Do Politicians Discriminate Against Internal Migrants? Evidence from Nationwide Field Experiments in India

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February 25, 2018

Abstract

Rural-to-urban migration is reshaping the economic and social landscape of the global south. Yet many internal migrants struggle to integrate into destination cities. We conduct two audit experiments to test whether urban politicians discriminate against migrants in providing key constituency services. Signaling that a hypothetical citizen is a newcomer to a city, as opposed to a long-term resident, causes politicians to be significantly less likely to respond to a request for help. Evidence from follow-up experiments suggests that politicians' beliefs about migrants' low propensity to participate in urban elections underlie this representational gap. The results advance our understanding of the drivers of political discrimination against internal migrants. They also inform policy debates about how to improve migrant welfare.

Introduction

In recent decades, cities and towns across the global south have witnessed explosive population growth. This has been spurred in significant measure by rural-to-urban migration (Bell and Charles-Edwards 2013). While internal population movements help drive economic development, many migrants struggle to integrate into destination cities. Slum settlements—where migrants disproportionately reside—are characterized by poor housing quality and minimal infrastructure (Auerbach 2016). Migrants suffer from inadequate education and healthcare facilities (Coovadia et al. 2009). Owing to limited police protection, they are routinely victims of harassment and violent crime (Afsar 2003).

These problems have been well documented worldwide. Yet we lack a firm theoretical and empirical grasp on why they persist. In this paper, we address a potential explanation: systematic political neglect. Elected representatives serve as essential providers of core services in "patronage democracies" (Chandra 2004) Hence their willingness (or otherwise) to attend to migrant concerns may have a powerful effect on this group's welfare. But do urban politicians in fact discriminate against migrants from other parts of the country when providing basic services? If so, on what basis?

We develop and test competing theories regarding politicians' treatment of internal migrants in rapidly urbanizing democracies. For elected elites deciding how to optimally allocate scarce fiscal and political capital, new waves of migrants pose a dilemma. On the one hand, politicians in receiving cities face strong electoral incentives to play the "nativist" card, withholding benefits from migrants perceived as putting strain on jobs, public goods, and social relations (Weiner 1978; Katzenstein 1979). Conversely, politicians might see migrants as a fresh source of electoral support. Internal migrants have the right to vote in destination-area elections. Thus politicians should treat migrants and natives equally. For this argument to hold, however, incumbent politicians must believe that natives and newcomers participate in urban elections at similar rates. Recent migrants may be poorly informed about about local politics and voting logistics—factors that may reduce their likelihood of casting ballots in urban elections. If politicians believe this to be the case, they will face fewer incentives to cater to migrant interests.

To assess which of these logics is operative, we conducted two nationwide field experiments in India. We compiled lists of municipal councilors in 28 Indian cities. Municipal councilors act as intermediaries between citizens and the state. They are responsible for delivering a wide range of constituent benefits. At the same time, they enjoy considerable discretion in deciding how to target assistance (Oldenburg 1976; Berenschot 2010).

We subject councilors to an unobtrusive audit. In the first experiment, we mailed short letters to 2933 councilors. In the letters, we randomly varied the identities of fictitious citizens, and the problems for which they were requesting help. The main randomized manipulation involved signaling long-term residence in the city versus recent migration to the city from a different Indian state. The letters asked the councilors to give the citizen a callback at a number provided. We estimate that "native" requesters are 24.1 percent more likely than otherwise identical migrant requesters to receive a callback from their local councilor, substantiating the existence of anti-migrant discrimination.

What explains this representational gap? We performed an additional "mechanisms" experiment to find out. We sent text messages asking for help to the original sample of councilors. But this time we manipulated requesters' *political* attributes. We primed (a) whether the citizen claimed to be registered to vote in the councilor's electoral ward; and (b) whether they wrote that they voted for the incumbent councilor previously. "Registered" migrants were 32.4 percent more likely to receive callbacks compared to nominally unregistered migrants. Meanwhile, politicians' rates of response to registered migrants and natives were statistically indistinguishable. In a final survey, councilors viewed hypothetical migrants as 46.1 percentage points less likely to be registered to vote compared to natives. Taken together, the evidence paints a clear picture. Beliefs about migrants' low turnout propensity—resulting mainly from their (perceived) low registration levels—explain this group's relative inability to access key services from urban politicians.

We make three contributions. First, we identify an overlooked type of political inequality. The challenges faced by international immigrants have been extensively documented in recent work (Adida, Laitin, and Valfort 2010; Dancygier 2010; Hainmueller and Hangartner 2014). The analogous yet fundamentally distinct problems faced by internal migrants, meanwhile, have escaped scholarly attention. The world's urban population is set to increase by 2.5 billion people by 2050; 90 percent of that growth is expected to occur in Asia and Africa. Smoothing this group's integration into cities is an urgent task (United Nations 2014).

Second, apart from estimating the magnitude of anti-migrant discrimination, we also pin down a mechanism to explain it. The findings imply a remedial intervention. Encouraging recent migrants to (re)-register to vote, and informing politicians that this registration process is underway, should increase politicians' responsiveness to migrants, with knock-on effects for welfare.

Third, our findings shed light on other biases plaguing democracy. We also varied the gender, religion, and skill level of requesters. Politicians display a marked preference for citizens from better economic backgrounds, and for those belonging to the majority religion (Hinduism). The results are pessimistic about the prospects for marginalized groups to advance under representative institutions. More practically, the findings underscore the need to reduce the discretionary role played by individual politicians in distributing public services.

Internal migration: the politician's dilemma

A variety of citizen traits affect how elected elites target their resources and effort. Ascriptive identities like race and ethnicity matter (Butler and Broockman 2011; McClendon 2016; White, Nathan, and Faller 2015). So do more malleable types of kinship like partisanship and religion (Dunning and Nilekani 2013; Adida, Laitin, and Valfort 2010). Should we expect migrant/native status to give rise to differential responsiveness too? Prior literature has explored politicians' behavior with regard to international immigration. Inter alia, it focuses on legislative position-taking on restrictive immigration policy, the emergence of nationalist parties, and the usefulness of anti-immigrant rhetoric as a wedge issue for elites trying to knit cross-class coalitions (Dancygier 2010; O'Rourke and Williamson 1999; Pettigrew 1998; Schain 2006). Yet internal migrants—those who relocate within a country—differ crucially from international ones, because they possess the right to vote wherever they move.[1] As we now elaborate, this difference suggests that office-seeking politicians may face a dilemma when it comes to dealing with migrants: court local, "native" votes by working against migrants, or court migrants so as to win their votes.

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[1] Note, however, that some countries like Sweden and the United Kingdom do allow immigrants to vote in certain local elections.

Courting native votes

Elected officials are usually thought to promote the preferences of the social coalition that got them elected (Downs 1957). If they do not, they risk being punished at the next election. In urban contexts, local voters often worry about the impact of influxes of migrants. Politicians beholden to the "native" population for support may therefore channel nativist concerns in the actions they decide to take, and in choosing whom to help.

Why might long-term city residents be averse to in-migration? There are two broad classes of explanations. Cultural-based accounts posit that natives are wary of cultural dilution, and thus prefer ethnically "in-group" as opposed to "out-group" migrants (Brader, Valentino, and Suhay 2008; Hopkins 2013). Research has "without exception" identified "strong evidence of pervasive cultural concerns" undergirding antipathy toward immigration (Malhotra, Margalit, and Mo 2013, 392), although some recent studies are more circumspect.[2]

A second class of explanation, centered in economics, emphasizes migration's consequences for employment and public finance. Under the closed-economy factor proportions model, native workers experience a decline (increase) in real wages as immigrants with similar (different) skill competencies enter the labor market (Benhabib 1996), suggesting that natives should oppose inflows of workers with skill sets similar to their own (K. Scheve and Slaughter 2001). Fiscally, meanwhile, low-skilled migrants are expected to impose additional taxes on natives and cause a decrease in per capita transfers (Facchini and Mayda 2009; Hanson, Scheve, and Slaughter 2007). Overcrowded infrastructure, proliferating slum colonies, and hikes in property taxes to meet the demands of a growing population are everyday complaints in many developing country cities.

These theories yield empirical predictions. Officials will follow natives and not provide benefits to migrant newcomers. Politicians will be especially keen to withhold resources from newcomers seen as ethnic out-groups, and those endowed with fewer skills—migrant attributes viewed as most objectionable by natives. There is evidence that this is what sometimes happens. "Sons of the soil" parties have made significant inroads in several developing democracies (Hansen 2001). We do not know how general is this phenomenon, though, nor whether nativism colors the behavior of individual politicians.

Courting migrant votes

Another strategy that urban politicians might pursue is to court migrants' votes. Citizens who relocate from one region of their home country to another region invariably possess the right to re-register to vote in their new place of residence. Local politicians in receiver cities may be indifferent about whether their supporters are migrants or natives. This being the case, their best option may be to tap the fresh pool of migrant support. Such a strategy involves providing assistance and state benefits to migrants, which will evoke gratitude and translate into pro-incumbent voting (Bechtel and Hainmueller 2011, Thachil (2014)).

However, the merit of this strategy hinges on a critical assumption: that internal migrants appear on the voter rolls. Although internal migrants possess the right to register to vote in destination-city elections, actual rates of migrant registration might be lower than those of natives. Voter registration is a costly and cumbersome exercise, requiring proof of identity and residency documents, the completion of a local-language form, visits to government offices, and sometimes the payment of bribes (White, Nathan, and Faller 2015; Nickerson 2015). In many settings, citizens are also required to first de-register in their prior place of residence. Evidence of the special difficulties migrants face on this score comes from the United States. Analyzing the Moving to Opportunity experiment, Gay (2011) finds that migrants are 3 percentage points less likely to be registered to vote than a stationary control group and, among experimental compliers, 6.8 percentage points less likely to vote. In a similar vein, Braconnier, Dormagen, and Pons (2014, 31) document that self-initiated voter registration in France prevents a large fraction of citizens from voting, with vulnerable population groups—including immigrants—being the most excluded. These impediments are likely to be even more burdensome in developing countries where bureaucracies function poorly, and citizens frequently lack the time to engage with the state.

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[2]Recent work on local preferences over internal migration identifies conditional effects according to native majority/minority status (Gaikwad and Nellis 2014).

Hurdles to full electoral participation by marginalized groups can have far-reaching implications for welfare and public policy. Brazil's adoption of electronic voting technology helped empower poor voters, leading to greater public outlays on healthcare, as well as lower infant mortality rates (Fujiwara 2015). Similarly, low political engagement by African Americans is thought to adversely affect the well-being of black voters in the U.S. (Hero and Tolbert 2004). If politicians believe that internal migrants are less likely to be registered to vote in destination cities than natives, politicians will perceive few electoral benefits to helping this class of citizens, leaving migrants de facto disenfranchised. Summing up, the real and perceived political characteristics of migrants might affect whether or not politicians view migrants as a group worthy of assistance.

Context

Municipal corporations in India

Our study site is India. Indian cities are governed by municipal corporations, whose members (councilors) are elected to single-member wards for five-year terms.[3] Corporations have expansive responsibilities, including the maintenance of roads, public transportation, fire brigades, street lighting, and water and sewage systems. Corporations are also charged with slum rehabilitation, enforcing building codes, and contributing to public education and health services (Bhagat 2005).[4] Municipal administration is funded by local taxes and grants-in-aid from state and central governments. It is overseen by a wards committee, made up of the local sitting councilors.

Most councilors' work is informal. India's bureaucracy is overstretched, under-motivated, and often corrupt. Gaining access to officials is difficult for most citizens. For instance, residents in Ahmedabad, Gujarat, "often used the expression *dhakka khaavadave chhe* ('getting pushed around') to describe their experiences with the bureaucracy;" in one resident's words, "you have to visit the relevant officials again and again without any result" (Berenschot 2010, 889). In this context, municipal councilors act as intermediaries. When problems arise, citizens turn to their local councilor for help (Manor 2000). Councilors can notarize documents, put in calls and formal requests to zonal and ward-level staff, spend their discretionary funds to fix particular issues, or seek the intervention of higher-up politicians (Oldenburg 1976).[5]

What motivates councilors to take on these responsibilities? Re-election incentives appear critical.[6] In one councilor's words, "I don't say, now the elections are over, I'll talk to you after five years. Every day, I fight like the election were tomorrow" (Oldenburg 1976, 106). Yet councilors' time and resources are finite; few have budgets to employ a large staff. Since demand is typically high, councilors are forced to ration assistance.[7] Ethnographic research backs up this idea; for example:

> These party workers [who work for the municipal councilor] . . . do not help everybody. Their work seems to be geared towards those groups who will be helpful during elections . . . Pravin Dalal [a municipal councilor] targets the coalition of upper castes and upwardly mobile castes that the BJP relies on in Gujarat and barely entertains requests from the small section of Muslims in his electoral ward. The latter take their requests to a Congress politician from another area (Berenschot 2010, 895–6).

Ethnicity dictates whom politicians respond to in this quotation. Whether or not politicians also systematically disregard migrants is what we set out to test.

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[3]In the municipal electoral data we collected from cities in the experimental sample, we found that 12935 people had voted in the average ward.

[4]An illustrative list of council functions is given in the 74th amendment to the Indian constitution, a provision passed in 1992 that standardized urban governance across the country and put it on an legal footing.

[5]Some municipalities assign discretionary funds to councilors under the Municipal Councillor Local Area Development Funds (MCLADS) scheme; for example, see https://goo.gl/bJwXV8 (accessed 2/12/2018).

[6]Note that councilors in India are not term-limited.

[7]Wit (2009) reports low levels of citizen satisfaction with the work done by the Municipal Corporation of Delhi (MCD). Interviews by Swain (2012) reveal that few councilors had knowledge about corporation procedures and budgetary processes. "In MCD everyone from official to councillors is corrupt"; "Councillor X [sic] does not come to meet me. He does not work. He has got arrested once" (Wit 2009, 11).

Internal migration in India

The Indian constitution states that "All citizens shall have the right ... to move freely throughout the territory of India [and] to reside and settle in any part of the territory of India." There were 326 million internal migrants in India as of 2007–9, comprising 29 percent of the country's population (Government of India 2010). 35 percent of India's urban population was recorded as being migrants. Inter-state migration has increased in the past two decades. The biggest sending states are Bihar and Uttar Pradesh (in the north), while the largest receiving states are Delhi, Gujarat, and Maharashtra (Government of India 2010). Because official statistics typically exclude seasonal migrants—a group thought to number 100 million people—they likely underestimate the true extent of internal migration (Deshingkar and Akter 2009).

The migrant population is socially diverse. On the one hand, historically marginalized communities such as scheduled castes, scheduled tribes, and other backward castes, are overrepresented. On the other hand, official data reveal a greater incidence of long-term migration among households in higher income deciles (Rajan 2014, 232). Reflecting this disparity, the Gini coefficient for migrants is higher than that for non-migrants (Haan 2011, 11).

Still, migrants have poor welfare outcomes overall (Srivastava and Sasikumar 2003). According to the United Nations, "internal migration has been accorded very low priority by the [Indian] government, and existing policies of the Indian state have failed in providing legal or social protection to this vulnerable group" (UNICEF and others 2013). "Migrants remain on the periphery of society, with few citizen rights and no political voice in shaping the decisions that impact their lives" (Deshingkar and Akter 2009). Public health statistics show a negative association between migrant status and health indicators (for a summary, see Nitika, Nongkynrih, and Gupta (2014)). Thachil (2017) finds in a sample of Delhi construction workers that only one in five migrants had voted in city elections.

We now describe our empirical strategy for assessing the extent of migrants' political exclusion.

Research design

It is difficult to infer the responsiveness and bias of politicians using observational data. Few councilors keep records of their case loads.[8] In field interviews, councilors repeatedly told us that they did not show favoritism toward any class of citizens. One possible measurement approach could be to survey citizens about their past experiences with politicians, and whether they encountered prejudice. But this body of answers could be marred by self-selection: citizens from marginalized groups might expect a non-response (rightly or wrongly) from politicians, and thus fail to put in a request in the first place. To get past these issues, we conducted a controlled audit experiment, which we now describe.[9]

Sample

We began by compiling lists of all sitting municipal councilors in 28 Indian cities.[10] The sample includes the country's ten most populous cities, as well as all major state capitals. We estimate the combined population of these cities to be 113 million people.

We gathered information on each councilor's name, mailing address, and mobile phone number. Most of this

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[8] Oldenburg (1976, 238) found that councilors significantly exaggerated how much work they did.

[9] Audit experiments in political science originated with Putnam, Leonardi, and Nanetti (1994).

[10] The cities were: Agra, Ahmedabad, Amritsar, Bengaluru, Bhopal, Bhubaneshwara, Chandigarh, Chennai, Coimbatore, Dehradun, Delhi (East, North, and South Delhi corporations), Gulbarga, Hyderabad, Jaipur, Jalandhar, Kolkata, Lucknow, Ludhiana, Madurai, Mumbai, Panaji, Pune, Raipur, Ranchi, Shimla, Surat, Thane, Thiruvananthapuram, Mumbai, Hyderabad, Kolkata. We excluded cities in the contested North-Eastern states and in Jammu and Kashmir, where the security situation is precarious.

information was publicly available online.[11] Where possible, we also collected data on the councilor's political party and their margin of victory in the most recent municipal election.

Treatments

The next step was to produce letters to mail to councilors. Each letter claims to be from a citizen requesting help with a simple problem. The basic template of the letters was held constant. We varied five elements:

- **Migrant status.** Half of the letters claim to be from migrants, while the other half claim to be from natives. In the "native" condition, citizens write that they and their family are "native to this city" and have "lived here all our lives." In the "migrant" condition, citizens write that they and their family are native to another state and have "recently moved to this city." The purpose of mentioning migrants' families was to signal that they had undertaken a permanent—and not temporary—move to the city.

- **Religion.** Citizens introduce themselves by mentioning their first name: e.g., "Hello, my name is Arjun." Recognizably Muslim aliases were used in half of the letters, and Hindu aliases were used in the remaining half.

- **Gender.** Four of these names were female (two Hindu, two Muslim), and four were male (ditto).

- **Skill profile.** Citizens mention their occupation. There are six possible occupations: three were low-skilled, and three high-skilled. We chose jobs that are commonly held by both men and women.

- **Problem type.** We ask councilors for help with solving a simple problem. We generated a list of six problems. Three are "neighborhood" problems, having to do with community goods like street lamps; the others are "individual" problems, e.g. obtaining an income certificate.

- **Party supporter.** Half of the requesters mention that they have supported the councilor's political party in the past; for the remaining half of citizens, this line is omitted.

We prime multiple dimensions of citizen identity and not just migrant status. This makes interpretation easier. Migrant status may be associated with a swath of attributes—for example, poverty—in councilors' minds. If councilors without additional information tend to associate migrants with poor citizens, then attributing differential migrant/native callback rates to migrant status itself may be unjustified, since it could just be picking up class bias. Effectively controling for these "correlated threats" lets us better zero in on the impact of migration status per se, isolated from the bundle of cognate attributes. Further, examining how these characteristics interact with migrant status can shed light on mechanisms. They also allow us to quantitatively benchmark anti-migrant discrimination (if it exists) against other types of political inequality.

The full list of attributes is given in Table 1.

Table 1: Treatment conditions

Name	State of origin	Occuption	Problem	Prior party support
1 Ram	1 Native	1 Cleaner	1 Aadhar card	1 Always
2 Zafar	2 Bihar	2 Vegetable seller	2 job	2 Never
3 Seeta	3 Assam	3 Cook	3 Income Certificate	
4 Zahra	4 Maharashtra	4 Doctor	4 Drainage	
5 Arjun	5 Andhra Pradesh	5 Lawyer	5 Government Dispensary	
6 Salman		6 Engineer	6 Street Lamp	
7 Sushma				
8 Waheeda				

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[11]In the vast majority of cases, these lists were available on the websites of the municipal corporation, or in publicly available affidavits filed with the state election commissions. For two cities where this information was not readily available, we obtained contact details directly from the municipal corporations.
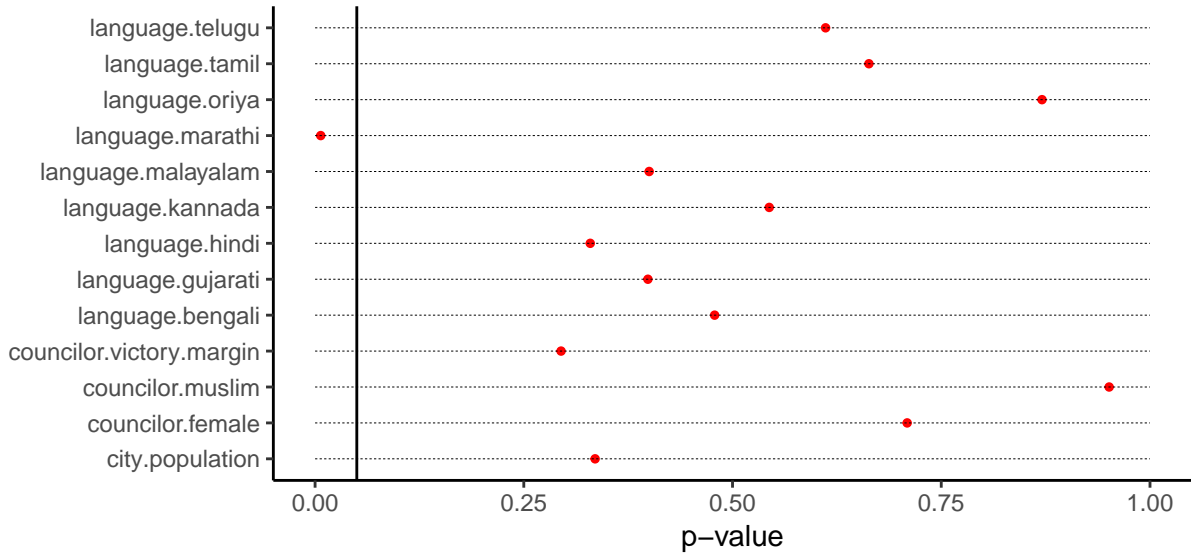
Figure 1: Balance on pre-treatment covariates. This figure plots the p-values from two-sided t-tests of differences in means. We assess whether pre-treatment covariates, listed on the vertical axis, are imbalanced across the migrant and native treatment conditions.

To illustrate, here is an example letter from a fictitious migrant:

> Hello, My name is Arjun and I live in your ward. My family and I are native to Maharashtra and we recently moved to this city. I work as a doctor. I am writing because I would like help getting an income certificate for myself. I have tried contacting many different people about this and also tried coming to see you, but you weren't available. Please could you or one of your assistants call me (LOCAL PHONE NUMBER) and let me what know I should do next? Thank you.

Randomization

Simple randomization was used to assign attributes to letters. We imposed one restriction. If the letter was randomly assigned to come from a migrant, then the migrant's state of origin (Table 1, column 2) could not be the same as as the state in which the letter was sent.[12]

Figure 1 suggests that the randomization was successful. We test whether "migrant letters" and "native letters" were sent to councilors with different background characteristics. We fail to reject the null hypothesis of no statistically significant difference for all but one covariate.[13]

Outcome

At the end of each letter we give a phone number and ask for a callback. Our main outcome is a binary variable denoting whether or not a callback was received. The telephone number was attached to a real SIM card with a local area code (i.e. local to the councilor receiving the letter). Enumerators at a call center

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[12]Due to an implementation error, letters from "migrants" arriving from Maharashtra and Andhra Pradesh were included in the sets of letters sent to councilors in Maharashtra and Andhra Pradesh, respectively. These observations are dropped in the main analysis. The inclusion of these cases does not impact the results.

[13]For robustness, we estimated the effects of migrant status on callbacks in a regression framework, including a vector of dummy variables indicating corporators' language. We find the results to be unaffected.

fielded the calls. They recorded the date and time of the call, and the councilor's name. Councilors were informed that the letters were sent as part of an academic research study, and thanked for their time.[14]

Realism

We took steps to make our treatments realistic. Before designing the letters, we asked ex-councilors from a large municipal corporation in northern India to show us a representative selection of letters they had received while in office. The letters varied in content and style. To mimic the "average" letter, we opted to keep the wording and sentence structures as simple as possible. We had 1,000 letters handwritten and the remainder typed.[15] All letters were sent in envelopes with handwritten names and addresses. To make sure the letters bore local post marks, we mailed them from the city of the addressee.

India is a multi-lingual country. Treatment letters were written in the main local language spoken in each city.[16] Individuals frequently migrate from one language region to another. Interacting with the state requires a lot of paperwork. Thus it is common for illiterates and non-native speakers to enlist scribes, friends, notaries, or local computer shop owners to pen documents on their behalf.[17]

Of course, letters are not the only means by which citizens solicit politicians' help. In-person meetings at councilors' houses or offices are also common. Our letters reference this fact, presenting the choice to write a letter as a last resort.

Ethics

Audit experiments involve deception. We judged that the insights likely to be gleaned from the experiment would far outweigh the small costs to public officials in terms of time and effort. (At most, councilors had to read a 5-line letter and a ca. 100-character text message, and make a phone call lasting about 20 seconds.) One worry is that our study might affect the prospects for researchers accessing politicians in future. The risk seems small. Very few scholars have previously investigated this tier of the Indian political system, and this was the first large-scale audit experiment of its kind in India. Our assessment was that the lessons learned from the experiment could potentially help improve the wellbeing of migrants in India and elsewhere.[18]

Results

Main experiment

The main results are based on one-sided t-tests of differences in means. We interpret them as intent-to-treat (ITT) effects—first, because some portion of the letters may not have reached their intended recipients, and second, because our experimental primes may not have always affected the ultimate variables we sought to manipulate.[19]

Of the 2933 letters mailed to councilors, 407 (13.9 percent) received a callback. For the set of requests that did receive a response, callbacks came 7.2 days after mailing on average. This response rate is low relative to

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[14]We focus our analysis on callbacks and not the *quality* of responses received. Quality measures cannot be analyzed experimentally because such an analysis requires conditioning on a post-treatment variable—namely whether or not a reply was received (Coppock 2018).

[15]Letters were randomly assigned to be handwritten or typed. Ultimately this variation did not lead to any detectable difference in response rate.

[16]Bengali, Gujarati, Hindi, Kannada, Malayalam, Marathi, Oriya, Tamil, and Telugu. Letters were translated and then independently reverse-translated by native speakers of these languages.

[17]See, for example, BBC, March 20, 2014.

[18]The experimental protocol was approved by by our university's institutional review board (Yale University protocol number 1403013586).

[19]For instance, a councilor skimming her correspondence might fail to notice the alias of the sender. In so doing, she would have failed to recognize the requester's gender and religion.
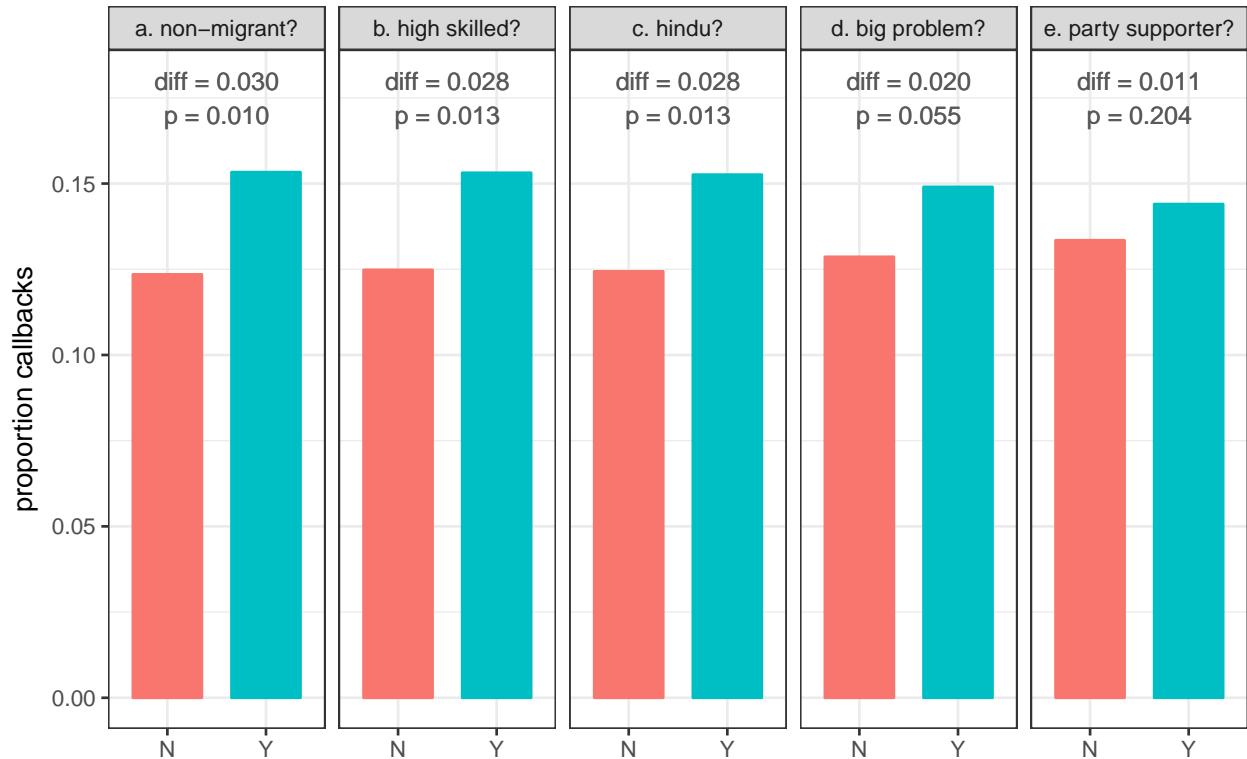
Figure 2: Estimated callback rates to request for help, across five randomized dimensions. Difference and p-values are based on one-sided t-tests.

politician audits conducted in other settings. In the United States, for example, the mean response rate to an audit measuring racial bias was 57 percent (Butler and Broockman 2011); in China, it was 32 percent (Chen, Pan, and Xu 2016); and in South Africa it was 21 percent (McClendon 2016). The local urban politicians we investigate are less professionalized and more junior than those targeted in previous studies.[20] In that sense, they are more akin to "street-level bureaucrats" (cf. White, Nathan, and Faller 2015; Einstein and Glick 2017) Moreover, earlier studies have relied on digital platforms to contact politicians, whereas we posted letters. The average response rate masks significant differences by treatment condition, to which we now turn.

Figure 2 plots the main results. We first examine differences in average callback rates to native versus migrant requesters. Our estimates suggest that putative natives are 24.1 percent (2.98 percentage points) more likely to get a callback than letters said to be from migrants (p=0.010). In Figure 3 we plot the Kaplan-Meier hazard function by native/migrant treatment status. The proportion of letters going unanswered ("surviving") in the migrant condition virtually always dominates the equivalent proportion in the native condition, for each day following the mailing of the letters. In short, we uncover strong evidence of unequal treatment.

How does the impact of migrant status impact compare with that of other requester characteristics? Figure 2 shows that citizens with highly skilled occupations are 22.7 percent (2.83 percentage points) more likely than those with low-skilled occupations to receive callbacks (p=0.013). Citizens with Hindu aliases are 22.7 percent (2.82 percentage points) more likely to receive a callback than Muslim-named citizens (p=0.013). Councilors were equally likely to reply to requesters with female versus male names. We detect some evidence that politicians are more reactive to problems that affect neighborhoods rather than individuals: neighborhood problems were 15.9 percent (2.04 percentage points) more likely to elicit a response than individual problems, although the effect is only marginally statistically significant (p=0.055). Expressing support for the councilor's political party has no distinguishable effect on callbacks. Looking at these estimates side by side, it is noteworthy that the penalty associated with being a migrant is the biggest one.

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[20]32.7 of callbacks were received from assistants rather than corporators themselves. In other words, the large majority of callbacks were handled by individual politicians.
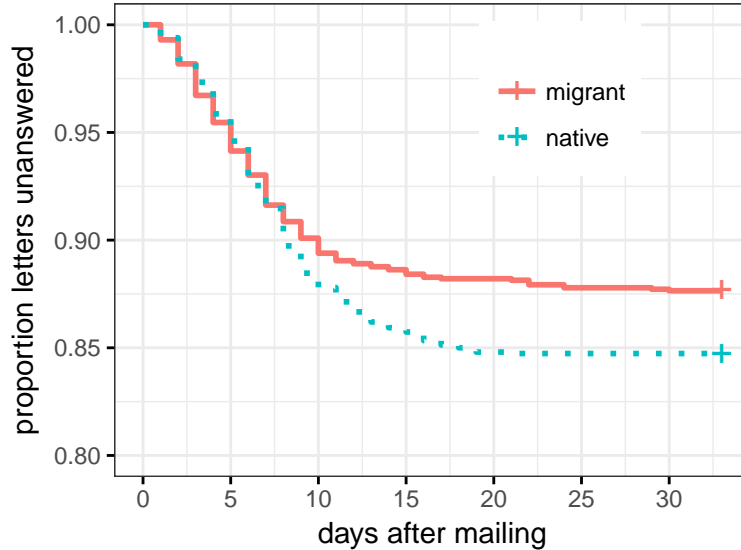
Figure 3: Survival analysis

What produces the bias against migrants? In particular, do nativist-type fears about the cultural and economic consequences of migration underlie the discrimination we observe?

Treatment by treatment heterogeneity

Tackling this question, we exploit the fully factorialized design to assess how migrant status shapes returns to other requester attributes. Figure 4 plots estimates of the average treatment effects produced by the additional characteristics, conditioning on migrant/native status. We find important asymmetries. For native requesters, occupation level and religious background prove to be highly consequential for callbacks. Requests from high-skilled natives are 5.5 percentage points more likely to get a response than requests from low-skilled natives (p=0.001), and Hindu natives are 4.3 more likely to get a callback than Muslim natives (p=0.01).

The preference that politicians display for Hindu requesters appears to result from expectations of coethnic voting (Chandra 2004; Dunning and Harrison 2010). When we re-run the analysis coding a new treatment indicator for a "match" between councilors' religion and the religion of the fictitious requesters, the estimated treatment effect grows substantially in size. It seems that politicians are declining to assist ethnic out groups, from whom they have no reasonable expectation of support. The bias toward high-skilled occupations is at odds with depictions of Indian democracy wherein poor voters—who vote in large numbers—receive the lion's share of politicians' attention (e.g. Varshney 1998). When it comes to providing constituency services, we do not find evidence of this. As we speculate further in the conclusion, this may be due to class bias, or because politicians hope to procure campaign contributions or favors from wealthier, more connected ward residents.

The most striking feature of Figure 4 is the non-effect of religion and skills among migrants. Politicians overlook these characteristics when deciding whether or not to follow up on migrants' requests; we see no statistically significant differences. Instead, politicians treat migrants as an undifferentiated mass. That said, we do observe effects for problem type and party support. Migrants gain from requesting assistance with a neighborhood (as opposed to an individual) problem (3.4 percentage points, p=0.026), and somewhat benefit from mentioning that they supported the councilor's political party in the past (2.3 percentage points, p=0.093). This may sit with the idea, raised in the theory section, that migrants are low-propensity voters. Politicians assist with group problems afflicting migrant communities—assistance for which *some* electoral returns might materialize—but not with individual problems, where the likelihood of gaining a vote is low. Signaling political activity, however, can partly offset this disadvantage.
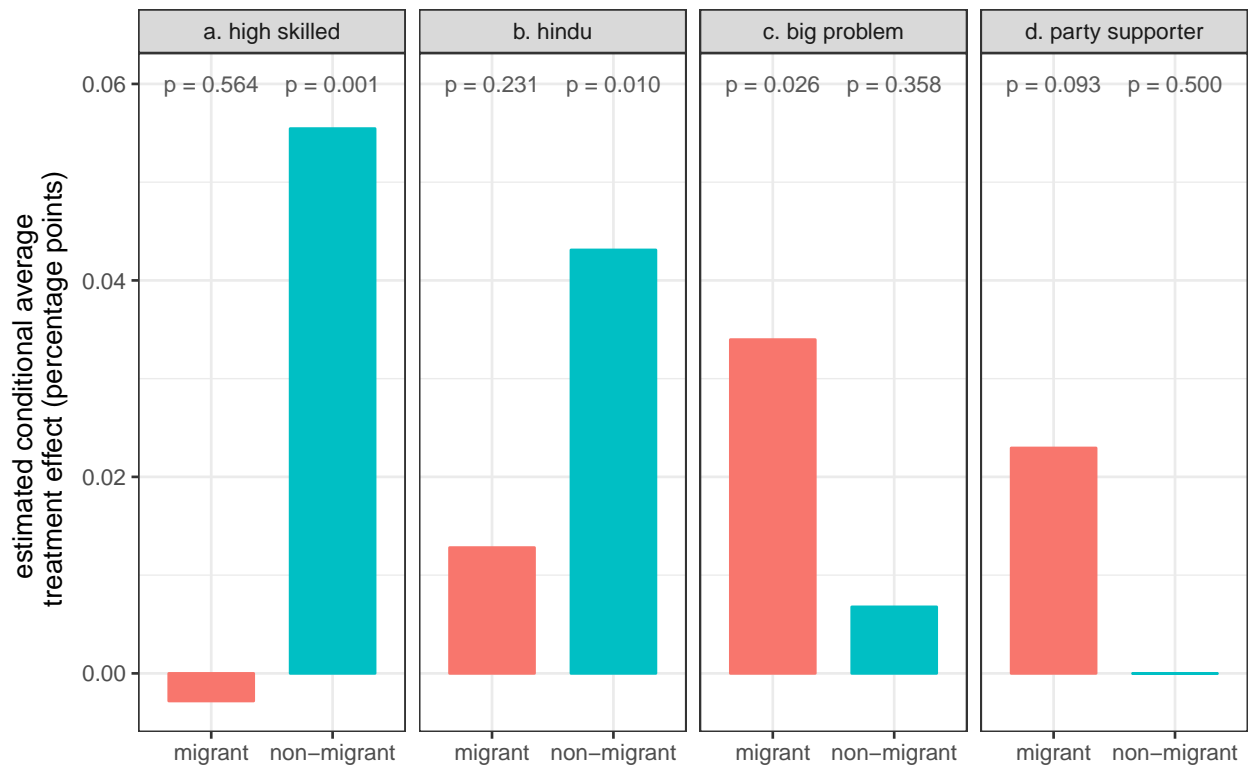
10

Figure 4: Estimated average treatment effects on callback rates conditional on signaling the requester to be a migrant or non-migrant. Estimates based on one-tailed t-tests.

Table 2: Margin of victory (MOV) and anti-migrant discrimination

	Dependent variable:			
	Callback received			
	(1)	(2)	(3)	(4)
Migrant(0/1)		−0.065***		−0.055**
		(0.025)		(0.023)
MOV(0–1)	−0.054	−0.170*		
	(0.064)	(0.088)		
Migrant(0/1) x MOV(0–1)		0.238*		
		(0.127)		
MOV(0/1)			−0.007	−0.032
			(0.016)	(0.023)
Migrant(0/1) x MOV(0/1)				0.051
				(0.033)
Constant	0.156***	0.188***	0.151***	0.178***
	(0.013)	(0.018)	(0.012)	(0.016)
Observations	1,883	1,883	1,883	1,883

Note: ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

Treatment by covariate heterogeneity

We now turn to explore heterogeneity in callback rates depending on the degree of electoral competition faced by councilors. Does politicians' desire for re-election moderate the impact of migrant status? We theorized that incumbents may believe that migrants, as recent arrivals in the city, will not participate in municipal elections at the same rate as natives—perhaps because migrants remain registered in their previous place of residence and fail to re-register locally. If this consideration drives incumbents' behavior, then we would expect incumbents exposed to tighter electoral competition to respond differently to natives versus migrants. Those facing competitive races should want to focus their efforts strategically, conserving scarce resources for those most likely to vote: in this case, long-term residents. By contrast, where politicians are secure in their position—that is, they have a high ex ante expectation of being re-elected—finely targeting assistance toward "voting" citizens (i.e. natives) is less imperative. High degrees of competition, then, should induce incumbents to expend effort and resources chiefly on natives.

To test this proposition, we gathered ward-level data on the margin of victory in the prior municipal corporation election for incumbents in our sample. A widely used proxy for the level of competition that incumbents *expect* to face in the next election is the narrowness of their victory in their most recent race. Collecting this data was challenging. Municipal elections are administered by state electoral commissions (SECs) whose paper records are often poorly kept. We enlisted teams of researchers to visit each SEC and attempt to locate ward-level election returns from previous years. In total we were able to obtain the data needed to compute margins of victory for 1,883 (out of 2,933) councilors in our estimation sample.[21] Our empirical strategy is to assess (a) whether competition by itself affects the likelihood of getting a callback, pooling across all requesters; and (b) whether migrant status moderates the effect of competition, as the theory implies.

The results are presented in Table 2. We use both a continuous measure of margin of victory as well as a dichotomized measure, partitioning the variable at its median value. Several conclusions emerge. We see in columns 1 and 3 that in aggregate there is no relationship between competition and callbacks. Since classic theory holds that competition should incentivize incumbents to work harder at supplying public goods and constituency services, so as to attract pivotal voters, this null result requires an explanation. Columns 2 may provide one. Here, we introduce the variable for migrant/native status as an interaction term. We now uncover evidence that competition is indeed impactful, but conditionally so. In column 2, the coefficient on the interaction term is positive and marginally statistically significant at conventional levels (p=0.061, two-sided test). This indicates that when confronted with native requests, politicians in competitive wards are more likely to respond compared to politicians in non-competitive wards. For native requests, we see that shifting from a zero to 100 percent margin of victory is associated, on average, with a 17 percentage point decrease in callback probability (p=0.052). But when the migrant indicator is set to one, margin of victory has no statistically significant impact on callbacks. The interaction can also be viewed from the perspective of migrant/native discrimination. When competition is high, migrants' requests get overlooked vis-a-vis natives', but when competition is low, migrant status does not affect callback chances. In column 4, we re-estimate the model using a binary coding of margin of victory variable. The results are qualitatively the same, although the coefficient on the interaction term falls short of significance (p=0.116).

What accounts for these differential effects by competition? As we have suggested, one candidate explanation is that politicians believe migrants are not likely to vote in the next municipal election, because they have not undertaken the steps to register. When incumbents find themselves in an electoral bind, it therefore makes sense to ration efforts and resources for likely voters only—i.e., natives.

It is important to stress that margin of victory is not randomly assigned in the Table 2 analyses, and the results are not highly significant. We now undertake a more fine grained test of the proposed mechanisms.
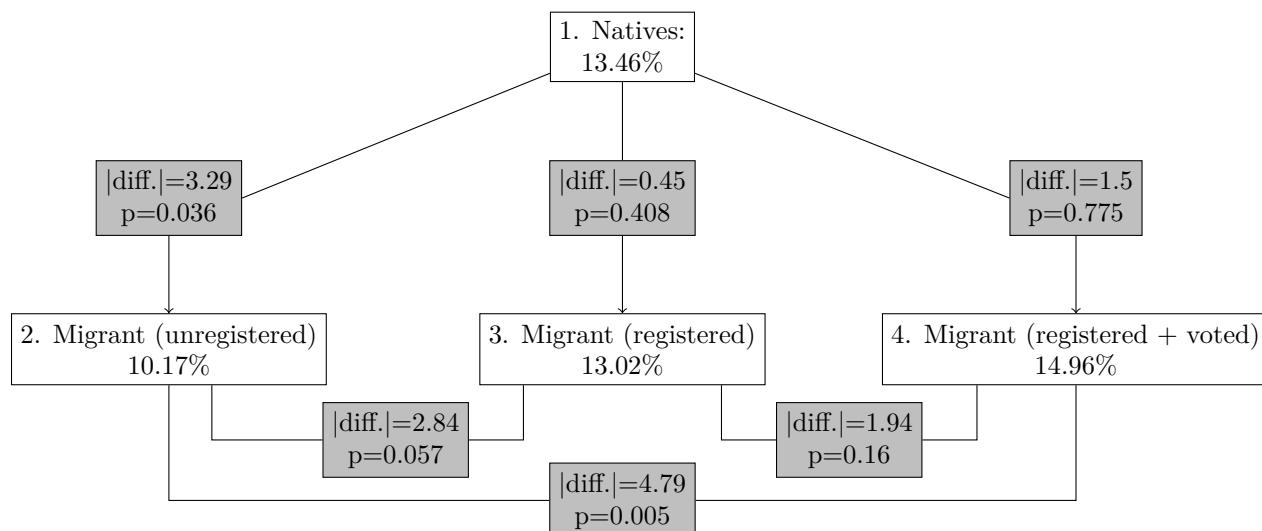
Figure 5: Average callbacks rates by treatment condition (white boxes) and results from one-sided t-tests between conditions (shaded boxes).

Mechanisms experiment

We find that migrants are treated unequally by municipal councilors: their chances of getting a callback are significantly lower than they are for natives. What explains this difference? Migrants' cultural and occupational backgrounds do not seem to matter. We therefore infer that politicians' behavior does not stem from the nativist preferences of local voters. Our discussion highlighted a further possibility, however. Councilors may not believe that migrants participate in urban elections at similar rates to natives. If correct, heeding migrant demands would be futile, because assistance will not generate votes.

To test this conjecture, we designed and implemented a second "mechanisms" experiment. It mirrors the first experiment in its basic set up. But this time we sent short text messages to councilors. Text messages were cheaper than letters. They also enable us to see whether average response rates and treatment effect estimates are sensitive to the medium used to contact councilors.

For logistical reasons, we limited the number of attributes randomized in this round. We employ two male names (Hindu/Muslim), two occupations (construction worker/engineer), two states of migrant origin (Bihar/Assam) and two problems (aadhaar card/street lamp fixed).[22] For the main treatments, we prime requesters' local voter registration status, and whether they claim to have voted for the councilor in the previous election. The first treatment sets out to test the hypothesis that voter registration status explains the shortfall in councilor responsiveness. If the theory about participation expectations is correct, we should see a responsiveness gap for unregistered migrants, but not for registered ones.

The four treatment groups are as follows. Unless otherwise indicated, the assignment probabilities are $\frac{1}{2}$ within each of the square brackets.

1. **Native** [$\Pr(assignment) = \frac{2}{5}$]**.** i'm [Arjun / Salman]-[construction worker / engineer] in ur ward. me & my family r originally from this city. we [are/aren't] registered 2 vote here. could u help me get [aadhaar card / street lamp fixed]?

2. **Migrant not registered to vote** [$\Pr(assignment) = \frac{1}{5}$]**.** i'm [Arjun / Salman]-[construction worker / engineer] in ur ward. me & my family r originally from [bihar / assam]. we aren't registered 2 vote

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[21]We were able to get electoral data for all cities except Ahmedabad, Amritsar, Bhopal, Jalandhar, Ludhiana, Pune, Raipur, Shimla, Surat, and Thane.

[22]The shift toward looking at only male citizen requests was prompted by the null effects seen for gender in the letters experiment.

here. could u help me get [aadhaar card / street lamp fixed]?

3. **Migrant registered to vote** $[\Pr(assignment) = \frac{1}{5}]$**.** i'm [Arjun / Salman]-[construction worker / engineer] in ur ward. me & my family r originally from [bihar / assam]. we're registered 2 vote here. could u help me get [aadhaar card / street lamp fixed]?

4. **Migrant registered to vote & voted previously for incumbent** $[\Pr(assignment) = \frac{1}{5}]$**.** i'm [Arjun / Salman]-[construction worker / engineer] in ur ward. me & my family r originally from [bihar / assam]. we're registered 2 vote here we've voted 4 u before. could u help me get [aadhaar card / street lamp fixed]?

We obtained mobile phone numbers for 2513 of the 2933 councilors messaged in the first experiment. As before, we recorded whether a councilor replied to the request for help, either by sending a return text message or by calling. The average response rate in the second experiment was 12.9 percent. This is very similar to the overall callback rate in the letters experiment (13.9 percent), suggesting that choice of contacting method is unlikely to limit the results' generalizability.

The main findings are presented in Figure 5. White boxes show average response rates under each treatment condition. Shaded boxes show the results of one-sided t-tests of differences in means. The results lend credence to the claim that beliefs about low electoral participation drive anti-migrant bias. Equivalent to the findings in the main (letters) experiment, a native is 3.29 percentage points—proportionally, 32.4 percent—more likely to receive a callback than an unregistered migrant. For "registered" migrants, however, the migrant penalty disappears. The likelihood of getting a callback is 2.84 percentage points (28 percent, p=0.057) higher for registered versus unregistered migrants, and not statistically different from natives.

Does providing a signal of migrants' past voting history boost politician responsiveness still further? Figure 5, box 4 shows the average callback rate for registered migrants who also claim to have voted for the councilor previously. Further communicating migrant's political support does not confer a measurable advantage relative to registered migrants. However, it is notable that a registered migrant claiming to have voted for the incumbent previously is 4.79 percentage points (47.1 percent, p=0.005) more likely to get a callback than an unregistered migrant—a sizable difference. Overall, there is strong evidence that variation in migrants' political attributes—and notably local registration status—affects politician responsiveness. This in turn suggests that discrimination is a function of politicians' re-election incentives.

We independently vary three further requester attributes in the text message experiment: religion, occupation, and problem-type. As before, these manipulations induce large differences in callbacks. A Hindu-named requester is 2.68 percentage points (23.15, p=0.023) more likely to receive a callback than a Muslim-named one; a neighborhood problem is 2.38 percentage points (20.31 percent, p=0.038) more likely to get a callback than an individual problem; and high-skilled requesters are 4.97 percentage points (47.68, p=0.000) more likely to receive callbacks than low-skilled requesters. These align closely with the results of the letters experiment. Migrant registration status partially moderates the returns to these additional attributes. Having a Hindu as opposed to a Muslim name is beneficial for registered migrants but inconsequential for unregistered migrants. There are no heterogeneous effects according to skill level or problem type.

Survey experiment

For a final test of the proposed mechanism, we implement a survey experiment to explore directly whether politicians believe that migrants and natives are differentially likely to be registered to vote in city elections. We attempted to contact 1500 councilors by telephone. Subjects were randomly sampled from the original list of 2933 councilors. In total, 427 councilors answered our calls and completed a brief survey. We included a vignette experiment at the start of each survey. Councilors were read the following text, with subjects being assigned to one of two treatment conditions (shown in square brackets) with equal probability:

Suppose a citizen living in your ward comes to you asking for help with some matter. [The citizen is originally from your city and has lived and worked in the city all his life / The citizen is originally from a different state and he has recently come to your city to live and work.]
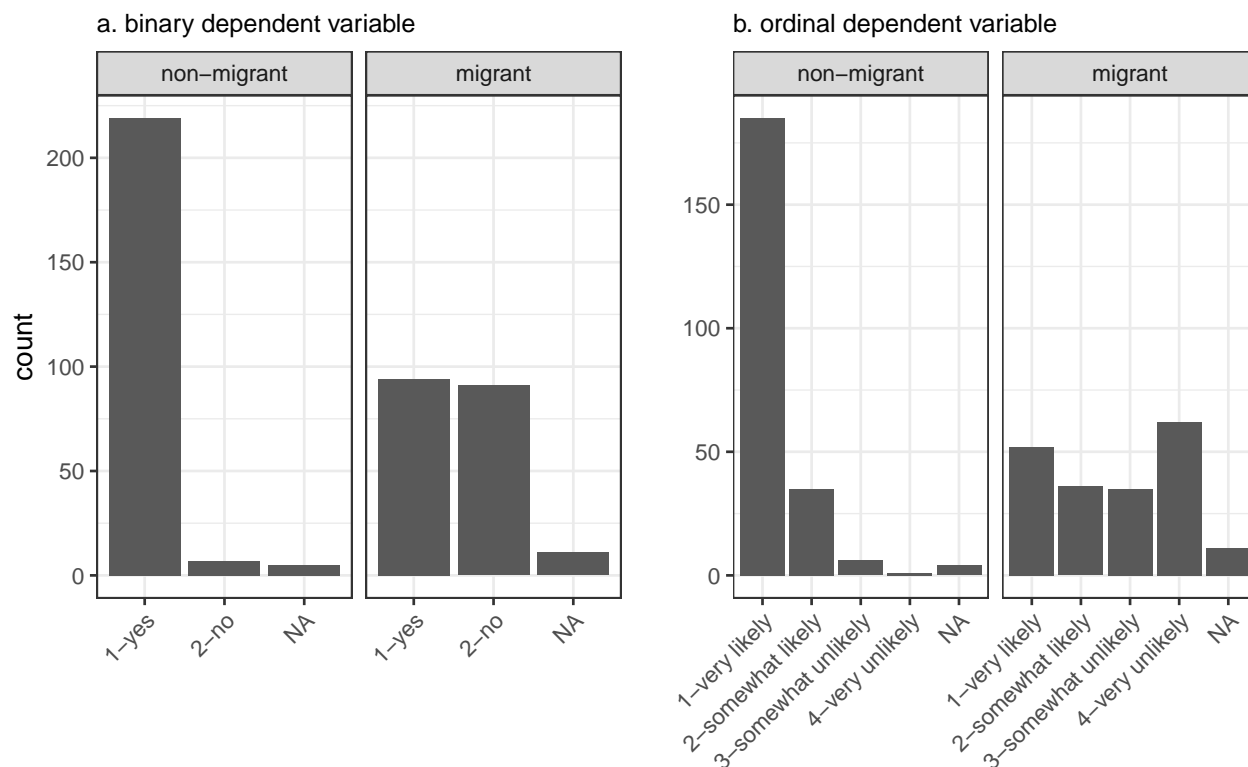
Figure 6: Survey experiment. Count of responses to each response option, broken down by migrant/native treatment condition.

There were two follow-up questions:

1. If you had to guess, and based on your experience, do you think that this [long-term resident/migrant] would have a local voter ID card allowing him to vote in municipal elections in this city? [Response options: Yes, No, Don't know]

2. How likely do you think it is that this [long-term resident/migrant] would have a local voter ID card allowing him to vote in municipal elections in this city? [Response options: Very likely, Somewhat likely, Somewhat unlikely, Very unlikely]

Figure 6 plots the counts of responses, broken down by treatment condition. We find large effects. 97 percent of councilors presented with a native citizen believed the citizen would be registered; the equivalent figure for migrant citizens is 51 percent—a difference of 46 percentage points (p=0.000, one-sided t-test). An analysis of answers to the second question yields the same conclusion. Mentioning that the citizen is a migrant instead of a native leads to answers that are 1.35 points lower, on average, than the corresponding answers for natives on a 4 point likelihood scale (p=0.000). We take this as compelling evidence that councilors harbor very different views about migrant and native citizens. Whereas natives are overwhelmingly believed to form part of the local electorate, migrants are not. For this reason,e politicians shirk in providing constituency services in response to requests from migrants, meaning that a large class of citizens goes under-represented.

Conclusion

We present the first large-scale study investigating anti-migrant discrimination in India. Using randomized experiments, we find that internal migrants suffer from unequal political representation. Requests to urban politicians for constituency service are more likely to go unanswered when they come from recent migrants to the city instead of long-term residents. The estimated bias is large: it exceeds in magnitude that associated

with religion, gender, occupation level, and problem type. Our follow-up experiments indicate that its major cause has little to do with conventional nativist worries about labor markets and culture. Rather, it stems from incumbents' electoral incentives. Most politicians believe that migrants are unregistered to vote in city elections. Accordingly, politicians devote their scarce time and resources to helping established, native city residents, whom they deem registered.

Migrants' de facto disenfranchisement may have adverse welfare consequences. In developing countries, citizens from marginalized population groups are especially dependent on the brokerage services that local politicians provide (Stokes et al. 2013; Nichter 2008; Auerbach and Thachil 2016). The cumulative welfare effects of anti-migrant bias are likely to be very considerable given the burgeoning number and size of cities in the global south.[23] The scope for discrimination against migrant communities looks set to rise substantially.

Looking forward, we can imagine two equilibria. In the first, migrants' political exclusion becomes chronic. Politicians believe that migrants will not vote and thus discriminate against them; in turn, migrants' failure to draw attention from incumbents alienates them from local politics, making registering seem futile. Political exclusion becomes self-fulfilling. Alternatively, however, if migrants do register and vote, politicians' calculus will change, and their responsiveness toward migrants should increase. Thus, while pessimistic, our paper's results suggest a solution. Encouraging migrants to register to vote in destination-city elections—for example, by running registration drives in migrant slum settlements—should improve political engagement, representation, and human development. Politicians' insouciance toward migrants is not due to "sticky" factors like taste or prejudice; it derives from instrumental electoral concerns, suggesting it should be fixable. The natural next step is to identify the constraints that prevent migrants from registering when they move.

Our study turns up additional results worthy of future research. Citizens in highly skilled occupations enjoy much better access to constituency services than citizens in low-skilled occupations. Several possible explanations come to mind. In line with "fiscal contract" theories, it could be that politicians see constituency services as part of a quid pro quo, and prefer to help citizens who pay more taxes. Richer citizens might also be plausible campaign contributors. Councilors themselves may come from wealthy social strata and may lean toward their own types. Or a councillor might think that wealthier, better-connected citizens are more likely to tattle to party leaders in the event that they do not receive a satisfactory response (Jaffrelot 2008). Future work should try to disentangle these mechanisms.

A wave of recent research has explored ways to facilitate communication between citizens and politicians using new technologies such as mobile phones and the internet. Our study employed two contacting methods—postal letters, and SMSes—and found near-identical, relatively low response rates for both. We conclude that the opportunities for new technology like mobile phones to "flatten political access" may be quite limited (cf. Grossman, Humphreys, and Sacramone-Lutz 2014).

The findings should apply broadly. A natural next step for researchers would be to run similar audits in other countries undergoing rapid urbanization.

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[23]For Africa, the United Nations projects an 80 percent growth in the number of cities with 500,000 or more inhabitants by 2030 (relative to 2016); the equivalent figure for Asia is 30 percent. See United Nations, "The World's Cities in 2016," https://goo.gl/triXo2 (accessed 2/24/2018).

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