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

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Abstract: *Rural-to-urban migration is reshaping the economic and social landscape of the Global South. Yet migrants often struggle to integrate into cities. We conduct countrywide audit experiments in India to test whether urban politicians discriminate against internal migrants in providing constituency services. Signaling that a citizen is a city newcomer, as opposed to a long-term resident, causes incumbent politicians to be significantly less likely to respond to requests for help. Standard "nativist" concerns do not appear to explain this representation gap. We theorize that migrants are structurally disposed to participate in destination-area elections at lower rates than long-term residents. Knowing this, reelection-minded politicians decline to cater to migrant interests. Follow-up experiments support the hypothesis. We expect our findings to generalize to fast-urbanizing democracies, with implications for international immigration too. Policywise, mitigating migrants' de facto disenfranchisement should improve their welfare.*

Verification Materials: The data and materials required to verify the computational reproducibility of the results, procedures, and analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: https://doi.org/10.7910/DVN/Q40RGF.

In recent decades, cities and towns across the Global South have witnessed explosive population growth (Post 2018). This has been spurred in significant measure by rural-to-urban migration (Bell and Charles-Edwards 2013; Thachil 2017). While internal population movements help drive economic development, many newcomers 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 (Deshingkar and Akter 2009). Owing to limited police protection, they are routinely victims of harassment and violent crime (Weiner 1978).

These problems persist even though most countries constitutionally enshrine the right to free movement within their borders. A growing literature suggests that socioeconomic conflict between locals and migrants is to blame (Bhavnani and Lacina 2015; Gaikwad and Nellis 2017). Yet many of the welfare shortfalls experienced by migrants appear to emanate from state inaction—in particular, states' failure to distribute core goods and services to migrant communities. This claim has not been subjected to theoretical or empirical scrutiny, however. In this article, we propose *systematic political neglect* as a new explanation for the hurdles faced by migrants. In developing democracies, elected representatives commonly hold broad discretionary sway over resource allocation.

As such, politicians' willingness (or otherwise) to attend to migrant concerns is likely to matter for this group's well-being (cf. Bussell 2019).

Do urban politicians discriminate against migrants from other parts of the country in providing basic services? If so, on what basis? We develop and test theories regarding politicians' treatment of recent 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 receiver cities face strong electoral incentives to play the "localist" card, withholding benefits from migrants seen as placing strain on jobs, public goods, and social relations. Conversely, politicians might view migrants as a fresh source of electoral support. Internal migrants are entitled to vote in destination-area elections. Thus, politicians should treat migrants and locals equally. Yet for this argument to hold, incumbent politicians must expect that long-term residents and newcomers participate in urban elections at similar rates. Recent migrants may be poorly informed about politics and voting logistics in destination regions, reducing 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 a nationwide field experiment in India. We compiled lists of sitting municipal councilors in 28 major Indian cities. Municipal councilors act as intermediaries between citizens and the state in the settings we study, and they are responsible for delivering a wide range of constituent benefits—from the provision of basic healthcare to helping citizens obtain ration cards and pension benefits. At the same time, they enjoy considerable latitude in deciding how to target assistance (Berenschot 2010; Oldenburg 1976). We subjected councilors to an unobtrusive audit. In the main experiment, we wrote and posted letters to 2,933 councilors, randomly varying the identities of fictitious citizens sending the letters, and the problems for which they were requesting help. The primary 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 requests from long-term residents are 3 percentage points—proportionally, 24%—more likely to receive a callback from the local councilor than requests from migrants, substantiating the existence of anti-migrant discrimination.

What explains this representational gap? We performed additional "mechanisms" experiments to find out. First, we sent text messages asking for help to the original sample of councilors. But this time, we manipulated the requesters' *political* attributes. We primed (a) whether the citizens 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 locals were 4 percentage points (proportionally, 41%) more likely to receive callbacks compared to unregistered migrants. Migrants' registration status proved highly consequential. "Registered" migrants were 3 percentage points (proportionally, 27%) more likely to receive callbacks compared to notionally unregistered migrants. Politicians' rates of response to registered migrants and registered natives were statistically indistinguishable. Flagging that the migrant had previously voted for the incumbent did not measurably boost the probability of a response. In a second mechanisms experiment, embedded in a survey we conducted, councilors viewed hypothetical migrants as 46 percentage points less likely to be registered to vote in local city elections than long-term residents. Taken together, the evidence paints a compelling picture. Beliefs about migrants' low turnout propensity—resulting primarily from their (perceived) low registration levels—helps explain this group's relative inability to garner services from urban politicians.

We identify an overlooked type of political inequality. The challenges faced by international immigrants have been extensively discussed in recent work (e.g., Adida, Laitin, and Valfort 2010; Dancygier 2010). The analogous yet fundamentally distinct problems faced by internal migrants have largely eluded scholarly attention. The world's urban population is set to increase by 2.5 billion people by 2050, and 90% of that growth is expected to occur in Asia and Africa (United Nations 2014). It is estimated that 120,000 people migrate to Asian cities each day (Lee, Helke, and Laczko 2015, 17). Smoothing this group's integration into cities is an urgent task.

Our findings imply a remedial intervention. Encouraging recent migrants to register to vote locally, and informing politicians that this registration process is underway, should increase politicians' responsiveness to migrants, with knock-on effects for welfare. There are potential implications for cross-country immigration too (cf. Alizade, Dancygier, and Ditlmann 2018). Naturalized immigrants typically evince low rates of political participation; in the United States, the gap between naturalized cross-border immigrants and native-born citizens ranged from 10 to 12 percentage points throughout the 2000s (Wang 2013). For immigrant communities, availing the full benefits of citizenship may require deeper and more visible electoral engagement.

The Challenge of Internal Migration

We begin by characterizing India's migrant population and situating it in cross-national perspective. 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 325 million internal migrants in India as of 2007–8, composing almost a third of the country's population; 35% of India's urban residents are migrants.[1] Interstate migration has increased over the past two decades. It doubled between 2001 and 2011 (World Economic Forum 2017, 24). The biggest sender states are Bihar and Uttar Pradesh, which are economically underdeveloped and populous. The largest receiver states are Delhi, Gujarat, and Maharashtra, which are more industrialized (Nayyar and Kim 2018, 9). In addition to permanent migrants, between 100 and 200 million people engage in seasonal or "circular" migration (Deshingkar and Akter 2009; Tumbe 2018). Historically marginalized communities—notably, Scheduled Castes, Scheduled Tribes, and Muslims—are overrepresented among circular migrants.

What motivates migration across states? We analyze data from the 2011 Census of India. Thirty-one percent of interstate migrants cited marriage as their reason for moving, 25% cited business or work, 25% moved with family, and 1% cited education.[2] Rural-to-urban migration predominates, constituting 38% of total cross-state movement (though not counting circular migrants). It is followed by urban-to-urban migration (33%), rural-to-rural migration (23%), and urban-to-rural migration (6%). Among urban-destination migrants, 29% give employment as their reason for moving; this figure is 52% for male migrants.[3] India's present urbanization rate is low by international standards—34% of Indians live in towns or cities, according to the World Bank.[4] Nevertheless, the world's 10 fastest-growing cities are Indian, and India's cities and larger towns will account for the lion's share of future growth in wage employment (Oxford Economics 2018).

What informs labor migrants' decisions about where to locate? Social networks—based on region of origin,

ethnicity, and religion—bear on migrants' occupational and residential choices. For example, migrants from the state of Karnataka staff South Indian Udupi restaurants in Mumbai, where they also cluster residentially (Tumbe 2018). Labor contractors significantly mediate circular migration; the 2011 Indian Human Development Survey shows that half of sampled circular migrants moved with the help of a middleman (Nayyar and Kim 2018). Language does not appear to be a major factor constraining destination choices (Government of India 2017, 265). Spatial concentration typifies migration: The main metropolises capture the largest absolute numbers of male migrants seeking work. Big cities appeal to both high-skilled and low-skilled migrants. High-skilled migrants to cities are slightly more likely to have originated in other urban areas (30%, in the 2011 census) than low-skilled migrants (26%).

India's migration experience parallels that of other developing nations. Young (2013) analyzes the Demographic and Health Surveys for 65 countries; on average, 22% of respondents who had resided in the countryside before the age of 12 were living in an urban area when surveyed. Urban migrants tend to have poor human development outcomes. This is true in India. 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 2013, 4). "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, 1). Public health statistics show a negative association between migrant status and health indicators; migrants do worse than natives in rates of vaccination, infant mortality, malnutrition, prevalence of sexually transmitted infections, and cardiovascular diseases (Nitika, Nongkynrih, and Gupta 2014). Migrant slums lack core public services (Auerbach 2016).

Migrant exclusion has many roots. One potential explanation, we posit, is the inattentiveness of urban political elites. In 2018, Gujarati politicians launched a campaign to hire local workers instead of "outsiders" in the state's industrial centers, contributing to violence and the exodus of Hindi-speaking migrants from Gujarat.[5] Heated rhetoric against northeastern Indians induced Assamese migrants to flee Bangalore in 2012 (Tumbe 2018). A political party built on an avowedly anti-migrant platform has held the elected mayoralty of Mumbai—a city of 22 million people—since 1996. These

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[1] Sanket Mohapatra, "Almost a Third of Indians, or Over 300 Million People, Are Migrants." *World Bank People Move*, available at bit.ly/2EiWpb6.

[2] 2011 Census of India, Table D-3, available at bit.ly/2x0zqRY.

[3] 2011 Census of India, Table D-7.

[4] Data are from the United Nations Population Division, *World Urbanization Prospects 2018* data set, available at bit.ly/2EoCerX.

[5] Hiral Dave, "Congress MLA Alpesh Thakor in the Eye of Gujarat's Migrant-Exodus Storm," *Hindustan Times*, October 10, 2018.

cases pinpoint the possibility that the adverse living conditions and precarity felt by India's migrants stem from the attitudes and behaviors of political incumbents: those tasked with allocating state benefits.

A Theory of Political Discrimination Against Internal Migrants

We hypothesize that urban politicians will be less responsive to internal migrants than to long-term city residents. Why might this be the case? We focus on the provision of constituency services. By helping constituents with basic requests, incumbent politicians can build a "personal vote" (Bussell 2019). We advance three reasons why politicians may be less likely to direct such services to migrants rather than locals.

First, politicians may discriminate against internal migrants for reasons of personal taste. Politicians may be prejudiced against new arrivals from other parts of the country, viewing them as "outsiders" who do not belong in the city. Ascriptive identities of citizen-petitioners, including their race (Butler and Broockman 2011; McClendon 2016), religion (Adida, Laitin, and Valfort 2010), and ethnicity (White, Nathan, and Faller 2015), have been shown to influence the responsiveness of public officials in multiple contexts. Animus might be attached to internal migrants too.

Second, politicians may not themselves harbor hostility toward migrants, but their constituents may. Politicians beholden to the local population for electoral support might channel locals' concerns about migrant influxes in their day-to-day activities, including decisions about whom to assist. Why might long-term city residents disdain in-migration? Culturalist accounts imply that locals are wary of social and ethnic "dilution." They object to the arrival of new city residents who do not share the same language, creed, or race as them. To the extent they tolerate migration, they prefer ethnically in-group migrants (Gaikwad and Nellis 2017). Meanwhile, materialist accounts emphasize migration's consequences for employment and public finances. Under the closed-economy factor proportions model, local workers' wages go down when similarly skilled migrants join the labor market (Scheve and Slaughter 2001). Fiscally, low-skilled migrants are expected to impose an additional tax burden on locals and cause a decrease in per capita transfers (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 cities. If

correct, this theoretical perspective implies that elected officials will discriminate against migrants overall, and be especially prone to turn away requesters lacking skills and belonging to ethnic outgroups—the attributes seen as most objectionable by locals.

A third line of reasoning highlights electoral incentives. The foregoing arguments were developed for understanding popular preferences over international immigration, and they may not directly shed light on elite behaviors with respect to internal migration. Crucially, internal migrants in democracies have the right to vote wherever they move. Since migrants present the prospect of new votes, politicians looking to expand their electoral base might reach out to migrants, soliciting their support by supplying assistance and representation. Under the logic of "one person one vote," therefore, anti-migrant discrimination may not be manifest.[6]

Yet the payoffs to politicians of this "migrant-agnostic" strategy turn on the assumption that internal migrants appear on local 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 long-term residents. Voter registration is typically a costly and cumbersome exercise, requiring proof of identity and residency documents, the completion of local-language forms, visits to government offices, and sometimes the payment of bribes. In many settings, citizens are also required to first deregister in their prior place of residence. Analyzing the Moving to Opportunity experiment in the United States, Gay (2012) 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 (2017) document that self-initiated voter registration in France prevents a large fraction of citizens from casting a ballot, with vulnerable population groups—including immigrants—being most affected. These impediments may be even more burdensome in developing countries where bureaucracies are often dysfunctional and the opportunity costs of engaging with the state are high. If the extra costs that migrants face to register are common knowledge, then time-strapped politicians may not offer help to migrants, from whom they do not expect to receive an electoral reward down the road. Thus, anticipation of migrants'

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[6]Noteworthily, poor, marginalized citizens in India vote in large numbers; Ahuja and Chhibber (2012) quote one poor citizen as saying, "If I don't vote, I am dead to the state." This suggests that vote-seeking politicians would be equally responsive to these groups.

low political participation might be a factor propelling discrimination.

Naturally, "migrant" is but one identity trait citizens carry. We additionally consider several other citizen attributes theorized to influence politicians' proclivity to supply services. We hypothesize that politicians will be more responsive to high-skilled requesters, those belonging to the majority ethnic group, those requesting help with a neighborhood (rather than an individual-level) problem, and those affirming membership of the politician's party.[7]

Evaluating the impact of these factors enables us to benchmark the magnitude of anti-migrant bias. More importantly, separately assessing these attributes' effects for migrant versus local requesters can help parse the theoretical mechanisms outlined above. In particular, if discrimination occurs because of politicians' personal prejudice against migrants, signaling party membership and neighborhood problems should minimally affect responsiveness; expressing animus against migrants trumps the appeal of additional migrant votes, which these signals convey. Next, if discrimination occurs because politicians channel locals' economic and cultural grievances surrounding in-migration, then low-skilled migrants and minority-group migrants should be particularly disfavored. Finally, if discrimination occurs because politicians believe that migrants are less likely to participate electorally than locals, then migrants' ethnicity and skill type should be inconsequential for responsiveness: The optimal strategy is to discriminate in blanket fashion.[8] By the same token, migrants who profess party membership may be *advantaged* under this mechanism if this characteristic is interpreted to suggest probable future electoral participation and support.

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[7]Prior work explains the significance of these features. Politicians employ economic class and income to make determinations about voting probabilities and who is likely to provide financial and other forms of support. Ethnic ties between politicians and constituents are known to be a key factor shaping voting choice in societies cleaved along ethnic lines (Chandra 2004). Greater electoral dividends accrue from services that benefit broader groups over excludable private goods that benefit individuals (Bussell 2019). Material support generally flows to core party supporters who are a "surer bet" in providing votes (Stokes et al. 2013).

[8]Note, though, that if politicians come to update their beliefs about migrants' likelihood of participating, we might expect "engaged" migrants belonging to a *majority* ethnic group to gain preferential treatment, as is the case for majority-group locals under models of coethnic voting.

Research Design

We have advanced three rationales for anti-migrant discrimination by urban politicians: (a) politician prejudice, (b) constituent prejudice, and (c) anticipation of migrants' low electoral participation. We now describe our empirical strategy for assessing the extent and causes of migrants' political exclusion.

Municipal Councils in India

Indian cities are governed by municipal councils, whose members (called councilors or corporators) are elected to single-member wards for 5-year terms. Councils have expansive responsibilities, including the maintenance of roads, public transportation, fire brigades, street lighting, and water and sewage systems. Councils are also charged with rehabilitating slums, enforcing building codes, and contributing to public education and health services. 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 local sitting councilors.

Most councilors' work is informal. India's bureaucracy is overstretched, undermotivated, and often corrupt (De Wit 2009). Gaining access to officials is difficult for most citizens. For instance, residents in Ahmedabad "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. Councilors can notarize documents, put in calls and formal requests to zonal and ward-level officers, spend their discretionary funds to fix particular issues, or seek the intervention of higher-up politicians to solve thornier problems.

What motivates councilors to take on these responsibilities? Reelection incentives appear critical. 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 usually high, councilors are forced to ration assistance. Ethnographic research backs up this idea:

> 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, 895–96)

Ethnicity and religion dictate whom politicians respond to in this quotation. Whether or not politicians also systematically disregard migrants—the identity class of interest in this study—is what we set out to test.

Empirical Strategy

Our goal is to estimate whether municipal councilors in India accord unequal treatment to internal migrants. It is difficult to infer the responsiveness and bias of politicians using observational data. Few councilors keep records of their caseloads, and Oldenburg (1976, 238) found that councilors being interviewed significantly exaggerated how much work they did. In in-depth field interviews that we conducted, councilors repeatedly told us that they did not show favoritism toward any category of citizens. One possible measurement approach is to survey citizens about their past experiences with elected officials, and whether they encountered prejudice. But this body of answers could be marred by self-selection: Citizens from marginalized groups might expect a nonresponse (rightly or wrongly) from politicians, and thus fail to put in a request in the first place. Prejudice might also be a subjective experience, complicating attempts to code it. To get past these issues, we conducted a controlled audit experiment, which we now describe.

Sample

We began by compiling lists of sitting municipal councilors in 28 Indian cities.[9] The sample includes the country's 10 most populous cities, as well as the major state capitals. We estimate the combined population of these cities to be 113 million people. The municipal councilors in our sample were directly accountable to these citizens.

We gathered each councilor's name, mailing address, and mobile phone number. Most of this information was available through the websites of the municipal councils or through official requests filed with the state election commissions. Where possible, we also collected data on the councilor's political party and his or her 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. Pursuant to the theoretical discussion, we varied six elements, with migrant status being the main cleavage of interest:

1. *Migrant Status.* Half of the letters claim to be from migrants, and the other half claim to be from long-term city residents. In the "local" 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 long-term—and not temporary—transition to the city.

2. *Religion.* Citizens introduce themselves by mentioning their first name, for instance, "Hello, my name is Arjun." Recognizably Muslim aliases were used in half of the letters, and Hindu aliases were used in the remaining half.[10]

3. *Gender.* Four of these names were female (two Hindu, two Muslim), and four were male (again, two Hindu, two Muslim).[11]

4. *Skill Profile.* Citizens mention their occupation. One of six occupations was assigned. Three of the occupation options were low-skilled, and three were high-skilled. We chose jobs that are commonly held by both men and women.

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[9]The cities were Agra, Ahmedabad, Amritsar, Bengaluru, Bhopal, Bhubaneswar, 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, and Thiruvananthapuram.

[10]Hindus are India's majority ethno-religious group, composing 80% of the population; Muslims make up 14% and have long been victims of sociopolitical discrimination.

[11]It is notable that "women constituted the majority of migrants in most cities between 2005 and 2006," and most came for reasons of marriage (Bhagat 2017, 37). Women appear to participate less than men in local urban politics; a study of recent Mumbai corporation elections found the female participation rate to be 26% compared to 35% for men (Parchure, Phadke, and Talule 2017, 43).

TABLE 1 Treatment Conditions for Letters Experiment

Name	State of Origin	Occupation	Problem	Party Member
1 Ram	1 Local	1 Cleaner	1 Aadhaar card	1 Yes
2 Zafar	2 Bihar	2 Vegetable seller	2 Job	2 Not mentioned
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				

5. *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 affecting only the requester, such as obtaining an income certificate.

6. *Party Member.* Half of the requesters mention that they are members of the councilor's political party; for the remaining half of the citizens, this line is omitted.

The full list of attributes is given in Table 1. We prime multiple dimensions of citizen identity and not just migrant status. This facilitates the interpretation of our results. In particular, migrant status may conjure an assortment of attributes (e.g., working class) in councilors' minds. If councilors without additional information tend to associate migrants with poverty, then attributing differential migrant/local callback rates to migrant status itself may be unjustified since it could just be picking up class bias. Effectively controlling for these "correlated threats" lets us better zero in on the impact of migration status per se, isolated from the bundle of cognate attributes. Critically, too, examining how these characteristics interact with migrant status helps illuminate theoretical mechanisms and adjudicate between potential causes of discrimination. They also allow us to quantitatively benchmark anti-migrant discrimination (if it exists) against other types of political inequality.

To illustrate, here is an example letter from a hypothetical 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 know what 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 (see Table 1) could not be the same as the state in which the letter was sent.[12]

Figure 1 suggests that the randomization was successful in creating similar groups of councilors. We test whether "migrant" letters and "local" letters were sent to councilors with different pretreatment 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, that is, local to the councilor receiving the letter. Enumerators at a call center fielded the calls.

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[12]Due to an implementation error, some 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. There were 80 such letters. These observations are dropped in all analysis that follows. As we show in Figure A1 in the supporting information (SI), the inclusion of these cases in the estimation does not impact the results in any qualitatively or statistically significant way.

[13]In SI Table A5, we estimate the effects of migrant status on callbacks in a regression framework, including the imbalanced covariate. The results are unaffected.

FIGURE 1 Balance on Pretreatment Covariates
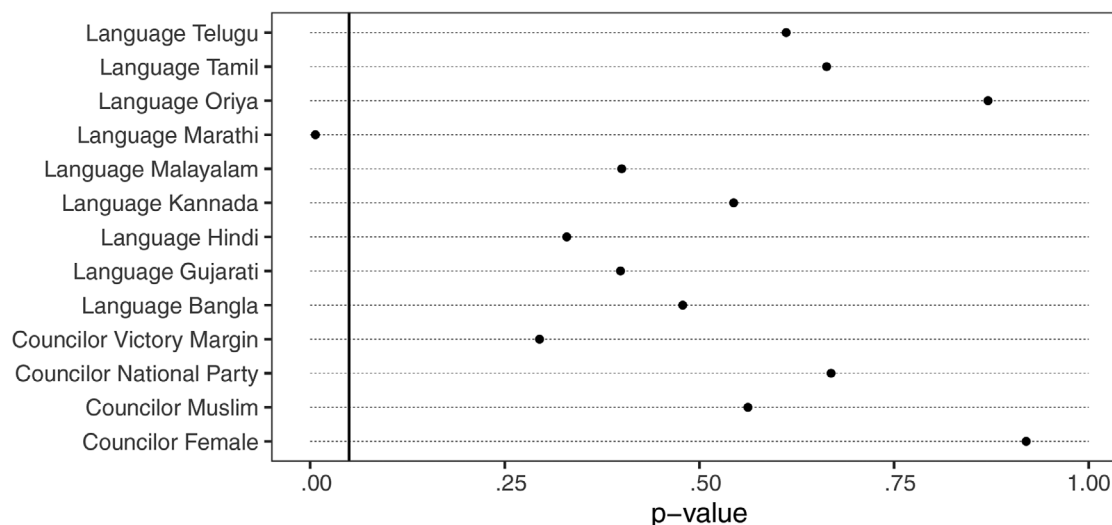
Note: This figure plots the p-values from two-sided t-tests of differences in means. We assess whether pretreatment covariates, listed on the vertical axis, are imbalanced across the migrant and local treatment conditions. N = 2,933 for all models except Councilor Victory Margin (N = 1,883) and Councilor National Party (N = 2,157).

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

Realism

We took several steps to make our treatments naturalistic. Before designing the letters, we asked ex-councilors from a large municipal council in northern India to show us a representative selection of letters they had received while in office. The letters varied in content and style—they were handwritten and typed, and asked for help with a wide range of issues. To mimic the "average" letter, we opted to keep the wording and sentence structures as simple as possible. We had 1,100 letters handwritten and the remainder typed.[14] All letters were sent in envelopes with handwritten names and addresses. To make sure the letters bore local postmarks, we mailed them from the city of the addressee.

India is a multilingual country. Treatment letters were written in a main local language spoken in each city.[15] Individuals frequently migrate from one language

region to another. Interacting with the state requires a lot of paperwork. Thus, it is common for illiterate individuals and non-native speakers to enlist scribes, friends, notaries, or local computer shop owners to pen documents on their behalf.

Naturally, letters are not the only means by which citizens solicit politicians' help. In-person meetings at councilors' homes 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 outweigh the small costs to public officials in terms of time and effort. At most, councilors had to read a five-line letter and a roughly 100-character text message, and make phone calls lasting about 20 seconds.[16]

We address two concerns. One is that the responsive politicians, who came to be informed about the research, would be less inclined to respond to subsequent requests from migrants for assistance. The average councilor, according to our end-line survey, received 293 requests for help per week. We deemed it unlikely that a brief audit would upend their workflow. Further, we did not disclose that the purpose of the study was to test for

[14]Letters were randomly assigned to be handwritten or typed. This variation did not lead to a detectable difference in response rate.

[15]Local languages include Bengali, Gujarati, Hindi, Kannada, Malayalam, Marathi, Oriya, Tamil, and Telugu. We infer that the language spoken by the corporators is a primary language spoken in the city where they serve.

[16]The experimental protocol was approved by Yale University's institutional review board (Protocol Number 1403013586).

the unequal treatment of migrants; given the number of attributes mentioned, the study's true objective would have been hard to discern from any single message. Another concern is that our study might affect the prospects for researchers accessing politicians in the future. This risk also seemed small. Very few scholars had previously investigated this little-understood but vital tier of the Indian political system, and this was the first countrywide audit experiment of its kind in India.

Our assessment was that the lessons learned from the experiment could potentially help improve the well-being of migrants, who are among the world's most disadvantaged communities—in India and elsewhere. To add to the research's expected benefits, we commit to communicating our substantive findings to policy makers and other relevant stakeholders. Concretely, we will (a) produce a policy brief to be circulated to the three commissioners of the Electoral Commission of India and to each of India's state electoral commissions; (b) participate in a dissemination event in India, in which we will present the study results; and (c) convene a workshop for migrant advocacy nongovernmental organizations to discuss an action plan for addressing the issues diagnosed here. The making of such commitments in a published academic article strikes us as good practice for researchers going forward.

Experimental Results

The main results are based on one-sided t-tests of differences in means. We estimate 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.[17]

Of the 2,933 letters mailed to councilors, 407 (13.9%) 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 politician audits conducted in other settings. In the United States, for example, the mean response rate to an audit measuring racial bias was 57% (Butler and Broockman 2011); in China, it was 32% (Chen, Pan, and Xu 2016); and in South Africa, it was 21% (McClendon

**FIGURE 2 Callback Rates for Requests for Help from Locals versus Migrants**
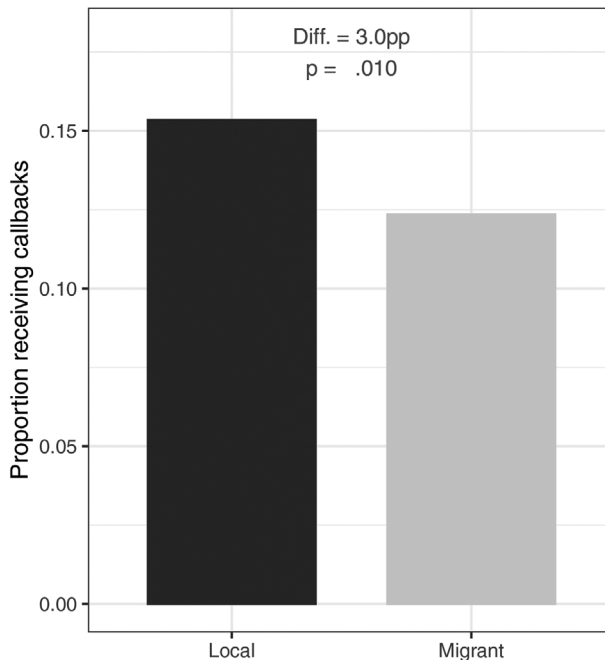
Note: Difference and p-value are based on a one-sided t-test. N = 2,933.

2016). The local urban politicians we investigate are less professionalized and more junior than those targeted in previous studies. In that sense, they are more akin to "street-level bureaucrats." Moreover, earlier studies have relied on digital platforms (especially emails) to contact politicians, whereas we posted handwritten and typed 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 local versus migrant requesters. Our estimates suggest that putative locals are 3.0 percentage points—proportionally, 24.1%—more likely to get a callback than putative migrants (p = .010). This is substantively meaningful in a context where receiving *any* form of response from politicians is challenging for citizens in general. In Figure 3, we plot the Kaplan-Meier hazard function by local/migrant treatment status. The proportion of letters going unanswered ("surviving") in the migrant condition virtually always dominates the equivalent proportion in the local condition, for each day following the mailing of the letters.[18]

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[17]Formally, we estimate average marginal component effects, assessing the average marginal effect of a treatment over the joint distribution of the other treatments. In the language of Muralidharan, Romero, and Wüthrich (2019), we employ "short" models. We use one-sided tests since our hypotheses, based on a large body of theory, are all unidirectional.

[18]Intriguingly, unequal treatment materialized mostly after the 6-day mark. One possibility is that councilors who answered messages later are under a greater cognitive strain, when prejudiced behaviors are more likely to occur.

FIGURE 3 Survival Analysis
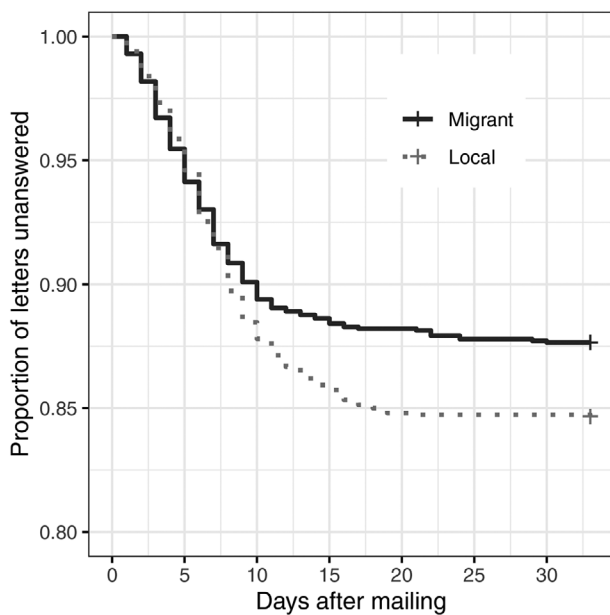
Note: This plot shows the proportion of letters that are unanswered for each day after mailing. N = 2,933.

In short, we uncover evidence of unequal treatment, with migrants at a disadvantage.

How does the impact of migrant status compare with that of other requester characteristics? Figure 4 shows that citizens with highly skilled occupations are 22.7% (2.8 percentage points) more likely than those with low-skilled occupations to receive callbacks (p = .013). Citizens with Hindu aliases are 22.7% (2.8 percentage points) more likely to receive a callback than Muslim-named citizens (p = .013). Councilors were equally likely to reply to requesters with female versus male names.[19] We detect some evidence that politicians are more reactive to problems that affect neighborhoods rather than individuals: Neighborhood problems were 2.0 percentage points (15.9%) more likely to elicit a response than individual problems (p = .055). Expressing membership in 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. This is an important finding in the study of political discrimination in countries experiencing rapid urbanization.

What produces the anti-migrant bias? In particular, do the results appear consistent with mechanisms emphasizing councilors' taste-based animus toward migrants, a channeling of locals' fears about the cultural and economic consequences of migration, or politicians' beliefs about the differential political participation of locals and migrants?

We exploit the fully crossed, factorialized design to assess how migrant status shapes returns to other requester attributes. Figure 5 plots estimates of the average treatment effects produced by the additional characteristics, conditioning on the migrant/local treatment condition (see SI Table A7 for formal interaction tests).

The pattern of results appears inconsistent with an explanation highlighting politicians' personal distaste for migrants. Recall that if politicians discriminate against migrants on the basis of animus, then their beliefs about migrants' political preferences and behaviors should minimally affect their willingness to respond to migrants' needs. Instead, Figure 5 shows that migrants gain from requesting assistance with a neighborhood (as opposed to an individual) problem (3.4 percentage points, p = .026), and they somewhat benefit from mentioning that they belong to the councilor's political party (2.3 percentage points, p = .093). That politicians assist with group problems afflicting migrant communities—assistance from which electoral returns might materialize—but not with individual problems, where the likelihood of picking up a vote is low, suggests that strategic vote-seeking is at work.

A second mechanism theorized was that politicians mirror the "localist" preferences of urban constituents in deciding whom to assist. This implies that low-skilled and ethnically outgroup migrants would be particularly disfavored. Figure 5 belies this prediction. Panels (a) and (b), respectively, demonstrate no evidence that politicians discriminate against migrants possessing these characteristics. Politicians do not seem to be focused on turning away migrants who carry the threat of "cultural dilution," fiscal burden, and job competition.

For local requesters, however, religious background and occupation level *do* prove impactful for callbacks (see Figure 5). Requests from Hindu locals are 4.3 percentage points more likely to get a callback than Muslim locals (p = .010), and high-skilled locals are 5.5 percentage points more likely to get a response than low-skilled locals (p = .001). What explains this gap? As discussed, the preference that politicians display for Hindu requesters may result from expectations of coethnic voting. When political competition divides along ethnic lines, voters face incentives to back coethnic representatives; politicians, in turn, expect to draw support from members of their own communities, and not others. We rerun the analysis coding a new treatment indicator for a "match" between

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[19]SI Tables A12 and A13 investigate co-linguistic and co-gender bias in responses, respectively. In SI Figures A2 and A3, we provide p-values corrected for multiple comparisons using the Benjamini-Hochberg method.

FIGURE 4 Callback Rates for Requests for Help across Four Randomized Attributes
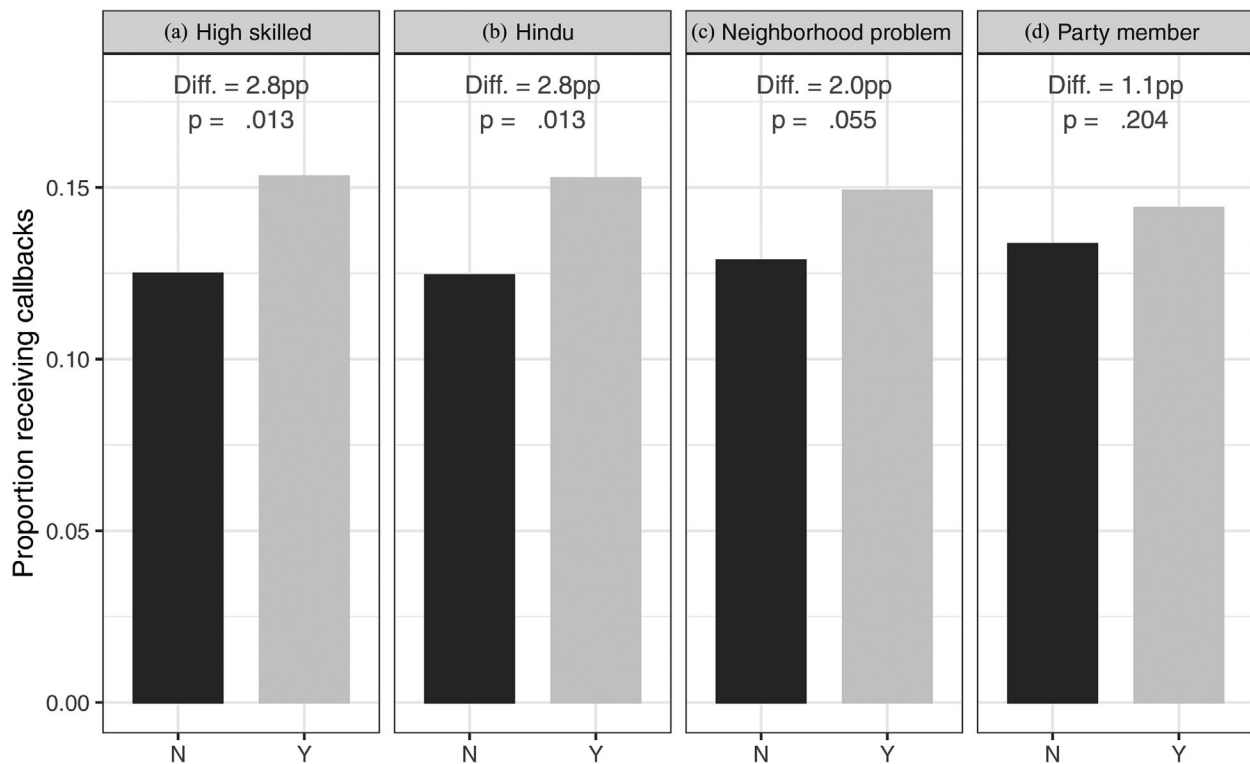
Note: Differences and p-values are based on one-sided t-tests. N = 2,933 for all models.

councilors' religion and the religion of the fictitious local requesters.[20] The statistically significant estimated treatment effect persists. In our interpretation, politicians may be declining to assist ethnic outgroups, from whom they do not anticipate future electoral support. The bias toward high-skilled occupations may be because politicians are more attentive to citizens who are net contributors to the welfare state, or because politicians hope to procure campaign contributions or other favors from wealthier, more connected local residents—points that we discuss further in the conclusion.

A remaining explanation for the unequal treatment faced by migrants is that it stems from expectations about political participation. If migrants are believed to participate less than locals, then migrants' ethno-religious and skills attributes should not matter for callbacks: What matters is the simple fact that they will not vote. This is what we observe. Politicians appear to be ignoring migrant requests in blanket fashion, regardless of migrants' skill and religion. At the same time, they do respond more upon receiving a signal that the migrant is polit-

ically active. Taken in conjunction, this pattern of results suggests that strategic electoral concerns are a compelling candidate mechanism for explaining politicians' decisions about whom to help.[21]

Testing Mechanisms: The Role of Voter Registration

We find that migrants are treated unequally by municipal councilors: Their chances of getting a callback are significantly lower than those for locals. Our discussion has highlighted a possible explanation. Councilors may not believe that migrants participate in urban elections at similar rates to locals. If correct, heeding migrant demands would be futile because assistance will not generate votes.

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[20]Specifically, this indicator takes 1 when requester and councilor both have or both do not have a distinctively Muslim name, and 0 otherwise.

[21]We additionally investigate whether politicians affiliated with regional parties are more biased against migrants than those affiliated with national parties. We find, in fact, *national* party politicians to be more biased against migrants (SI Table A9). These politicians are also much more responsive to requests overall. It may be that national party politicians are more competent, professionalized, and strategic in deciding whom to help.

FIGURE 5 Estimated Average Treatment Effects on Callback Rates Induced by Other Randomized Attributes, Conditional on Signaling the Requester to Be a Local or a Migrant
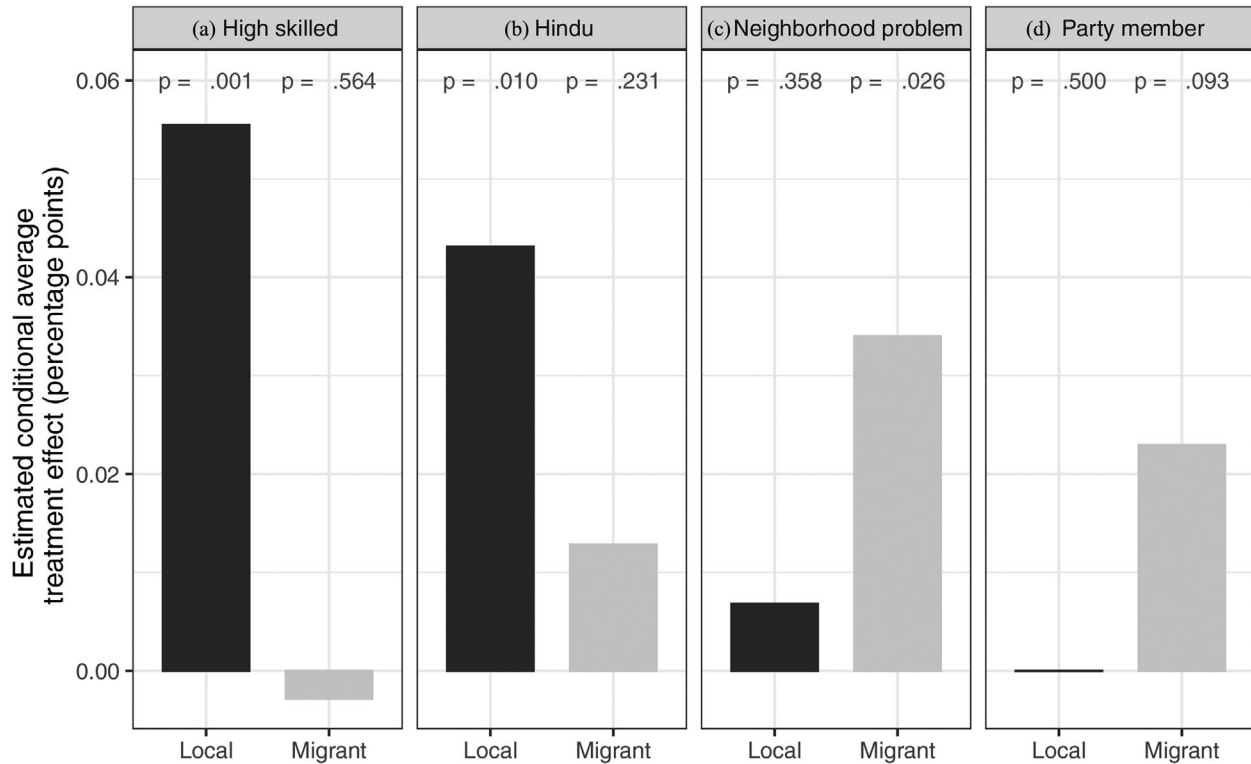
Note: Coefficient estimates and p-values are based on one-sided t-tests. N = 1,500 for *Local* models and 1,433 for *Migrant* models.

SMS Audit Experiment

To test more thoroughly the conjecture that councilors' beliefs about voter registration status underlie anti-migrant discrimination, we designed and carried out a second "mechanisms" experiment. It mirrors the first experiment in its basic setup. But this time, we sent short text messages to councilors. Text messages are cheaper than letters. They also allow us to see whether average response rates and treatment effect estimates generalize to other mediums commonly 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* [identification] card/street lamp fixed). For the main treatments, we prime requesters' local voter registration status and, among migrants, whether they claim to have voted for the councilor in the previous election.[22] The first treat-

ment sets out to test the hypothesis that voter registration status explains the disparity in councilor responsiveness. If the theory about participation expectations is correct, we should see a responsiveness shortfall for unregistered migrants, but not for registered ones. The second manipulation tests a further possibility: that anti-migrant bias stems from politicians' uncertainty about migrants' partisan preferences and whether migrants' votes can be swayed by the provision of constituency service.

The five treatment groups are as follows:[23]

1. *Local-registered* [*Pr(assignment)* = $\frac{1}{8}$]. "i'm [Arjun / Salman]-[construction worker / engineer] in ur ward. me & my family r originally from this city. we are registered 2 vote here. could u help me get [aadhaar card / street lamp fixed]?"

conveying registration status explicitly may signal deep political engagement and a greater likelihood of enforcing accountability in the event of a nonresponse.

[22]We vary registration status for both locals and migrants; even for locals generally presumed by politicians to be registered to vote,

[23]Note that the assignment probabilities are $\frac{1}{2}$ for each of the sub-treatment alternatives.

2. *Local-unregistered* [*Pr(assignment)* $= \frac{1}{8}$]. "i'm [Arjun / Salman]-[construction worker / engineer] in ur ward. me & my family r originally from this city. we aren't registered 2 vote here. could u help me get [aadhaar card / street lamp fixed]?"

3. *Migrant-unregistered* [*Pr(assignment)* $= \frac{1}{4}$]. "i'm [Arjun / Salman]-[construction worker / engineer] in ur ward. me & my family r originally from [bihar / assam]. we aren't registered 2 vote here. could u help me get [aadhaar card / street lamp fixed]?"

4. *Migrant-registered* [*Pr(assignment)* $= \frac{1}{4}$]. "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]?"

5. *Migrant-registered-vote* [*Pr(assignment)* $= \frac{1}{4}$]. "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 2,513 of the 2,933 councilors messaged in the first experiment.[24] 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%. This is very similar to the overall callback rate in the letters experiment (13.9%), suggesting that the choice of contacting method is unlikely to limit our results' generalizability.

Table 2 presents the findings. Each row gives the results of a t-test of difference in means between pairs of treatment conditions. The results lend credence to the claim that beliefs about migrants' low electoral participation drive anti-migrant bias. A registered local is 4.2 percentage points—proportionally, 41%—more likely to receive a callback than an unregistered migrant, a highly statistically significant difference (Table 2, row A). Among "registered" migrants, however, the migrant penalty disappears: We cannot reject the null hypothesis of no difference in callback rates between registered locals and registered migrants (Table 2, row B) or between unregistered locals and unregistered migrants (Table 2, row C). Politicians appear to be treating natives and migrants

equally, conditional on their believing that each applicant type possesses the same registration status. That said, the data do indicate that politicians are somewhat more forgiving of natives being unregistered than migrants. The likelihood of getting a callback is 2.8 percentage points higher for registered versus unregistered migrants (Table 2, row D); meanwhile, the equivalent (but insignificant) responsiveness gap between registered and unregistered locals is 1.7 percentage points (Table 2, row G).[25]

Does providing a signal of migrants' past voting history boost politician responsiveness still further? Table 2, rows E and F show the average callback rate for registered migrants who also claim to have voted for the councilor previously to be 15%. Communicating a migrant's political support does not confer a measurable advantage relative to registered migrants (Table 2, row E). However, it is notable that a registered migrant claiming to have voted for the incumbent previously is 4.8 percentage points more likely to get a callback than an unregistered migrant—a sizable difference (Table 2, row F). Thus, compelling evidence emerges that variation in migrants' political attributes—above all, local registration status—affects politician responsiveness.

We independently varied three more 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 2.7 percentage points (p = .023) more likely to receive a callback than a Muslim-named one; high-skilled requesters are 5 percentage points (p = .000) more likely to receive callbacks than low-skilled requesters; and a neighborhood problem is 2.4 percentage points (p = .038) more likely to get a callback than an individual problem. These align closely with the results of the letters experiment.

In additional tests (not shown), we find that migrant registration status partially shapes the returns to these other attributes. Having a Hindu name as opposed to a Muslim name proves advantageous for registered migrants but inconsequential for unregistered migrants. This accords with the notion that simple electoral arithmetic may lie behind callback decisions.[26] All else equal,

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[24]In SI Table A10, we confirm there is no evidence of spillovers between the two studies. Note that because the contact medium and message wording of the second experiment differed from the first, the risk of detection is negligible. SMS messages were sent approximately four months after the letters used in the first experiment.

[25]It should be emphasized, though, that the difference between these differences is not itself significantly different, and we have fewer observations to detect a within–local registration effect in Table 2, row G.

[26]We note that it is conceivable that politicians are ethnically biased for taste-based reasons, but that we do not *observe* this bias for unregistered citizens because politicians' overriding desire for votes causes them to overlook requests from unregistered citizens altogether.

TABLE 2 Differences in Proportions of Callbacks Received across Pairs of Treatment Conditions in the SMS Experiment

Comparison (C1 vs. C2)	C1 Prop.	C2 Prop.	C1 − C2	p-value	N
A. Migrant-unregistered vs. Local-registered	0.102	0.144	−0.042	.040	924
B. Migrant-registered vs. Local-registered	0.130	0.144	−0.014	.290	915
C. Migrant-unregistered vs. Local-unregistered	0.102	0.127	−0.025	.130	963
D. Migrant-unregistered vs. Migrant-registered	0.102	0.130	−0.028	.057	1,269
E. Migrant-registered vs. Migrant-registered-votes	0.130	0.150	−0.019	.160	1,265
F. Migrant-unregistered vs. Migrant-registered-votes	0.102	0.150	−0.048	.005	1,274
G. Local-unregistered vs. Local-registered	0.127	0.144	−0.017	.267	609

Note: Each row represents one model. Differences and p-values are based on one-sided t-tests.

favoring coethnic petitioners is worthwhile given the tendency for ethnic groups to vote en bloc for coethnic candidates; yet it is only worthwhile, we suggest, when the petitioner in question is a probable voter. Registered migrants fit this bill, whereas unregistered ones do not. This chimes with the results from our letters experiment. We saw that only locals—who are presumed to be more likely to turn out to vote than migrants—suffer discrimination on the basis of religion, a finding that does not apply to migrants.

FIGURE 6 Survey Experiment Assessing Councilors' Beliefs about (a) Whether and (b) How Likely It Is That Different Types of Citizens Are Registered to Vote Locally
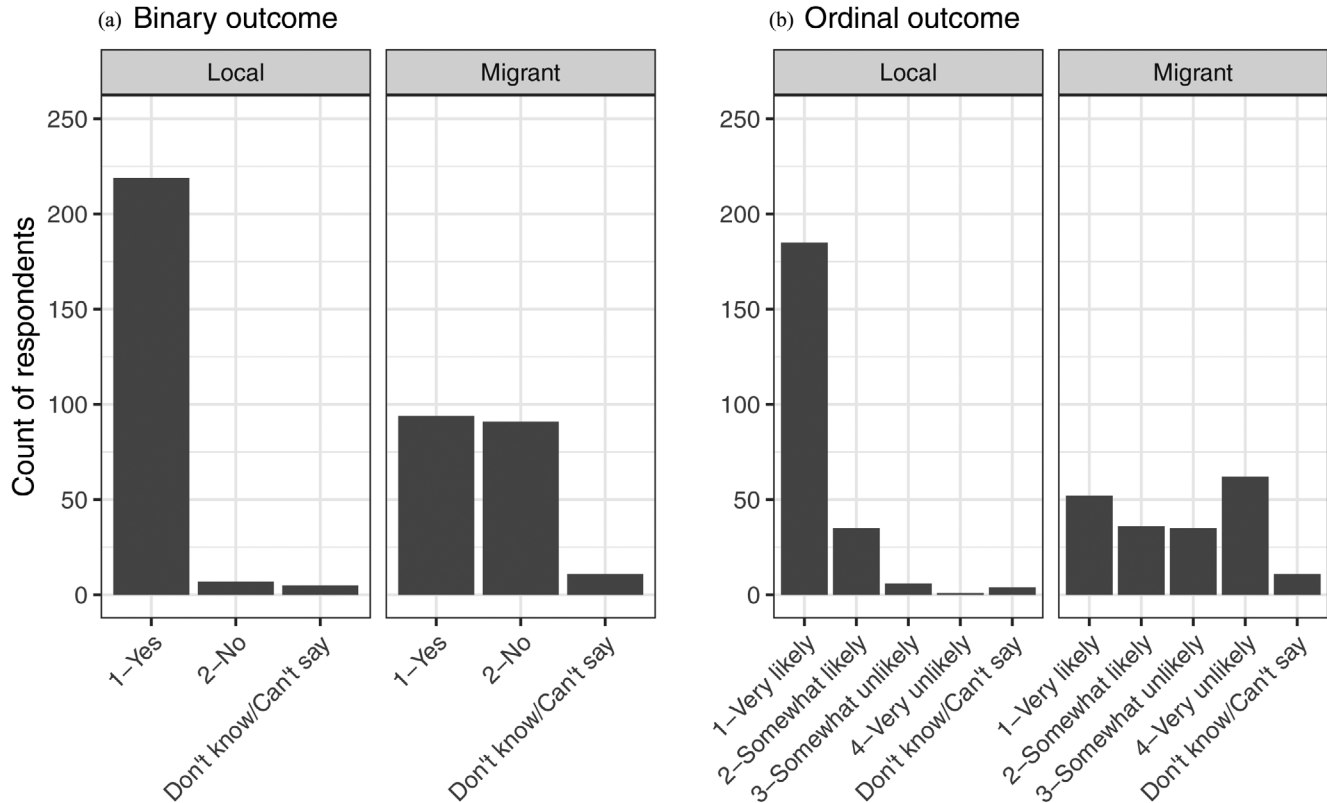
Note: The plots show counts of responses to each response option, broken down by local/migrant treatment condition. Overall N = 427. See main text for question wording.

Politicians' Beliefs about Migrant/Local Registration: A Survey Experiment

For a final test of the proposed mechanism, we implemented a survey experiment to explore directly whether politicians believe that migrants and locals are differentially likely to participate in city elections. We attempted to contact 1,500 councilors by telephone. Subjects were randomly sampled from the original list of 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.]

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?
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?

Figure 6 plots the counts of responses to each question, broken down by treatment condition. We find large effects: 97% of councilors presented with a long-term resident believed the citizen would be registered; the equivalent figure for migrant citizens is 51%, a difference of 46 percentage points (p = .000). An analysis of answers to the second question yields the same conclusion. Mentioning that the citizen is a migrant instead of a local leads to answers that are 1.36 points lower, on average, than the corresponding answers for locals on a 4-point likelihood scale (p = .000).

We take these results as persuasive evidence that councilors maintain very different views about migrants and local citizens. Whereas locals are overwhelmingly thought to form part of the local electorate, migrants are not. For this reason, politicians shirk more in providing constituency services in response to requests from recent migrants, meaning that a large class of citizens goes underrepresented.

Conclusion

We present the first large-scale study investigating anti-migrant discrimination in India. Using a series of 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 the bias associated with religion, gender, occupation level, and problem type. Our follow-up experiments indicate that it derives in significant measure 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 city residents, who they believe are more likely to be registered.

Migrants' de facto disenfranchisement may have adverse welfare consequences. In developing countries, citizens from vulnerable population groups are especially dependent on the brokerage services that local politicians provide (Stokes et al. 2013). The cumulative welfare effects of anti-migrant bias are likely considerable given the burgeoning number and size of cities in the Global South. The scope for discrimination against migrant communities looks set to rise substantially.

We identify anti-migrant discrimination, and a probable explanation for it, when looking at constituency service delivered privately to voters by politicians. Yet candidates may continue to echo nativist citizen sentiments in other politicking in which they engage—policy formulation, campaign literature, and speeches at rallies, to name three. For these activities, there is ample evidence that politicians exploit popular prejudice against migrants to rouse support (Gaikwad and Nellis 2017; Weiner 1978). Public acts of this sort may be governed by a different strategic calculus.

Our inferences come from the domain of internal migration, yet the electoral trade-offs we identify are also instructive for the study of international immigration politics. In several countries, such as the United Kingdom and Sweden, immigrants hold the right to vote in local elections (Dancygier 2010). Elsewhere, immigrants are frequently in a position to naturalize and thus register to participate in elections. In these contexts, immigrants might serve as appealing vote banks for politicians, while also being hurt by their group's reputation for low turnout, hinting at our thesis's wider applicability.

It is worth considering possible longer-term effects. Looking forward, we can imagine two states of the

world. In the first, migrants' political exclusion becomes chronic. Politicians believe that migrants will not vote and thus discriminate against them; consequently, migrants' failure to draw attention from incumbents alienates them from local politics, making registration seem pointless. Political exclusion and suboptimal welfare outcomes become self-fulfilling. Alternatively, however, if migrants do register and vote, politicians' calculus will change and their responsiveness toward migrants should increase, leading to a boost in migrants' sense of political efficacy.

Therefore, while pessimistic, our article's results indicate a path forward. Encouraging migrants to register to vote in destination-city elections (e.g., by running registration drives in migrant slum settlements or easing registration laws) may improve political engagement, representation, and human development. Politicians' insouciance toward migrants appears to flow primarily from short-term electoral concerns. That said, the effectiveness of such campaigns is not preordained. We cannot rule out the possibility that migrants encounter discrimination at the point of registration. The officials who oversee registration may harbor animus toward migrants; the local bureaucracy might also be captured by politicians eager to forestall certain groups from registering. On top of that, and even assuming success, politicians' stereotypes about migrants might endure for a long time.

Our results also carry a certain irony. Moves to become more like locals—as by registering—may help migrants obtain more benefits on average. Yet, at the same time, it may also spur politicians to start heeding migrants' additional attributes (e.g., religion), which politicians use to sort locals when directing assistance. In other words, escaping one kind of discrimination may open the door to other varieties. Understanding these constraints should be a priority for future research.

Our study illuminates several other dimensions of inequality, besides migrant status, in India's political system. Hindus and higher-skilled citizens enjoy much better access to constituency services than Muslims and lower-skilled citizens. The results on religion buttress a large body of qualitative literature and some quantitative studies—although, to our knowledge, this constitutes the most direct test of religious-based discrimination by Indian politicians to date. Our findings on occupation- and class-based discrimination are novel. Several possible explanations come to mind for the result we observe. 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 more plau-

sible campaign contributors. Politicians themselves may come from wealthy social strata and may lean toward their own types. Or a politician might think that wealthier, connected citizens are better positioned to complain to party leaders in the event that no satisfactory response is received. Future work should try to disentangle these mechanisms.

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

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Appendix B: Additional data description
Appendix C: Regression estimation of main results for the letters experiment
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Appendix I: Effects in letters experiment by party type
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Appendix K: Testing for alternative in-group preferences in the letters experiment