

The Salience of Ethnic Categories:  
Field and Natural Experimental Evidence from Indian Village Councils

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This version: April 26, 2009

Prepared for the Faculty Colloquium in Comparative Politics, Princeton University

Acknowledgements: I am grateful to Drs. Veena Devi and Ramana, their students and collaborators at Bangalore University, and especially to Dr. B.S. Padmavathi of the international Academy for Creative Teaching (iACT) for assistance with fieldwork. Janhavi Nilekani and Rishabh Khosla of Yale College, with whom I am collaborating on related projects, provided superb research assistance. I also received useful advice or help from Jennifer Bussell, Raúl Madrid, Jim Manor, SS Meenakshisundaram, Nandan Nilekani, Rohini Nilekani, Sunita Parikh, Vijayendra Rao, Sandeep Shastri, Drs. Shaymla and Jeffer of the Karnataka RDPJ, and S.K. Singh of the NIRD in Hyderabad. In-kind support from Kentaro Toyama at Microsoft Research India and financial support from Yale's Whitney and Betty MacMillan Center for International and Area Studies and the Institution for Social and Policy Studies are gratefully acknowledged. This research was approved by Yale's Human Subjects Committee under IRB protocol #0812004564.

**Abstract:** Many scholars emphasize that both electoral institutions and the sanctioning of particular ethnic categories by the state may shape the political role of ethnicity, as well as the salience of different forms of ethnic identification. Yet because electoral institutions and state-sanctioned categories may themselves be shaped by patterns of ethnic identification, such causal claims are typically challenging to evaluate empirically. This paper reports results from a field experiment implemented in rural villages in the Indian state of Karnataka, in which the caste relationship between subjects and political candidates in videotaped political speeches was experimentally manipulated. The experimental design allows me to compare the influence on candidate evaluations of sub-caste (*jati*) ties with the effect of larger, state-sanctioned legal categories (such as Scheduled Caste). Embedding this field experiment within the natural experiment provided by the reservation of the presidencies of some village councils (a kind of electoral quota for lower-caste politicians) then allows me to examine how political empowerment based on larger categories shapes the salience and nature of caste relationships that matter. The field experimental results underscore the role of sub-caste ties, yet also show that reservation increases the political salience of larger caste categories. A survey of council presidents and members helps explain why caste may strongly shape affective evaluations of candidates for Indian village councils.

## **I. Introduction**

A large literature in comparative politics emphasizes that electoral institutions, patterns of political competition, and the sanctioning of particular ethnic categories by the state may all shape the political role of ethnicity, as well as the salience of different forms of ethnic identification. An abundance of observational evidence seems to support this basic proposition. For instance, the recent election of Bolivia's first indigenous president, Evo Morales, coincides with a large increase in the percentage of Bolivians who identify as indigenous in public opinion surveys (LAPOP 2008: xxx-xxiii; see also Madrid 2008: 485, 490). In the United States, the creation of a census category for Hispanics is seen to have created a unifying identity, around which people of disparate national origins (Dominican, Ecuadorian, Argentine, and so on) can mobilize politically (see e.g. Rodríguez 2000). Students of African politics emphasize the role of both colonialism and post-independence political competition in shaping the salience of tribal and other ethnic categories (Bates 1983, Laitin 1986, Posner 2004, 2005). As constructivist scholars of ethnic politics have emphasized, the very conceptual existence of an ethnic group may reflect as much as it produces categorization by the state.

Yet because electoral institutions and state-sanctioned categories may themselves be shaped by patterns of ethnic identification, such causal claims are typically challenging to evaluate empirically. The examples mentioned in the previous paragraph illustrate this point. The growth of indigenous identity in places such as Bolivia and Ecuador is also the fruit of successful but only relatively recent mobilization along ethnic rather than class lines (Yashar 2005); such mobilization may have produced secular changes in the proportion of Bolivians who identify as indigenous and may have also

facilitated the election of the country's first indigenous president. The creation of census categories such as Hispanic typically reflects active campaigning by pressure groups who seek to shape the way that the state conceptualizes and measures ethnic categories (Nobles 2000). And European colonialists hardly found a tabula rasa from which to construct linguistic or tribal identities in Africa. Thus, the extent to which changes in the salience of various ethnic categories reflect other confounding processes, rather than innovations in electoral rules or the sanctioning of legal categories by the state, is an open question.

I contribute to this literature by presenting empirical research in which the causal effects of electoral rules and political leadership on the salience of different ethnic categories may be identified, using both field and natural experimental evidence from rural village councils in the Indian state of Karnataka. In the field experiment, I recruited residents from 160 villages,<sup>1</sup> who were shown videotaped speeches given by actors posing as candidates for a local village council, that is, a Gram Panchayat.<sup>2</sup> The experimental manipulation consisted of what subjects were told about the candidate's surname. Because the surnames indicate the candidate's caste, changing the candidate's surname manipulates the caste relationship between subjects and candidates.<sup>3</sup> In

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<sup>1</sup> An additional 40 villages were selected for an initial, smaller experiment and survey, after which (inter alia) the experimental treatments could be refined. Village selection and subject recruitment are described below.

<sup>2</sup> It is often convenient to use the word panchayat instead of council; whereas the latter seems to refer only to the governing body, the former indicates both the governing body and its administrative jurisdiction. In other words, village residents live in panchayats but not councils.

<sup>3</sup> Following the literature, and as discussed further below, I refer here to "caste" as an ethnic identity (see Chandra 2006).

particular, the experimental design was structured to allow comparison of three kinds of caste relationships. In one treatment condition, subjects and candidates come from both the same sub-caste (*jati*) and the same larger, state-sanctioned legal category (such as Scheduled Caste).<sup>4</sup> In a second treatment condition, subjects and candidate come from different sub-castes but from the same larger category. And in the third condition, subjects and candidates come from different sub-castes and different larger categories. The experimental design therefore allows estimation of the effect of various kinds of caste relationships on preferences for the candidates.

I also embed this field experiment within the natural experiment provided by the reservation of the presidencies of some village councils (a kind of electoral quota for lower-caste politicians). In contrast to what is asserted in some of the existing literature (e.g., Munshi and Rosenzweig 2008), reservation of presidencies in these village councils is not randomly or “as-if” randomly assigned in many Indian states, as required in a valid natural experiment (Green and Gerber 2008; Dunning 2008). However, because reservation is determined by a strict rule based on population proportions of the relevant lower-caste group, it is possible to construct a regression-discontinuity design, in which only village councils just above or below the threshold for assignment to reservation are included in the study population.

By using a regression-discontinuity approach to select the villages in which to implement the field experiment, I can recover and exploit a natural experiment, in which reservation is for all intents and purposes assigned at random among the villages in the

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<sup>4</sup> For purposes of distinguishing *jati* from larger categories like Scheduled Caste, I refer to “sub-caste” throughout, even though “caste” may be a more faithful translation.

study population.<sup>5</sup> This empirical strategy allows inquiry into how political empowerment based on larger group categories—such as Scheduled Caste or Scheduled Tribe—causally shapes preferences for candidates along both sub-caste and caste-category lines. In each selected village, I also implemented a survey of the council president, two other council members, and the executive secretary (local bureaucrats), which allows me to investigate how reservation shapes the workings of village councils.

The results from the field experiment underscore the role of caste relationships as a whole in shaping preferences for candidates, in both expected and more surprising ways; they also helpfully reveal how different dimensions of caste identity shape candidate evaluations. For example, I find that politicians from subjects' sub-castes are significantly preferred to politicians from other castes. Strikingly, however, in the experimental population at large, evaluations of candidates from different sub-castes—but the same larger caste category—are statistically indistinguishable from evaluations of candidates who are from both different sub-castes and different caste categories. The evidence therefore establishes the basic primacy of sub-caste (*jati*) relations in these Indian villages, using an experimental design in which the chances that the effects of sub-caste on candidate preferences are confounded are minimal.

The field experiment also lends insight into why voters prefer candidates of their own castes. Consistent with previous research on the relationship between caste and

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<sup>5</sup> I thus do not claim that causal effects estimated in the surveyed villages necessarily generalize to, say, all villages in the selected districts. On the other hand, it is not obvious that the village selection procedure would produce a highly unrepresentative sample of villages: because reservation takes place within taluks (an administrative division below the district), there is substantial observed heterogeneity in the experimental population on many observed variables, including the proportion of Scheduled Caste or Scheduled Tribe respondents (that is, the assignment covariates).

political preferences in India (inter alia, Chandra 2007), subjects do expect to receive greater access to government jobs and benefits from candidates with whom they share either a sub-caste or caste category. In addition, sharing a sub-caste category with a candidate significantly boosts evaluations of a candidate's credibility—in particular, subjects' confidence that a candidate's post-election behavior will be consistent with pre-election promises. However, in contrast to findings about the mechanisms that account for co-ethnic preferences in a different setting in which a similar experimental design was used (Dunning and Harrison 2008), here I find that sub-caste relationships also shape affective evaluations of candidates: for example, politicians who share subjects' sub-castes are found to be more impressive and competent, and they also are judged to share subjects' own preferences to a greater extent than do other candidates.

Finally, and perhaps most interestingly, the evidence suggests that reservation both heightens the effects of caste on candidate preferences and, in particular, shapes the dimension of caste identity that is most important. First, the estimated effects of caste on political preferences in the experiment are statistically significant only in reserved panchayats; in other words, the aggregate finding in the study population at large is driven by the greater salience of caste in reserved villages. Thus the evidence suggests that reservation heightens the effect of caste, as measured through the field experiment. Difference-of-means tests, in which estimated field experimental effects in unreserved villages are subtracted from estimated effects in reserved villages, suggest a causal effect of reservation, both on affective evaluations of politicians from other castes and on the extent to which politicians of various castes are perceived to share residents' preferences.

Second, reservation shifts the relative salience of sub-caste versus larger group ties. On the one hand, there is some evidence that reservation heightens competition between sub-caste groups who belong to the same larger caste category, as one would predict from the idea that ethnic groups or entrepreneurs construct minimum winning coalitions (Posner 2004); in particular, subjects presented with candidates from their own sub-caste are significantly more likely to report that they will receive a government job or benefit than subjects exposed to a candidate from a different sub-caste but the same larger category, but this is true only in reserved villages. (Interestingly, this holds both for subjects from Scheduled Castes and Scheduled Tribes—that is, the reserved groups—and for subjects from dominant Backward Castes—the unreserved groups; I discuss interpretations of this result below.)

On the other hand, there is also evidence of a solidarity effect, in which reservation intensifies affective or emotive preferences for candidates from different sub-castes who are part of the same larger category. For example, while in the experimental population at large, there is no statistically discernible difference in candidate evaluations, on average, across subjects exposed to candidates from the same sub-caste and different category and subjects exposed to candidates from different sub-castes and different categories, there is such a difference in reserved villages. That is, reservation increases the attractiveness and likeability of candidates who do not come from subjects' sub-caste but do come from the same caste category. Although subjects' anticipation of the differential benefits they will receive from politicians of different castes does seem to play a role, reservation seems to have the biggest effect in shaping political preferences on these emotive or affective grounds.

The survey of council members and presidents may suggest one reason that distributive politics seem to play a relatively limited role in the field experimental data. While my analysis of these survey data is still preliminary, the evidence suggests little effect of reservation on the working of village councils, and in particular, little effect on distributive outcomes—for example, the extent to which Scheduled Castes or Scheduled Tribes are targeted for policy benefits. This finding contrasts with previous studies that have found that reservation has strong distributive effects (Bardhan et al. 2005; Besley et al. 2004, 2007; Duflo and Topalova 2004; though see Banerjee and Pande 2007, also Bardhan and Mookherjee 2006). Instead, the results may perhaps more consistent with the literature arguing for the importance of a “politics of dignity” in understanding ethnic conflict and nationalism (Varshney 2003, Rao and Walton 2004). I return to the interpretation of these results below, after further describing the empirical context, research design, and experimental results.

## **II. Decentralization, reservation, and caste politics**

As many authors have noted, the 73<sup>rd</sup> and 74<sup>th</sup> constitutional amendments passed in 1993 by the Indian parliament represented an important step in democratic decentralization, as state governments were required to regulate the holding of regular elections for those bodies and also to pursue mechanisms for transferring resources and decision-making authority to village and town councils. (The 73<sup>rd</sup> amendment governed village councils, while the 74<sup>th</sup> concerned municipal bodies. In some states, like Karnataka or West Bengal, these federal amendments appeared merely to strengthen local

institutions that already had long histories, while in other states the state laws that followed the federal amendments represented a sharper break from the past.<sup>6</sup>

These constitutional amendments also required state legislatures to reserve representation in village councils for members of marginalized groups, including Dalits (former Untouchables, or Harijans in the Gandhian parlance) and other castes included among the Scheduled Castes in many Indian states. “Scheduled” refers to an official list of caste names, the list being attached as a schedule to separate legislation passed by each of the Indian states; inclusion on the list (as with lists of Scheduled Tribes, and, in some policy domains, of Other Backward Castes) can confer educational, employment, and political benefits in the form of quotas. Reservation of elected offices for politicians from particular communal or caste groups dates from the colonial period in India, when reservation of assembly seats for Muslim or Scheduled Caste candidates was seen as a preferable alternative to creating electoral jurisdictions in which only voters from those groups would vote (Hasan 1998). After independence, reservation was extended to both the national parliament (the Lok Sabha) and to state legislative assemblies (Parikh 1997). The 93<sup>rd</sup> constitutional amendment and subsequent state laws and regulations extended the principle of reservation to elections for members and presidents and panchayat councils. In many states, both the presidency of village councils and some of the members’ seats were to be reserved for Scheduled Caste, Scheduled Tribe, and sometimes Other Backward Castes (a category that includes castes deemed less marginal

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<sup>6</sup> A useful review can be found in Chaudhuri (2003).

or disadvantaged than SCs or STs but more marginal than forward castes), with reservation to rotate between village councils in different electoral terms.

Both the strengthening of the panchayats and the specific extension of caste and gender reservation to these councils has spawned a large and growing literature.<sup>7</sup> Yet there are several important issues about caste voting that do not appear to be broached in this literature. One is that the mechanisms that lead to caste voting do not appear to have been fully explored. Suppose, for example, that people vote for co-ethnics because they believe the campaign promises of politicians from their caste to distribute benefits to group members. This could well be because they believe that politicians from their caste sincerely prefer to distribute benefits to them, through a kind of co-ethnic altruism. On the other hand, politicians might fear that a failure to distribute benefits to caste members could lead to greater sanctioning, perhaps because of superior capacity for monitoring by co-ethnics. Whatever the specifics, the general issue is that the mechanisms that make the campaign promises of caste members have not been fully elucidated or empirically tested.<sup>8</sup>

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<sup>7</sup> Among many others, see Banerjee and Pande 2007, Bardhan et al. 2005, Bardhan and Mookherjee 2006, Besley et al. 2004, 2007, or Duflo and Topalova 2004. On reservation and state-level outcomes, see, e.g., Pande 2003 or Prakash 2007.

<sup>8</sup> In an important recent contribution, for instance, Munshi and Rosenzweig (2008) suggest that voters in Indian villages may be able to discipline and sanction elected politicians from their own sub-castes, if politicians diverge from the policies preferred by the median member of the sub-caste. However, although these authors speculate that increased information flows associated with endogamous marriage rules may reduce commitment problems within sub-caste groups (see Munshi and Rosenzweig 2008, 9), the ability of politicians to commit to voters in their sub-caste (but not to other voters) is a modeling assumption; neither the fact of co-ethnic commitment nor the mechanisms that would allow co-ethnics greater ability to commit to policies are tested empirically.

Panchayat governance provides an opportunity to study the connection between distributive politics and ethnic identification, because the distribution of benefits in villages typically has a clear discretionary character. In principle, central and state governments mandate that some funds be used for particular purposes.<sup>9</sup> In practice, as one interviewee emphasized to me, most local spending through the panchayats can have a discretionary character, with even local public goods such as roads and water pumps playing the role of private transfers: a section of road might be improved, or a water-pump installed, near a temple used by the village's Lingayath community (a dominant Backward Caste) or instead near its Scheduled Caste colony.<sup>10</sup> (Discrimination can make such apparent public goods excludable, too; for instance, former Untouchable castes are sometimes banned from high-caste temples).

Beyond the topic of caste voting and distributive politics, however, lies another intriguing issue: while much of the anthropological and political science literatures on village politics appears to focus on the political role of sub-caste or *jati*, reservation clearly empowers larger categories of more disparate groups, such as the heterogeneous category of Scheduled Castes. How, then, does reservation shape the relative salience of these various ethnic categories, and in particular the salience of sub-caste identities relative to the larger group category?

Karnataka is a particularly valuable case in which to examine the relative salience of sub-caste and larger, state-sanctioned caste categories, in part because of the structure

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<sup>9</sup> Although panchayats raise a small proportion of their funds from local taxes and fees, most resources come from transfers from central and state governments.

<sup>10</sup> Dr. SS Meenakshisundaram, Interview, Bangalore, January 17, 2009.

of caste categories and caste relations in the state. First, as a large literature attests, a predominant role tends to be played in both village and state politics by two dominant backward castes, Vokkaligas and Lingayaths (Manor 1989, Charsley and Karanth 1998, Shastri 2009). These two castes tend to both prevalent and especially dominant in the districts from which the villages in the experimental population were selected, in the southern (Mysore) region of Karnataka as well as the central and western parts of the state. The proportion of forward castes (Brahmins) is very low in these villages, at less than 10 percent; Vokkaligas and Lingayaths instead play the role of dominant castes, even though they are “backward”. In Karnataka, Weiner’s (2001: 221) general observation that “some of the most acute conflicts take place take place not between Dalits [former Untouchables included among the Scheduled Castes] and Brahmins and other forward castes, but between Dalits and OBCs and other intermediate castes” seems particularly apt.

Second, in Karnataka’s villages, the Scheduled Caste category also tends to be comprised of two main sub-caste groups, that is, the Holayas and the Madigas.<sup>11</sup> As Charsley and Karanth (1998: 38) put it, “Karnataka is the state with the longest list of Scheduled Castes and a frequent conviction that there are only two which are really Untouchable.”<sup>12</sup> According to some scholars, there is a history of competition and even antagonism between these two distinct sub-caste groups, both comprised of former

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<sup>11</sup> These terms, particularly Holaya or Holeya, can carry pejorative connotations but are widely used and well-understood.

<sup>12</sup> One reason the list is long in Karnataka is that Telugu, Marathi, and Malayalam speaking minorities, who live near the state borders with, respectively, Andhra Pradesh, Maharashtra, and Kerala, are included.

Untouchables.<sup>13</sup> Each group tends to have distinct heroes (with the Scheduled Caste leader Dr. Ambedkar tending to be especially celebrated by Holayas), and residential segregation occurs in many villages, with Holayas and Madigas living in separate colonies. Yet both Holayas and Madigas are empowered by political reservation and by government benefits (welfare schemes) for which eligibility depends on membership in the larger caste category, and politicians (such as Ambedkar himself) have mobilized Scheduled Castes as Scheduled Castes — suggesting the potential importance of the larger category for political preferences.

Thus, in Karnataka two important Scheduled Caste groups co-exist with two dominant Backward Caste groups, with each sub-caste maintaining its separate identity but each also linked to the larger state-sanctioned category. As I discuss further below, this structure of caste relations permits a ready examination of how reservation shapes the relative salience of sub-caste and state-sanctioned caste categories.<sup>14</sup> (Although I focus much of the analysis below on the four sub-castes mentioned above—two dominant Backward Castes and two Scheduled Castes—and although the experimental population came predominantly from these groups, I do also examine the relative salience of categories for other groups as well, including Scheduled Tribes as well as other backward

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<sup>13</sup> See discussion in the chapters of the Charsley and Karanth (1998) volume. While accompanying two Holaya research assistants to villages in Davanagere district, I was regaled with tales of the improprieties of Madigas, who (I was told) are mostly drunks.

<sup>14</sup> The selection of Karnataka may have some advantages in terms of external validity as well, in terms of generalizing effects for village elections to politics at larger levels of aggregation. For instance, while sub-caste groups may in general be too small to play a dominant role as a basis for coalition or party formation in state elections (Chhibber 1999), in Karnataka the dominance of Vokkaligas and Lingayats implies a predominant influence on state politics of these two sub-caste groups (Shastri 2009).

and forward caste groups.) I now turn to describing the research design, including both the field and natural experiments.

### **III. Research Design**

#### ***Field Experimental Design***

In the experiment, videotaped political speeches were shown to experimental subjects, who were recruited in rural villages in six districts in the southern, central and western parts of the state of Karnataka. Subjects were told that the speechmaker was considering running for a local Gram Panchayat council and that he would like to be the council president (*adhyaksha*).<sup>15</sup> We then asked subjects to evaluate the quality of the speech and the attractiveness of the candidate along various dimensions. Although, as described below, we presented speeches with two distinct scripts, and while we used one actor in the southern and central parts of the state and a different actor in the western part of the state (due to differences in spoken Kannada across different parts of Karnataka), speeches viewed by the subjects were otherwise identical. The translated text of the speech and other aspects of the experimental protocol will be posted online.<sup>16</sup>

The experimental manipulation consisted of what subjects were told about the politician's surname. Because last name conveys information about the sub-caste (*jati*) to which the politician belongs, and because belonging to a particular *jati* also implies membership in a larger caste category, varying the politician's last name generates the

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<sup>15</sup> Most Gram Panchayat constituencies (wards) elect multiple members. Council presidents are then elected indirectly by members.

<sup>16</sup> The URL is <http://pantheon.yale.edu/~td244/research.html>.

three treatment conditions depicted in Table 1. In the first condition, subjects and politicians belong to the same *jati* and the same caste category. In the second, they belong to the same sub-caste but to different caste categories. Finally, in the third condition, the subject and politician belong to different caste categories, as well as to different sub-castes.<sup>17</sup>

Experimental subjects were assigned at random to these three treatment conditions using a computer-generated list of pseudo-random integers between 1 and 3 (inclusive). The experimental stimuli, in particular the manipulation of the subject-politician ethnic relationship in the context of a videotaped speech, are similar to those used by Dunning and Harrison (2008) in Mali and by Dunning in an ongoing project in Brazil; the embedding of this field experiment in the natural experiment provided by reservation represents a methodological innovation in this study, relative to those studies.<sup>18</sup>

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<sup>17</sup> In related experimental research conducted in Mali, Dunning and Harrison (2008) included two control conditions, including one in which no ethnic information about the politician was offered. I did not do this here, since estimating the effect of ethnic identification relative to baseline evaluations of candidate quality—that is, evaluations of the quality of the speech or candidate, absent information about the candidate’s caste—seemed of limited interest. Instead, it made more sense to bolster statistical power by allocating a greater number of subjects to the three conditions in which the candidate’s surname was provided.

<sup>18</sup> Beaman et al. (2008) also use both field and natural experimental evidence drawn from Indian panchayats, though they are interested in the effects of gender rather than caste; they find that men rate a hypothetical leadership speech more negatively when the speaker’s voice is experimentally manipulated to be female, rather than male, but that “randomly assigned exposure to a female leader (due to mandated political representation for women) reduces such prejudice by 50-100% depending on the measure.”

**Table 1. Experimental Design: Treatment Conditions**

	<b>Subject and politician are from same caste category</b>	<b>Subject and politician from different caste categories</b>
<b>Subject and politician are from same sub-caste (<i>jati</i>)</b>	N=458	
<b>Subject and politician are from different sub-castes (<i>jatis</i>)</b>	N=470	N=525

In order to control the random assignment of subjects to each of the three treatment conditions depicted in Table 1, I needed a way to expose each subject to the appropriate stimulus—that is, to a politician’s patronym that corresponds to the relevant cell of Table 1, for a given subject’s *jati* and caste category. To do this, I reviewed the secondary literature (see, for instance, Charsley and Karanth 1998: 38-41 and other chapters in that volume) and conducted interviews with experts on caste in Karnataka. This allowed me to catalogue the politicians’ surnames associated with each of the treatment conditions, for the common sub-caste and caste groups we expected to encounter in our selected villages in Karnataka. Each row of the matrix depicted in Table 2 corresponds to a subject *jati* and caste category and each column of which gives the politicians’ surnames associated with the appropriate treatment condition. As discussed

below, I then used a smaller experiment (N=312) to test and refine a preliminary version of this matrix.

The surnames included in each column of Table 2 are not intended to be exhaustive: for instance, the second and third columns of the matrix do not include all possible surnames associated with each treatment condition. Rather, the intention was to use politician surnames for which the caste relationship would hopefully be well-understood and widely-recognized by subjects, so that we could accurately manipulate the stimulus to which subjects were exposed.

Due to the nature of the experimental design as well as the structure of caste groups in Karnataka, some cells of Table 2 have multiple entries. For instance, Holaya and Madiga are the two major Scheduled Caste groups in the areas of Karnataka where fieldwork was conducted, while Vokkaliga and Lingayath are the two dominant OBC castes. Thus, for example, a Holaya subject assigned to the third, “different *jati*, different caste category” condition could be exposed either to a Vokkaliga surname (we used Gowda, a very common surname for that *jati*) or to a Lingayath surname. In cells with multiple names, the politician's last name was selected at random from the names listed in the cell. (In these cases, we will estimate the average effect of being exposed to the two surnames included in that column, relative to other treatment conditions.) On the advice of interviewees and field investigators, I added some additional rows to Table 2 to accommodate the castes prevalent in the western part of the state (near Mangalore, in Dakshin Kannada); in those areas, I expected to encounter substantial numbers of citizens from the Bunt and Billava castes.

One additional issue is that, based on interviews with experts on the distribution of castes in the region, I expected field investigators to encounter an overwhelming preponderance of Nayaka tribes among the STs; in villages in which other tribes do not live, using a non-Nayaka ST surname for the different *jati*, same larger category condition might well sacrifice realism. I therefore opted to use SC surnames (Madiga and Holaya) for STs (Nayakas) exposed to the “different *jati*, same category” condition, even though of course reservation occurs separately for SC and ST council presidents. It is useful to keep in mind that the design really involves multiple experiments, one for each subject *jati*; the results reported below are largely robust to excluding all subjects except for Holayas and Madigas (the two main SC groups) and Lingayaths and Vokkaligas (the two dominant Backward Castes). As shown below (see Table 3), these four *jati* comprise the large majority of the experimental population.

Field investigators applied the experimental stimulus by introducing the politician’s name, prior to showing the videotaped speech (and after random assignment to the relevant treatment condition), and then by repeating the politician’s surname in every question asked about the politician and his speech.

I used speeches with two different contents in the experiment, one a more “programmatically” message and another a more “clientelistic” message that encouraged subjects to think about the individual benefits they would receive if the candidate were elected. The version of the speech was assigned at random with equal probability in each experimental trial. Initial data analysis suggested no discernible effects of the speech content, however, which is interesting when compared to the other effects discussed

below; for purposes of much of the discussion below, I pool across these two treatments involving different versions of the speech.

It bears emphasis that for several reasons, we might expect treatment effects to be rather weak in this experiment. One issue has to do with the use of surnames to deliver the experimental stimulus, namely, perceptions of caste. First, while surnames do almost universally indicate caste in rural villages, some citizens (and even politicians) do not regularly use their last names, especially in rural villages. Second, people from different *jatis* vary in their propensity to use surnames. Several interviewees noted that members of dominant castes like the Vokkaliga tend to proudly affix surnames, like Gowda, while surnames tend to be less widely used by members of Scheduled Castes (and some are even considered pejorative). This had implications for the experimental stimuli. In the initial, smaller experiment, for example, some of the surnames proved too subtle as a means of stimulating perceptions of caste relationships: last names such as Chennadasa (for the Holaya *jati*) and Malimath (for the Madiga *jati*), while described as unique to those castes by academic experts I interviewed, were not widely recognized by many subjects. In the larger experiment reported in this paper, I therefore retained only those surnames that were widely recognized in the pilot experiment (such as Gowda, for the Vokkaliga sub-caste, or Deshpande, for Brahmins). In other cases, I replaced the surname, sometimes using the name of the caste itself (which is not an unusual practice). These factors implied that there was some variation in the kinds of surnames included in Table 2.

**Table 2: Politician Surnames Used in Each Treatment Condition**

Subject's subcaste ( <i>jati</i> )	Subject's caste category	Condition 1: Subject and politician are from same <i>jati</i> and caste category	Condition 2: Subject and politician are from different <i>jati</i> , same caste category	Condition 3: Subject and politician are from different <i>jati</i> and caste category
Madiga	SC	Madiga	Holaya	Gowda Lingayath
Holaya	SC	Holaya	Madiga	Gowda Lingayath
Lambani	SC	Lamani	Madiga Holaya	Gowda Lingayath
Nayaka or other tribe	ST	Nayaka	Madiga Holaya	Gowda Lingayath
Lingayath	BC	Lingayath	Gowda	Madiga Holaya
Vokkaliga	BC	Gowda	Lingayath	Madiga Holaya
Kumbara	BC	Kumbara	Gowda Lingayath	Deshpande
Bunt	BC	Bunt	Gowda Lingayath	Madiga Holaya
Brahmin	Forward	Deshpande	Gowda Lingayath	Madiga Holaya

SC = Scheduled Caste. ST = Scheduled Tribe. BC = Backward Caste. Forward caste respondents (Brahmins) are grouped with the dominant Backward Castes for treatment assignment purposes.

In addition to the issue of surnames, there are other ways in which the stimulus is somewhat artificial—for example, watching videotapes speeches is obviously different from attending a real political rally—and thus may not fully stimulate perceptions of caste on the party of experimental subjects. On the other hand, the believability that our actor could be a candidate for the local panchayat is bolstered by the fact that most

panchayats consist of several villages, so it is conceivable that a resident in one village does not know all the residents of other villages in the panchayat.

Subjects were recruited from 200 villages in southern and central Karnataka. (The procedure for village selection is described below.) The experimental protocol called for 10 citizens in each village to participate in the experiment. Participants were administered a screening questionnaire, which included questions about sub-caste (*jati*) and caste category, which allowed assignment of the treatment stimulus; they were then shown the videotaped speech, which was introduced by the field investigator using the surname of the politician to which the subject had been assigned. Then, after answering post-treatment questions (that is, questions relating to the candidate and his speech), each participating citizen was also asked a range of survey questions about aspects of local panchayat governance, his or her participation in politics, and other topics unrelated to the experimental stimuli. The research design also called for the local panchayat president (*adhyaksha*), two other panchayat council members, and the executive secretary (the state-level bureaucrat who assists the panchayat) to be interviewed, using separate survey instruments.

Of the 200 villages in the original pool, 40 were set aside for a smaller experiment, which allowed the experimental stimuli and other aspects of the design to be tested and subsequently improved. While I do not report results from that smaller experiment, in which significant problems of implementation arose (see discussion below), I do analyze data on citizens' answers to observational survey questions in these villages, as well as data from the *adhyaksha*, member, and secretary questionnaires.

(These data were unaffected by the pilot phase of the experiment.) The study population for the experiment reported in this paper is 1,453 citizens.<sup>19</sup>

Within each village, field investigators were instructed to select at random four Scheduled Caste residents—ideally, two from the Holaya sub-caste and two from the Madiga sub-caste—and one Scheduled Tribe resident. If necessary, investigators were permitted to substitute sub-castes among these categories (for example, in communities with very few Madigas, three Holayas and one Madiga might be recruited; where no Scheduled Tribe residents existed, students could substitute a fifth Scheduled Caste participant.) The other five participants in each village were to be selected at random from the general and backward caste population. Because villages in rural Karnataka tend to be residentially segregated along caste and even sub-caste lines, stratifying the population for sampling purposes was relatively straightforward. In recruiting a Holaya respondent, for example, the investigators were told to go to the Holaya colony in the village (or, in villages where no sub-caste colony existed, to the Scheduled Caste colony), the location of which could be ascertained from the panchayat’s executive secretary or from a local resident. Investigators were then instructed to pick a house in the middle or corner of the lane or street corresponding to the caste category in question, attempt to recruit a respondent, and skip two houses before recruiting another.

In effect, then, the experimental group consists of a probabilistic sample of the residents of local villages, with an oversample of Scheduled Castes—who comprise less

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<sup>19</sup> The experimental population does not reach its intended size of 1,600 participants for numerous reasons, including data entry error, failure on the part of field investigators to record the experimental stimulus, and the presence of villages in which fewer than 10 subjects participated.

than 18 percent of the population of Karnataka, rather than 4 in 10 as in our sample—and Scheduled Tribes—who comprise well less than 1 in 10 residents in the selected villages. Of course, the population being sampled is limited to those residents from the sub-castes in the first column of Table 2. (These sub-castes comprise most of the population of rural villages in the areas in which fieldwork was conducted). The distribution of ethnicities in the experimental study group is depicted in Table 3. One can see that the experimental population overrepresents Scheduled Castes, who in fact represent less than 18 percent of the population statewide.

**Table 3: Ethnic Distribution of Experimental Population**

<b>Caste category</b>	<b>Sub-caste (<i>jati</i>)</b>	<b>N</b>	<b>Percent</b>
<b>Scheduled Caste</b>	Holaya	331	22.87
	Madiga	228	15.76
	Lambani	23	1.59
<b>Scheduled Tribe</b>	Nayaka	133	9.19
<b>Dominant Backward Castes</b>	Lingayath	267	18.45
	Vokkaliga	246	17.00
	Bunt	42	2.90
<b>Other Backward Castes</b>	Kumbara	77	5.32
<b>Forward Caste</b>	Brahmin	97	6.70

The interviews were conducted by around forty field investigators, most of whom are M.A. students in political science at Bangalore University, working in teams of two;

including the 40 villages in the pilot study, the twenty teams therefore visited on average ten villages each. I accompanied two teams of investigators to villages in Chamarajanagar district (Kollegala taluk) and Davanagere district (Harapanahalli taluk); many teams also typically included one more senior and experienced investigator. The fieldwork for the project was undertaken from December to February 2009.

### **Village Selection: A Regression-Discontinuity Approach**

In this section, I describe the rationale and procedure for selection of 200 villages in southern Karnataka (40 villages for the pilot phase of the study, and 160 for the larger experiment reported in this paper.) A growing empirical literature has sought to take advantage of the apparent natural experiment provided by reservation of the Gram Panchayat presidencies to estimate the causal effects of electoral rules and political leadership.<sup>20</sup>

In fact, caste reservation does not appear to be randomly or “as if” randomly assigned (Dunning 2008) in most Indian states. While there is substantial variation in reservation rules across states—the 93<sup>rd</sup> and 94<sup>th</sup> constitutional amendments leave the details to state legislative assemblies and bureaucrats—the rules promulgated in many states require bureaucrats to rank Gram Panchayats in descending order by the population proportions of the relevant group (such as Scheduled Caste or Scheduled Tribe). In the first elections held for village councils after creation of these lists, council presidencies or members’

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<sup>20</sup> Seats are also reserved for SC, ST, and BC members in panchayats, using the population of each group in the village to determine the number of seats to be reserved. However, because panchayat wards (usually comprising separate villages) typically elect multiple members—some reserved, some not—exploiting reservation to understand the effect of members’ reservation on caste relations in villages is less feasible.

seats would then be reserved in those panchayats at the top of the lists, for each relevant caste category; in subsequent elections, bureaucrats were to work down the lists, rotating reservation of council presidencies or members' seats to the next batch of panchayats. Clearly, comparisons between all reserved and all unreserved Gram Panchayats after any given election could lead to biased inferences about the causal effects of reservation, since reserved panchayats would tend to have larger proportions of Scheduled Caste or Scheduled Tribe residents than unreserved panchayats (and could conceivably differ in many unobserved ways as well).

However, notwithstanding the lack of random or “as if” random assignment to reservation, the strict observance of population proportions in determining reservation eligibility does permit construction of a regression-discontinuity design to assess the causal effects of reservation. The idea is that in any election, those panchayats the population proportions of which place them just above the floor for inclusion among the group of reserved panchayats will be, on average, very similar to those panchayats just below the floor. Indeed, factors other than reservation that might influence response variables of interest (such as the role or salience of caste in local politics) should in expectation be balanced across the two groups. Suppose the floor of the population proportion required for reservation in any given election is, say, 26 percent. Then whether 26.1 percent of panchayat residents are Scheduled Caste (thus prompting reservation) or instead just 25.9 percent (thus leaving the panchayat unreserved) may be something akin to a coin toss. In the neighborhood of the threshold, assignment of panchayats to reservation may be plausibly “as-if” random, as required by a natural experiment (Green and Gerber 2008; Dunning 2008). Because assignment to reservation

via the construction of population lists occurs within multiple, discrete administrative units, one may then select panchayats that are near the threshold from many such units and thereby construct a study population in which reservation is essentially randomly assigned.

The Karnataka example demonstrates the necessity for close attention to the idiosyncracies of procedure rules in constructing a regression-discontinuity design. In Karnataka, reservation happens at the taluk level, that is, the administrative unit below the district, and bureaucrats known as district collectors or other top officials are charged with conducting reservation in each taluk in their jurisdiction. State regulations call for district officials to rank the panchayats in each taluk by the proportion of Scheduled Castes, and also by proportion of Scheduled Tribes, in descending order. Reservation for each category then proceeds by working down the two lists, starting with the list of Scheduled Caste proportions.<sup>21</sup>

To select villages for inclusion in the study, I mimicked this reservation process, using census data on the proportion of Scheduled Castes and Scheduled Tribes in each taluk in selected districts. By sorting the lists in descending order and observing which were reserved in the most recent election cycle (these data were provided by the Karnataka State Election Commission), it was possible in each taluk to choose the village

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<sup>21</sup> Various institutional safeguards may help to protect the integrity of the procedure, by making the selection criteria transparent. For instance, the panchayats whose presidencies are selected for reservation are presented to panchayat members in taluk-level assemblies, and the rules are explained by the district collector (or other bureaucrat).

just above the threshold and the village just below the threshold, for each category of reservation. Villages were selected in this fashion from six districts (which were chosen because the castes discussed above are amply distributed there): Bangalore Rural, Chamarajanagar, Dakshin Kannada (Mangalore), Davanagere, Mandya, and Ramanagar.

After selection of the 200 villages and the initiation of field work, interviews revealed that some district collectors appeared to use a slightly different approach to reservation: the number rather than the proportion of seats reserved for members was used as a proxy for the size of the reserved group.<sup>22</sup> Since the proportion of Scheduled Castes or Scheduled Tribes in each panchayat is used for arriving at the proportion of members' seats to be reserved for each category, use of the proportion of seats would accord perfectly with the legal principle for reservation (and the one I used in village selection). However, ranking the panchayats in a taluk in descending order by the number of seats reserved should tend, in principle, to place larger panchayats at the top of the list. (Each panchayat is typically required to have one additional member for each 400 village residents). While it is unclear how widespread this alternative reservation practice is, it should not lead to bias, as long as there is no strong relationship between village size and the proportion of Scheduled Caste or Scheduled Tribe residents; the observed correlation between these variables is only 0.009 across the state of Karnataka.<sup>23</sup> Thus, unreserved and reserved villages with very similar proportions of Scheduled Caste or Scheduled

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<sup>22</sup> Interviews, State Election Commission, January 2009.

<sup>23</sup> This calculation uses census data for the state of Karnataka, merged to coincide with panchayat boundaries.

Tribe residents should have similar numbers of members' seats reserved; in particular, the size of the village should be independent of reservation near the threshold.

To assess the claim of as-if random assignment, Table 4 presents a randomization or balance check, comparing reserved and unreserved villages on measured pre-treatment covariates (and also across the assignment covariates, namely, SC and ST proportions). As the table shows, reserved and unreserved villages are statistically indistinguishable on these covariates, which is a necessary condition for a valid natural experiment (Dunning 2008). In particular, reserved and unreserved villages are balanced with respect to village size, as well as other pre-treatment drawn from the census, such as literacy rate and the number of workers. Reserved and unreserved villages are also tightly balanced on the assignment covariates (SC and ST proportion) used to construct the regression discontinuity, reflecting their location near the threshold for reservation.

This regression-discontinuity design therefore allows the construction of a population of 200 ex-ante comparable villages, 100 of which were exposed to reservation of the council president and 100 of which were not. (I sampled 40 of these villages for inclusion in the initial, smaller field experiment; this left 160 villages for the larger experiment, 81 of which were reserved). Notice that the design has one advantage relative to another frequent strategy for generating natural experiments, which is to exploit "as if" random jurisdictional borders to study units on either side of the border. In Posner's (2004) excellent study, for instance, relations between two ethnic groups (Chewas and Tumbukas) were studied on either side of the border between Zambia and Malawi. Through careful evaluation of historical evidence, Posner argues, convincingly, that differences in inter-group relations in each country have to do with electoral

competition and the size of the relevant electoral jurisdiction, rather than other variables (Dunning 2008). Yet with a single randomization and many possible treatments, this cannot be estimated from the data at hand. In contrast, the type of regression-discontinuity evaluated here generates a much larger number of groups, each exposed to a single treatment (reservation or lack thereof).

One feature to note about the regression-discontinuity design analyzed here is that it is possible, though perhaps unlikely, that some unreserved panchayats included in the experimental population would have been previously exposed to reservation. After the passage of the 1993 federal constitutional amendment, district bureaucrats began at the top and then worked down the lists of population proportions in each reservation category. Reservation of the presidency (though not of members' seats) has been rotated halfway through the five-year panchayat term since 2000, when a new law was passed in Karnataka that required midterm rotation; in consequence, each panchayat has had five presidents since the passage of the 73<sup>rd</sup> amendments, with terms beginning in 1994, 2000, 2002, 2005, and 2007. (The reservation data used in this paper come from the 2007 term). Should the number of reservations required in any taluk, across these five presidencies, exceed the total number of panchayats, the district collector will have reached the bottom of the list of population proportions, at which point he or she is required to start again at the top of the list; this gives rise to the possibility that unreserved or reserved villages could have been exposed to reservation in the past (though probably not within the previous decade). I have obtained and am having data translated on the history of reservation since 1994, for all districts and taluks in Karnataka, and thus will be able to evaluate this question empirically. For the moment, it

is useful to note that a history of past reservation in the unreserved panchayats analyzed here might weaken contrasts between reserved and unreserved panchayats (and thus limit the estimated effects of reservation).

**Table 4: Reservation of *Panchayat* Presidencies: Randomization Checks**

	Group 1: Reserved for SC or ST <b>(A)</b>	Group 2: Unreserved or reserved for OBC <b>(B)</b>	Difference of Means <b>(A) - (B)</b>	p-value (two- sided)
Mean population (Standard error)	5684.17 (200.44)	6055.3 (180.60)	-371.13 (269.80)	0.17
Mean male population (Standard error)	2873.2 (103.03)	3064.41 (92.96)	-191.21 (138.77)	0.17
Mean SC population (Standard error)	1119.21 (91.91)	1114.16 (67.84)	5.05 (114.23)	0.96
Mean ST population (Standard error)	505.52 (56.70)	444.85 (43.86)	60.67 (71.69)	0.40
Mean number of literates (Standard error)	3076.63 (111.46)	3315.61 (114.5)	-238.98 (159.79)	0.14
Mean number of workers (Standard error)	2860.12 (103.03)	3017.59 (92.41)	-157.47 (138.40)	0.26
<b>Assignment Covariates:</b>				
Mean SC proportion	0.18 (0.01)	0.18 (0.01)	0.00 (0.02)	0.97
Mean ST proportion	0.09 (0.01)	0.08 (0.01)	0.01 (0.01)	0.26
N	100	100		

The unit of analysis is the Gram Panchayat. Village-level census data were combined to construct the Gram Panchayat data. P-values give the probability of observing a t-statistic as large in absolute value as the observed value, if Group 1 and Group 2 have equal means.

#### IV. Analysis and Results

I now turn to the experimental analysis, which is conducted in terms of Neyman's (1923) potential response model. For each post-treatment question and each of the two speeches, subjects have one response if assigned to view a speech by a politician from their own sub-caste and caste group; another response if assigned to view a speech by a politician from a different sub-caste but from their own caste group; and a third response if assigned to view a speech by a politician from a different sub-caste and caste group.<sup>24</sup> Responses are deterministic, and only one response is observed for each subject. For any two treatments, the average causal effect is the difference between the average response, if all subjects were assigned to the first treatment, minus the average response of all subjects, if all were assigned to the second treatment. An unbiased estimator for the average causal effect is the average response of subjects randomly assigned to the first treatment, minus the average response of subjects randomly assigned to the second treatment. (The analysis relies on the intention-to-treat principle, because I focus on response to treatment assignment).

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<sup>24</sup> A minor issue is that some subjects, for some treatments, are assigned to one of two politician last names (see Table 2). The model is easily extended in this case: for instance, each such subject has one response for one last name and another response for the other last name. A similar extension handles the fact that two speech texts were used.

After viewing the videotaped speech, subjects were asked the extent to which the politician's speech made them want to vote for the candidate, on a scale of 1 to 7.<sup>25</sup> To reinforce the experimental stimulus, the surname of the candidate was repeated as a part of the question (as with every other question having to do with the politician and his speech). Descriptive statistics on responses to this and all other post-treatment questions are presented in the Appendix.

Table 4 shows average responses to this question, by treatment assignment category. The evidence demonstrates a causal effect of assignment to the same sub-caste on candidate preferences: respondents assigned to view a speech by a member of their own *jati* are significantly more likely to want to vote for the candidate than those assigned to the other treatment conditions. The size of the estimated average causal effect—0.22 points relative to the second treatment, and 0.20 points relative to the third treatment, which is a little less than one-quarter of one standard deviation—is somewhat smaller but in the neighborhood of the estimated effects of co-ethnicity in other contexts, in which the same question was asked and similar experimental designs were used (see Dunning and Harrison 2008).

Perhaps more striking, while the evidence suggests a causal effect of sub-caste relationships, there is little evidence here that sharing a larger caste category influences candidate preferences. Indeed, subjects' evaluations of candidates who come from a different *jati* but from the same caste category are statistically

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<sup>25</sup> Respondents were shown a ladder with 7 steps asked “Now, suppose the first step of the ladder means ‘no, not at all’ and the 7th step means ‘yes, completely.’ Where would you put your answer to the following question: Does the speech of (*name of politician*) make you want to vote for this candidate?”

indistinguishable from their evaluations of candidates who come from both a different *jati* and a different caste category. This difference holds not only for the whole experimental population (as reported in Table 4) but also for the sub-population of Scheduled Caste and Scheduled Tribe respondents (though sample sizes are smaller in these sub-groups, and the estimated effects of sharing a *jati*, relative to the other two conditions, are significant only at the 0.1 level).

**Table 5: The Effects of Treatment Assignment**

	<b>Quality of Speech</b>	<b>Vote Preference</b>
Subject and politician are from the same sub-caste ( <i>jati</i> ) and the same caste category <b>(1)</b>	4.81 (0.07)	4.46 (0.07)
Subject and politician from different sub-castes ( <i>jati</i> ) and the same caste category <b>(2)</b>	4.70 (0.06)	4.24 (0.07)
Subject and politician are from different sub-castes and different caste categories <b>(3)</b>	4.57 (0.06)	4.26 (0.06)
<b>Difference of Means (1-3)</b>	<b>0.24 (2.71)</b>	<b>0.20 (2.29)</b>
<b>Difference of Means (1-2)</b>	0.11 (1.20)	<b>0.22 (2.17)</b>
<b>Difference of Means (2-3)</b>	0.13 (1.52)	-0.02 (-0.20)

In the first three rows, standard errors are in parentheses. In the final three rows, t-statistics are in parentheses. Boldface type indicates that the estimated effect is significant at standard levels ( $p < 0.05$ ).

### **Why are co-ethnics preferred?**

In addition to the questions regarding overall candidate and speech evaluations, subjects were asked to evaluate candidates along a range of dimensions, such as their likeability, competence, and so on. Comparing answers to these questions across the three treatment conditions provides insight into *why* co-ethnics are preferred; in particular, it allows us better to understand the factors that make politicians from one sub-caste more attractive than those from a different sub-caste but the same caste category or, alternately, a different caste category.

I combine these various questions into several linear indices, all normalized to run from 0 to 1. (See the Appendix for the full list of questions). Questions tapping the candidate's likeability, competence, intelligence, and impressiveness are included in *affection*, that is, a variable measuring affective evaluations. Evaluations of the candidate's trustworthiness, motivations, capacity to face the challenges of office, likelihood of doing a good job if elected, and willingness to fight for his ideals and defend others are combined in *credibility*, a variable that taps subjects' expectations about the politician's post-election behavior. (Dunning and Harrison 2008 use similar indices in their study of co-ethnicity and cousinage relations in Mali). Three additional variables tap other specific aspects of candidate evaluations. First, *monitoring* combines separate questions about whether the subject would know if the candidate broke his campaign promises, and whether the subject could hold him accountable. Next, *preferences* taps whether the candidate is perceived to care about people like the subject, and also care about the same things as the subject. Finally, *benefits* measures the

likelihood that the subject would gain access to benefits (welfare schemes) or government jobs if the candidate were elected.

The first three rows of Table 6 report average values of these variables by treatment assignment category; the final three rows conduct difference-of-means tests to estimate the causal effect of treatment assignment. The analysis suggests several conclusions. First, several different kinds of mechanisms seem to explain why politicians from one's own sub-caste are preferred to politicians from a different category altogether (penultimate row of the table): the difference-of-means is significantly different for *affection*, *credibility*, *monitoring*, *preferences*, and *benefits*, with only answers to the *monitoring* questions statistically indistinguishable from zero. The size of the effects is between one-sixth and one-fourth of a standard deviation (see the Appendix for the SDs).

Second, expectations about distributive benefits seems to play a particular role in distinguishing same sub-caste politicians from different sub-caste, same category politicians. Recall from subjects express a significantly stronger vote preference for politicians from the former condition, relative to the latter. Yet among the summary indices, only the *benefits* variable statistically distinguishes these two treatment conditions. Finally, subjects from a different sub-caste but the same caste category are found more credible—that is, subjects have more positive expectations about post-election performance—than subjects from a different category.

The limited role of *monitoring* is also striking. A number of theoretical and empirical accounts emphasize the superior ability of co-ethnics to monitor one another (e.g., Fearon and Laitin 1996; also Habyarimana et al. 2008). It is therefore surprising, in

**Table 6. What Explains the Effects? Summary Indices, by Treatment Assignment**

	<i>Affection</i>	<i>Credibility</i>	<i>Monitoring</i>	<i>Preferences</i>	<i>Benefits</i>
Subject and politician from same <i>jati</i> and same caste category  <b>(1)</b>	0.51 (0.01)	0.58 (0.01)	0.63 (0.01)	0.57 (0.01)	0.58 (0.01)
Subject and politician from different <i>jati</i> but same caste category  <b>(2)</b>	0.49 (0.01)	0.56 (0.01)	0.61 (0.01)	0.55 (0.01)	0.54 (0.01)
Subject and politician from different <i>jati</i> and caste categories  <b>(3)</b>	0.48 (0.01)	0.54 (0.01)	0.61 (0.01)	0.53 (0.01)	0.52 (0.01)
<b>Difference of Means</b>  <b>(1-2)</b>	0.01 (1.04)	0.02 (1.73)	0.02 (1.11)	0.02 (1.42)	<b>0.06</b> <b>(3.64)</b>
<b>Difference of Means</b>  <b>(1-3)</b>	<b>0.03</b> <b>(2.79)</b>	<b>0.04</b> <b>(3.84)</b>	0.02 (1.00)	<b>0.04</b> <b>(2.89)</b>	<b>0.04</b> <b>(2.33)</b>
<b>Difference of Means</b>  <b>(2-3)</b>	0.02 (1.76)	<b>0.02</b> <b>(2.06)</b>	-0.00 (-0.15)	0.02 (1.52)	0.02 (1.20)

Standard errors are in parentheses in the first three rows of the table. In the final three rows, t-statistics are in parentheses. Boldface type indicates that the estimated effect is significant at standard levels ( $p < 0.05$ ).

a setting like rural villages that should be favorable to monitoring, to find that caste relationships do not influence the ability to monitor. Of course, the favorability of the setting could be the issue: average levels of monitoring are very high across all treatment conditions, substantially higher than for the other variables in Table 6.

## **VI. The effects of reservation**

If caste relationships affect subjects' evaluations of political candidates, and if the category of relationship that matters differs for different response variables, it may well be that the caste of the village council president shapes the role of caste in each surveyed village. How, then, does reservation shape the effect of caste relationships on evaluations of politicians and, in particular, the salience of different caste categories?

To investigate this topic, I estimate the intention-to-treat effects reported in Tables 5 and 6 separately for reserved and unreserved panchayats. Notice that the difference between these estimates, for reserved panchayats and unreserved panchayats, is an unbiased estimator for the causal effect of reservation, provided that reservation is "as if" randomly assigned. Table 7 reports the estimated effects for reserved and unreserved villages in the first two columns; the differences of means in the third column give reservation's estimated effects, for each of the response variables. In each column, t-statistics are in parentheses.

The analysis suggests several interesting findings. First, reservation heightens the influence of caste relationships on candidate preferences. With one exception, estimated treatment effects in the field experiment are only statistically significant in reserved panchayats (compare columns 1 and 2 of Table 7). In other words, the sub-group

analysis demonstrates that the aggregate findings in Tables 5 and 6 are driven mostly by the greater salience of caste in reserved villages. As the difference-of-means tests in the third column of Table 7 show, reservation has a significant estimated effect on affective evaluations of politicians from other castes, and on the extent to which politicians of various castes are perceived to share residents' preferences. Thus, reservation for lower-caste groups appears to heighten the general role of caste in shaping political preferences.

Second, reservation shifts the relative salience of different caste categories, increasing the overall importance of larger group ties. As Table 7 suggests, reservation intensifies distinctions between politicians from one's own larger caste category and politicians from a different caste category. Recall that in the aggregate, politicians from subjects' sub-castes are significantly preferred to politicians from a different caste category. However, as Table 7 shows, this is true only in reserved villages, at least for the *vote preference*, *affection*, *credibility*, *preferences*, and *benefits* variables. (The exception to this finding is the *credible* variable, where the estimated effect remains significant in unreserved panchayats as well). In addition, reservation sharpens distinctions between politicians who are from a different sub-caste, but from the same caste category, and politicians from a different caste category. In reserved villages, politicians with whom subjects share a larger category but not a sub-caste are evaluated significantly more positively than politicians from different larger category, at least for the *affection* and *preferences* variables.

Distinctions among politicians from the same larger category are also blurred, with one exception. Recall that in the aggregate, politicians from subjects' own sub-castes are significantly preferred to politicians from different sub-castes but the same

caste category. This does not remain true in the separate analysis of reserved and unreserved villages (where, of course, the smaller sample size in each group does limit statistical power). For variables that tap affective evaluations, subjects' perceptions of the candidate's credibility, and opinions about the extent to which the candidate cares about the same things as the subject, politicians from the subject's own larger caste category are statistically indistinguishable, whether or not they come from the subject's sub-caste.

On one dimension, however, reservation does not blur distinctions between sub-castes from the same larger category and even seems to intensify them: politicians from subjects' sub-castes are evaluated significantly more positively than politicians from a different sub-caste (but the same caste category) on the *benefits* variable, which combines two questions about whether subjects expect to receive jobs or benefits if the politician in the video were elected. Moreover, the estimated effect on *benefits* is significant in reserved panchayats but insignificant in unreserved panchayats. This may provide at least some evidence that reservation heightens competition between sub-caste groups who belong to the same larger caste category. It may be that since reservation mandates that council presidents come from the Scheduled Castes or Scheduled Tribes, competition for resources between sub-castes from these larger categories becomes more intense, which seems consistent with the idea that ethnic groups or entrepreneurs construct minimum winning coalitions (Posner 2004).<sup>26</sup> (Interestingly, however, the finding that reservation heightens competition within categories along the benefit dimension holds

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<sup>26</sup> Note, however, that reservation does not seem to shape effects on *vote preference*, where the comparison is between own-sub-caste and different sub-caste-but-same-category politicians.

both for subjects from Scheduled Castes and Scheduled Tribes—that is, the reserved groups—and for subjects from dominant Backward Castes—the unreserved groups.)

Yet as a whole, rather than mostly shaping expectations about post-election performance or about the distribution of benefits, reservation most strongly shapes affective evaluations of politicians, and the extent to which politicians of different castes are deemed to share the preferences of subjects. While puzzling in some ways (given that many observers have linked the political importance of caste to issues of material distribution), this evidence about the importance of affective evaluations is consistent with observations of a number of experts about the “politics of dignity” in Indian villages. For instance, Weiner (2001: 219-20) asserts that “at the local level, Dalit [former Untouchable] activists...are concerned less with getting benefits from the state and changing public policies than they are in promoting the mobilization of scheduled castes against upper-caste domination...The cry for ‘social justice’ is as much a demand for respect and equal treatment in ordinary everyday relationships as it is a demand for economic benefits.” As Kohli (2001: 16) also puts it, “the politics of caste is often the politics of dignity; goals sought are less broad-based education or health, but more respect, equality of treatment, and symbolic gains. As a result, inclusion of caste leaders into visible positions of power has often satisfied...the demands of lower-caste groups” (see also Varshney 2003, Rao and Walton 2004).

The evidence presented in this section puts an addendum on this observation, however. While reservation may satisfy some demands of lower-caste groups, it also appears to politicize caste differences, at least at the affective level. Indeed, reservation heightens caste-based preferences over political candidates. Perhaps most importantly, it

**Table 7: The Effect of Reservation on the Experimental Effects of Caste**

(1: same *jati* and group; 2: Different *jati*, same group; 3: different *jati* and group)

	Estimated effect, reserved panchayats <b>(A)</b> (t-statistic)	Estimated effect, unreserved panchayats <b>(B)</b> (t-statistic)	The effect of reservation <b>(A-B)</b> (t-statistic)
<b>Vote preference (1-2)</b>	0.20 (1.45)	0.23 (1.77)	-0.02 (-0.11)
<b>Vote preference (1-3)</b>	<b>0.31</b> <b>(2.30)</b>	0.12 (0.91)	0.19 (1.03)
<b>Vote preference (2-3)</b>	0.10 (0.77)	-0.11 (-0.88)	0.21 (1.18)
<b>Affection (1-2)</b>	0.03 (1.50)	-0.00 (-0.06)	0.03 (1.15)
<b>Affection (1-3)</b>	<b>0.06</b> <b>(4.07)</b>	-0.00 (-0.06)	<b>0.06</b> <b>(2.93)</b>
<b>Affection (2-3)</b>	<b>0.04</b> <b>(2.48)</b>	-0.00 (-0.00)	0.04 (1.79)
<b>Credibility (1-2)</b>	0.03 (1.82)	0.01 (0.62)	0.02 (0.88)
<b>Credibility (1-3)</b>	<b>0.06</b> <b>(3.63)</b>	<b>0.03</b> <b>(1.93)</b>	0.029 (1.28)
<b>Credibility (2-3)</b>	0.03 (1.76)	0.02 (1.30)	0.01 (0.05)
<b>Monitoring (1-2)</b>	0.02 (1.13)	0.01 (0.46)	0.01 (0.43)
<b>Monitoring (1-3)</b>	0.03 (1.30)	0.01 (0.25)	0.02 (0.70)
<b>Monitoring (2-3)</b>	0.00 (0.14)	-0.01 (-0.23)	0.01 (0.27)
<b>Preferences (1-2)</b>	0.01 (1.12)	0.02 (0.85)	0.01 (0.23)
<b>Preferences (1-3)</b>	<b>0.07</b> <b>(3.28)</b>	0.02 (0.72)	<b>0.06</b> <b>(1.94)</b>
<b>Preferences (2-3)</b>	<b>0.05</b> <b>(2.29)</b>	-0.00 (-0.18)	0.05 (1.79)
<b>Benefits (1-2)</b>	<b>0.05</b> <b>(2.17)</b>	0.02 (1.11)	0.03 (0.79)
<b>Benefits (1-3)</b>	<b>0.08</b> <b>(3.72)</b>	0.03 (1.49)	0.05 (1.63)
<b>Benefits (2-3)</b>	0.01 (0.32)	0.03 (1.48)	0.02 (0.81)

In reserved panchayats, the council presidency is reserved for Scheduled Castes or Scheduled Tribes. In unreserved panchayats, the presidency is open (general category) or reserved for Other Backward Castes (categories A or B). Boldface type indicates that the estimated effect is significant at standard levels ( $p < 0.05$ ).

also shifts the locus of difference that is most politically relevant from the sub-caste to the caste category, that is, to the level of ethnic aggregation that is officially sanctioned by the state.

## **VII. Does Reservation Shape Distributive Outcomes?**

There may be other reasons that reservation seems particularly to shape affective evaluations, however. In contrast to the evidence provided in some previous studies, results from my surveys of council members and presidents suggest relatively little effect of reservation on distributive outcomes, as well as little influence on the measured workings of panchayat councils. While this evidence should be considered preliminary, it does suggest reasons that distributive factors play a relatively muted role in the evidence from the field experiment.<sup>27</sup> In this penultimate section, I briefly discuss this evidence, after first describing the surveys.

In each village, in addition to administering the experiment to residents, field investigators administered surveys to two members and the panchayat president. If the panchayat president was SC or ST, researchers were required to include one SC or ST members among the two they interviewed; on average, then, around one-third of respondents should be SC or ST members or presidents. The surveys tapped members'

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<sup>27</sup> The results should be considered preliminary because I have not yet merged survey data from the 40 selected villages included in the pilot version of the field experiment. I am also working with field investigators to correct some apparent data miscodings, which have reduced the effective sample size somewhat. These factors imply that statistical power is low for the analysis presented in this section.

and presidents' perceptions of the workings of the Gram Panchayat, as well as the effectiveness Grama Sabhas (biannual village meetings), Ward Sabhas (ward meetings), and Panchayat Jambandi (social audits undertaken by district bureaucrats in the form of an open meeting). The surveys also measured other variables, such the extent to which SCs and STs are effectively served or given priority by the council, in the opinion of members and the president; the perceived role of caste in village politics; and the extent to which the president has independent power relative to other members as well as to the executive secretary (local bureaucrat).

Table 8 presents the average answers of council members and presidents to the survey questions, stratified by the reservation status of their panchayats (columns 1 and 2). Because reservation is assigned "as if" at random, comparing these variables across reserved and unreserved villages allows us to estimate the causal effect on the workings of the village council of the reservation of the presidency for SC or ST leaders. The difference-of-means tests in the third column estimate the causal effect of reservation on the perceptions of council members and presidents.

As the table suggests, there is little evidence that reservation strongly affects members' and presidents' perceptions of distributive outcomes. Indeed, none of the difference-of-means in the third column of Table 8 are statistically significant. The sample size is relatively small (N=372, with 167 from reserved panchayats and 205 from unreserved panchayats), thus limiting statistical power. Nonetheless, average answers from members and presidents of reserved and unreserved panchayats are remarkably balanced, on such variables as whether the council gives the greatest priority to SC and ST castes in allocating council funds; whether council members work together well as a

group; and whether the council president has more independent power to determine expenditures and choose beneficiaries than other council members. There is some limited evidence that reserved panchayats respond more effectively to the needs of SC and ST residents, though the effect is only significant at the 0.10 level.

The evidence presented in Table 8 contrasts with the findings of a number of previous studies that the identity of the council president shapes distributive outcomes (Bardhan et al. 2005; Besley et al. 2004, 2007; Duflo and Topalova 2004). One possibility is that in at least some previous studies, caste reservation is inappropriately understood to be assigned as if at random; as discussed in the third section above, if all reserved villages are compared to all unreserved villages, then the greater presence of Scheduled Castes or Scheduled Tribes in the former group of councils may bias estimates of the effect of reservation on, for instance, spending on Scheduled Caste groups. (Some spending is mandated for Scheduled Castes, so panchayats with a higher proportion of SC residents may naturally spend more on Scheduled Caste groups). However, this contrast between my preliminary evidence and previous studies remains somewhat puzzling. Further investigation of this topic will incorporate data on actual spending by village councils, which was gathered from surveys of executive secretaries (bureaucrats).

If reservation of the council presidency does not, in fact, strongly shape distributive outcomes, however, it may also help to explain the relatively important impact of reservation on affective evaluations. In other words, it may help account for the predominant role of the “politics of dignity” noticed by many analysts of the Indian panchayats.

**Table 8: Reservation and Distributive Outcomes: What do Council Members Say?**

	Members and Presidents, Reserved Panchayats <b>(A)</b> (standard error)	Members and Presidents, Unreserved Panchayats <b>(B)</b> (standard error)	The Effect of Reservation <b>(A-B)</b> (t-statistic)
<b>Council responds effectively to the needs of SC/ST citizens</b> 1-5, ascending scale	4.37 (0.06)	4.22 (0.06)	0.15 (1.73)
<b>SC/ST castes have more influence over the council than any other group</b> % saying yes (open-ended)	0.12 (0.03)	0.14 (0.03)	-0.02 (-0.51)
<b>The council gives greatest priority to SC/ST castes in allocating funds</b> % saying yes (open-ended)	0.19 (0.03)	0.23 (0.03)	-0.04 (0.77)
<b>In general, members favor their own castes in allocating jobs and benefits</b> % saying yes	0.38 (0.04)	0.46 (0.04)	-0.08 (1.46)
<b>Voters tend to favor candidates from their own sub-caste (<i>jati</i>)</b> % saying yes	0.34 (0.04)	0.31 (0.04)	0.03 (0.63)
<b>Council members work together well as a group</b> 1-5, ascending scale	4.37 (0.06)	4.42 (0.05)	-0.05 (-0.60)
<b>Number of council meetings in previous six months</b> 0-10 or more	4.64 (0.14)	4.52 (0.11)	0.12 (0.68)
<b>Effectiveness of council in undertaking works desired by constituents</b> 1-5, ascending scale	4.17 (0.08)	4.32 (0.06)	-0.16 (1.60)
<b>Number of full days per week worked by president (asked of members)</b> 0-7	4.15 (0.12)	4.42 (0.11)	-0.28 (1.68)

<b>What members disagree most about</b> % who say: which citizens will benefit from welfare schemes	0.38 (0.04)	0.30 (0.04)	0.08 (1.36)
<b>How disagreements are resolved</b> % who say: the president decides	0.21 (0.03)	0.18 (0.03)	0.03 (0.67)
<b>Independent power of the president, relative to other members, to set the council's expenditure priorities</b> 1-5, ascending scale	4.02 (0.08)	4.09 (0.06)	0.07 (0.69)
<b>Independent power of the council president to determine beneficiaries of welfare schemes</b> 1-5, ascending scale	3.89 (0.07)	3.89 (0.07)	0.00 (0.01)
<b>Independent power of the executive secretary (bureaucrat), relative to the president to set expenditure priorities</b> 1-5, ascending scale	3.81 (0.08)	3.78 (0.07)	0.04 (0.03)
<b>Independent power of the executive secretary (bureaucrat), relative to the president to determine beneficiaries of welfare schemes</b> 1-5, ascending scale	3.63 (0.07)	3.75 (0.07)	-0.12 (1.20)
<b>The respondent's most important priority for the council is the council's actual priority (presidents only)</b> % of presidents who say yes	1.00 (undefined)*	1.00 (undefined)*	0.00 (undefined)*

\* Council presidents were asked what the priority of the village council should be and were then asked what the actual priority is. Every president, in reserved and unreserved council's alike, identified the priority the council should have in his or her view as the council's actually priority.

### VIII. Conclusion

Analysts of ethnic politics have emphasized that ethnic identification, and even the ethnic categories that are politically salient, are endogenous to political competition, electoral rules, and the sanctioning of particular ethnic categories by the state. Yet such

causal claims are typically difficult to evaluate empirically, because the relative salience of ethnic categories and patterns of ethnic identification can themselves shape political competition and political institutions. By embedding a field experiment that measures co-ethnic preferences inside a regression-discontinuity design, in which reservation is “as if” randomly assigned, this paper is able to identify sources of variation in the salience of caste in shaping evaluations of political candidates for Indian village councils.

I find that reservation of council presidencies for lower-caste politicians causally shapes the effect of caste on candidate preferences. In particular, while the field experimental results as a whole underscore the importance of sub-caste (*jati*) ties, reservation makes the distinction between larger caste categories more salient. The experiment also usefully underscores the reasons *why* caste matters. I find that although distributive politics play some role, caste relationships most strongly shape affective evaluations of politicians.

This finding on the importance of affective ties contrasts with several recent experimental studies in other contexts—for instance, Habyarimana et al. (2007) or Dunning and Harrison (2008)—in which co-ethnic altruism or affective evaluations were not found to be key sources of co-ethnic advantages in providing public goods or in seeking votes. The results thus also score the utility of experimental replication in disparate contexts, allowing results to be accumulated, compared and eventually explained.

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**Appendix: Descriptive Statistics, Post-Treatment Variables**

<b>Variable Name</b>	<b>Survey Question/Definition</b>	<b>Range</b>	<b>Mean (Standard Deviation)</b>
<b>Quality of Speech</b>	“Please look at this ladder, which has 7 steps. Suppose the first step of the ladder means ‘very bad,’ and the 7th step means ‘very good.’ On what step would you place the quality of the speech of ( <i>name of politician</i> ) that you just heard?”	1-7 (ascending scale)	4.73 (1.35)
<b>Vote Preference</b>	“Now, suppose the first step of the ladder means ‘no, not at all’ and the 7th step means ‘yes, completely.’ Where would you put your answer to the following question: Does the speech of ( <i>name of politician</i> ) make you want to vote for this candidate?”	1-7 (ascending scale)	4.34 (1.42)
<b>Likeable</b>	“In your opinion, is the politician in the video, ( <i>name of politician</i> ), very unlikeable, unlikeable, neither likeable nor unlikeable, likeable, or very likeable?”	1-5 (ascending scale)	2.99 (1.16)
<b>Intelligent</b>	“In your opinion, is the politician in the video, ( <i>name of politician</i> ), not at all intelligent, a little intelligent, intelligent, quite intelligent, or very intelligent?”	1-5 (ascending scale)	2.76 (0.91)
<b>Competent</b>	“In your opinion, is the politician in the video, ( <i>name of politician</i> ), not at all competent, a little competent, competent, quite competent, or	1-5 (ascending scale)	2.68 (0.97)

	very competent?”		
<b>Trustworthy</b>	“In your opinion, is the politician in the video, ( <i>name of politician</i> ), not at all trustworthy, a little trustworthy, trustworthy, quite trustworthy, or very trustworthy?”	1-5 (ascending scale)	2.75 (0.98)
	<p><i>THE FOLLOWING INSTRUCTIONS WERE GIVEN FOR THE REST OF THE POST-TREATMENT QUESTIONS:</i></p> <p>“Please look again at the ladder with seven steps. Suppose that the first step means ‘I do not agree at all’ and the 7th step means ‘I agree completely.’ Where on the ladder would you place your degree of agreement with the following statements?”</p>		
<b>Impressed</b>	“You were impressed by the candidate, ( <i>name of politician</i> )”	1-7 (ascending scale)	4.39 (1.43)
<b>Ideas</b>	“You agree with the political ideas of ( <i>name of politician</i> )”	1-7 (ascending scale)	4.63 (1.50)
<b>Motives</b>	“The candidate, ( <i>name of the politician</i> ), has good motives for running for office.”	1-7 (ascending scale)	4.83 (1.44)
<b>Challenges</b>	“The candidate, ( <i>name of the politician</i> ), will be capable of facing the challenges of office.”	1-7 (ascending scale)	4.66 (1.40)
<b>GoodJob</b>	“If he were elected, ( <i>name of the politician</i> ) would do a good job in office.”	1-7 (ascending scale)	4.17 (1.47)
<b>FightIdeals</b>	“If he were elected, ( <i>name of the politician</i> ) would defend	1-7 (ascending scale)	4.25

	others and fight for his ideals.”	scale)	(1.51)
<b>BrokenPromises</b>	“If he were elected, ( <i>name of the politician</i> ) would keep his promises.”	1-7 (ascending scale)	4.33 (1.53)
<b>BrokenPromisesKnowledge</b>	“If ( <i>name of the politician</i> ) broke his promises, people like you would know about it.”	1-7 (ascending scale)	4.76 (1.59)
<b>HoldAccountable</b>	If ( <i>name of the politician</i> ) broke his promises, people like you could hold him accountable.”	1-7 (ascending scale)	4.64 (1.73)
<b>CaresPeopleLikeMe</b>	“The candidate, ( <i>name of the politician</i> ), cares about people like you.”	1-7 (ascending scale)	4.28 (1.61)
<b>CaresSameThings</b>	“The candidate, ( <i>name of the politician</i> ), cares about the same sorts of things as you do.”	1-7 (ascending scale)	4.40 (1.5)
<b>WelfareSchemes</b>	“If ( <i>name of the politician</i> ) were elected, people like you would receive more benefits from the welfare schemes of the government.”	1-7 (ascending scale)	4.39 (1.53)
<b>GovernmentJob</b>	“If ( <i>name of the politician</i> ) were elected, people like me would have a better chance of getting a job with the government.”	1-7 (ascending scale)	4.20 (1.67)
<i>Affection</i>	Linear scale combining <b>Likeable, Intelligent, Competent, and Impressed</b>	0-1	0.49 (0.16)
<i>Credibility</i>	Linear scale combining <b>Trustworthy, GoodMotives, FaceChallenges, GoodJob, and FightIdeals</b>	0-1	0.56 (0.18)
<i>Monitoring</i>	Linear scale combining <b>BrokenPromisesWouldKnow</b>	0-1	0.62

	and <b>HoldAccountable</b>		(0.24)
<i>Preferences</i>	Linear scale combining <b>CaresPeopleLikeMe</b> and <b>CaresSameThings</b>	0-1	0.56 (0.23)
<i>Benefits</i>	Linear scale combining <b>WelfareSchemes</b> and <b>Government Job</b>	0-1	0.55 (0.24)

The table reports average values across all three treatment conditions.