchase, 46% can place a value in INR on this increase, and 93.5% expect the value of the property to increase further in the future.

²⁰Travel 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.

ing new habits surrounding the lottery apartments. One respondent, for example, said that “we just pay attention to what is happening with the BMC [MCGM].” Another respondent explained that after visiting some MCGM ward offices, she had developed a new interest in how the municipal government works. “I now just like to know what is going on, even where I live,” she said.

Alternative explanations

Here, I consider alternative explanations for the effects seen above, particularly those that are unique to this specific intervention and therefore threaten the external validity of the study. It is possible, however, that there are other mechanisms through which transfers affect claim-making more generally, and it is beyond the scope of this paper to provide evidence for each and every one.

There are a number of alternative, and possibly simpler, ways to interpret the results shown in this paper. They could be a mere mechanical result of the fact that winners need to interact with local government to obtain the apartments. These outcomes, however, are measured 3-5 years after individuals win the lottery. They do not reflect one-time actions, but rather longer-term changes to behavior. It is also unlikely that effects are driven by a demand for services in exchange for a property tax, as the program does not require winners to pay property taxes within the time period of the study. The intervention, unlike land-titling programs for slum-dwellers, also does not generate a shock to property rights, as the target group itself is lower-middle class and tends to have secure property rights.

It is possible that effects are driven by disgruntled members of the control group who no longer want to participate in local politics after failing to win. This seems 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 participate in the lottery repeatedly, much like someone in the US can

repeatedly buy lottery 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.

Increased participation may be driven by relocation and the increased existence of informal institutions for, or norms surrounding, claim-making in the lottery apartments. Recall, however, that outcomes are measured for the areas in which respondents live; similar rates of claim-making among landlords and owners (Table 7) make this alternative explanation less likely. Meeting participation among owner-occupiers is slightly higher than landlords, yet the difference in meeting attendance between landlords and the control group is much higher still, indicating that the availability of meetings at lottery apartments is not solely responsible for the results. Indeed, I observed that local area improvement meetings were ubiquitous in the control group neighborhoods I visited.

The results may also arise from dissatisfaction with service delivery in the new apartment locations. Owner-occupiers experiencing worse services in the new buildings could organize to demand improvements in their new communities. I look at responses to questions that ask if individuals are satisfied with services in the neighborhoods in which they live, and actually see greater satisfaction with the delivery of most services among lottery winners (Figure 2). Much of the claim-making in lottery buildings surrounded issues with which control group members remained unconcerned, particularly illegal fruit and vegetable vendors on the streets. This suggests that the housing subsidies generated *new* claims that non-beneficiaries may not have been sufficiently able or motivated to make.

Moreover, I observed that the lottery apartment buildings are well maintained, particularly in comparison to the areas where the members of the control group live. The roads are free of garbage and human waste, the complexes have several overhead water tanks apiece, and electricity service is regular. The all have high walls that prevent the visibility and entry of those on the street. In fact, rather than participation being driven by poor service delivery in the new apartments, it is possible that landlords seeing better services in

the apartment buildings had greater expectations of government as a result and organize to demand improvements in their baseline communities. As argued by Kruks-Wisner (2018), the visibility of effective service provision by the government may be another mechanism by which transfers lead to claim-making more generally, and is worth exploring in future research.

Conclusion

Low- and middle-income countries are sites of rapid innovation in policies to mitigate poverty and inequality, particularly wealth and income transfers in the form of pensions, conditional cash transfers, and subsidies for various items. Existing literature yields ambiguous expectations about the effects of income and wealth transfers on claim-making, a form of everyday political participation. I exploit subsidized home-price lotteries in Mumbai and show that benefitting from this large wealth transfer leads individuals to increase their reported participation in claim-making and knowledge of local government. I argue that the results arise from increased political capacity due to beneficiaries' newfound wealth, and changing motivations to participate in claim-making due to their desire to protect this wealth. Beneficiaries indeed report greater happiness with their finances, longer time horizons, increased perception of their own status, and greater interest in local issues when making choices at election time. While I study the effects of one policy only, other wealth and income transfers that effectively boost beneficiaries' political capacities and change their motivations possibly have similar effects.

The behavior observed among lottery winners is similar to that described by research on homeowners in the US who participate in local politics to defend their property values (e.g. Portney 1991; Dear 1992; Fischel 2001; Schively 2007; Hankinson 2018; Marble and Nall 2018; Einstein *et al.* 2019). The US-based literature focuses on a “not-in-my-backyard” (NIMBY) behavior, or the negative externalities of homeownership. Owners defect from land use policies that are of general benefit to a municipality because they impose costs

(in the form of land depreciation or externalities such as crowds and pollution) on the very local communities in which individuals own homes. In the context described in this study, homeownership and a desire to protect property values potentially has positive externalities, particularly in communities with mixed housing tenure and low levels of baseline service provision. Nevertheless, homeownership can have negative NIMBY-type externalities in urban India as well. Many have documented the urban middle class’s attempts to clear slums and “beautify” cities; such actions likely share the same underpinnings as NIMBYism in that they benefit homeowners at the expense of others in the city (Fernandes 2006; Heller, Mukhopadhyay, and Walton 2016).

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

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