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Leader Identity and Coordination*

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Abstract

This paper examines policy effectiveness as a function of leader identity. We experimentally vary leader religious identity in a coordination game implemented in India, and focus upon citizen reactions to leader identity, controlling for leader actions. We find that minority leaders improve coordination, while majority leaders do not. Alternative treatment arms reveal that affirmative action for minorities reverses this result, while intergroup contact improves the effectiveness of leaders of both identities. We also find that minority leaders are less effective in towns with a history of intergroup conflict. Our results demonstrate that leader and policy effectiveness depend upon citizen reactions, conditioned by social identity and past conflict.

JEL codes: P16, D70, D91, J78

Keywords: Leader identity, religion, coordination failure, affirmative action, intergroup contact, conflict, India

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1. Introduction

We examine the role of leader identity in influencing economic outcomes and policy effectiveness in societies marked by social diversity. In a sharp departure from previous work on leader identity that focuses upon leader preferences and actions, we use a field experiment to isolate the role of citizen reactions. We find that citizen reactions to a leader's social identity can significantly impact social outcomes, over and above any direct effect of the leader's actions. Specifically, leaders belonging to the religious minority achieve greater group coordination. This is because individual (citizen) choices respond not only to economic payoffs but also to leader identity, minority citizens are more responsive to leader identity than majority group citizens, and majority group citizens anticipate this. So identity moderates behavior and, once coordination is involved, beliefs over the behavior of other members of the group matter. We extend the analysis by randomizing two policy treatments designed to improve social integration, namely affirmative action and intergroup contact. We find that these policy treatments modify citizen reactions to leader identity and play a significant role in determining their effectiveness. By virtue of conducting the experiment in areas with varying levels of previous intergroup conflict, we further identify a role for the history of intergroup conflict in shaping the effectiveness of minority group leaders.

The main outcome we investigate is economic coordination. Coordination is distinct from cooperation as measured, say, in public goods games. Cooperation relies on voluntary contributions by individuals, driven by prosocial motivations such as altruism or trust, or specific strategies such as conditional cooperation. Coordination, on the other hand, relies on individually rational (self-interested) choices to collectively act to achieve a common goal, which may or may not result in Pareto-superior outcomes. Coordination can be critical to resolving collective action problems and market failures. It has been shown to be important for conflict prevention, halting the transmission of viruses, changing social norms, escaping poverty traps, optimizing resource use on common land,

and raising the provision of public goods (Schelling, 1980; Coleman, 1987; Kremer, 1993; Hoff, 2000; Hoff and Stiglitz, 2001; Adda, 2016; Bowles and Halliday, 2020). Global public goods are often characterized as having weakest-link properties and thereby being susceptible to coordination failure (Sandler, 1998; Nordhaus, 2006). Coordination failure as a barrier to economic development is highlighted in the field work of Dreze and Gazdar (1997) and Dreze and Sharma (1998) in the Indian state of Uttar Pradesh, the site of our study. They observe, for instance, that villages fail to coordinate on simple tasks of community value such as sanitation or the timing of the planting of crops to maximize output, with severe welfare consequences. While coordination problems are rife in most societies, they are of particular importance in developing countries where formal institutions to coordinate individual actions are weak and externalities from infectious disease or pollution are large.

In this study we make a unique contribution by connecting the literature on politician identity and policy outcomes with the literature on leadership and coordination, by introducing the relevance of social identity in achieving coordination. Our identification of citizen reactions as a new mechanism by which leader identity influences policy outcomes is relevant to understanding the effectiveness of leaders, and the conditions for the success of policies aimed at helping minority groups such as affirmative action or improving intergroup interaction via residential desegregation or mobility policies like Moving to Opportunity in the United States. We also highlight that the success of identity-based policies will vary with the history of conflict between the two groups, a result that is relevant to many post-conflict settings.

We implement a large laboratory-style experiment in a field setting in India, and experimentally vary the religious identity of the leader. Addressing this research question with observational data is difficult for several reasons. First, leader identity is typically not randomly assigned and will tend to be correlated with citizen preferences. Second, with observational data there is no straightforward way

of disentangling the effects of leader preferences from those of citizen reactions. Third, it can be difficult to obtain secondary data on coordination outcomes.

We use a weakest link coordination game (as in Brandts and Cooper, 2006), a key feature of which is that an individual's payoff depends positively upon the minimum effort in the group and negatively on their own effort. This game has multiple Pareto-ranked equilibria. Coordination is measured as the minimum level of effort achieved in the group. Payoffs are designed such that coordination tends to occur at the lowest effort level, in a Pareto-inefficient equilibrium. Leaders can potentially affect a shift to a better equilibrium by proposing a (non-binding) effort level. We conducted this coordination game with 1028 participants in mixed-religion groups, across 44 towns in India's largest state of Uttar Pradesh. Our sample includes Muslims, who are a religious minority in India, and Hindus who form the religious majority.

To guide interpretation of our findings, we develop a stylized theoretical framework that allows for two types of individuals--rational types, who simply maximize their economic payoff in the game and behavioral types, who additionally respond to leader identity. The model predicts that minimum group effort will vary with leader identity if the expected fraction of behavioral types differs by citizen identity. In particular, individual choices are guided by beliefs about how others will respond to leader identity. For instance, in choosing their actions, Hindus not only directly respond to leader identity, they also account for how they think Muslims will respond to leader identity. If population minorities are expected to be more responsive to leader identity, the expected fraction of Muslim behavioral types will be higher than the expected fraction of Hindu behavioral types, and minimum group effort (and hence coordination) will be higher under Muslim leaders than under Hindu leaders.

Our main finding is that the introduction of Muslim leaders increases minimum group effort by 31%, coordinating group outcomes to a Pareto-superior equilibrium, while the introduction of Hindu leaders has no significant impact on coordination. This result is robust to the inclusion of town fixed

effects and to several specification checks. This novel finding identifies a potential gain in aggregate output associated with having minority leaders and establishes the relevance of social identity to coordination outcomes.

Investigating mechanisms, we show, consistent with our theoretical framework, that behavior is primarily driven by citizen reactions. In particular, it is shaped by Muslims having stronger in-group bias and Hindus expecting this to be the case, with in-group bias being primed by leader identity. We rule out other possibilities: we establish that higher coordination under Muslim leaders does not arise from their proposing higher effort (a proxy for leader preferences), from a perception that Muslims are more competent leaders, or a perception that Muslim leaders themselves supply higher effort.

The results discussed thus far describe reactions to leader identity in the control arm. We now discuss the influence of leader identity on coordination outcomes in the two treatment arms, offering the first analysis of affirmative action and intergroup contact in the same experimental setting. Towns were randomly assigned to either treatment or control arms, and within each town, we randomly assigned some groups to have Muslim leaders and others to have Hindu leaders. This experimental design allows us to identify whether the stronger coordination under a Muslim leader that we documented in the control arm is modified after affirmative action or intergroup contact is introduced. We also stratify by the intensity of recent religious conflict.

Under the affirmative action (AA) treatment, the main result is reversed: we now find that coordination improves under Hindu but not Muslim leaders. This is consistent with the treatment making salient for the Hindu majority that the Muslim minority is being favored by design, thereby priming Hindu identity. We verify this by showing that individual effort levels of Hindus increase under Hindu relative to Muslims leaders. This result demonstrates how identity-based policies modify citizen reactions to leader identity.

We facilitated intergroup contact by having mixed-identity groups collaborate in solving a puzzle before the coordination game was played. We find that this improves coordination in groups led by leaders of both religions, but with Hindu-led groups showing a significantly larger improvement. Indeed, this effect serves to counter-balance the control group difference so that, following intergroup contact, Hindu and Muslim leaders achieve similar levels of coordination. This is consistent with contact leading to an increase in the fraction of Hindu and Muslim behavioral types. In other words, identity becomes salient after contact with a member of the other religion, and more so among majority group members. This is consistent with majority group members having more limited exposure to minorities in a pre-treatment setting.

We re-estimated the impact of leader identity in the control group and in the two policy arms of the experiment, distinguishing towns in neighboring districts with a high vs low intensity of Hindu-Muslim conflict in the preceding thirty years. We compare coordination across arms *within* each district, so that district-specific unobservable characteristics do not contaminate our findings. We find that a history of intergroup conflict has a detrimental effect on the effectiveness of minority (Muslim) leaders under all policy regimes. Specifically, AA decreases Muslim leader effectiveness significantly in high conflict areas and intergroup contact increases Muslim leader effectiveness only in low conflict areas. In contrast, conflict history does not significantly influence Hindu leader effectiveness.

Our findings are relevant to research in several areas that are currently distinct from one another, including leader identity, coordination, quotas, intergroup contact and social or identity-related motivations for economic choices. Previous work on leader identity typically identifies the combined impact of leader preferences (or actions) and citizen reactions.¹ Our experimental design uniquely

¹ Many papers have examined the impact of a politician's personal identity (e.g. gender, ethnicity, caste) on policy outcomes. See, among others, Chattopadhyay and Duflo, 2004; Chin and Prakash, 2011; Bhalotra and Clots-Figueras, 2014; Brollo and Troiano, 2016; and Bhavnani, 2017. Citizen-candidate

allows us to isolate citizen reactions, and we find that they are a very important determinant of leader effectiveness. Prior work has tended to focus on whether leaders redistribute public goods or transfers towards their own group (Pande, 2003; Burgess et al., 2015), while our focus on coordination provides a measure of the aggregate economic impact of leader identity. Further, our study reveals that citizen reactions to leader identity can be changed by commonly implemented policies such as AA or intergroup contact, with important implications for the success of these policies. No previous work has investigated the effectiveness of these policies as a function of leader identity. Finally, we contribute novel evidence on the relevance of conflict history, a marker of intergroup hostility, in shaping the effectiveness of leaders and of policies.² Overall, our work pushes forward the frontier on the open question of what makes some leaders more effective than others (Brandts et al., 2007; Brandts et al., 2015; Weber et al., 2001).

Our study also takes forward the experimental literature on coordination failure. While previous laboratory experimental evidence suggests that leaders are effective in improving economic outcomes (Guth et al., 2007; Levy et al., 2011; Brandts et al., 2015; Jack and Recalde, 2015; Brandts et al., 2016; Heursen et al., 2019),³ our results make clear that such effects are significantly mediated by social identity in diverse societies. Previous research has not examined how the effectiveness of leaders may be modified by policies designed to assimilate minorities or promote minority group representation. Previous leader-coordination studies have almost entirely been conducted in the laboratory, while the few examples of coordination games in the field have not studied the role of leaders (Brooks et al., 2018; Afridi et al., 2020; Polania-Reyes and Echeverry, 2020). Our implementation of the coordination

models (Osborne and Slivinski, 1996; Besley and Coate, 1997) allow leader identity to affect policy outcomes, in contrast to Downsian models where only the identity of the median voter matters.

² Previous research primarily focuses on the origins of conflict (see Blattman and Miguel, 2010, for a review) and its impact on growth (Rodrick, 1999), credit markets (Fisman et al., 2020), human capital (Miguel and Roland, 2011) and cooperation (see Bauer et al., 2016 for a review).

³ See Hogg (2001) for a theoretical analysis and Devetag and Ortmann (2007) for a literature survey.

game in the field enables us to use real social identities rather than lab-assigned identities, to conduct the analysis in a developing country where strong formal institutions to enable coordination are less present, and to investigate how past conflict influences group behavior.

We contribute to the literature on AA policies, where there is no previous evidence on how citizens react to AA under leaders of different religious or ethnic identities; the existing literature has been dominated by the analysis of gender quotas.⁴ Of particular interest here are Gangadharan et al. (2016) and Bagues et al. (2017), who find that male identity is strengthened by gender quotas and committee-level exposure to women respectively, a result similar to our finding that Hindu identity is primed by quotas for Muslim leaders and by intergroup contact with Muslims. Similarly, no previous paper has analyzed the effectiveness of intergroup contact in improving coordination, nor how this varies with leader identity (and conflict history). The previous literature on intergroup contact finds that contact can change attitudes and prejudice towards the out-group, as documented for anti-Muslim prejudice in India (Barnhardt, 2009).⁵ We differ from this literature by measuring group coordination outcomes rather than individual attitudes; note that such coordination takes into account others' actions and attitudes. The question of whether the social identity of leaders matters, and how affirmative action or intergroup contact policies play out is interesting not only in the Hindu-Muslim setting (which is

⁴ Many papers have examined how gender quotas influence policy outcomes, gender norms, women's aspirations and political participation (see, among others, Chattopadhyay and Duflo, 2004; Adams and Ferreira, 2009; Beaman et al., 2009; Ahern and Dittmar, 2012; Beaman et al., 2012; Iyer et al., 2012; Matsa and Miller, 2013). Experimental research has primarily focused on whether gender quotas encourage women to take part in tournaments (Balafoutas and Sutter, 2012; Niederle et al., 2013; Leibbrandt et al., forthcoming).

⁵ See Allport (1954) for an articulation of the potential effects of intergroup contact, and Paluck et al. (2018) for a review of the empirical literature, as well as Pettigrew and Tropp (2006), Ashraf and Bandiera (2017) and Bertrand and Duflo (2017). Only a few recent studies use random assignment of groups to examine intergroup contact in a developing country setting (Corno et al., 2018; Lowe, 2017; Rao, 2019; Scacco and Warren, 2018). Bhavnani et al. (2014) study the effects of intergroup contact on violence in Jerusalem using observational data.

about a seventh of the world population), but also in many other societies where progress is held back by societal divisions.

Finally, we contribute to research in psychology, sociology and economics showing that social identity affects individual economic choices, and that the influence of identity on behavior varies with primes that make group identity salient (see, among others, Akerlof and Kranton, 2000; Benjamin et al., 2016; Hungerman, 2014; Chen and Chen, 2011). We differ from this literature by studying group outcomes rather than individual economic choices, and by using real identities rather than laboratory-primed ones. Further, no previous study has sought to trace the manner in which variation in leader identity activates the group identity of citizens and, thereby, the degree of coordination failure under alternative policy regimes. The rest of the paper is structured as follows. Section 2 provides contextual information on religion and leadership in India. Section 3 describes the data collection and Section 4 delineates the experimental design. Section 5 lays out a theoretical framework to structure and interpret our results, Sections 6 and 7 present the empirical results, and Section 8 concludes.

2. Context: Religion and Leadership in India

India is a religiously diverse country, with Hindus constituting the majority religion (79.8% of the population in the 2011 census), Muslims forming the largest religious minority (14.2% of the population) and the rest being comprised of Christians, Sikhs, Jains, Buddhists and others. With 172 million Muslims in 2011, India has the third largest Muslim population in the world. Close to 40% of Muslims live in urban areas relative to 29% of Hindus. The standard of living of Muslims is generally lower than that of Hindus: 43% of Muslims are illiterate relative to 36% for Hindus, 33% of Muslims are employed relative to 41% for Hindus and 25% of Muslims fall below the poverty line compared to 22% for all Indian citizens (NITI Aayog, 2016).

Violence between Hindus and Muslims has occurred frequently in India generating insecurity, displacement, segregation and loss of property and life. Previous research indicates that violence tends

to disproportionately impact Muslims (Mitra and Ray, 2014). For instance, between 1985 and 1987, in the ten states with significant Muslim populations, Muslims experienced 60% of all deaths related to religious riots, 45% of all injuries and 73% of property damage (Wilkinson, 2004, p 30). Previous research has shown that Muslims (and Hindus) exhibit positive in-group bias in trusting behavior when they form a minority of the population, and that such in-group bias is increased by the degree of identification with the group (Gupta et al., 2018). In Indian elections (where Muslims form a minority of voters), Muslims are more likely to vote for Muslim candidates, but there is no evidence that Hindus discriminate against Muslim candidates (Heath et al., 2015).

Compared to their population share, Muslims are under-represented in leadership positions: Muslims comprised only 7.6% of state legislators over the period 1980-2010 (Bhalotra et al., 2014), less than 3% of national ministers (The Economist, 2016), and only 4% of the police force and the judiciary (Jaffrelot et al., 2019). This low representation has substantive consequences, since the religious identity of politicians has been shown to matter for policy outcomes such as health, education and sex-selective abortions (Bhalotra et al., 2014; Bhalotra et al., 2019). There are no quotas for Muslims in central government positions. However, some quotas for Muslims have been implemented in the civil service and educational institutions in five states (Government of India, 2014).

3. Site Selection and Subject Recruitment

We conducted field work in two pairs of neighboring districts in Uttar Pradesh, India's largest state with more than 200 million inhabitants. Each district pair was composed of one high conflict and one low conflict district. The experiment was conducted in 44 towns in July 2017 with 1028 subjects. We focus on towns rather than villages, as Muslims are more likely to live in urban areas. This has the added benefit of comprising a more educated populace, reducing issues surrounding the understanding of the experimental tasks.

The 44 sites were randomly assigned to three different treatment arms: 14 sites were retained as control, and 15 each were assigned to the intergroup contact and the affirmative action treatments (see Appendix Table A1). The assignment was performed within each district (i.e. implicitly stratifying by high/low conflict status); we further stratified by Muslim population proportion and total population. Our research assistants then recruited study participants from both Hindu and Muslim sections of each town (see Appendix B1 for further details on site selection and subject recruitment).

Upon arrival, participant names were recorded on a participant list that was not visible to other participants. In India, it is relatively easy to identify Muslims from their names; our research assistants were easily able to recruit an equal number of Hindus and Muslims from the list to participate in the study. Participant religion was verified using the pre-experiment questionnaire (see below). There were only two misclassifications of religion based on name (these participants were compensated and replaced with others prior to the experiment). The selected individuals were assigned an ID number, assembled in a room and randomly seated on mats, with four participants and a research assistant on each mat (see Appendix Figure A2). Subject ID numbers were never matched with participant names, and the list of participant names were destroyed at the end of each session. Participants were given no information about the identities of other participants in the experiment.

4. Experimental Design

Each experimental session contained a pre-experiment survey and three tasks: a puzzle task, a weakest link coordination task (run across six rounds), and a social norms elicitation task. Out of the three tasks, one was chosen randomly for payment. The average payoff was ₹ 610 including a ₹ 200 show up fee. This constitutes about 2.5 days' wage for a semi-skilled laborer. The session concluded with a survey of attitudes and respondent characteristics. Subjects knew that the session had multiple stages but were not given instructions about any particular stage until reaching that stage (see Appendix B2 for complete experiment instructions).

4.1. Pre-Experiment Survey and Puzzle Task. Prior to commencement of the incentivized tasks, subjects answered a brief survey about their personal characteristics, namely height, eye color, hair color and religion. The questionnaire was primarily designed to check the religion of the participant (see layout in Appendix B2), and other questions were included to avoid making the research question explicitly salient, which might induce socially desirable response bias or experimenter demand effects. Using surveys and lists to make identity implicitly salient without making it explicitly salient is standard procedure in the identity salience literature (Steele and Aronson, 1995; Shih et al., 1999; Benjamin et al., 2016; Cohn et al., 2015). While it is difficult to know how participant responses would change if they were explicitly aware that the experiment was about religion, we should note that all subjects filled out the same questionnaire. So, even if the questionnaire made the religious focus of our research explicitly salient, this would be the same across different types of leaders or different policy arms.

After the pre-experiment survey, all subjects participated in a 12-piece jigsaw puzzle task. Participants completed the task individually in the control and affirmative action treatment groups, and in pairs in the intergroup contact treatment group. Our objective was to suppress competitiveness and have cooperative intergroup contact, since the latter has been shown to reduce prejudice (Paluck et al., 2018; Lowe, 2017). The time given for the puzzle assembly was twelve minutes and almost all participants were able to complete the task successfully in this time.

4.2. Weakest Link Coordination Task. The task structure is closely related to the minimum effort corporate turnaround game designed by Brandts et al. (2006), which is based on the minimum effort or weakest link coordination game of Van Huyck et al. (1990).⁶ It was conducted after the puzzle task.

⁶ Coordination is different from cooperation, as typically usually measured by the willingness to contribute in a standard public goods game. In the latter, payoffs depend on the sum of other players' contributions and there is only one pure-strategy Nash equilibrium. Non-zero contributions in public goods games can be related to altruism, trust or other prosocial motivations, and the leader's role can be thought of as encouraging such motivations. In the "weakest link" coordination game, by contrast, there are multiple pure-strategy Nash equilibria that can be Pareto-ranked, non-zero effort is

4.2.1. Group Formation. Individuals were assigned to four-member groups (each called a “firm”), comprised of two Hindu and two Muslim “employees.” Participants did not know, nor could they observe, who the other three in their group were. We did not provide information to participants on the identity of their group members, and they were explicitly told that the people sitting on their mat were not part of their firm.

4.2.2. Effort Choices and Payoffs. The task is run across six periods. In each period employees decide how many hours (x) to devote to firm activities. Their choices vary between 0 and 20 in intervals of 5: $x_i \in \{0,5,10,15,20\}$. It is noteworthy that choices are not actual hours worked but effort choices with payoff consequences. Employees’ payoffs for each period depend negatively on their own effort choice and positively on the minimum effort of all individuals in the group:

$$(1) \pi_i = 500 - 25x_i + [\min(x_i, X_{-i}) * 40]$$

where x_i is player i ’s own effort (number of hours) and X_{-i} is the vector of all other players’ effort choices. The payoff table is illustrated in Appendix Table A3, where the units are Indian rupees (1USD \approx ₹68). Participants were shown the payoff table but not the payoff equation. Under this payoff structure, coordinating on any of the available effort levels is a Nash equilibrium.

Note that it is only worthwhile for profit maximizing employees to raise their own effort level if this will increase the minimum effort of the firm.⁷ Given this, previous work has found that play often evolves towards the payoff-dominated equilibrium in which all players choose the lowest possible effort level (Brandts et al., 2006). The task is split into two stages. The first stage repeats the coordination game described above across four periods (rounds). Employees work in the same firm

individually rational and the leader’s role is to act as a focal point to guide the group towards a specific equilibrium.

⁷ For a profit maximizing employee to increase their effort by 1 unit, for example, from 0 to 5 hours, they must believe there is an 85.5% probability that each of the other three employees increase their effort. To derive this probability, we solve for p where $500 = 375(1 - p^3) + 575(p^3)$.

across all periods. At the end of each period subjects are informed of the firm's minimum effort. Employees are never informed of individual firm members' effort choices. The first stage is designed to induce coordination on an inefficient equilibrium with low levels of effort, which we label "coordination failure" (Brandts et al., 2015).

4.2.3. Leader Assignment. The second stage introduces a leader, and runs for two further periods. The leader's role within each firm is to suggest a non-binding number of hours to work. Leaders do not have the scope to communicate with their employees beyond proposing an effort level, similar to other papers in the "leading by example" literature (Güth et al., 2007, Gächter et al., 2012; Levy et al., 2011). All leaders are appointed and participants cannot elect or change the leader. Firm employees are informed of the leader's proposal, but not the actual effort choice of the leader. Employees are also provided information about the characteristics of their firm leader taken from the pre-experiment survey, namely height, eye color, hair color and religion. The only characteristic that varies across leaders is their religion.

Half of the firms in each session are assigned Hindu leaders and half are assigned Muslim leaders. Leader identity is randomly allocated, and the player in each group who will be the leader is also randomly selected conditional on their religion. Our estimates for the impact of introducing a leader are thus specific to leader identity. It is important to remember that the religious composition of firms is the same across all firms, regardless of the leader's religion. Participant characteristics with regard to demographics, education, income and religiosity are balanced across groups with Hindu or Muslim leaders (Appendix Table A4). Leader characteristics other than religion, in particular, gender, age, and family income are balanced across Hindu and Muslim leaders, though Muslim leaders are less likely to have gone to college and more likely to pray several times a day, similar to the overall population.

After being informed of the leader's effort proposal and leader characteristics at the start of the fifth period, similar to the earlier periods, employees are informed of their group's minimum effort in

the previous period. All employees including the leader must then decide how many hours to work. The leader's effort, just like the effort of other employees, is not visible to the group. The coordination game is repeated for two periods with the same leader, but with a new effort proposal by the leader in each period. If this task is selected for payment, players are paid their coordination game payoffs from two randomly selected periods. We have two additional treatment arms (described below) where the same weakest link game is played, but with changes to the environment in which the leaders operate, designed to mimic commonly proposed policy interventions.

4.2.4. Affirmative Action (AA) Treatment. Affirmative action policies, such as quotas, are common in both government and business to increase participation of disadvantaged or minority groups. As described earlier, 15 of our 44 towns were randomly assigned into an AA treatment arm. The game is conducted exactly as in the control arm described above, with one important exception. Upon the introduction of a leader at the beginning of period 5, subjects are told that “similar to many government positions, 50% of the leadership positions in this game will be reserved. Reservation will be made based on some characteristic in the initial survey.”

Along with information on the leader's characteristics (height, eye color, hair color, religion), employees with a Muslim (Hindu) leader are also informed that their leader is in a reserved (unreserved) position.⁸ While participants are not explicitly told that the reservation is based on religion, about 70% of respondents in the post-experiment survey correctly identified that the reservation was done on the basis of religion, with the rest citing other leader characteristics or saying “don't know.”⁹ It is important to reiterate that here, as in the control arm, by design all groups contain

⁸ Leadership positions could in theory also be reserved for Hindus. However, we do not analyse this possibility since we are not aware of the existence of such a policy in any part of India.

⁹ Our estimates, which are intent-to-treat estimates, will therefore under-estimate the impact of religion-based reservation. Sample size considerations precluded the inclusion of a treatment with Hindu leaders being reserved and, in practice, quotas are usually for population minority groups.

two Hindu and two Muslim participants. By comparing the control and the AA treatment arms (and thus effectively comparing a Muslim leader with a Muslim leader who is leading through a quota), we can measure whether people behave differently when they believe their leader is in a position due to an affirmative action policy. In our setting, AA does not change the composition of leaders, it only makes the reservation policy salient in participants' minds.

4.2.5. Intergroup Contact Treatment. We also investigate the impact of a randomized intervention that increases intergroup contact on citizen responses to leaders of different religious identity. The key difference between this treatment and the control arm is the implementation of the puzzle task. Unlike in the control arm and AA treatment, where puzzles are assembled individually, subjects in the contact treatment assemble the jigsaw puzzle with a partner from the other religion. The puzzle partner is a person sitting on the same mat as the participant, and therefore not a member of the same firm (see Appendix Figure A3). Subjects are encouraged to talk with their partner during the 12 minutes allowed for the puzzle. By comparing outcomes across the control arm and the contact treatment arm, we can infer the impact of intergroup contact on the ability of leaders to coordinate groups.

Subjects in the sample towns often live in separate Hindu and Muslim neighborhoods, which limits interaction between the two communities. Nevertheless, our survey confirms that only 14% of participants incorrectly identified the religion of their puzzle partner. A potential concern is that interacting with anyone, not necessarily from a different religion, prior to the coordination game may affect coordination. As we shall see below, we can reject this concern because we see no differences in minimum effort in the contact vs the control arms in the periods *before* the leader is introduced.

4.3. Norms Elicitation Task

To measure Hindu and Muslim religious norms we follow Krupka and Weber (2013) and Gangadharan et al. (2016) and conduct a social norms coordination task. Participants in this task are

asked a set of questions related to behavior in the weakest link coordination task. Participants are asked to rate the social appropriateness of a Hindu or Muslim employee working 0, 10 or 20 hours under a Hindu or Muslim leader. Appropriateness ratings are measured on a 4-point ordered scale, consisting of the following options: very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate and very socially appropriate. These questions can thus be used to evaluate what people within our sample towns believe are the appropriate behaviors between Muslims and Hindus.

5. Theoretical Framework

In order to explain our results, we describe a stylized theoretical framework to help us understand the role of leader identity in changing individual behavior and hence the group outcome in the coordination game. Specifically, our model incorporates the role of citizen reactions to leader identity in shaping aggregate outcomes. Akerlof and Kranton (2000) introduced the role of identity in economic decision making. In a setting broadly related to ours, Benjamin et al (2016) show that priming religious identity can change individual economic choices, but utility maximization in their model does not involve strategic interactions as in our coordination game. We build on an extensive reputational literature by introducing “behavioral types”—players who choose their effort based not only on their economic payoff but also on leader identity. Rational players, in contrast, maximize their payoff, taking into account the presence of behavioral types. As is standard in the reputational literature, we assume that behavioral types constitute only a tiny fraction of the population.

Recall that individuals in our field experiment are either Hindus or Muslims, and four players are randomly chosen from the population to constitute a group. Individuals do not know the identity or religion of other individuals in their group. The group plays the coordination game as described earlier,

with individuals choosing among possible effort choices over a continuum $[0, W]$.¹⁰ All players selecting any effort level in this range produces a Nash equilibrium. We assume that in situations where there is no leader, all individuals will use the concept of *risk-dominance* as an equilibrium selection device in the coordination game. This means that players have uniform beliefs over the others' effort strategies and that this guides their own effort choice. This assumption is similar to that made in coordination games with investment decisions under incomplete information.¹¹ The coordination game payoff is maximized when the individual player matches the minimum effort of the other players. So, the optimal effort choice involves calculating the expected value of the minimum of the other three players. Under the risk-dominant criterion, players assume that all the other players are randomizing uniformly over $[0, W]$, and hence the equilibrium effort choice can be calculated as $x^* = \frac{W}{4}$.

The optimization is different for behavioral individuals. Behavioral individuals will follow the leader's proposal if the leader is from their own religion and will choose effort level x^* when the leader is from the other religion. We assume that the leader's proposal will be greater than the minimum effort in the no-leader equilibrium x^* , indeed we show that this is the case.¹² Rational players will choose an effort level that optimizes their coordination game payoffs, keeping the responses of behavioral types in mind. A fraction a_H of Hindu individuals are behavioral types, as are a fraction a_M among Muslims. The overall fraction of behavioral types in the population is thus $\tilde{a} := pa_M + (1 - p)a_H$, where p is the population share of Muslims. a_H and a_M are small enough that the

¹⁰ In our experiment, we have discrete effort choices for ease of implementation in the field.

¹¹ See the literature on equilibrium selection and global games e.g. Carlsson and van Damme (1993), Morris and Shin (1998) and Morris, Shin and Yildiz (2016), among others.

¹² This is true in our data: less than 2% of all leader proposals in rounds 5 and 6 are lower than the minimum group effort in round 4 (prior to leader identity being announced).

probability of more than one behavioral individual in a group of 3 or 4 is close to zero, so that rational players can behave as though there is at most one behavioral player in the rest of the group.¹³

The optimal effort choices for a rational player when there is a Hindu leader (x_H), and when there is a Muslim leader (x_M), can then be derived as follows:

$$(2) \quad x_H = (1 - \tilde{a})^3 x^* + 3(1 - \tilde{a})^2 \left((1 - p)a_H \frac{w}{3} + pa_M x^{**} \right)$$

$$(3) \quad x_M = (1 - \tilde{a})^3 x^* + 3(1 - \tilde{a})^2 \left(pa_M \frac{w}{3} + (1 - p)a_H x^{**} \right)$$

Here the first term reflects the probability of all three other players being rational (and the optimal choice is therefore x^*); the second term is the expected optimal choice under the assumption of one player in the group being behavioral. Under Hindu leaders, if the behavioral player is Hindu (which happens with probability $(1 - p)a_H$), that player will follow the leader and choose effort above x^* , and hence the rational player only needs to match the expected minimum effort of the other two rational players, which is $\frac{w}{3}$ ($> x^*$). On the other hand, if the behavioral player is Muslim (which happens with probability pa_M), that player chooses x^* . Then the rational player needs to match the expected minimum value of x^* and the choices of the other two players, which can be calculated as $x^{**} = \frac{9}{16}x^* + \frac{3}{16}\frac{x^*}{2} + \frac{1}{16}\frac{x^*}{3} < x^*$. A similar logic applies to computation of x_M .

Examination of equations (2) and (3) tells us that optimal effort in the presence of a leader will be higher than the optimal effort without a leader only under certain conditions, and that the change in optimal effort will depend on the identity of the leader as well as the fraction of behavioral individuals in each religion. This is a contribution we make to the coordination literature that typically overlooks social diversity within groups.

¹³ For instance, if \tilde{a} is 0.10, then the probability of more than one behavioural player is only 5.23% in a group of size 4, and 2.8% in a group of size 3. If \tilde{a} is 0.05, these probabilities are 1.4% and 0.7% respectively.

We can be more specific in our predictions: as long as pa_M is sufficiently greater than $(1 - p)a_H$, minimum group effort will be higher under Muslim leaders after leader identity is made public, and the increase in minimum group effort under Muslim leaders (compared to the situation without a leader) will be greater than the increase in minimum group effort under Hindu leaders. Note that one way this sufficient condition is satisfied is if the fraction of behavioral types among Muslims (a_M) is much greater than the fraction among Hindus (a_H). This is likely to be the case, since previous literature has established that members of population minority groups are more likely to display “in-group bias” and majority groups are unlikely to do so (Bisin and Verdier, 2001; Gupta et al, 2018; Berge et al., 2019). We now proceed to test these hypotheses using the data from our field experiment.

6. The Impact of Leader Identity on Coordination Outcomes

6.1. Regression Specification

Our main outcome variable is the minimum effort exerted in the group in each round. This is the key determinant of player payoffs and is the standard measure of coordination in the weakest link literature. We test whether leaders improve coordination by estimating the following specification:

$$(4) \quad \text{MinGroupEffort}_{kjt} = \alpha + \beta \text{Leader}_{kjt} + G'_{kj} \gamma + \varepsilon_{kjt} ; t=1,2,\dots,6$$

where $\text{MinGroupEffort}_{kjt}$ is the minimum effort exerted by group k in town j in period t , and Leader_{kjt} is a dummy variable that takes value one for periods 5 and 6, when a leader is introduced. This regression therefore compares the group’s minimum effort in periods with a leader to periods without a leader. G_{kj} is a suite of control variables that includes town fixed effects, demographic controls (average age, education, gender mix and monthly household income of the group members) and a control for religiosity based on prayer frequency. Standard errors are clustered at the group level to account for within-group correlation in outcomes across different periods.

We run specification (4) separately for Hindu and Muslim leaders to test whether leader identity matters in achieving better coordination. Since comparisons between later and earlier rounds maybe

influenced by round effects and because our main interest is in the comparison between Hindu and Muslims (where pre-leader rounds have similar rates of coordination) we also run a regression on the combined data, and include an interaction term $Leader_{kjt} * MuslimLeader_{kjt}$ in order to test whether the increase in minimum effort under Muslim leaders is higher than under Hindu leaders. Recall that half of all groups *within* each town are randomly assigned to have Muslim leaders. We also run a robustness test in which we restrict the analysis to periods 5 and 6, and control for the group-specific minimum effort in period 4 and the leader’s proposals in periods 5 and 6. This enables us to test whether the impact of the policies can be attributed to differences in the leader’s proposal, or to differences across groups in the coordination outcome in previous rounds.

6.2. Leader Identity and Coordination in the Control Group

Consistent with the corporate turnaround game literature, we find that groups coordinate on the low-effort equilibrium in the absence of a leader, which we call “coordination failure.” The average minimum group effort is less than three hours at the end of period 4 (Figure 1A). We find that introducing Muslim leaders significantly improves minimum group effort in periods 5 and 6 (Figure 1A). The efficiency gain in Muslim-led groups is large: minimum group effort increases by 1.07 hours, compared to the pre-leader average of 3.45 hours in periods 1 through 4 (Table 1, column 1).¹⁴ The estimates are robust to controlling for the demographic and religious characteristics of group members, consistent with our randomized assignment of leader identity (column 3). In contrast, the introduction of Hindu leaders does not improve minimum group effort (Figure 1A), leading to a statistically insignificant decline of 0.488 hours (Table 1, column 2). The difference in coordination

¹⁴ The effect of Muslim leadership on minimum effort is lower than in the lab experiment of Brandts et al. (2015) who find that a randomly selected leader increases minimum effort from 3.33 to 11.25 (using a rescaled measure for comparison purposes). The lower impact of leaders in our context could be due to differences in the context (developed country lab experiment vs developing country field setting) or because leaders in our experiment could only communicate a numerical proposal while those in Brandts et al. (2015) had the ability to send more detailed messages to participants.

gains between Muslim and Hindu leaders is statistically significant (column 5). Our results are consistent with a higher expected fraction of behavioral types among Muslims-- our model predicts that Muslim leaders will have a bigger impact than Hindu leaders under this situation.

The results are robust to using town random effects instead of town fixed effects, using an ordered probit specification rather than OLS, and to controlling for town*mat fixed effects to ensure that participants are correctly responding to the effort choices of their firm members rather than the effort choices of those seated on the same mat during the experiment. In a post-game survey, we elicited each participant's trust of people of the other religion by asking whether they would like to have a neighbor of a different religion. We find no evidence that our results are driven by differences in cross-religion trust, see Appendix Table C1.

6.3. Mechanism: Citizen Reactions to Leader Identity

Our model generates predictions based on individual responses to leader identity (among the "behavioral types") and other players' optimal strategies in light of these expected reactions. We show that our results cannot be explained by alternative hypotheses, such as the reactions to leader proposals or to the perceived competence of leaders. First, we verify that our results cannot be explained by differences in leader proposals. Our data reveal that Muslim leaders propose 10.5 hours on average, compared to 9.4 hours for Hindu leaders (Appendix Table A5). This difference is not statistically significant (Appendix Table A6, column 1), and a Kolmogorov-Smirnov test shows that the distributions of proposals by leader identity are not statistically different (p value= 0.452; Appendix Figure A4). We confirmed that the significantly different results under Muslim leaders compared to Hindu leaders hold even when we control for leader proposals and for minimum effort in period 4 by restricting the sample to periods 5 and 6 (Table 1, column 6). Second, we find that our results are not driven by perceptions of higher competence of Muslim leaders, by greater or lesser exposure to real-life Muslim leaders, or by beliefs about the hours worked by the leader (see Appendix Table A7).

We now show two pieces of evidence consistent with our theoretical framework. Our theoretical framework predicts that we should see increased effort in Muslim-led groups when pa_M is sufficiently greater than $(1 - p)a_H$. When is this more likely to happen? Based on previous research, we expect a lower fraction of Muslim behavioral type individuals (a_M) in towns where Muslims form a higher fraction of the population, i.e. where p is high. This means that the inequality is less likely to be satisfied when p is very high or very low, and more likely to be satisfied at intermediate values of p . In Appendix Figure A5, we graph the increase in minimum effort under Muslim leaders against the town’s Muslim population share. The results are in line with this prediction, namely that towns with very high or very low Muslim population share show smaller increases. This is suggestive rather than conclusive as, with a small number of towns, the differences are not statistically significant.

Importantly, we show that our results are consistent with the extent to which individuals expect others to react to leader identity. As part of the social norms task, we asked respondents to rate (on a scale of 1 to 4, with 4 being the highest) how “socially appropriate” others would consider it to be for a Hindu or Muslim employee to choose the maximum effort level under a Hindu or a Muslim leader. We find that Hindu subjects rate a maximum effort as less “socially appropriate” from a Muslim employee when faced with a Hindu leader instead of a Muslim leader, a statistically significant difference (Appendix Table A8, panel A). In other words, Hindus *expect* Muslims to significantly change their behavior based upon leader identity. In contrast, Muslim employees do not expect any difference in effort from themselves under Hindu vs Muslim leaders. They do rate Hindus providing maximum effort under Muslim leaders to be less socially appropriate than under Hindu leaders, but the difference is not statistically significant (Appendix Table A8, panel B).

Given this structure of beliefs, our model predicts that rational Hindu employees are much less likely to increase effort under Hindu leaders (since they are more likely to expect the Muslims to be “behavioral”), as compared to Muslim employees under Hindu leaders. The reverse is unlikely to be

true, since Muslim subjects do not assign a statistically different rating to Hindu employees' appropriateness under different leaders. We can investigate these predictions by examining how individual effort decisions respond to leader identity:

$$(5) \text{ IndividualEffort}_{ikjt} = a + b\text{Leader}_{kjt} + G'_{ikj}g + w_{ikjt}; t = 1, 2, \dots, 6$$

where $\text{IndividualEffort}_{ikjt}$ is the effort choice of individual i in group k (of town j) and period t . As before, G_{ikj} includes town fixed effects, demographic controls and religion, and standard errors are clustered at the group level. Note that individual effort choices depend not only on their expectations of how other individuals in the group will react to the leader's proposal and the leader's identity, but also the individual's own reaction to leader identity (if they happen to be a behavioral type).

Consistent with our model predictions, we find that Hindu employees show a decline in effort when a Hindu leader is introduced, while Muslim employees do not show any significant change in effort (Table 2, columns 1 and 2). The difference in response between Hindu and Muslim employees is significant at the 10% level. In contrast, Muslim employees exhibit a statistically significant increase in effort under Muslim leaders, while Hindu employees show a non-significant increase in effort (Table 2, columns 3 and 4). However, the difference between Hindu and Muslim employee response to a Muslim leader is not statistically significant. To summarize, our investigations indicate that our main finding, that Muslim leaders induce greater coordination towards Pareto-superior equilibria, is primarily driven by citizen reactions that are shaped by expectations of Muslims having stronger in-group bias (i.e., they are more likely to expect the Muslims to be "behavioral").

7. Coordination Responses to Leader Identity under Policy Assignment

7.1. Regression Specification

We examine whether leader effectiveness varies across policy environments by comparing coordination outcomes for a given leader identity across the different treatment groups as follows:

$$(6) \text{MinGroupEffort}_{kjt} = \beta_0 + \beta_1 \text{Leader}_{kjt} + \beta_2 \text{Leader}_{kjt} * \text{AA}_j + \beta_3 \text{Leader}_{kjt} * \text{Contact}_j + X'_{kj} \gamma + \varepsilon_{kjt} ; t = 1, 2, \dots, 6$$

In equation (6), AA_j is a dummy that equals one if town j was randomly assigned to the affirmative action treatment and Contact_j equals one if the town was randomly assigned to the contact treatment. We estimate equation (6) separately for Muslim and Hindu leaders. β_1 then captures the impact of the leader on coordination in the control arm, β_2 estimates the differential impact of the leader in an AA environment and β_3 estimates the differential impact of the leader in an environment with pre-game contact between members of the different religions.

7.2. Affirmative Action

Our empirical results show that affirmative action (AA) policies, a commonly suggested solution to improve integration and opportunities for minorities, can in fact strongly reduce the effectiveness of minority leaders. Minimum effort increases by a statistically insignificant 0.227 hours (1.067-0.840) under Muslim leaders (Table 3, column 1). In contrast, minimum group effort increases by a large and statistically significant 2.391 hours under Hindu leaders (column 2).

Our model suggests that this may result from an increase in the expectation that Hindus include behavioral types (a_H), which would lead to increased minimum effort under Hindu leaders. This will be more likely to happen if a_H increases so much that the sufficient condition for our hypothesis is reversed, i.e. $(1 - p)a_H \gg pa_M$. Such a change in behavior is consistent with previous research which finds that AA policies result in a strengthening of in-group bias among AA non-recipients (Gangadharan et al., 2016 show this in the context of gender quotas in India). This effect is likely to be heightened in contexts where people believe that quota recipients are not suitable for leadership roles because they are less skilled or not truly disadvantaged (Ip et al., 2019). To investigate the

behavior of Muslim and Hindu individuals, we examine individual effort as a function of the religion of the group leader across different treatment arms:

$$(7) \text{IndividualEffort}_{ikjt} = f_0 + f_1 \text{Leader}_{kjt} + f_2 \text{Leader}_{kjt} * \text{AA}_j + f_3 \text{Leader}_{kjt} * \text{Contact}_j + X'_{ikj} \mathbf{g} + z_{ikjt} \quad ; t = 1, 2, \dots, 6$$

Our estimates show that Hindu employees chose significantly higher effort levels in Hindu-led groups under the AA treatment compared to the control group i.e. $f_2 > 0$ (Appendix Table A9, column 4). The results are sharper when we restrict to periods 5 and 6, and include controls for the leader's proposal and for minimum group effort in the pre-leader period, similar to columns 3 and 4 of Table 3. We see that both Hindu and Muslim employees reduce effort under Muslim leaders in the AA environment compared to the control group (Appendix Table A9, columns 5 and 6). Similarly, both Hindu and Muslim employees increase effort under Hindu leaders in the AA environment (columns 7 and 8). Though the change in effort is statistically significant only for Hindu employees, the fact that both types of employees' effort choices move in the same direction is consistent with a generalized perception of greater a_H and lower a_M under AA. Note also that the higher response of Hindu employees to both Hindu and Muslim leaders is consistent with a greater fraction of behavioral types among Hindus under AA.

As with the control group results, we verify that these differences do not arise because of differences in leader proposals across treatment arms. Muslim leaders do not make statistically different proposals from Hindu leaders across any of the treatment arms (Appendix Table A6, column 2). To address the possible concern that the groups assigned to the different policy environments undergo different rates of learning over the course of the game, we restrict the estimation sample to the last two periods and control for leader proposals and minimum effort in the previous period of the game, period 4 (Table 3, columns 3 and 4). The concern is allayed, and the coefficients on $\text{Leader} * \text{AA}$ are now statistically significant for both Muslim and Hindu leaders.

7.3. *Intergroup Contact*

Another common policy to improve integration is to encourage contact and interaction between groups. We find that intergroup contact improves minimum group effort under both Muslim and Hindu leaders compared to the control group, but the difference is larger for Hindu leaders. For Muslim leaders, intergroup contact increases minimum group effort by an additional 1.007 hours compared to the control group, but this difference is not statistically significant (Table 3, column 1). Under Hindu leaders, intergroup contact results in a large and statistically significant increase of 2.755 hours of minimum group effort, compared to the control group (column 2). This difference counterbalances the better performance of Muslim leaders in the control group—minimum group effort in the contact treatment is almost the same across Muslim and Hindu leaders (see Figures 1B and 1C).

This result in the context of our framework, corresponds to an increase in both a_H and a_M , and a particularly large increase in a_H . In other words, both Hindus and Muslims are more responsive to leader identity after intergroup contact, but the effect is stronger for Hindu individuals. Examining potential mechanisms and in particular individual effort again, we find no significant increase in individual effort levels under Muslim leaders, but a strong and significant increase in individual effort for both Hindus and Muslims under Hindu leaders (Appendix Table A9, columns 5-8). Our results suggest that expectation of change in Hindu behavior after contact with Muslims is higher than the expectations of change in Muslim behavior after contact with Hindus. This asymmetry probably arises because Muslims are a minority community overall, and hence interactions with Hindus may be more common for them than the reverse. The mechanism our results indicate—namely the increase in identity-based response—is different from that emphasized in previous research on intergroup contact which has focused on whether such contact can reduce prejudice or affect attitudes such as pro-sociality, trust or egalitarianism (Rao, 2019, Finseraas et al. 2020, Paluck et al., 2018).

As before, we verify that leader proposals are not driving our results. We again find no significant differences in leader proposals under intergroup contact compared to the control group (Appendix Table A6, column 2). The coefficients of interest are larger in magnitude and statistically significant after controlling for leader proposals (Table 3, columns 3 and 4). In fact, once we control for leader proposals, the improvement in coordination in Muslim-led groups becomes statistically significant.¹⁵

7.4. Does Conflict History Matter?

The results discussed so far average across areas with different histories of Hindu-Muslim conflict. In this section, we examine how the relationship between coordination outcomes and leader identity varies in the control vs treatment arms with whether the district had high or low exposure to intergroup conflict over the period 1980-2010. This is pertinent since such policies may be implemented to ameliorate historical grievances. The aim of this exercise is not to compare outcomes in high and low conflict areas directly, since many other observable and unobservable characteristics may differ across high conflict and low conflict areas. Instead, we take advantage of the fact that we randomly allocated towns to different treatments within districts characterized by high vs low conflict and we compare behavior across these randomized treatments *within* each conflict setting. Thus, we do not aim to identify the causal impact of conflict history, but rather the casual impact of the two policies under Muslim vs Hindu leaders across areas with different conflict histories.

We find a consistent pattern of results that suggest a lower responsiveness of Muslims to leader identity in high conflict areas, and a higher responsiveness of Hindus to leader identity in those same areas (Figure 3). In particular, we see that the increases in minimum group effort under Muslim leaders are uniformly higher in low conflict areas for all the three policy environments (see Table 4, columns

¹⁵ The gender of an individual's puzzle partner may also be important. We re-estimate equation (4) for the contact group participants, interacting leader with female puzzle partner. We find that participants with female puzzle partners respond less to the introduction of a leader, but the difference is not statistically significant (see Appendix Table C2).

1 and 2). In particular, this means that our earlier finding that Muslim leaders improve coordination in the control and contact treatments emerges mostly from low conflict areas (column 1), and there is a significant decline in coordination in the AA arm in high conflict areas (column 2). This is consistent with a_M being uniformly lower in high-conflict areas. One reason for this may be that Muslims are less willing to exercise their religious preferences precisely because of the history of religious conflict, in which existing evidence suggests that they are usually the victims (Mitra and Ray, 2014).

The impact of Hindu leaders on coordination is less sensitive to the history of conflict in the district, with the exception that coordination improvements in the contact treatment arm are in fact higher in high conflict areas (Table 4, columns 3 and 4). This is consistent with higher a_H , potentially because of behavioral type Hindus reactions to leader identity being primed by intergroup contact in areas where religious tensions are stronger. Our earlier findings that Hindu leaders do not improve coordination in the control arm, but do improve it under both the AA and the contact treatments, holds in high and low conflict areas. All of these results are robust to controlling for leaders' proposals, and therefore they are primarily driven by citizen reactions to leader identity (see Appendix Table C3). Overall, our results suggest that the prior history of intergroup conflict matters for the effectiveness of leaders as well as for the effectiveness of policies. Of particular policy relevance is the result that AA, which often aims to increase leadership roles for minorities, may in fact be detrimental for minority leader effectiveness.

8. Conclusions

We provide the first investigation of how leader effectiveness in achieving economic coordination in diverse societies varies with leader identity. We implemented a lab-in-field experiment in India's largest state, where we randomly assigned towns to two policy treatments and a control group. The random assignment was stratified by district, to allow us to compare the impact of policy treatments across districts with a history of high vs low intergroup conflict.

We find that minority leaders improve coordination (measured as minimum group effort), but majority leaders do not. This is primarily the result of citizen reactions to leader identity, rather than differences in leader actions. The mechanism driving citizen reactions appears to be higher expectations of responsiveness to leader identity among members of the minority group. In contrast to the greater responsiveness of Muslims to leader identity in the control group, we find that identity-based responsiveness to leaders appears to increase for members of the majority group in the presence of policies designed to improve social integration of the minority group. In fact, the control group results are reversed under affirmative action that is perceived to assign leadership posts to Muslims, with coordination becoming higher under Hindu leaders. A policy of intergroup contact leads to higher coordination under all leaders, but the gains are larger under Hindu leaders.

We find that social integration policies enable minority leaders to be more effective in low conflict areas. Specifically, in low conflict areas and *only* in low conflict areas, Muslim leaders serve to improve coordination outcomes in each of the three experimental arms. The effectiveness of Hindu leaders remains evident in the AA and contract arms but does not vary significantly by conflict history.

Pulled together, our findings provide compelling evidence that social identity influences behavior in a way that leads to different economic choices than would emerge from maximization of individual economic payoffs. Our particular contribution is to demonstrate this in the context of leadership. We show that citizen reactions to leader identity influence coordination outcomes. In general, we find that the minority group shows stronger responsiveness to leader identity, except in areas with a history of intergroup conflict. However, policies designed to integrate minorities tend to activate social identity responses in the majority group in ways that can potentially reduce the effectiveness of these policies.

Our findings contribute novel evidence to research on leader identity, coordination failure, social integration policies and religious conflict, bridging key aspects of these domains of research. They provide unique evidence on the role of citizen reactions to leader identity in heterogeneous

communities using a non-student sample. They further provide useful guidance for policy, identifying conditions under which leaders of minority vs majority groups may be constrained in resolving coordination problems.

Our results suggest many directions for future research in field settings. These include allowing leaders to use unrestricted communications rather than specific proposals (as in Brandts et al., 2016), examining whether raising the monetary stakes or changing group sizes affects leaders' effectiveness, and whether our results for Hindus and Muslims in India generalize to other social majority or minority groups. Some recent research suggests that social identity itself may be amenable to policy initiatives (Miguel, 2004; Blouin and Mukand, 2019). While this is beyond the scope of our paper, it can also be a fruitful direction for future research.

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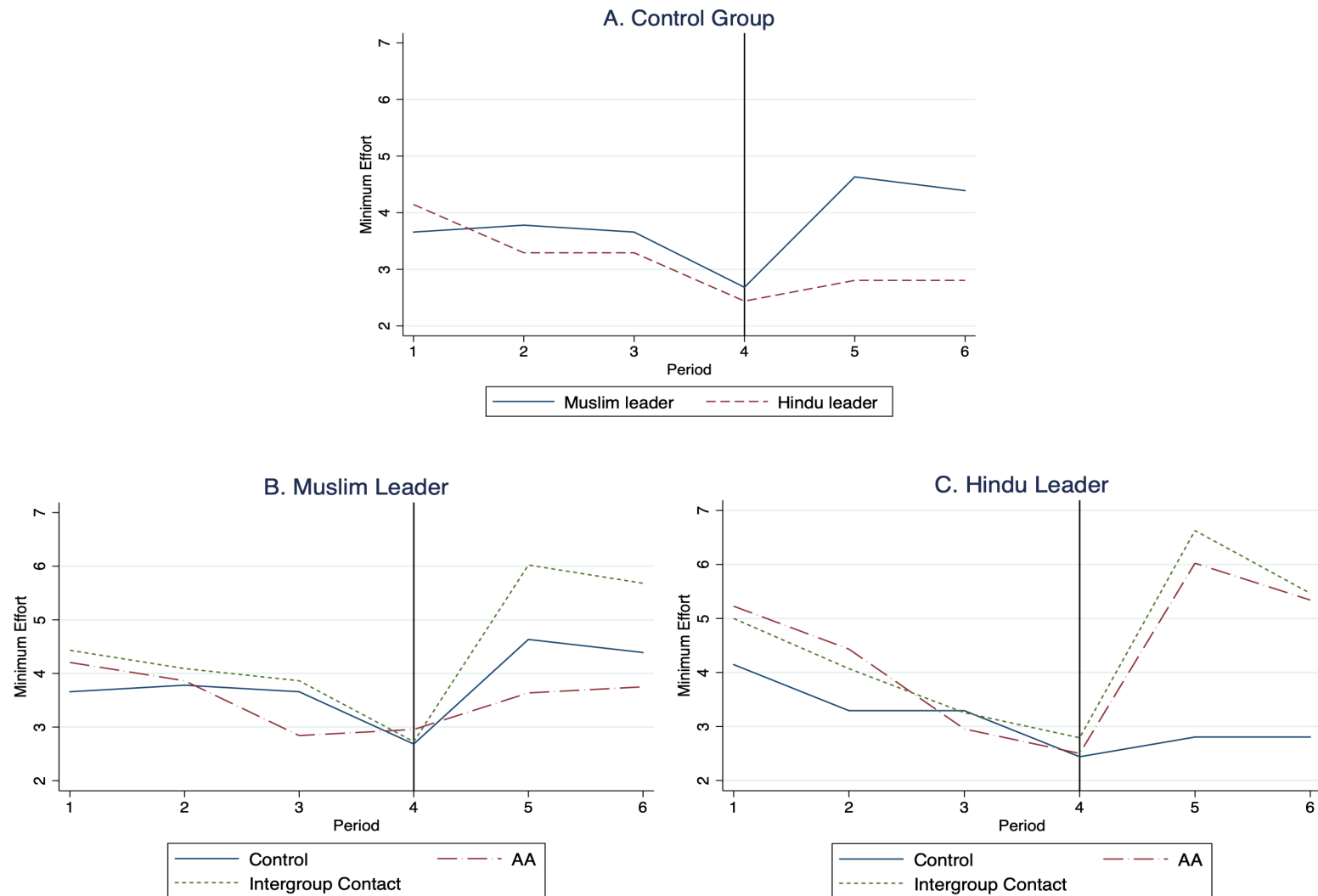
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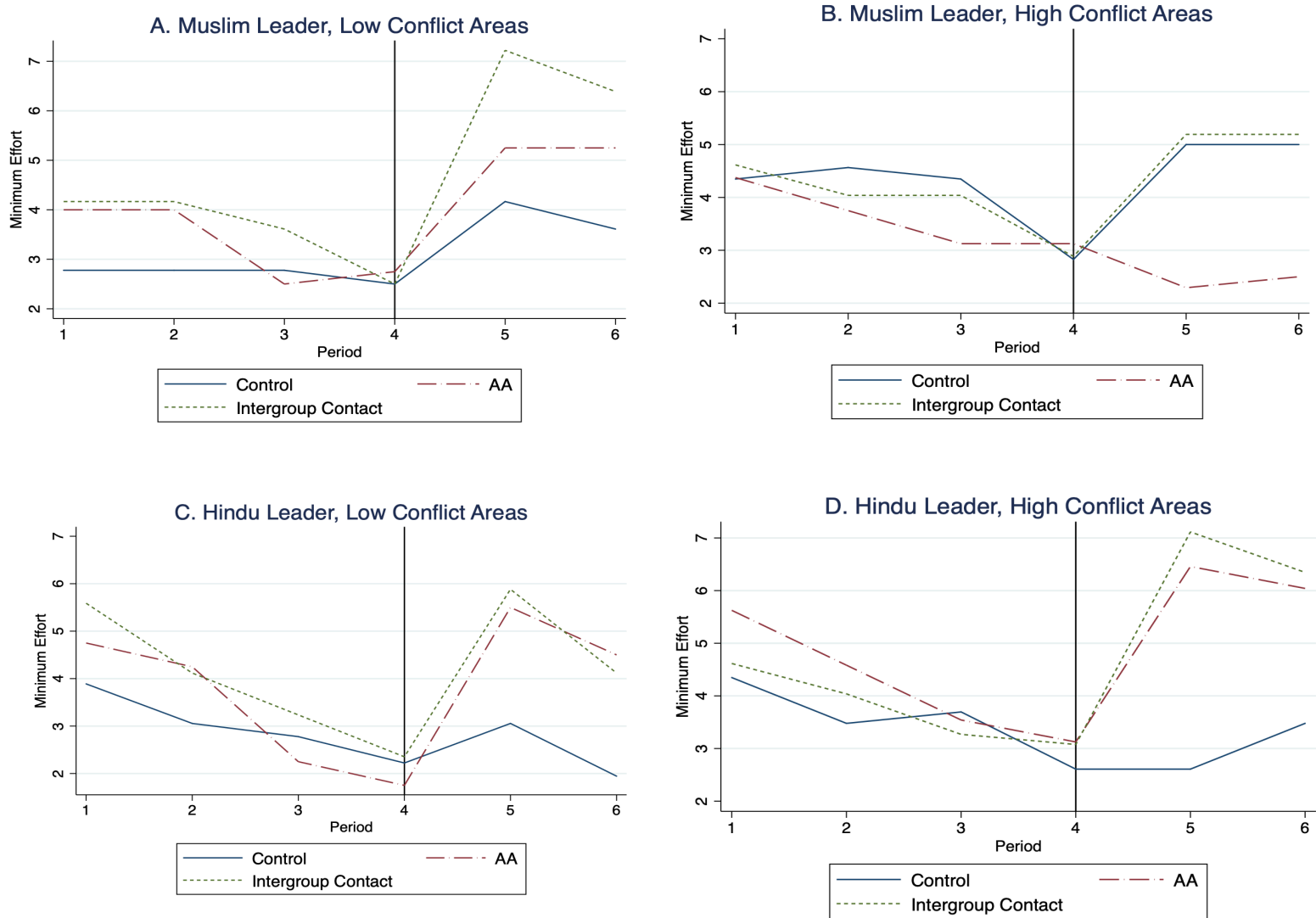
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Figure 1
Leader Identity and Minimum Group Effort in Different Policy Environments



The figure shows the average minimum effort for groups with Muslim leaders and groups with Hindu leaders. Periods 1-4 are prior to leader assignment, periods 5 and 6 show outcomes after leader identity and leader proposals are revealed to participants.

Figure 2
Leader Effectiveness Across Policy Environments and Conflict Histories



The figure shows the average minimum effort for groups with Muslim leaders and groups with Hindu leaders. Periods 1-4 are prior to leader assignment, periods 5 and 6 show outcomes after leader identity and leader proposals are revealed to participants.

Table 1
Leader Identity and Minimum Effort (Control Group)

Dependent variable: Minimum Effort in the Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Muslim Leaders	Hindu Leaders	Muslim Leaders	Hindu Leaders	All Leaders	All Leaders
Leader (Period>4)	1.067 (0.494)	-0.488 (0.381)	1.067 (0.508)	-0.488 (0.392)	-0.488 (0.379)	
Muslim Leader * (Period>4)					1.555 (0.620)	
Muslim Leader					-0.492 (0.616)	1.272 (0.618)
Observations	246	246	246	246	492	164
R-squared	0.281	0.258	0.477	0.435	0.309	0.536
Town FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes	Yes
Religious Controls	No	No	Yes	Yes	Yes	Yes
Experimental Controls	No	No	No	No	No	Yes

Standard errors in parantheses, clustered at group level. Demographic controls include gender, age, education and monthly household income; religious controls include dummies for whether the participant prays several times a day or once a day; experimental controls include the leader's proposal and the group minimum effort in period 4. Columns 1-5 include data from all periods; Column 6 is restricted to periods 5 and 6 only.

Table 2
Leader Identity and Individual Effort (Control Group)

	(1)	(2)	(3)	(4)
	<i>Individual Effort</i>			
	Hindu Leaders		Muslim Leaders	
	Muslim Employees	Hindu Employees	Muslim Employees	Hindu Employees
Leader (Period>4)	0.015 (0.517)	-1.156 (0.408)	1.157 (0.434)	0.377 (0.531)
p-value (M employee = H employee)	[0.093]		[0.281]	
Observations	486	480	486	498
R-squared	0.242	0.280	0.309	0.278
Town FE	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
Religious Controls	Yes	Yes	Yes	Yes

Standard errors in parantheses, clustered at group level. Demographic controls include gender, age, education and monthly household income; religious controls include dummies for whether the participant prays several times a day or once a day. Data includes effort choices of both leaders and employees in columns (1)-(4).

Table 3
Policy Environments and Leader Effectiveness

Dependent variable: Minimum Effort in the Group

	(1) Muslim Leaders	(2) Hindu Leaders	(3) Muslim Leaders	(4) Hindu Leaders
Leader (Period>4) β_1	1.067 (0.495)	-0.488 (0.382)		
Leader (Period>4)*AA β_2	-0.840 (0.814)	2.391 (0.671)	-1.625 (0.837)	2.370 (0.627)
Leader (Period>4)*Contact β_3	1.007 (0.752)	2.755 (0.651)	1.738 (0.801)	2.850 (0.676)
p-value for $\beta_2 = \beta_3$	0.034	0.634	0.000	0.476
Observations	774	768	258	256
R-squared	0.258	0.340	0.365	0.399
Town FE	Yes	Yes	No	No
Demographic Controls	Yes	Yes	Yes	Yes
Religious Controls	Yes	Yes	Yes	Yes
Experimental Controls	No	No	Yes	Yes

Standard errors in parantheses, clustered at group level. Demographic controls include gender, age, education and monthly household income; religious controls include dummies for whether the participant prays several times a day or once a day; experimental controls include the leader's proposal and the group minimum effort in period 4. Columns 1 and 2 include data from all periods; Columns 3 and 4 are restricted to periods 5 and 6 only. Leader dummy is always equal to one in columns 3 and 4.

Table 4**Does a History of Conflict Matter for Leader Effectiveness across Policy Environments?***Dependent variable: Minimum Effort in the Group*

	(1)	(2)	(3)	(4)
	Muslim Leaders	Muslim Leaders	Hindu Leaders	Hindu Leaders
	Low Conflict Areas	High Conflict Areas	Low Conflict Areas	High Conflict Areas
Leader (Period>4)	1.181 (0.577)	0.978 (0.770)	-0.486 (0.555)	-0.489 (0.537)
Leader (Period>4)*AA	0.757 (1.194)	-2.176 (1.036)	2.236 (1.020)	2.520 (0.912)
Leader (Period>4)*Contact	2.014 (1.100)	0.320 (1.027)	1.663 (0.780)	3.470 (0.939)
Observations	336	438	330	438
R-squared	0.417	0.282	0.401	0.328
Town FE	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
Religious Controls	Yes	Yes	Yes	Yes

Standard errors in parantheses, clustered at group level. Demographic controls include gender, age, education and monthly household income; religious controls include dummies for whether the participant prays several times a day or once a day.