

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

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March 15, 2016

*We thank Peter Aronow, Jennifer Bussell, Thad Dunning, Gregory Huber, Kenneth Scheve, Tariq Thachil, Michael Weaver, Steven Wilkinson, and participants at Yale University, the University of California–Berkeley, Stanford University, the American Political Science Association 2015 Meeting, and the International Political Economy Society 2015 Meeting for useful comments. Yashwant Deshmukh and Gaura Shukla provided indispensable assistance in implementing the experiments. For funding, we gratefully acknowledge the CVoter Foundation and the International Growth Center.

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Abstract

In recent decades, the global south has witnessed an explosive increase in the number of people relocating from rural to urban areas. Yet many migrants struggle to integrate into destination cities, facing severe hurdles to accessing adequate housing, as well as essential public goods and services such as healthcare and education. We posit that a key explanation for these difficulties lies in unequal political representation. We conduct two audit experiments to test whether urban politicians discriminate against internal migrants vis-à-vis long-term residents (“natives”) in providing essential constituency services. We find that fictitious migrants are 23% less likely to receive a callback from a councilor in response to a mailed letter request for assistance compared to an otherwise similar native. What mechanisms explain this effect? In a second experiment using SMS, we show that migrants signaling that they are registered to vote in municipal ward elections receive callbacks at much higher rates than migrants signaling they are unregistered. Even more strikingly, signaling that migrants are registered to vote closes the migrant-native callback gap documented in the first experiment. We take this to indicate that politicians’ beliefs about migrants’ generally low participation in city elections leads them to ignore requests by migrants for help, because they foresee no electoral returns to providing assistance. Overall, this paper informs policy debates about how to improve the welfare of internal migrants, who count among the world’s most marginalized population groups.

In recent decades, cities and towns across the global south have witnessed explosive population growth—a trend spurred, in significant measure, by rural-to-urban migration (Bell and Charles-Edwards, 2013). Fast-paced urbanization generates sizable economic gains. Economists from Adam Smith to Karl Marx viewed metropolitan expansion as both the “natural outcome of the development of the productive forces as well as the launch pad for sustaining that development” (Merrifield, 2013, 22), while modern commentators have dubbed cities “our greatest invention” (Glaeser, 2011). Yet—and as Marx famously recognized—such rapid demographic transformations carry the potential to dramatically reconfigure social and political life. Across the urban centers of the developing world, migrants hungry for opportunity and advancement contribute to a burgeoning, marginalized underclass (Davis, 2006). Teeming informal settlements—characterized by high crime levels, as well as inadequate infrastructure, housing, healthcare, and education—are hallmarks of megacities such as Rio de Janeiro, Lagos, and Mumbai (Auerbach, 2016). With the world’s urban population projected to increase by 2.5 billion people by 2050, and with 90 percent of that surge concentrated in Asia and Africa, the task of integrating internal migrants ranks among the most urgent challenges confronting governments across the global South today (United Nations, 2014).

What accounts for patterns of government neglect in cities undergoing rapid growth? In particular, do elected officials charged with providing essential goods and services to urban citizens respond differently to migrant newcomers compared to long-term city residents? And if so, on what basis? Despite recent attempts to document the hurdles encountered by international immigrants in Western Europe and the United States (Adida, Laitin and Valfort, 2010; Dancygier, 2010; Hainmueller and Hangartner, 2014), existing scholarship remains blind to the parallel challenges faced by internal migrants in poorer countries. Given that domestic population flows numerically far outstrip international immigration movements, this represents a serious omission—one that we set out to rectify.

This paper theorizes and tests the determinants of politician behavior with respect to internal migration. From the standpoint of elected urban elites deciding how to optimally allocate scarce fiscal and political capital, internal migrants present a unique dilemma. On the one hand, urban politicians face strong electoral incentives to play the nativist card (Weiner, 1978; Katzenstein, 1979). Incumbents are first and foremost accountable to their base—i.e. the social coalition that got them elected (Lipset and Rokkan, 1967). Because long-term city residents fear the searing economic and cultural shifts wrought by high-volume in-migration, politicians will mete out unequal treatment to migrants in order to placate local voters and keep them onside. Conversely, we argue that a key distinguishing attribute of within-country migrants—namely, their constitutional right to vote in destination-city elections—points to the opposite conclusion. In so-called patronage democracies, electorates cast their ballots for politicians capable of providing selective state benefits (Stokes et al., 2013). Hence a basic electoral logic dictates that domestic newcomers, as a fresh source of potential support for vote-maximizing incumbents, will receive equal treatment relative to their native counterparts. For these virtuous incentives to operate, however, incumbent politicians must perceive that natives and migrants participate in urban elections at similar rates. If migrant populations as a group evidence below-average levels of voter registration and turnout, officeholders will foresee few electoral returns to catering to migrant interests.

To adjudicate which of these accounts holds sway, we conducted two nationwide field experiments in India—an emerging economy whose urban population is expanding at lightening speed. We compiled comprehensive lists—including names, mailing addresses, telephone numbers, and background characteristics—of sitting municipal councilors in 28 major Indian cities. Municipal councilors are responsible for granting access to myriad individual and neighborhood goods and services, from the provision of basic primary healthcare to helping constituents obtain income certificates, ration cards, and pension benefits. In this capacity, they serve as crucial intermediaries between citizens and the

state. At the same time, councilors operate in a highly competitive electoral environment and enjoy considerable discretion in targeting resources and assistance (Oldenburg, 1976; Berenschot, 2010). By subjecting councilors to an in-depth audit, we seek to uncover what motivates politician behavior on the issue of internal migration.

In the first experiment, we mailed a mix of short handwritten and printed letters (“chits”) to 3,013 councilors for whom we had postal addresses. Within the letters, we randomly varied both the identity of the hypothetical petitioner, and the problem for which they were requesting help. The main manipulation involved petitioners signaling long-term residence in the city, versus recent migration to the city from a different Indian state. In addition—and independent of migrant status—we varied the gender, religion, and skills-status of the requester. The letters concluded by asking the councilor to call back the citizen at a mobile phone number. Since all attributes of the requesters were randomized, comparing average callback rates across the various treatment conditions yields consistent estimates of the marginal effect of switching requester identities. The principal result to emerge from the first experiment is that “native” requesters are 23.4% more likely than otherwise identical migrant requesters to receive a callback from their local councilor, leading us to reject the null hypothesis that politicians accord equal benefits to migrant and native citizens.

What drives anti-migrant discrimination? To elucidate the underlying mechanism, we performed a second experiment, conducted four months after the first one on a subset of 2,513 urban politicians drawn from our original sample. We sent text messages (a.k.a. Short Message Service, or SMS) to councilors’ mobile phones. Once again, each message contained a brief request for help, and the identity of the requester was signaled to be either native or migrant. On this occasion, however, we manipulated requesters’ *political* attributes. Most significantly, the requester reported being either registered or unregistered to vote in the councilor’s electoral ward. Tabulating the results, we find compelling evidence that politicians’ perceptions about migrants’ registration status

underlie the observed discrimination. Migrants reporting that they are *not* registered to vote were 24% less likely than natives to receive a callback. But migrants reporting that they *are* registered to vote received callbacks at a statistically indistinguishable rate from that seen for otherwise comparable natives. Put differently, after clarifying migrants' voter registration status, the responsiveness gap closes and discrimination vanishes. We take this as strong evidence for the claim that the representational shortfall migrants suffer at the hands of urban politicians is not due to animus; rather it is an outcome of a simple calculation made by politicians, based on their beliefs about migrants' low propensity to vote in city elections.

A follow-up survey experiment on 412 of the councilors provides the final piece of corroborating evidence. We presented each politician with a fictitious citizen—signaled at random to be either a native or a migrant—and asked for the interviewee's assessment of how likely it was that such a citizen would be registered to vote locally. Migrants are seen as 46 percentage points less likely to be registered compared to natives. Taken in conjunction, these three experiments paint a clear picture: India's urban politicians perceive internal migrants as inactive voters, and neglect to assist them for this reason.

While this paper tells a straightforward story, its implications are far-reaching. We identify a previously overlooked form of unequal political representation in the world's largest democracy and provide a mechanism that appears to largely explain its cause. The finding suggests a low-cost policy intervention that could significantly ameliorate the problem. Providing assistance to recent migrants in registering to vote, and informing politicians that this registration process is underway, can reliably be expected to increase politicians' responsiveness to this vulnerable population group.

Internal Migration: The Politician’s Dilemma

Unequal representation on the basis of group characteristics is a prevalent feature of democratic politics in modern states. Extensive evidence demonstrates the importance of shared traits for the targeting of material resources by elected elites. Both ascriptive identities—including race (Butler and Broockman, 2011; McClendon, 2012), religion (Adida, Laitin and Valfort, 2010), and ethnicity (White, Nathan and Faller, 2015)—as well as more malleable types of social kinship such as partisanship (Dunning and Nilekani, 2013) have been shown to matter in this regard. But should we expect migrant/native status to elicit differential responsiveness on the part of sitting urban officials? Prior literature has explored politicians’ behavior with regard to international immigration, focusing (inter alia) on legislative position-taking on restrictive immigration policy, the emergence of far-right nativist parties, and the utility of anti-immigrant rhetoric for elites seeking to forge cross-class coalitions (Dancygier, 2010; O’Rourke and Williamson, 1999; Pettigrew, 1998; Schain, 2006). Yet the internal migration context sets into relief a sharp trade-off that office-seeking politicians must face: courting local votes versus courting the votes of migrants themselves. We now elaborate this dilemma.

Courting Native Votes Political actors are commonly presumed to aggregate and advance the interests of the societal coalition that elected them (Downs, 1957). If officeholders fail to calibrate their behavior to voter preferences, they invite punishment at the ballot box. For reasons we now outline, long-term city residents are frequently hostile toward in-migration—in particular the high-volume kind typified by rural-urban population flows.

The cross-national immigration literature supplies two broad classes of explanations for popular antagonism toward migration. Culturalist accounts—anchored in psychology and sociology—posit that natives are wary of cultural diversity, 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 recent studies urge more skepticism.¹ A second class of explanation, centered in economics, emphasizes the labor-market and fiscal consequences of migration. 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 influxes of workers with skill sets similar to their own (Scheve and Slaughter, 2001). Fiscally, meanwhile, low-skilled migrants are expected to impose additional taxes on natives and lead to a decrease in per capita transfers (Facchini and Mayda, 2009; Hanson, Scheve and Slaughter, 2007). Overcrowded infrastructure, strained public services, proliferating slum colonies, and hikes in property taxes to meet the demands of an enlarging city population are oft-heard complaints in developing country metropolises.

These theories yield several empirical predictions. In a passive sense, officials will acquiesce to native biases and abstain from channeling benefits to migrant newcomers. Politicians will be especially prone to withholding resources from newcomers perceived as ethnic out-groups, and those endowed with fewer skills (migrant attributes viewed as most objectionable from natives’ perspective). In line with this prediction, scholars in the immigration domain have showed how underlying nativist fears in the citizenry find echo in politicians’ legislative and day-to-day activities (Goldin, 1994; O’Rourke and Williamson, 1999). In a more active sense, politicians may attempt to actively galvanize support by playing the nativist card—that is, stoking local resentments against migrant outsiders as a means of winning votes. Such strategies are endemic to rightist political parties in western Europe (Howard, 2010; Pettigrew, 1998). “Sons of the soil” parties have made significant inroads in developing democracies too (see, for example, the Shiv

¹Recent work on local preferences over internal migration identifies more conditional effects (Gaikwad and Nellis, 2014).

Sena in India; Weiner 1978; Katzenstein 1979).² The core expectation we derive is that elected officials will discriminate against internal migrants versus otherwise comparable natives.

Courting Migrant Votes Contra the foregoing account, an alternate electoral strategy for urban politicians seeking to maximize their re-election chances may be to capture migrants' votes. Citizens who relocate from one region of their home country to another region possess the formal, constitutionally-guaranteed 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—after all, a vote is a vote. This being the case, the best option may be to tap the fresh pool of migrant support—particularly in situations where migrant votes are seen as pivotal to the outcome of the election (Dancygier, 2010). Practically speaking, such a strategy entails the provision of selective assistance and state benefits to migrants—which, as extensive studies show, is liable to evoke voter gratitude and boost pro-incumbent voting (Bechtel and Hainmueller, 2011; Thachil, 2014*a*).

However, the strategy of pursuing migrant votes hinges on a critical assumption—namely 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 may fall well below those of their native counterparts. Voter registration is a costly and cumbersome exercise for many citizens, standardly requiring proof of identity and residence, the completion of a local-language form, visits to government offices, dealing with prejudiced and under-motivated staff, and sometimes the payment of bribes (White, Nathan and Faller, 2015; Nickerson, 2015). In many settings, citizens are required

²Importantly, the mere presence of anti-immigrant parties can have a “contagion” effect, shifting entire political systems to the right as even centrist politicians are forced to take a stance on the issue of migration (van Spanje, 2010; Bale, 2008).

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 political effects of the Moving to Opportunity experiment, Gay (2011) finds that migrants (“movers”) are 3 percentage points less likely to be registered to vote than a stationary control group (“stayers”) 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 the citizenry from voting, with the greatest exclusion afflicting vulnerable population groups including immigrants.

Though seemingly mundane, bureaucratic impediments 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 and 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 internal migrants are *de facto* disenfranchised in a similar fashion—owing to low rates of voter registration in destination cities—politicians will perceive few electoral benefits to helping this class of citizens. In sum, the real and perceived political characteristics of migrants might moderate politicians’ inclination to view migrants as a source of votes—a proposition we carefully explore.

Context

Municipal Corporations in India

India operates two forms of representative local government. Rural areas are governed by elected panchayats, while urban areas are governed by municipal corporations (MCs). The elected members of India’s MCs are referred to as councilors or corporators. These are the officials whom we seek to audit. Councilors are elected to single-member districts

(“wards”) approximately once every 5 years under simple plurality rules. The total number of councilors, and the magnitudes of the wards they represent, vary across cities (Bhavnani, 2009). City revenues are drawn from property taxes, entertainment and vehicular taxes, fees and fines, returns on municipal assets and investments, and grants-in-aid from state and central governments.

The 74th Amendment of the Indian Constitution grants MCs expansive formal powers and responsibilities. These include construction and maintenance of roads, water supply, drainage, fire brigades, public lighting, sewage systems, in addition to education and public health (Bhagat, 2005). MCs also perform crucial regulatory functions, such as the enforcement of building codes.

Apart from carrying out their formal duties, municipal councilors play an important informal role functioning as mediators between constituents and the state. Put differently, they facilitate access to the goods and services which the government provides. Like in most developing countries, India’s bureaucracy is overstretched and encumbered with problems such as corruption and shirking. Citizens find it difficult to directly interface with public officials. In Ahmedabad, for example, “residents often used the expression ‘*dhakka khaavadave chhe*’ [“getting pushed around”] to describe their experiences with the bureaucracy ... you have to visit the relevant officials again and again without any result” (Berenschot, 2010, 889). In the face of such representational hurdles, municipal councilors act as “fixers” or middlemen connecting India’s city residents to government agencies (Manor, 2000). When an individual or neighborhood problem arises, citizens’ first port of call is often to their local councilor. Councilors then endeavor to solve the problem using a mix of instruments: notarizing documents, making calls and formal requests to zonal and ward-level staff, disbursing money from their discretionary funds,³ and contacting higher-level politicians. One councilor claimed that he “does as much work as an MLA and an MP put together” (Oldenburg, 1976, 240).

³These include the Municipal Councillor Local Area Development Funds (MCLADS).

What incentivizes India’s councilors to perform these formal and informal duties? According to ethnographic research, electoral motivations hold the key. Councilors are not term-limited.⁴ Therefore, the desire to win re-election—and/or the desire to build up local support so as to compete in elections for higher office—spurs many councilors to win favor with voters by attending to the development of the ward, and by doing case work on constituents’ behalf. In Berenschot’s analysis, “as citizens have come to rely on services that the state provides, politicians are judged on the basis of their capacity to provide access to these services” (Berenschot, 2010, 888). As one councilor put it, “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).

To be sure, several studies cast doubt on the competence of many politicians within this tier of elected officials. Using survey data, de Wit (2009) reports low levels of citizen satisfaction with the work done by the Municipal Corporation of Delhi (MCD). Buttressing this countervailing view, Swain (2012) interviewed a large number of councilors and detected minimal knowledge about the MC procedures and budgetary processes. Rent seeking might explain this uneven performance. As de Wit (2009) explains in the Delhi context, “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.” In other words, councilors are by no means uniformly public spirited.

While councilors exhibit varying levels of commitment to their constituencies, they also differ in which citizens they select to help. Ethnographic evidence strongly suggests that constituent-level discrimination is rampant. This discrimination is often manifested in economic or ethnic forms, yet appears to be driven by electoral considerations. Consider the following quotations from Berenschot (2010):

⁴Note that the rotation of reservations for women can impose de facto term limits. That said, there is widespread “capture”—many of female corporators are the wives of the former corporators, who are the de facto power holders.

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. Since control over the resources of the hospital is an important instrument to gain electoral support, the political competition outside the hospital shapes the daily struggle for hospital beds and cheap treatment within the hospital (895).

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 (896).

Inferring responsiveness—let alone discrimination—from observational data is challenging. Fieldwork suggests that few, if any, councilors keep systematic records of their case loads, and Oldenburg (1976, 238) found that councilors were prone to exaggerating the extent of their contact with citizens. We ourselves interviewed a number of councilors in-depth, and in no case did a councilor admit to discriminating against *any* class of individuals. Citizen reports or surveys, too, may be unreliable. For example, they would be uninformative if citizens expect low responsiveness from politicians, and thus do not go to them for help. The need for a large-scale, systematic evaluation of urban politicians in India thus helps motivate this study.

India’s Rural-Urban Migrants

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.” According to the National Sample Survey (NSS), there were 326 million internal migrants in India as of 2007–9, comprising 29% of the country’s population. 35% of India’s urban population were recorded as being migrants. Inter-state migration has been a major area of migrant growth in the past two decades, with the biggest sending regions being the two northern states of Bihar and Uttar Pradesh, while the largest receivers are the fast-growing states of Delhi NCT, Gujarat, and Maharashtra. Impressive as these figures appear, official statistics dramatically underestimate the true extent of internal

migration, and particularly labor mobility. This is because government operational definitions exclude seasonal migrants—i.e. those who relocate to cities in search of work during periods of low demand for agricultural labor. This group is purported to number 100 million individuals, most of whom, it is believed, now spend the majority of the year in their destination cities (Deshingkar and Akter, 2009).

The social profile of the migrant population is variegated (Srivastava and Sasikumar, 2003). On one tail of the distribution, historically marginalized communities such as scheduled castes, scheduled tribes, and other backward castes, are overrepresented. At the other tail, NSS data reveal a greater incidence of long-term migration among households in higher income deciles compared to lower ones (Rajan, 2014, 232). Reflecting this disparity, the Gini coefficient for migrants is higher than that for non-migrants (de Haan, 2011, 11).

With respect to migrant welfare, the general picture is one of deprivation. On Deshingkar and Akter (2009)’s assessment, “migrants remain on the periphery of society, with few citizen rights and no political voice in shaping the decisions that impact their lives.” A United Nations report concurs, emphasizing that “internal migration has been accorded very low priority by the government, and existing policies of the Indian state have failed in providing legal or social protection to this vulnerable group” (UNICEF et al., 2013). Statistics corroborate this claim. Public health research documents an adverse association between migrant status and health outcomes in India (for a summary, see Nitika, Nongkynrih and Gupta 2014). Compared to natives, migrants display much lower vaccination rates, higher child mortality, worse malnutrition, higher alcohol consumption, greater prevalence of sexually transmitted infections, as well as cardiovascular diseases. Other forms of exclusion also obtain. Thachil (2014*b*) finds in a sample of Delhi construction workers that only one in five migrants had voted in city elections, and 80% had only village voter ID cards. Migrant slums are characterized by government neglect (Auerbach, 2014). Of course, many migrants prosper in cities, particularly those with

high skills. But for many poor Indian migrants, life is permeated with hardship.

Research Design

Audit experiments have emerged as a valuable tool for detecting systematic biases among employers, bureaucrats, and politicians (Putnam, Leonardi and Nanetti, 1994). In the standard set-up, researchers generate a set of communication documents—for example, resumes or petitions for assistance—and, within those documents, randomly manipulate relevant characteristics of the document’s fictitious author (the “requester”). These documents ask the subjects of the experiment to respond to the request, and provide the necessary contact information. The documents are then mailed to the subject pool. Because requester identities are randomly assigned to subjects, a comparison of average response rates across requester-identity groups yields a consistent estimate of the effect of requester identity on the likelihood of response, with differential rates suggesting discrimination.

We implemented two audit experiments to test for discrimination against internal migrants. The research design proceeded in several steps. To start, we compiled lists and accompanying information for all municipal councilors in 28 of the largest Indian cities.⁵ We sought to include all state capitals in the sample, as well as the ten most populous cities in the country. Together, our cities represented migrant-receiving destinations in states with a combined population of over one billion people; the municipal councilors in

⁵These cities were: Agra, Ahmedabad, Amritsar, Bengaluru, Bhopal, Bhubaneswara, 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 in order to guarantee the safety of our research team.

our sample were directly accountable to an urban population totaling over 113 million citizens. Our lists contained information on the councilor’s name, mailing address, and mobile telephone number. 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.

The second step was to produce a bank of letters to mail to councilors. These letters were written by fictitious citizens and asked for help with various problems that they were facing. Our goal was to generate realistic letters that would effectively signal the migrant/native status of the citizen-requester, in addition to other attributes. To achieve this realism, we asked several former councilors from one large corporation to provide us with examples of real letters that they had received while in office. The letters they gave us turned out to be highly varied in content and style—they were handwritten and typed, long and short, and asked for help with a wide range of issues. We used these to make a letter template for our experiment. Our letters were written in Bengali, Gujarati, Hindi, Kannada, Malayalam, Marathi, Oriya, Tamil, and Telugu, depending on the lingua franca of each urban region.⁶ A randomly chosen 1,000 letters were handwritten; the remainder were typed.⁷ The physical appearance of the letter—a small, simple chit in a plain envelope—was intended to look home-made. Each envelope was inscribed with handwritten names and addresses. They were then stamped and postmarked from a local post office in each respective city.

⁶We carefully translated and reverse-translated our letters in each of these languages.

⁷Due to budget and logistical constraints, we could not write out by hand all of our letters. Based on our discussions with councilors, however, both types of letter presentations were plausible and commonly encountered.

Independent Variables

We vary a battery of other letter attributes besides the migrant/native characteristic. We relate these additional variations to the theoretical discussion, and draw on them to illuminate the mechanisms behind predicted anti-migrant bias. They also help to overcome inferential challenges stemming from correlated threats. In simple terms, if councilors without additional information tend to associate migrants with some other class of citizens (perhaps low-skilled), then attributing differential callback rates between migrants and natives to migrant status per se would be problematic (and would mask, in this case, a skills-related bias). Including additional clarifying information about the requester counteracts this danger. Last, because these auxiliary attributes are assigned independently of one another, they are amenable to separate analysis and thus provide insight into other sources of unequal representation. It is important to emphasize that the inclusion of further manipulations does not compromise the interpretation of our primary outcome of interest: native/migrant status.

The varied attributes were assigned with equal probability as follows:

- **Migrant/Native.** To convey *native* status, we specified that the citizen and his/her family were “native to this city” and had “lived here all our lives.” By contrast, to convey *migrant* status, we mentioned that the citizen and his/her family were native to another state and had “recently moved to this city.” We selected four migrant states of origin, which are representative of the major regions of India: Bihar (north), Andhra Pradesh (south), Assam (north east), and Maharashtra (north west). Naming specific states was important for adding concreteness to the request. It also allows for tests of regional discrimination.
- **Religion and Gender.** The names selected are distinctively Hindu or Muslim, and either male or female.
- **Skills.** Citizens were assigned two skills levels, high and low, and assigned occupa-

tions to enhance the realism of each category. We selected jobs that might plausibly be carried out by both men and women, since gender was an attribute we wished to manipulate.

- **Problem Type.** The fictitious citizens petitioned their representatives for help with a particular “problem.” An implication of the electoral incentives argument is that politicians should be more eager to assist in providing a neighborhood good as against an individual good. Based on interviews, we made a list of issues and classified them as problems afflicting individuals or communities. From these, we selected six.

The full list of attributes is given in Table 1. Randomization was performed by a random-number generator integrated within a computer platform.

Dependent Variable

At the end of the letter, the fictitious citizen provided a phone number and asked for a callback. The telephone number was attached to a real SIM card that had a local area code. This was important to signal local residence, and to keep the monetary costs of replying as low as possible for councilors. Local calls are very cheap in India—a one minute call or one text message costs approximately Rs.1 (\$0.015), although inter-state calls can be expensive. Enumerators at a central call center fielded the calls. They recorded the date and time of the call, and the councilor’s (or his/her assistant’s) name. Enumerators informed the councilor that the letter was fictitious and was part of an academic research study, and thanked them for their time. In a few instances of missed calls, enumerators phoned back and elicited the key information.

Our main dependent variable, therefore, is an indicator variable for whether or not a callback was received.⁸ We are the first to admit that this outcome measure is coarse,

⁸We also employ the number of days elapsed between the sending of the letter and

although taking the trouble to make contact with citizens shows that the councilor is motivated to help. This does not capture the quality or depth of assistance that the politician would be willing to provide a citizen, but obtaining these richer measures of responsiveness would have involved engaging in undue levels of deception and imposing a substantial burden on the councilors' time.

Example Letters

Example of letter from 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 (971729XXXX) and let me what know I should do next? Thank you."

Example of letter from native: Hello, My name is Zafar and I live in your ward. My family and I are native to this city and we have lived here all our lives. I work as a vegetable seller. I am writing because I would like help getting a government dispensary set up in our neighborhood. 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 (981043XXXX) and let me what know I should do next? Thank you.

Additional Data

We collected additional data at the councilor and city level. We used councilors' names to classify them according to gender, and whether or not they had a Muslim-sounding name.

Plausibility and Additional Methodological Features

Several additional features of the letters and experimental design warrant clarification.

the receipt of a reply as an outcome variable.

Language India is a multi-lingual country in which 30 languages are spoken by at least 5 million people each. Migrants’ native language, therefore, might differ from the dominant language spoken in their destination cities, and, by extension, from the language spoken by the elected officials whom we were auditing. Evidently, letters addressed to politicians must employ the politician’s language, otherwise there would be little chance of obtaining a reply.

One might worry that letters written by migrants in the councilors’ local language (i.e. not the migrants’ primary language) might be seen as less plausible than letters from natives. Similarly, one might question whether migrants would attempt to communicate with councilors in this way. However, several features of our local context make this highly unlikely to be the case. First, in India, it is common practice for illiterates, non-native speakers, and those who are unfamiliar in formal written language to ask scribes, friends, notaries, or local computer shop owners to pen petitions on their behalf.⁹ Therefore, migrants can easily procure letters in the local language of their destination cities. Second, migrants tend not to move to arbitrarily selected cities; rather, they migrate to places where they have linguistic abilities, and/or places where they have established social networks containing people who can assist in public communications. Thus, we view letters from migrants and natives letters as equally plausible. As a robustness test, we replicate our analysis after restricting our sample to migrants from areas with closely cognate languages that employ the same script as the language used in the migrants’ destination cities, and find similar results.

We note, additionally, that letters written by migrants in their primary language (i.e., languages foreign to councilors) would likely be subject to additional discrimination by councilors. By signaling a willingness to engage with the councilor in his/her own

⁹Scribes—or professional letter writers—are a common feature of urban life in Indian cities. They are found easily, from market to mosque, and charge reasonable fees for their services. See, for example, *The British Broadcasting Corporation*, March 20, 2014.

language, we likely bias downward estimates of potential discrimination.

Mode of Contact To enhance realism, we employed simple wording and sentence structures, avoiding complicated (and, in particular, heavily Sanskritized) language. Citizens can, of course, interact with politicians using several channels, including in-person meetings. The content of our letter acknowledges this fact by explicitly presenting the choice to write a letter as a last resort. It is made to seem as if the requester has attempted a number of different avenues for getting in touch with the councilor and has finally opted to write and mail a letter.

A limitation of our study is that is not informative about the discrimination citizens might encounter in face-to-face interactions with politicians. Still, we see our choice of letters—and later on text messages—as an advantage of our study. Visiting political offices is usually a protracted and frustrating experience. From a citizen’s perspective, having information about the efficacy of more impersonal and “modern” contacting technologies is valuable. It is also instructive about the potential for the emergence of more routinized citizen-politician interactions in poorer countries—a topic of hot debate (e.g. Grossman, Humphreys and Sacramone-Lutz, 2014).

Ethical Considerations

Careful thought was given to the ethics of the experiment (Teele, 2014). As with all audit experiments, deception was unavoidable. However, we judged that the very small costs to public officials in terms of time and effort—at the most, reading a 5-line letter, and making a ca. 20-second phone call—would be far outweighed by the insights to be gleaned about this little-understood but vital tier of the Indian democratic system. The topic is one of considerable public interest, and the lessons learned could conceivably help ameliorate the wellbeing of migrants in India and elsewhere, who are among the world’s most disadvantaged communities. We also carefully evaluated the concern that our study

might unduly draw on a subject pool that is of interest to other scholarly researchers. Given that ours was the first large-scale audit experiment of its kind in India, and given that politicians in India receive high-volume correspondence from citizens on a day-to-day basis, we judged the risk of our study affecting other scholarly research to be minimal. The protocol was approved by our institution’s IRB (reference number 1403013586).

Experiment 1: Results

The statistical results are based on simple differences-in-means tests. Note that point estimates reflect intent-to-treat (ITT) rather than treatment-on-the-treated (TOT) effects. This is because some proportion of the letters may not have been delivered to the intended councilors due to factors outside our reach, such as postal-service errors, or misplacement by councilors themselves.¹⁰ It is also plausible that some councilors did not associate our experimental manipulations with the underlying characteristics these manipulations were intended to conjure. For instance, a councilor reading her correspondence in a hurry may have failed to properly notice the alias of the sender. In so doing, she would have failed to recognize the requester’s gender and religion. Both considerations imply that the results place a lower bound on the true parameters of interest.

Of the 3,013 letters mailed to councilors, 418 (14%) received a callback, and these 418 responses took an average of 7.3 days to arrive after mailing. The low responsiveness we observe resonates with other scholarly work that points to the generally low capacity of India’s political system. It also suggests the difficulty of establishing routinized communications between citizens and elected officials in developing-country settings.¹¹

¹⁰We sent letters by regular mail, judging that registered mail would not have been realistic for several of the categories of fictitious constituents in our experimental design.

¹¹Our response rates are below the average response rates obtained for parallel audit studies conducted in the United States and Europe (e.g. Butler and Broockman, 2011;

We shall see later that this response rate remains stable when we use an alternate contacting method. It is also important to emphasize that the average response rate masks significant heterogeneity by treatment conditions—the matter to which we now turn.

We are primarily interested in differences in average response rates for migrant-versus-native citizens. The main results are graphed in Figure 1. Recall that if discrimination were absent, there would be no tangible differences in average response rates across treatment conditions. We reject the null hypothesis of no significant effect. Letters from purported migrants were 23% (3.0 percentage points) less likely to receive a callback than letters from purported natives ($p < 0.01$), yielding evidence of substantial anti-migrant discrimination.

Figure 2 graphs the Kaplan-Meier hazard function by native/migrant treatment condition for the time elapsed between the mailing of the letters and the receipt of callbacks. The chart can be interpreted as the proportion of letters going unanswered (“surviving”) by treatment group, for each day following the mailing of the letters.¹² It provides additional confirmation of the unequal handling of requests.

In Figure 3, we report the main effects of additional requester characteristics on callback rates. Gauging these overall impacts is important in its own right; it also provides a useful benchmark for assessing the magnitude of the anti-migrant bias revealed in Figure 1. Citizens declaring a high-skilled occupation are 22.4% (2.8pp) more likely than those declaring low-skilled occupations to receive callbacks ($p = 0.013$). Meanwhile, citizens bearing Hindu aliases are 20.1% (2.8pp) more likely to receive a callback than Muslim-named citizens ($p = 0.014$). But the data suggest that the requester’s gender is White, Nathan and Faller, 2015). It is worth stressing, however, that our mode of communication was postal letters, rather than emails (which have always been employed in prior studies). Also, the resources available to politicians to help citizens varies considerably between the Indian context and western contexts.

¹²The study ends at day 33, when the phone lines were closed.

inconsequential for responsiveness: councilors were equally likely to reply to requesters with female versus male names. We find evidence that politicians are more reactive to problems that affect neighborhoods rather than individuals: neighborhood problems were 16% more likely to elicit a response than individual problems. Professed support for the councilor’s political party had no net measurable effect on callbacks. The key takeaway from this inventory of results is that the migrant “penalty” exceeds in magnitude the effects on skill, religion, and problem-type, making this an important new addition to our understanding of political discrimination in countries experiencing rapid urbanization.

How does migrant status itself affect returns to these other attributes—that is, how does it condition their effects? Figure 4 sheds light on this issue by plotting the treatment effects of the auxiliary characteristics for migrants and natives separately. Broadly speaking, an asymmetric pattern emerges. Discrimination by skill level and religious background is targeted wholly toward *native* requesters: high-skilled natives are 5.5pp more likely to get a callback than low-skilled natives ($p < 0.01$), and Hindu natives are 4.3pp more likely to get a callback than Muslim natives ($p = 0.010$). In short, politicians’ behavior regarding religion and skills-based discrimination adheres to theoretical expectations when dealing with natives requesters: they favor co-ethnics and they privilege richer, high-skilled constituents, who contribute most to municipal budgets.

Strikingly, however, politicians overlook these characteristics when determining whether or not to follow up on migrants’ requests. They tend to treat migrants as an undifferentiated mass, and do not discriminate amongst migrants based on ethnicity and skills. The same is not true for the effects of the two remaining treatments: party membership, and type of problem. Migrants gain from requesting assistance with a neighborhood (as opposed to an individual) problem (3.4pp, $p = 0.025$). There is some evidence that migrants benefit from mentioning that they belong to the councilor’s political party (2.3pp, $p = 0.094$); we discuss the interpretation of this result below, and we design a second

experiment to explore in greater detail potential electoral factors that might be play.¹³

One illustrative exercise to help gauge the magnitude of the discrimination we measure is to compare callback rates for the “best” migrants with those of the “best” native. For example, a Hindu native who is high-skilled, a party supporter, and who asks for help with a neighborhood problem is called back 25% of the time. An otherwise identical migrant is called back just 13% of the time. This is a striking difference.

Our results from the first experiment indicate that politicians abide by expectations insofar as they discriminate strongly against migrants. Migrants’ cultural and economic attributes fail to offset this discrimination, even while politicians continue to show special patronage toward co-ethnic and high-skilled natives. Yet, as we noted earlier, politicians’ relative uncertainty about migrants’ electoral participation and political preferences could induce non-responsiveness by undercutting political incentives to court migrant votes. Mitigating uncertainty about these factors, therefore, should be expected to alter political responses to migration. In the following section, we take up this issue in greater detail.

Experiment 2: Text Messages

The mailing experiment generated evidence of extensive discrimination against internal migrants. But the mechanisms driving this effect remain ambiguous. The experiment embedded one manipulation that directly addressed political mechanisms—namely, support for the councilor’s party. However, if politicians believe that migrants are unlikely to

¹³Additionally, we look for the presence of differential callback rates by region of migrant origin. In Figure 5 we plot average callbacks for natives and migrants from each of the four chosen states of origin. While all four migrant groups fall short of the callbacks received by natives, it is purported migrants from Bihar and Assam who arouse the greatest animosity. We view this as an informal validation check, since, anecdotally, migrants from Bihar and north-east India have tended to trigger the strongest antipathy.

be registered to vote in destination cities—whether due to voter re-registration costs or difficulties in de-registering in home villages—then having migrants profess support for the councilor’s party might not be enough to convince a politician that helping migrants will be electorally remunerative.

We designed a second experiment to evaluate the hypothesis that councilors’ expectations about voter registration status underlie anti-migrant discrimination. Our second approach parallels that of the letters experiment in its basic aspects, but now involves sending short text messages to councilors’ mobile phones. Sending SMSes instead of letters for the second experiment had several advantages from an implementation standpoint. Because we were relying on the same set of councilors, sending additional letters may have provoked suspicion. (It is important to stress that the SMSes were sent at least four months after the letters and included very different wording. The chances of detection were minimal, therefore, and we received no complaints.) Substantively, SMSes also enable us to get a sense of the relative efficacy of different modes of contacting politicians, thereby giving a point of comparison for the callback rates observed in the letters experiment, and enhancing the generalizability of the research.

In this round, our aim was to generate a set of SMSes from fictitious citizens who share similar traits to the requesters depicted in the letters. But this time citizens also had differing political attributes. Specifically, they varied along the dimensions of self-declared registration status (yes/no) and an expression of past political support for the individual councilor.

For logistical reasons, we were forced to limit the number of attributes randomized in the SMS phrase of the project. We employ two male names (Hindu/Muslim), two largely male occupations (construction worker/engineer), two states of migrant origin (Bihar/Assam) and two problems (aadhaar card/street lamp fixed). The shift toward looking at only male citizen requests was based on the null effects of gender in the letters experiment.

The 5 main treatment groups are as follows:

1. *Native registered to vote* [$Pr(\text{Assignment}) = 1/8$]: 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. *Native not registered to vote* [$Pr(\text{Assignment}) = 1/8$]: 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]?
3. *Migrant not registered to vote* [$Pr(\text{Assignment}) = 1/4$]: 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 to vote* [$Pr(\text{Assignment}) = 1/4$]: 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 to vote & supports party* [$Pr(\text{Assignment}) = 1/4$]: 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]?

The analytic strategy is to compare average callbacks between natives and migrants, as well as between different types of migrants. The results of the second experiment are presented in Figures 6 and 7. The larger white boxes present mean response rates, with italicized numbers corresponding to the treatment conditions just enumerated. The shaded boxes present formal pairwise comparisons of these averages using one-sided t-tests. The average callback rate for natives was 13.5%. Reassuringly, this is almost identical to the average callback rates obtained in the letters experiment, suggesting that low responsiveness is not confined to a single contacting method.

The data displayed in Figures 6 and 7 tell a straightforward story—one that meshes with the voter registration hypothesis. Whether using both unregistered *and* registered natives as the comparison group (Figure 6) or only registered natives (Figure 7), unregistered migrants suffer discrimination. Indeed, a request from a registered native

is 4.2 percentage points—proportionally, 42%—more likely to evoke a callback than a request coming from a homologous unregistered migrant. This effect is sizable and exceeds in magnitude the anti-migrant discrimination observed in the letters experiment. Crucially, however, after introducing a minor manipulation change for the hypothetical migrant—from “we aren’t” to “we are registered 2 vote here”—the apparent migrant penalty disappears. The likelihood of callback is 27.5% (2.8pp, $p = 0.057$) greater for registered migrants versus unregistered ones. More notably still, the difference between registered natives and unregistered migrants goes away after signaling that migrants are registered (in both Figures 6 and 7). This difference is no longer statistically significant. In short, there is strong evidence that migrants’ registration status constitutes a potent explanation for the unequal treatment meted out to these citizens by municipal councilors.

The theory section posited an additional electoral-incentives based explanation for why politicians might be less responsive to migrant requests. Plausibly, migrants’ political preferences are more obscure to politicians, owing to the fact that politicians are less acquainted with this diverse and unfamiliar group. As a corollary, and conditional on migrants being registered to vote, politicians may be less certain that providing constituency service to migrants will translate into electoral support. One way to evaluate the veracity of this conjecture is to provide a signal of migrants’ political preferences and to test for a potential boost in callbacks. We do this in treatment group 5, which depicts a registered migrant, yet one who also claims to have voted for the councilor in the prior election. Clearly, adding a clarifying statement about the migrant’s political preferences does not confer a statistically significant advantage to migrants who are registered to vote in city elections.

As in the letters experiment, we exogenously varied three other attributes in the SMS study: the requester’s religion, his occupation, and the type of problem. The overall impacts of these treatments are large and significant in the expected direction. A Hindu-

named requester is 23% (2.7pp) more likely to get a callback than a Muslim-named one; the neighborhood problem was 21% (2.4pp) more likely to get a callback than the individual problem; and high-skilled requesters were 48% (5.0pp) more likely to receive callbacks than low-skilled requesters.

Does migrant status affect the returns to these additional attributes? Recall that the letters experiment revealed that signaling “positive” individual characteristics does not offset the disadvantages associated with being a migrant. A crucial difference in the SMS experiment, however, is that migrants come in two definite varieties, registered and unregistered voters. We find that migrants’ registration status does affect returns to individual attributes. The results for *unregistered* migrants in our SMS study are akin to the results for migrants in our letters experiment. But *registered* migrants in our SMS study fare more similarly to the natives in our letters experiment. Having a Hindu as opposed to a Muslim name is beneficial for registered migrants but inconsequential for unregistered migrants. Being such, the treatment accorded to registered migrants is now equivalent to that accorded to natives.

The phenomenon of co-ethnic voting provides a potential explanation for this pattern of results. If voters prefer to cast their vote as an ethnic bloc for “in-group” candidates, then politicians might preferentially help co-ethnics, since they are the group(s) most easily mobilized to the politician’s side (Dunning and Harrison, 2010). Yet once again, in order for this logic to operate, both politicians and their ethnic coalitions of native citizens must believe that migrants are registered to vote and hold the potential of influencing ethnic voting trends in city elections. When migrants are unregistered to vote, they do not alter ethnic electoral coalitions; consequently, politicians see little reason to discriminate on the basis of ethnicity among these unregistered citizens. When migrants are registered to vote however, they can potentially reshape ethnic electoral coalitions in the city. In these situations, politicians wishing to augment their coalition alliances are likely to assist the co-ethnic migrants whose votes they seek.

We note next that registered migrants' skill levels in the SMS experiment play an important role in determining who does and does not get a callback, with high-skilled, high-income migrants receiving a pronounced bounce in support from politicians in our sample. Theories of fiscal burden discussed earlier indicate that natives prefer migrants who are net contributors to the welfare state to migrants who are net beneficiaries of the state. Elected representatives who advance the interests of their societal coalitions are expected to hew to these nativist concerns. Given that councilors' budgets and spheres of influence directly depend on the amount of resources that they are able to collect from local tax-paying citizens, they might be especially attuned to these considerations.¹⁴ Because these budgets are financed by local property and sales taxes, high-skilled migrants who relocate to cities on a more permanent basis are expected to contribute more to such taxes. Meanwhile, poor migrants might be viewed as net drains on the welfare state, since in India, permanent migrants are accorded access to social protection in the form of food and other benefits via the Public Distribution System (Bhatia and Chatterjee, 2010). Together, these considerations reasonably help explain why politicians are particularly responsive to high-skilled migrants.

The results from our letters and SMS experiments are consistent with Dancygier (2010), who argues that natives compete with migrants in the political arena only when migrants are politically enfranchised and pivotal to electoral outcomes. Our letters experiment shows that when migrants are not expected to vote in city elections, politicians ignore migrant attributes such as ethnicity and skills that typically provoke hostility on the part of natives. In these cases, politicians treat migrants as an undifferentiated mass; they discriminate against migrants in a blanket fashion because they do not anticipate any electoral support from migrant voters. When migrants are registered to vote, however, politicians do not discriminate against migrants because migrants hold the possibility of

¹⁴The size of these budgets can be large; for example, the Mumbai Municipal Corporation by itself has an annual budget of Rs.310 billion and employs 108,000 citizens.

electoral support. But because migrants now compete (or form alliances) with natives in the political arena, politicians reflect the preferences of their electoral coalitions and begin to discriminate against migrants based on individual attributes. Thus, migrants' registration status appears to play a central role in explaining when politicians decide to court migrant votes and when they find it profitable to embrace coalition strategies in their constituent-level interactions with migrants.

Experiment 3: Councilors Survey Experiment

Our evidence thus far strongly suggests that councilors are less responsive to migrant requesters owing to prior beliefs about migrants' registration status. Because councilors generally think that migrants are unregistered to vote in city-wide elections, they expect minimal electoral returns to assisting this class of citizens. Our interpretation is based on comparative evidence from the letters and SMS experiments. The results from our letters experiment (in which we did not provide information on the migrants' registration status) closely mirror the results from the SMS treatment which specified that migrants are not registered to vote, but differ from the SMS treatment in which migrants signal their eligibility to vote in city elections. Although politicians' perceptions about migrants' registration status appears to be a reasonable explanation for this representation gap, we have not yet supplied a direct test of this mechanism.

To provide such a test, we conducted a telephone-based survey experiment on a subsample of councilors. We attempted to interview 1,500 councilors by telephone. In total, 412 councilors answered our calls and completed the brief survey, making for a response rate of 27 percent. At the start of each survey, councilors were read the following vignette in their native language by our enumerators:

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.]

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? [*Choose from: Yes, No, Don't know.*]

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? [*Choose from: Very likely, Somewhat likely, Somewhat unlikely, Very unlikely.*]

Within the vignette, respondents were randomly assigned to one of the two treatments (native/migrant), indicated in bold.

The frequency distributions of responses to both questions, broken down by treatment condition, are shown in Figures 8 and 9. The results are clear. 96% of councilors presented with a native citizen believe the citizen to be registered; the equivalent figure for councilors presented with a migrant citizen is 50 percent—a difference of 46 percentage points ($p < .000$). The same finding emerges from an analysis of answers to the second question (“How LIKELY do you think it is...”). We regress the four-category outcome on the dichotomous treatment variable. The ordered log-odds regression coefficient is 2.674 ($p < .000$), firmly suggesting that councilors believe migrants are less likely to be registered to vote than natives.

The findings from our third experiment helps paint a fuller picture of the electoral foundations of anti-migrant prejudice in rapidly urbanizing democracies such as India. Politicians typically do not expect migrants to be registered to vote in city elections. Consequently, they treat migrants as an undifferentiated underclass and discriminate against migrants in favor of native city residents while providing constituency services. These forms of political exclusion might even create self-fulfilling cycles of underrepresentation to the extent that migrants rely on political elites for voter registration and political integration in destination cities. But when migrants are able to overcome representational barriers and register to vote in city elections, politicians stop discriminating

against migrants in the hope of winning their electoral support.

Conclusion

We offer the first set of large-scale experiments to probe the determinants of anti-migrant discrimination by politicians in a major developing country. There is a near consensus about the importance of rural-to-urban migration for economic growth and human development, yet in most emerging economies migrants lag on key welfare indicators and struggle to integrate into destination cities. This paper demonstrates that one source of these hindrances lies in unequal political representation. Our first experiment reveals that migrants are 23.4% less likely to receive help from their elected representatives compared to otherwise similar long-term city residents. The magnitude of this effect is larger than that of other key attributes highlighted in the literature, such as ethnic-group identity or economic status. A key finding to emerge from our second experiment, however, is that when migrants convey that they are registered to vote in politicians' electoral wards, anti-migrant discrimination evaporates. These results—which are further supported by a survey experiment—point to a simple electoral logic explaining the representational shortfall experienced by migrants, who count among the world's most marginalized population groups.

This study illuminates a new and substantively significant source of political inequality in developing democracies undergoing rapid urbanization. The Universal Declaration of Human Rights and the constitutions of almost all democratic nations guarantee citizens the right to free movement and equal representation within national boundaries. Our study documents how and why government behavior might fail to live up to these high ideals. Lower registration (and thus turnout) rates among migrants disincentivize political elites from serving migrant interests. As a consequence, migrants face *de facto* disenfranchisement. The real-world implications of such neglect should not be minimized.

The municipal councilors we audit shoulder responsibility for local public health, sanitation, clean water supply, education, roads, lighting, and a vast portfolio of related government services. Yet, against this pessimistic picture, the paper's results are salutary insofar as they suggest a pathway by which states can enhance migrants' well-being. By putting in place initiatives that encourage migrants to register to vote in their destination cities, municipal governments (and perhaps non-governmental organizations too) can augment the political representation of migrant communities, and thereby lock in incentives for politician responsiveness going forward. Other disadvantaged groups also perceived to be inactive in the electoral arena may profit from similar interventions.

A contribution of our study is to highlight an under-appreciated theoretical dilemma confronting politicians. In explicating the sources of discrimination against migrants, scholarship has largely focused attention on popular bases of support for nativist parties (Weiner, 1978; Katzenstein, 1979; Pettigrew, 1998). According to this perspective, political entrepreneurs espouse anti-migrant platforms to rally nativist sentiment. Our work points to a different electoral mechanism. Politicians anticipate few electoral dividends from catering to migrant voters because migrants remain politically disenfranchised. That is, political nativism might stem not only from anti-migrant preferences among native city residents but also from lower rates of electoral mobilization on the part of migrants.

Our findings pertain to the domain of internal migration, yet the electoral tradeoffs that we identify are instructive for the study of cross-border immigration politics. In several countries, such as the United Kingdom and Sweden, immigrants hold the right to vote in local elections (Dancygier, 2010). Other countries such as the United States feature high immigrant naturalization rates. In these cross-border contexts, immigrants might serve as appealing vote banks for politicians, hinting at our thesis' wider applicability.¹⁵

¹⁵Recent voter registration drives among Hispanic immigrants in the United States are indicative in this regard.

We suggest two fruitful areas for research going forward. The first is to evaluate the relative cost-effectiveness of different voter registration campaigns in poorer settings. This could be modeled after the Get-Out-The-Vote (GOTV) literature in the United States and Europe, and its registration-specific extensions. Second, and more ambitiously, researchers should rigorously assess the welfare impacts of voter registration efforts on migrants themselves. If our interpretation of the paper’s results is correct, registration may be expected to produce substantial welfare gains. At the same time, it would be important to elucidate the broader societal impact of these changes. For instance, sudden large-scale migrant voter registration could deepen native-migrant tensions sufficient to offset any other welfare improvements (Dancygier, 2010).

Beyond migration, our findings provide new insights on the interface between urban political elites and citizens. A rising body of literature shows that new technologies such as cell phones have mixed consequences for how citizens engage politically.¹⁶ Do technological advancements make politicians more responsive to citizens? Our study employed two contacting methods—postal letters, and SMSes—and found near-identical response rates for both. Because our mode of contact was not randomly assigned, we cannot draw causal inferences about which contacting method is more effective. Going forward, researchers could fruitfully investigate whether and how modern technologies alter citizen-state interactions.

We conclude by noting that our study illuminates several other dimensions of inequality (apart from migrant-native status) in India’s political system. Most seriously, higher-skilled citizens and Hindus enjoy much better access to constituency services than the lower-skilled citizens and Muslims. The results on religion accord with a substantial

¹⁶Some studies show that cell phones are not very likely to boost political participation among citizen groups (Grossman, Humphreys and Sacramone-Lutz, 2014). Others show, by contrast, that cell phones can have a big impact on collective action (Pierskalla and Hollenbach 2013, see also Shapiro and Weidmann 2015).

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 income-based discrimination are novel. Fiscal burden theories lead us to expect that politicians prefer net contributors to the state over those that pose a drain on government resources. Our evidence indicates that politicians do indeed discriminate in favor of high-income citizens. It could be that politicians are more responsive to citizens who provide most tax revenues. Other factors might also be at play—for instance, urban political elites might anticipate campaign contributions or quid pro quo favors from richer constituents in exchange for help. Future work should probe these possibilities.

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Tables and Figures

Table 1: Treatments in Letter Experiment

Name	Native/Migrant	Occupation	Problem Type	Party member
Ram	Native	Cleaner	Aadhar card	Always
Arjun	Migrant (Bihar)	Vegetable Seller	Income Certificate	Never
Seeta	Migrant (Assam)	Cook	Job	
Sushma	Migrant (Maharashtra)	Doctor	Drainage	
Zafar	Migrant (Andhra Pradesh)	Lawyer	Government dispensary	
Salman		Engineer	Street lamp	
Waheeda				
Zahra				

Table 2: Treatments in SMS Experiment

Name	Occupation	Problem Type	Native/Migrant Registered/Registered/Voted
Arjun	Engineer	Aadhar card	Native (not registered to vote)
Salman	Construction worker	Street lamp	Native (registered to vote)
			Migrant-Bihar (not registered to vote)
			Migrant-Bihar (registered to vote)
			Migrant-Bihar (registered to vote, voted before)
			Migrant-Assam (not registered to vote)
			Migrant-Assam (registered to vote)
			Migrant-Assam (registered to vote, voted before)

Table 3: Letters experiment: Do attributes of councilors, letters, and cities predict migrant/native treatment? OLS estimates

Dependent variable:	Migrant Treatment
Female corporator	-0.003 [0.020]
Muslim corporator	0.001 [0.037]
City-wide population	0.000 [0.000]
Language: Hindi	0.004 [0.030]
Language: Kannada	-0.029 [0.041]
Language: Telugu	-0.040 [0.064]
Language: Bengali	0.019 [0.050]
Language: Gujarati	0.019 [0.053]
Language: Malayalam	0.032 [0.056]
Language: Marathi	-0.070* [0.036]
Language: Oriya	0.002 [0.076]
Constant	0.500*** [0.027]
<hr/>	
<i>R</i> -squared	0.00
Observations	2,584
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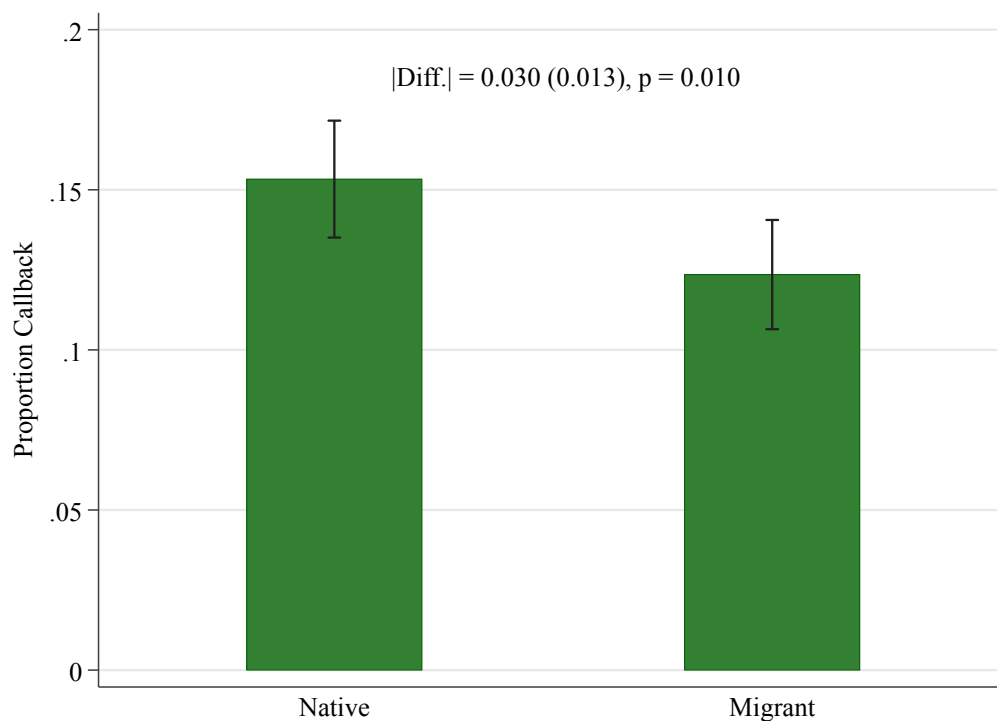
Notes: Robust standard errors in brackets.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: SMS experiment: Do attributes of councilors, letters, and cities predict assignment to SMS treatment conditions? Multinomial logit estimates

	Treatment:						
	Native	Migrant					
	Reg.	Unreg.		Reg.		Reg.+Supporter	
<i>Migrant state:</i>		Assam	Bihar	Assam	Bihar	Assam	Bihar
Female corporator	-0.067 [0.176]	0.174 [0.179]	0.107 [0.173]	0.038 [0.178]	0.258 [0.174]	0.185 [0.173]	0.003 [0.177]
Muslim corporator	-0.270 [0.343]	-0.145 [0.339]	0.216 [0.307]	0.181 [0.320]	0.055 [0.324]	0.067 [0.316]	-0.133 [0.337]
City-wide population	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	-0.000 [0.000]	-0.000 [0.000]	0.000 [0.000]
Language: Hindi	-0.101 [0.255]	0.433 [0.277]	-0.140 [0.259]	-0.241 [0.258]	-0.022 [0.258]	-0.157 [0.254]	-0.126 [0.257]
Language: Kannada	-0.145 [0.330]	0.261 [0.352]	-0.071 [0.332]	-0.306 [0.337]	-0.230 [0.344]	0.054 [0.320]	-0.056 [0.329]
Language: Telugu	-0.210 [0.538]	0.500 [0.528]	0.524 [0.476]	-0.298 [0.548]	0.195 [0.511]	0.131 [0.504]	-0.198 [0.543]
Language: Gujarati	-0.478 [0.452]	0.282 [0.444]	-0.037 [0.419]	-0.402 [0.443]	0.071 [0.417]	-0.412 [0.443]	-0.091 [0.420]
Language: Malayalam	-0.267 [0.451]	0.437 [0.448]	-0.031 [0.435]	-1.015* [0.537]	0.018 [0.434]	-0.371 [0.452]	-0.591 [0.482]
Language: Marathi	-0.330 [0.290]	-0.120 [0.320]	-0.155 [0.291]	-0.171 [0.286]	-0.001 [0.287]	-0.305 [0.286]	-0.298 [0.291]
Language: Oriya	-0.185 [0.616]	0.291 [0.626]	0.232 [0.569]	-0.934 [0.736]	-0.379 [0.648]	-0.007 [0.578]	-0.071 [0.597]
Constant	0.208 [0.223]	-0.400 [0.251]	0.108 [0.226]	0.214 [0.224]	0.062 [0.229]	0.194 [0.222]	0.225 [0.221]
Log-likelihood	-4,644.03						
Chi-square p-value	0.980						
Observations	2,247						

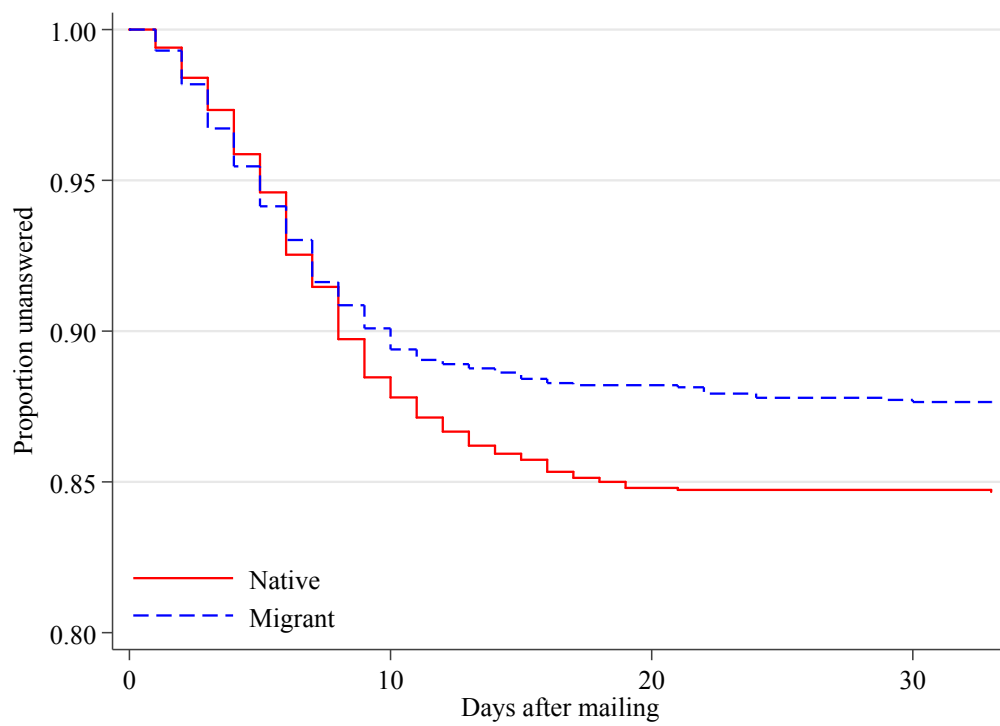
Notes: Robust standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 1: Impact of migrant status on politician callbacks to letter requests



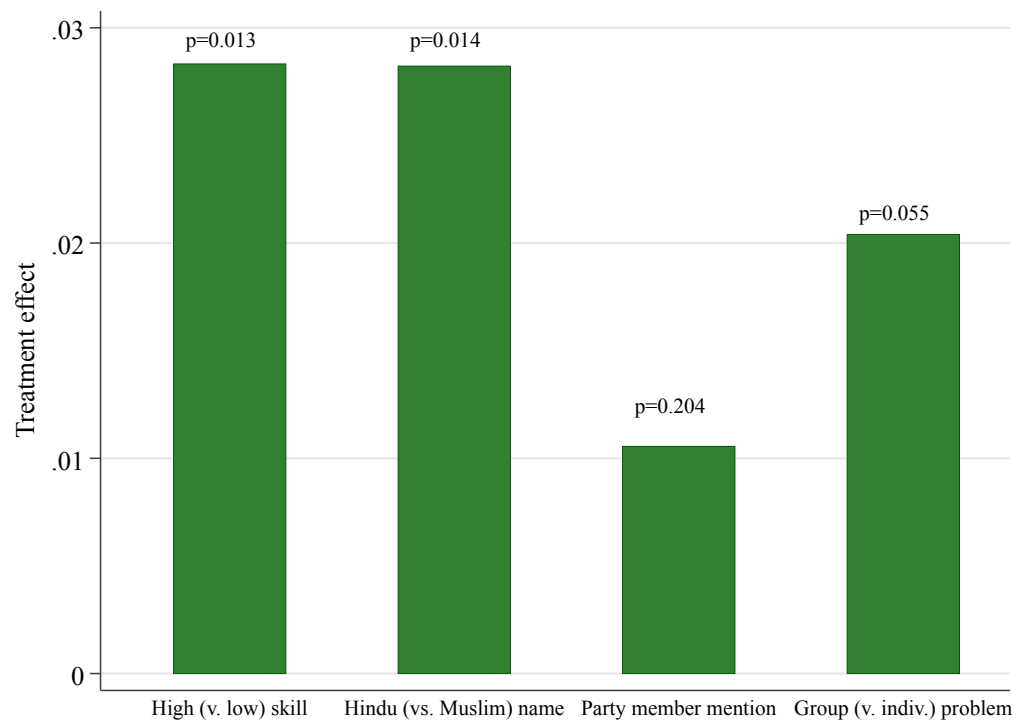
Notes: This graph presents average callback rates for requesters signaled to be natives and migrants. The reported difference in callbacks, associated standard error (in parentheses), and p-value are based on one-tailed t-tests.

Figure 2: Survival analysis



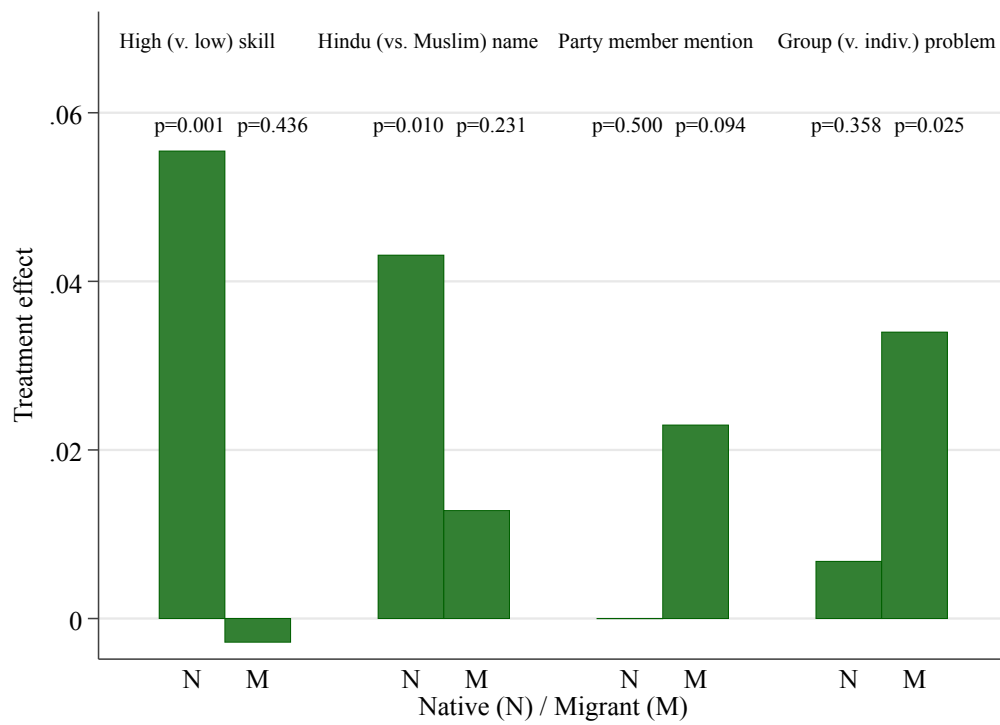
Notes: This graphs presents the average *non*-response to requests by natives and migrants for each day after the letters were mailed.

Figure 3: Impact of additional requester attributes on politician callbacks to letter requests



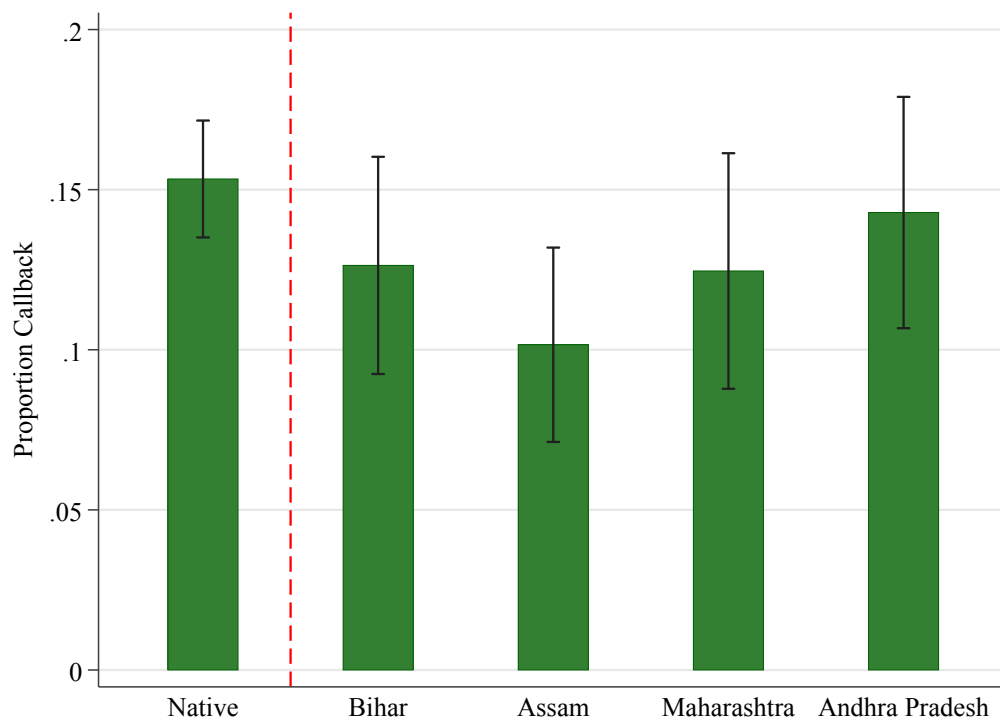
Notes: This graph presents the treatment effects on callback rates of four other attributes randomized in the letters. Results are derived from one-tailed t-tests.

Figure 4: Impact of additional requester attributes on politician callbacks to letter requests, by native/migrant status



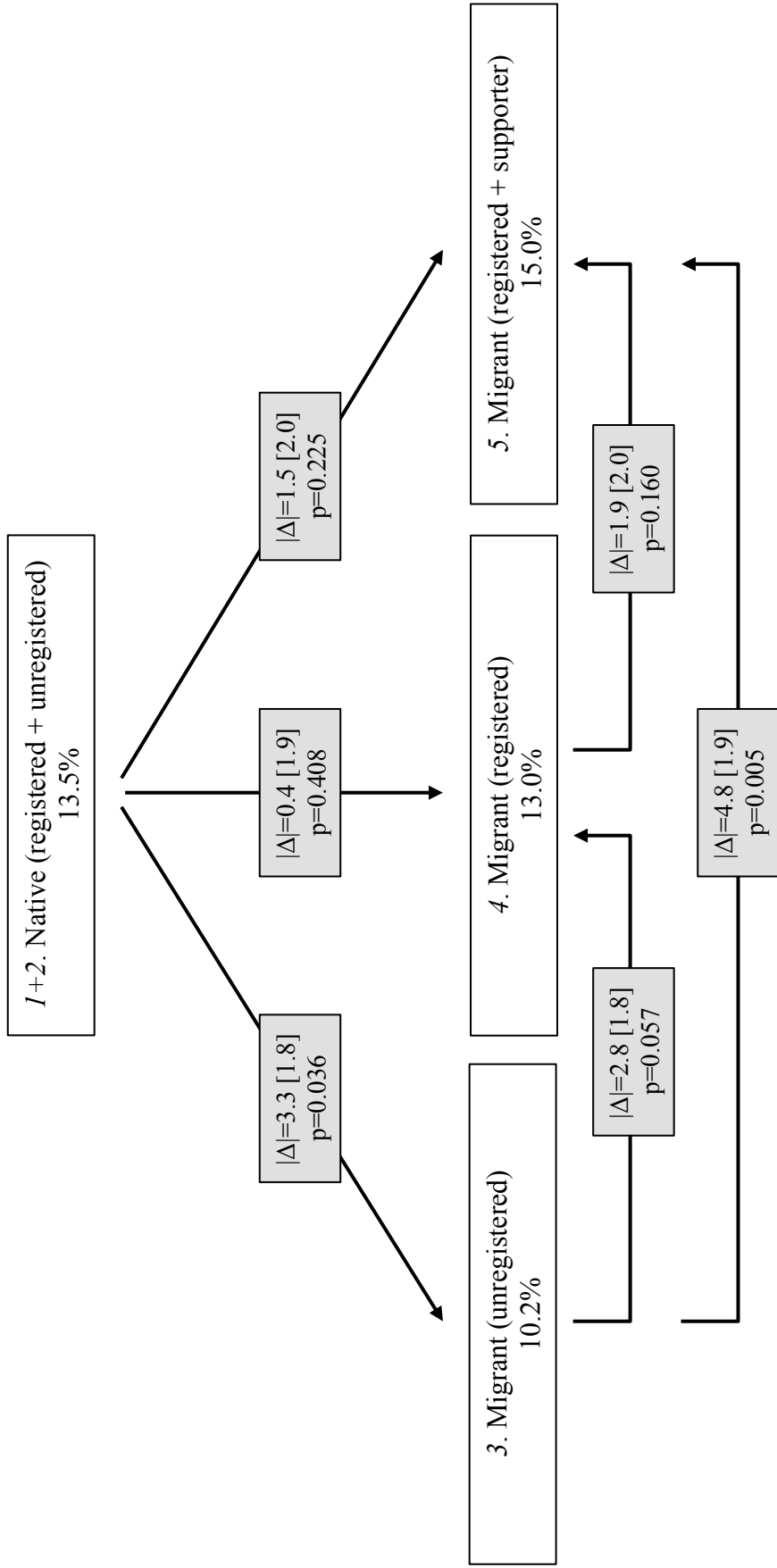
Notes: This graphs presents the treatment effects on callback rates for natives and migrants of four other attributes randomized in the letters. Results are derived from one-tailed t-tests.

Figure 5: Impact of migrant region of origin on politician callbacks to letter requests



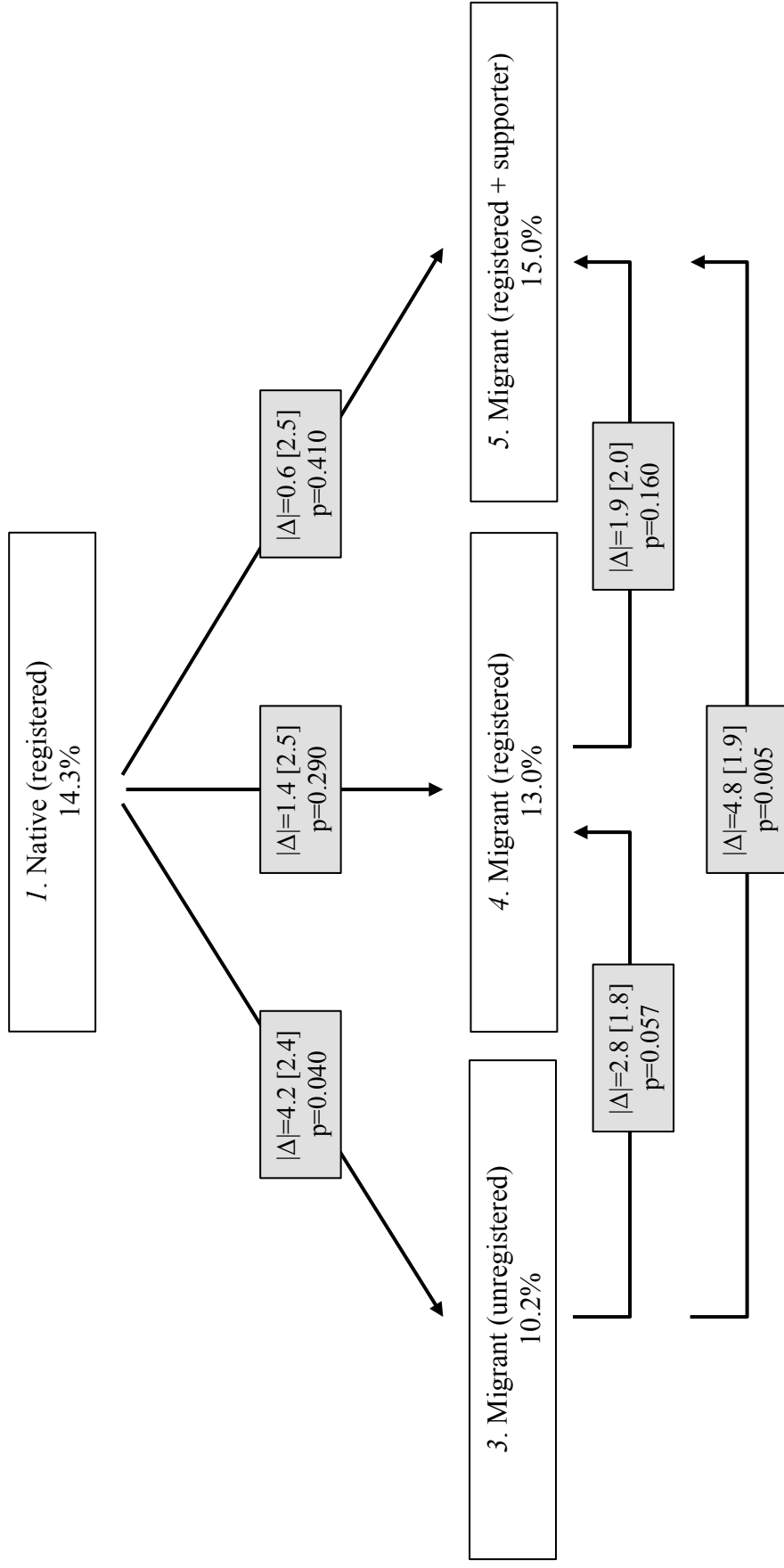
Notes: This graph presents average callback rates for requesters signaled to be natives, and for migrants signaled to come from four different regions.

Figure 6: SMS response rates and treatment effects by requester type



Notes: This chart displays the average callbacks rates for each treatment condition (in large white boxes) and the results of one-sided t-tests between these conditions (in smaller gray boxes). Standard errors are in square parentheses.

Figure 7: SMS response rates and treatment effects by requester type



Notes: This chart displays the average callbacks rates for each treatment condition (in large white boxes) and the results of one-sided t-tests between these conditions (in smaller gray boxes). Standard errors are in square parentheses.

Figure 8: Survey experiment: frequency of responses by treatment condition (binary outcome)

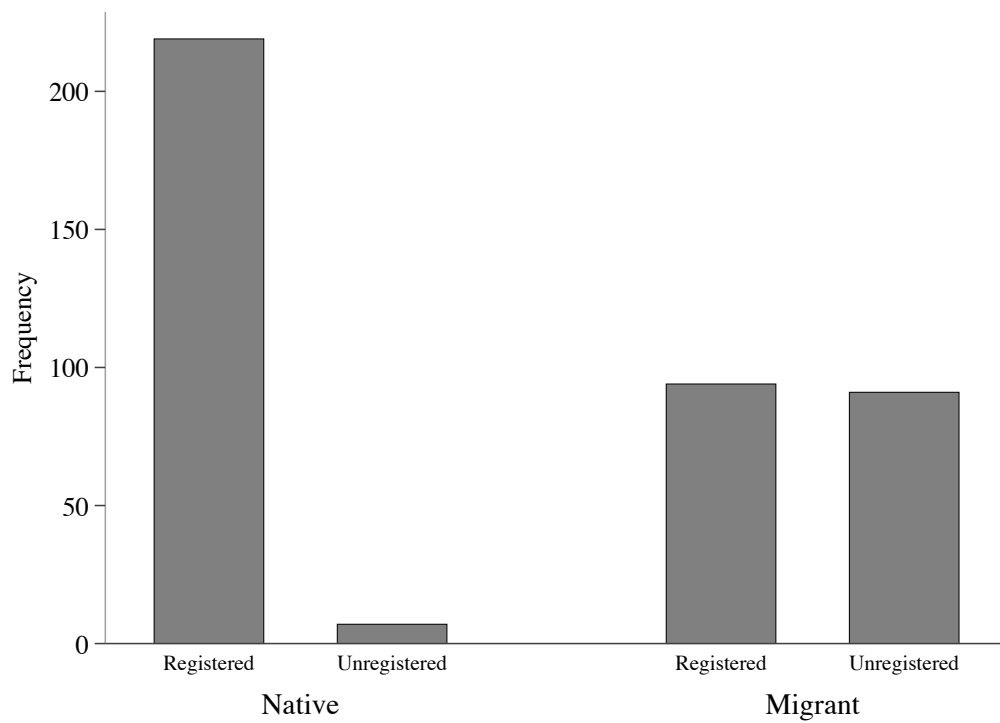


Figure 9: Survey experiment: frequency of responses by treatment condition (ordinal outcome)

