DISCUSSION PAPER SERIES

DP16158

Leader identity and coordination

Sonia Bhalotra, Joseph Vecci, Lakshmi Iyer and Irma Clots Figueras

DEVELOPMENT ECONOMICS

CEPR

ISSN 0265-8003

Leader identity and coordination

Sonia Bhalotra, Joseph Vecci, Lakshmi Iyer and Irma Clots Figueras

Discussion Paper DP16158
Published 16 May 2021
Submitted 14 May 2021

Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

This Discussion Paper is issued under the auspices of the Centre's research programmes:

- Development Economics

Leader identity and coordination

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 Classification: N/A

Keywords: N/A

Sonia Bhalotra - sonia.bhalotra@warwick.ac.uk
University of Warwick and CEPR

Joseph Vecci - joseph.vecci@economics.gu.se
University of Gothenburg

Lakshmi Iyer - liyer@nd.edu
University of Notre Dame

Irma Clots Figueras - i.clots-figueras@kent.ac.uk
University of Kent

Leader Identity and Coordination*

Sonia Bhalotra, University of Essex
Irma Clots-Figueras, University of Kent
Lakshmi Iyer, University of Notre Dame
Joseph Vecci, University of Gothenburg

November 12, 2020

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

---

* Email addresses: srbhal@essex.ac.uk; I.Clots-Figueras@kent.ac.uk; liyer@nd.edu; joseph.vecci@economics.gu.se. We are immensely grateful to Aditya Kuvalekar for his help, insights, and guidance. We acknowledge financial support from the International Growth Center, ESRC Grant ES/L009153/1 awarded to the Research Centre for Micro-Social Change at ISER, University of Essex (Bhalotra), the Ramon y Cajal Fellowship (Clots-Figueras), the University of Notre Dame (Iyer) and the Swedish Research Council Project no. 348-2014-4030 (Vecci). We thank Debayan Pakrashi and the Indian Institute of Technology (Kanpur) for their hospitality and logistical support. Nidhila Adusumalli, Kunal Dixit, Gunjita Gupta, Ashwin Nair and Aradhana Singh provided exceptional field supervision. We thank three anonymous referees, Amrit Amirapu, Jordi Brandts, Antonio Cabrales, Utteeyo Dasgupta, Inge van den Bijgaart, Saumitra Jha, Anandi Mani, Eva Ranehill, Farah Said, Zaki Waahaj, and seminar and conference participants at several universities for extremely useful comments. This study is registered in the AEA RCT Registry and the unique identifying number is "AEARCTR-0002353."

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 was 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 identity 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:

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

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 ≈ ₹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 $\tilde{a} \coloneqq pa_M + (1-p)a_H$, 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:

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

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

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:

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

where $MinGroupEffort_{kjt}$ 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:

$$(5)\quad IndividualEffort_{ikjt} = a + bLeader_{kjt} + G'_{ikj}g + w_{ikjt};\ t = 1,2,\ldots,6$$

where $IndividualEffort_{ikjt}$ 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) $MinGroupEffort_{kjt} = \beta_0 + \beta_1 Leader_{kjt} + \beta_2 Leader_{kjt} * AA_j + \beta_3 Leader_{kjt} * Contact_j + X'_{kj}\gamma + \varepsilon_{kjt}$; $t = 1,2,\ldots,6$

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:

$$(7)\ IndividualEffort_{ikjt} = f_0 + f_1 Leader_{kjt} + f_2 Leader_{kjt} * AA_j + f_3 Leader_{kjt} * Contact_j + X'_{ikj} g + z_{ikjt} \quad ; t = 1,2,\ldots,6$$

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 Leader*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 counter-balances 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.

References

Adams, R. B. & Ferreira, D. (2009). Women in the Boardroom and Their Impact on Governance and Performance. *Journal of Financial Economics*, 94(2): 291-309.

Adda, J. (2016). Economic Activity and the Spread of Viral Diseases: Evidence from High Frequency Data. *Quarterly Journal of Economics*, 131(2): 891-941.

Afridi, F., Dhillon, A., Li, S.X. & Sharma, S. (2020). Using Social Connections and Financial Incentives to Solve Coordination Failure: A Quasi-Field Experiment in India's Manufacturing Sector. *Journal of Development Economics*, 144.

Ahern, K.R., & A. Dittmar. (2012). The Changing of the Boards: The Impact on Firm Valuation of Mandated Female Board Representation. *Quarterly Journal of Economics* 127:1:137–197.

Akerlof, G. & Kranton, R.(2000). Economics & Identity. *Quarterly Journal of Economics*, 105(3): 715–53.

Allport, G. (1954). *The Nature of Prejudice*. New York, NY: Basic Books.

Ashraf, N. & Bandiera, O. (2017). Social Incentives in Organizations. Working Paper.

Bagues, M., Sylos-Labini, M. & Zinovyeva, N. (2017). Does the Gender Composition of Scientific Committees Matter? *American Economic Review*, 107(4): 1207-1238.

Balafoutas, L. & Sutter, M. (2012). Affirmative Action Policies Promote Women and Do Not Harm Efficiency in the Lab. *Science*, 335: 579-582.

Barnhardt, S. (2009). Near and Dear? Evaluating the Impact of Neighbor Diversity on Interreligious Attitudes. WP.

Bauer, M., C. Blattman, J. Chytilová, J. Henrich, E. Miguel, & T. Mitts. (2016). Can War Foster Cooperation? *Journal of Economic Perspectives,* 30(3): 249–74.

Beaman, L., Chattopadhyay R., Duflo E., Pande R. & Topalova P. (2009). Powerful Women: Does Exposure Reduce Bias? *Quarterly Journal of Economics,* 124(4): 1497-1540.

Beaman, L., Chattopadhyay R., Duflo E., Pande R. & Topalova P. (2012). Female Leadership Raises Aspirations and Educational Attainment for Girls: A Policy Experiment in India. *Science*, 335(6068): 582-586.

Benjamin, D.J., Choi, J.J. & Fisher, G. (2016). Religious Identity and Economic Behavior. *Review of Economics and Statistics*, 98(4): 617–637.

Berge, L, Bjorvatn, K., Galle, S., Miguel, E., Posner, D., Tungodden, B. & Zhang, K. (2019). Ethnically Biased? Experimental Evidence from Kenya. *Journal of the European Economic Association*, 17: 1-31.

Bertrand, M. & Duflo, E. (2017). Field Experiments on Discrimination. *Handbook of Economic Field Experiments*, 1:309–393.

Besley, T. & Coate, S. (1997), An Economic Model of Representative Democracy, *The Quarterly Journal of Economics*, 112(1): 85-114.

Bhalotra, S., & Clots-Figueras, I. (2014). Health and the Political Agency of Women. *American Economic Journal: Economic Policy*, 6 (2): 164-97.

Bhalotra, S., Clots-Figueras, I. & Iyer, L. (2019). Religion and Abortion: The Role of Politician Identity. IZA Working paper.

Bhalotra, S, Cassan, G, Clots-Figueras, I & Iyer, L (2014). Religion, Politician Identity & Development Outcomes: Evidence from India. *Journal of Economic Behavior and Organization*, 104: 4–17.

Bhavnani, R. (2017). Do the Effects of Temporary Ethnic Group Quotas Persist? Evidence from India. *American Economic Journal: Applied Economics*, 9(3): 105-23.

Bhavnani, R., Donnay, K., Miodownik, D., Mor, M. & Helbing, D. (2014). Group Segregation and Urban Violence. *American Journal of Political Science*, 58: 226-245.

Bisin, A., & T. Verdier (2001). The Economics of Cultural Transmission and the Dynamics of Preferences, *Journal of Economic Theory*, 97: 298 – 319.

Blattman, C., & Miguel, E. (2010). Civil War. *Journal of Economic Literature*, 48 (1): 3-57.

Blouin, A. & Mukand, S. W. (2019). Erasing Ethnicity? Propaganda, Nation Building, and Identity in Rwanda. *Journal of Political Economy,* 27(3): 1008-1062.

Bowles, S. & Halliday, S. (2020). *Microeconomics: Competition, Cooperation & Conflict*. Oxford University

Brandts, J, & Cooper, D (2006). A Change Would Do You Good... An Experimental Study on How to Overcome Coordination Failure in Organizations. *American Economic Review*, 96(3): 669-693.

Brandts, J., & Cooper, D. J. (2007). It's What You Say, Not What You Pay: An Experimental Study of Manager-Employee Relationships in Overcoming Coordination Failure. *Journal of the European Economic Association*, 5(6): 1223-1268.

Brandts, J., Cooper, D. J., & Fatas, E. (2007). Leadership and Overcoming Coordination Failure with Asymmetric Costs. *Experimental Economics*, 10(3): 269-284.

Brandts, J., Cooper, D. J., & Weber, R. A. (2015). Legitimacy, Communication, and Leadership in the Turnaround Game. *Management Science*, 61(11): 2627-2645.

Brandts, J., Rott, C. & Solà, C. (2016). Not Just Like Starting Over - Leadership and Revivification of Cooperation in Groups. *Experimental Economics*, 19: 792–818.

Brollo, F. & Troiano, U. (2016). What happens when a woman wins an election? Evidence from close races in Brazil. *Journal of Development Economics*, 122: 28-45.

Brooks, B., K. Hoff & P. Pandey (2018). Cultural impediments to learning to cooperate: An experimental study of high- and low-caste men in rural India. *Proceedings of the National Academy of Sciences*, 115 (45): 11385-11392.

Burgess, R., Jedwab, R., Miguel, E., Morjaria, A. & Padró i Miquel, G. (2015). The value of democracy: evidence from road building in Kenya. *American Economic Review*, 105(6): 1817-1851.

Carlsson, H & van Damme, E (1993). Global Games & Equilibrium Selection. *Econometrica* 61: 989 1018.

Cartwright, E., J. Gillet & M. Van Vugt. (2013). Leadership by Example in the Weak-Link Game. *Economic Inquiry*.

Chattopadhyay, R. & Duflo, E. (2004). Women as policymakers : evidence from a randomized policy experiment in India. *Econometrica,* 72(5):1409–1443.

Chen, R., & Chen, Y. (2011) The Potential of Social Identity for Equilibrium Selection, *American Economic Review,* 101, 2562– 2589.

Chin, A. & N. Prakash. (2011). The Redistributive Effects of Political Reservation for Minorities: Evidence from India. *Journal of Development Economics*, 96 (2): 265-277.

Cohn, A., Marechal, M. & Noll, T. (2015). Bad Boys: How Criminal Identity Salience Affects Rule Violation. *Review of Economic Studies,* 82: 1289–1308.

Coleman, J. S. (1987). Norms as social capital. In G. Radnitzky & P. Bernholz (Eds.), *Economic Imperialism* (pp. 133-155). New York: Paragon

Corno, L., E. La Ferrara & J. Burns. (2018). Interaction, stereotypes and performance: Evidence from South Africa. Working paper.

Devetag, G. & Ortmann, A. (2007). When and why? A critical survey on coordination failure in the laboratory. *Experimental Economics*, 10: 331-344.

Finseraas, H., Hanson, T., Johnsen, A. A., Kotsadam, A. and Torsvik, G. (2020). Trust, Ethnic Diversity, and Personal Contact: A Field Experiment. Working paper.

Fisman, R., Sarkar, A., Skrastins, J., & Vig, V (2020). Experience of Communal Conflicts and Intergroup Lending. *Journal of Political Economy,* 128(9): 3346-3375.

Gächter, S., Nosenzo, D., Renner, E., & Sefton, M. (2012). Who makes a good leader? Cooperativeness, optimism, and leading-by-example. *Economic Inquiry* 50: 953-967.

Gangadharan, L., T. Jain, P. Maitra, & J. Vecci (2016). Social Identity and Governance: The Behavioral Response to Female Leaders. *European Economic Review*, 90: 302–25.

Government of India (2014). Status of Reservation of OBC in Various States. Ministry of Social Justice and Empowerment. GOI. http://pib.nic.in/newsite/PrintRelease.aspx?relid=108754

Gupta, G, Mahmud, M., Maitra, P., Mitra, S., & Neelim, A. (2018). Religion, Minority Status and Trust: Evidence from a Field Experiment. *Journal of Economic Behavior and Organization*, 146: 180 - 205.

Güth, W., M. V. Levati, M. Sutter, & E. Van Der Heijden. (2007). Leading by Example with and without Exclusion Power in Voluntary Contribution Experiments. *Journal of Public Economics*, 91: 1023–42.

Heath, O., Verniers, G. & Kumar, S. (2015). Do Muslim voters prefer Muslim candidates? Co-religiosity and voting behaviour in India. *Electoral Studies* 38: 10-18.

Heursen, L., Ranehill, E. & Weber, R. (2019). Are women less effective leaders than men? Evidence from experiments using coordination games. Working paper.

Hoff, K. (2000). Beyond Rosenstein-Rodan: the modern theory of coordination problems in development. *Proceedings of the Annual World Bank Conference on Development Economics*, 145-188.

Hoff, K., & Stiglitz, J. (2001). Modern economic theory and development. In G. M. Meier & J. E. Stiglitz (Eds.), *Frontiers of development economics: The future in perspective*. New York: OUP.

Hogg, M.A. (2001). A social identity theory of leadership. *Pers Soc Psychol Rev,* 5: 184–200

Hungerman, D M., (2014). Do Religious Proscriptions Matter? Evidence from a Theory-Based Test, *Journal of Human Resources*, 49 (2014), 1053–1093.

Ip, E., Leibbrandt, A., & Vecci, J., (2018). How Do Gender Quotas Affect Hierarchical Relationships? Complementary Evidence from a Representative Survey and Labor Market Experiments. *Management Science*, forthcoming.

Iyer, L., A. Mani, P. Mishra, & P. Topalova (2012). The power of political voice: Women's political representation and crime in India. *American Economic Journal: Applied Economics,* 4(4): 165–193.

Jack, K. & Recalde, M. (2015). Leadership and the voluntary provision of public goods: Field evidence from Bolivia. *Journal of Public Economics* 122: 80-93.

Jaffrelot, C., Verniers, G. & Das, S. (2019). The Dwindling Presence of Religious and Other Minorities from India's Public Sphere. Work in progress.

Kaysser, N., Bhalotra S., Clots-Figueras I., & Iyer L. (2015). Hindu-Muslim violence in India 1950-2010: an update of the Varshney-Wilkinson dataset on Hindu-Muslim violence in India. Mimeo

Kremer, M. (1993). The O-Ring Theory of Economic Development. *Quarterly Journal of Economics*, 108(3): 551-575.

Krupka, E., & Weber, R., (2013). Identifying social norms using coordination games: why does dictator games haring vary? *Journal of the European Economic Association*. 11(3),495–524.

Leibbrandt, A., Wang, L.C., Foo, C., (forthcoming). Gender Quotas, Competitions, and Peer Review: Experimental Evidence on the Backlash Against Women. *Management Science.*

Levy, D.M., Padgitt, K., Peart, S.J., Houser, D. & Xiao, E. (2011). Leadership, cheap talk and really cheap talk. *Journal of Economic Behavior and Organization*, 77(1): 40-52.

Lowe, M. (2017). Types of contact: A field experiment on collaborative and adversarial caste integration. *Working Paper.*

Matsa, D. A., & Miller, A. R. (2013). A female style in corporate leadership? Evidence from quotas. *American Economic Journal: Applied Economics*, *5*(3): 136-69.

Miguel, E. & Roland, G. (2011). The Long-Run Impact of Bombing Vietnam. *Journal of Development Economics*, 96: 1-15.

Miguel, E. (2004). Tribe or Nation?: Nation Building and Public Goods in Kenya versus Tanzania. *World Politics* 56(3): 327–62.

Mitra, A. & D. Ray. (2014). Implications of an Economic Theory of Conflict: Hindu-Muslim Violence in India. *Journal of Political Economy*, 122 (4): 719–65.

Morris, S. & H.S. Shin. (1998). Unique equilibrium in a model of self-fulfilling currency attacks. *American Economic Review*, 88: 587–597.

Morris, S., H.S. Shin, & M. Yildiz. (2016). Common Belief Foundations of Global Games. *Journal of Economic Theory* 163: 826-848.

Niederle, M., Segal, C. & Vesterlund, L. (2013). How costly is diversity? Affirmative action in light of gender differences in competitiveness. *Management Science*, 59(1): 1-16.

NITI Aayog (2016). Eliminating Poverty: Creating Jobs & Strengthening Social Programs. Occasional Paper 2. http://niti.gov.in/writereaddata/files/OccasionalPaper_No2_Poverty.pdf.

Nordhaus, W. D. (2006). Paul Samuelson and global public goods. In Szenberg, M., Ramrattan, L., and Gottesman, A., editors, *Samuelsonian Economics*, pp 88–98. Oxford: OUP UK.

Osborne, M. & Slivinski, A. (1996). A Model of Political Competition with Citizen-Candidates, *The Quarterly Journal of Economics*, 111(1): 65-96

Paluck, E, Green, S & Green, D (2018). Contact Hypothesis Revisited. *Behavioural Public Policy,* 1-30.

Pande, R. (2003). Can Mandated Political Representation Provide Disadvantaged Minorities Policy Influence? Theory and Evidence from India. *American Economic Review*, 93(4): 1132-1151.

Pettigrew, T. F. & Tropp, L. R. (2006). A meta-analytic test of intergroup contact theory. *Journal of Personality and Social Psychology*, 90(5):751.

Polania-Reyes, S. & Echeverry, D. (2020). Coordination as unintended benefit: Lab-in-the-field evidence from a conditional cash transfer program. Working Paper.

Rao, G. (2019). Familiarity Does Not Breed Contempt: Diversity, Discrimination and Generosity in Delhi Schools. *American Economic Review*, 109(3).

Sandler, T. (1998). Global and regional public goods: A prognosis for collective action. *Fiscal Studies*, 19(3): 221–247.

Rodrik, D. (1999). Where Did All the Growth Go? External Shocks, Social Conflict, and Growth Collapses. *Journal of Economic Growth* 4: 385–412.

Scacco, A. & S. Warren (2018). Can Social Contact Reduce Prejudice and Discrimination? Evidence from a Field Experiment in Nigeria. *American Political Science Review* 1-24.

Schelling, T. C. (1980). *The Strategy of Conflict*. Harvard University Press.

Shih, M., Pittinsky, T. L., & Ambady, N. (1999). Stereotype Susceptibility: Identity Salience and Shifts in Quantitative Performance. *Psychological Science*, 10(1), 80–83.

Steele C. M. & Aronson, J. (1995). Stereotype threat and the intellectual test performance of African Americans. *Journal of Personality and Social Psychology*, 69(5): 797–811.

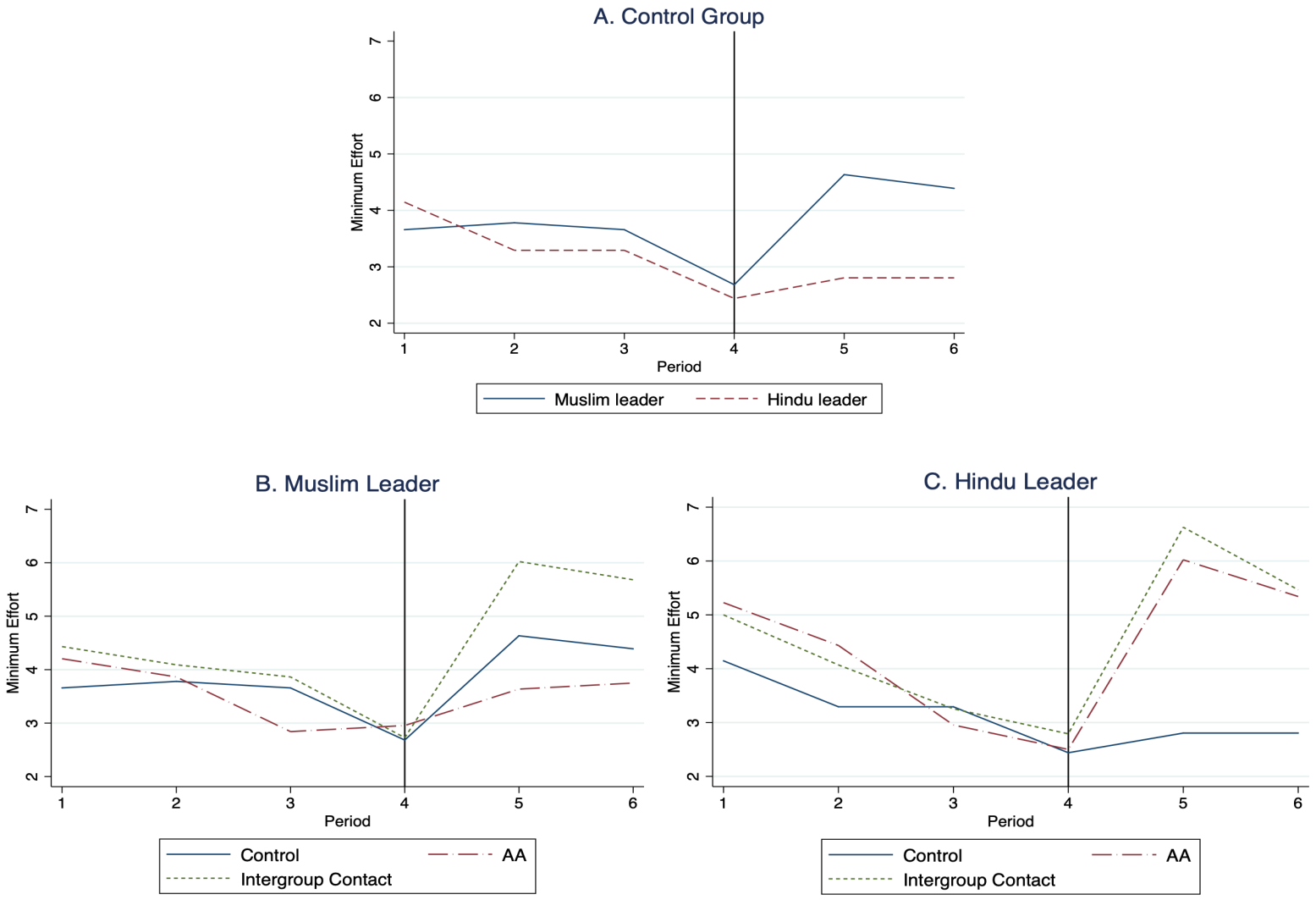
The Economist (2016). India's Muslims: An uncertain community, October 29. Available: https://www.economist.com/asia/2016/10/29/an-uncertain-community

Van Huyck, J. B., R. C. Battalio, & R. O. Beil (1990). Tacit Coordination Games, Strategic Uncertainty, and Coordination Failure. *American Economic Review*, 80: 234-248.

Weber, R., Camerer, C., Rottenstreich, Y. & Knez, M. (2001). The Illusion of Leadership: Misattribution of Cause in Coordination Games. *Organization Science*.

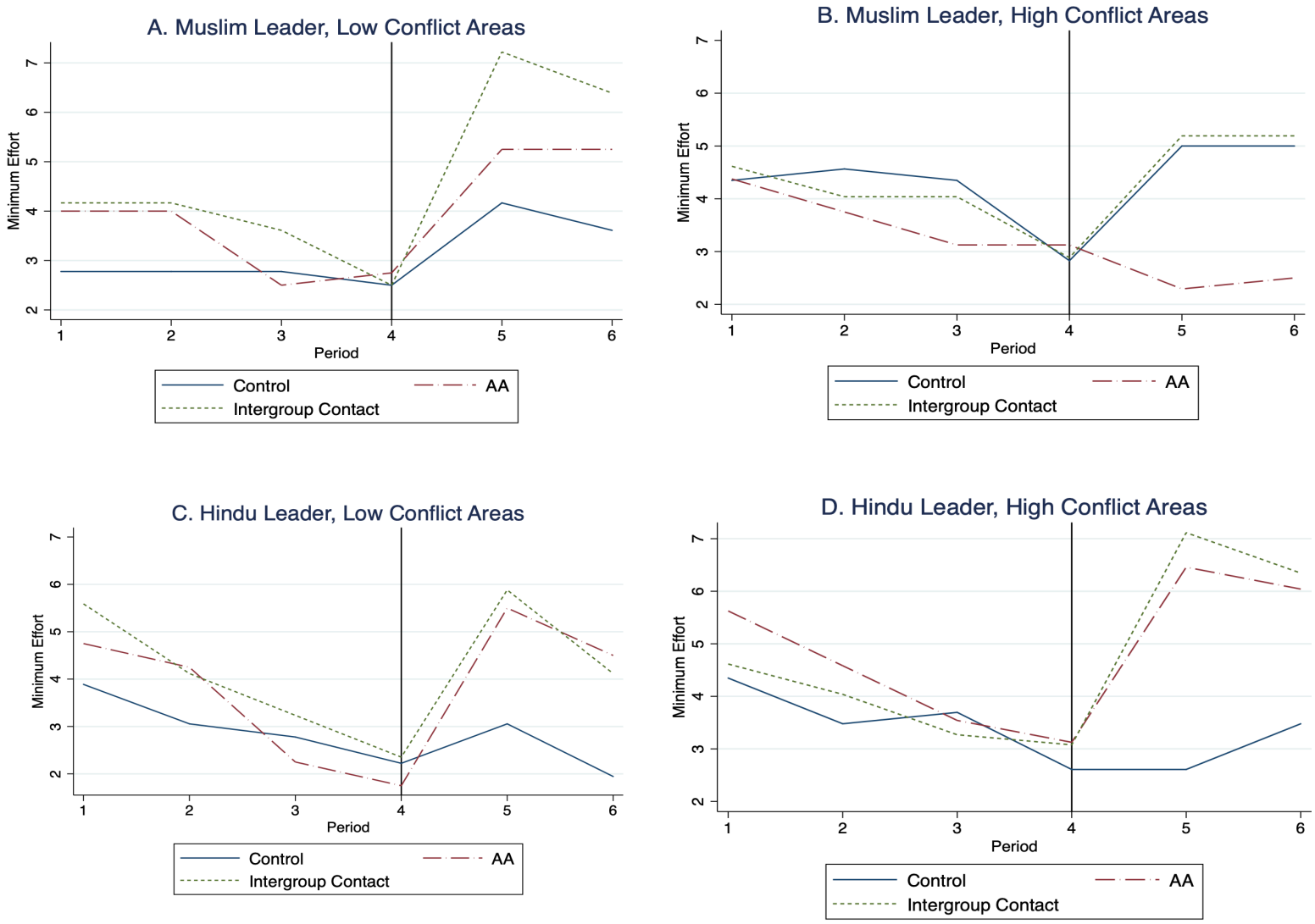
Wilkinson, S. (2004). *Votes and Violence: Electoral Competition and Ethnic Riots in India*. Cambridge: CUP.

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) Muslim Leaders	(2) Hindu Leaders	(3) Muslim Leaders	(4) Hindu Leaders	(5) All Leaders	(6) All Leaders
Leader (Period>4)	1.067	-0.488	1.067	-0.488	-0.488	
	(0.494)	(0.381)	(0.508)	(0.392)	(0.379)	
Muslim Leader * (Period>4)					1.555	
					(0.620)	
Muslim Leader					-0.492	1.272
					(0.616)	(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)
	Individual Effort			
	Hindu Leaders		Muslim Leaders	
	Muslim Employees	Hindu Employees	Muslim Employees	Hindu Employees
Leader (Period>4)	0.015	-1.156	1.157	0.377
	(0.517)	(0.408)	(0.434)	(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	-0.488		
	(0.495)	(0.382)		
Leader (Period>4)*AA β_2	-0.840	2.391	-1.625	2.370
	(0.814)	(0.671)	(0.837)	(0.627)
Leader (Period>4)*Contact β_3	1.007	2.755	1.738	2.850
	(0.752)	(0.651)	(0.801)	(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) Muslim Leaders Low Conflict Areas	(2) Muslim Leaders High Conflict Areas	(3) Hindu Leaders Low Conflict Areas	(4) Hindu Leaders High Conflict Areas
Leader (Period>4)	1.181	0.978	-0.486	-0.489
	(0.577)	(0.770)	(0.555)	(0.537)
Leader (Period>4)*AA	0.757	-2.176	2.236	2.520
	(1.194)	(1.036)	(1.020)	(0.912)
Leader (Period>4)*Contact	2.014	0.320	1.663	3.470
	(1.100)	(1.027)	(0.780)	(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.

Leader Identity and Coordination

Sonia Bhalotra
University of Essex

Irma Clots-Figueras
University of Kent

Lakshmi Iyer
University of Notre Dame

Joseph Vecci
University of Gothenburg

Appendix A: Tables and Figures

Table A1
Number of Sites and Respondents Across Different Treatment Arms

	Control	Contact	Affirmative Action	Total
Total				
# sites	14	15	15	44
# groups	82	87	88	257
# respondents	*328*	*348*	*352*	*1028*
Low-conflict districts				
# sites	6	6	7	19
# groups	36	35	40	114
# respondents	*144*	*140*	*160*	*456*
High-conflict districts				
# sites	8	9	8	25
# groups	46	52	48	150
# respondents	*184*	*208*	*192*	*600*

Table A2
Demographic Characteristics Across Towns in Different Treatment Groups

	Control	Contact	AA	p-value of difference (Contact v Control)	p-value of difference (Control v AA)
Panel A: Town characteristics (Census 2011)					
Total Population	13709	14646	14011	0.727	0.917
Fraction Muslim	0.43	0.40	0.38	0.803	0.631
Women per 1000 men	913	908	902	0.665	0.415
Girls per 1000 boys (age 0-6)	919	911	904	0.738	0.463
Fraction literate	0.58	0.57	0.58	0.808	0.978
Fraction women literate	0.50	0.49	0.50	0.880	1.000
Fraction SC	0.13	0.13	0.13	0.986	0.949
Fraction ST	0.00	0.00	0.00	0.488	0.323
Work participation, male	0.46	0.47	0.46	0.355	0.837
Work participation, female	0.11	0.14	0.14	0.167	0.262
Fraction working men in household enterprise	0.07	0.08	0.07	0.612	0.912
Fraction working men in non-agri and non-HH enterprise	0.65	0.60	0.64	0.555	0.894
Panel B: Experiment participant characteristics					
Male	0.67	0.60	0.60	0.065	0.059
Age	24.1	24.7	22.5	0.382	0.014
Scheduled Caste or Scheduled Tribe	0.16	0.13	0.16	0.357	0.847
Other Backward Caste	0.58	0.50	0.51	0.055	0.091
Only primary education	0.13	0.12	0.12	0.646	0.686
Completed grade 10	0.23	0.19	0.24	0.132	0.909
Completed grade 12	0.39	0.35	0.37	0.347	0.659
Completed college	0.25	0.34	0.27	0.007	0.497
Family monthly income <=Rs 5,000	0.19	0.21	0.16	0.568	0.335
Family monthly income Rs 5,000-10,000	0.31	0.30	0.29	0.875	0.574
Family monthly income Rs 10,000-15,000	0.24	0.19	0.22	0.147	0.507
Family monthly income Rs 15,000-30,000	0.17	0.23	0.23	0.069	0.063
Family monthly income > Rs 30,000	0.09	0.07	0.11	0.302	0.550
Pray several times a day	0.26	0.28	0.25	0.412	0.856
Pray once a day	0.61	0.58	0.60	0.519	0.811
Pray less than once a day	0.14	0.13	0.15	0.889	0.577

Notes: p-value of difference is calculated by regression on the treatment dummies and using robust standard errors.

Table A3
Coordination Game Payoff Table

		Minimum of the Hours Spent by Other Employees				
		0	5	10	15	20
My Hours working	0	₹ 500	₹ 500	₹ 500	₹ 500	₹ 500
	5	₹ 375	₹ 575	₹ 575	₹ 575	₹ 575
	10	₹ 250	₹ 450	₹ 650	₹ 650	₹ 650
	15	₹ 125	₹ 325	₹ 525	₹ 725	₹ 725
	20	₹ 0	₹ 200	₹ 400	₹ 600	₹ 800

Table A4
Experiment Participant Characteristics Across Groups with Hindu and Muslim Leaders

	Hindu leader	Muslim leader	p-value of difference
Male	0.64	0.60	0.235
Age	23.7	23.8	0.755
Scheduled Caste or Scheduled Tribe	0.16	0.13	0.172
Other Backward Caste	0.51	0.55	0.172
Only primary education	0.12	0.12	0.812
Completed grade 10	0.23	0.21	0.333
Completed grade 12	0.37	0.37	0.770
Completed college	0.28	0.30	0.457
Family monthly income <=Rs 5,000	0.17	0.20	0.126
Family monthly income Rs 5,000-10,000	0.31	0.29	0.568
Family monthly income Rs 10,000-15,000	0.25	0.19	0.031
Family monthly income Rs 15,000-30,000	0.19	0.23	0.126
Family monthly income > Rs 30,000	0.09	0.09	0.818
Pray several times a day	0.26	0.27	0.780
Pray once a day	0.59	0.60	0.918
Pray less than once a day	0.15	0.14	0.619

Notes: p-value of difference is calculated by regression on the Muslim leader dummy and using robust standard errors.

Table A5
Summary Statistics of Key Variables

	Hindu Leaders			Muslim Leaders		
	Control	AA	Contact	Control	AA	Contact
Minimum effort in group (rounds 1-4)	3.29	3.78	3.78	3.45	3.47	3.78
	4.40	*4.14*	*3.96*	*4.34*	*3.44*	*4.28*
Minimum effort in group (rounds 5-6)	2.80	5.68	6.05	4.51	3.69	5.85
	3.92	*3.86*	*4.65*	*4.53*	*4.37*	*4.73*
Leader Proposal (rounds 5-6)	9.39	12.22	11.98	10.49	11.88	11.65
	6.23	*4.52*	*5.31*	*5.67*	*4.67*	*4.51*
Individual effort (Hindus)	7.41	9.45	9.67	9.75	9.19	9.36
	6.26	*5.82*	*5.75*	*6.46*	*6.11*	*5.72*
Individual effort (Muslims)	8.51	9.88	10.23	9.41	9.72	9.57
	6.35	*5.47*	*5.54*	*6.16*	*5.48*	*5.81*

Standard deviations in italics

Table A6
Do Leader Proposals Across Leader Identity and Treatment Arms?

Dependent variable: Leader Proposal (Periods 5 and 6)

	(1)	(2)
	Across Leader Identity	Across Treatments
	Control Group	All Groups
Muslim Leader	1.093	0.923
	(0.979)	(1.000)
Muslim Leader * AA		-1.855
		(1.329)
Muslim Leader * Contact		-1.730
		(1.442)
Observations	164	504
R-squared	0.512	0.308
Mean for Hindu leaders in control group	9.390	9.390
Town FE	Yes	Yes
Demographic Controls	Yes	Yes
Religious Controls	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.

Table A7
Alternative Explanations for Higher Coordination in Muslim-led Groups

Dependent variable: Minimum Effort in the Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Muslim Leader Competence		Muslim Leader Exposure		Beliefs about Leader Effort	
	Muslim Leaders	Hindu Leaders	Muslim Leaders	Hindu Leaders	Muslim Leaders	Hindu Leaders
Leader (Period>4)	0.925	-0.138	1.833	-0.167	1.067	-0.488
	(0.754)	(0.487)	(0.669)	(0.619)	(0.509)	(0.393)
Leader* Fraction believe that Muslim leaders are less competent	0.801	-2.492				
	(2.478)	(2.288)				
Leader*Town has Muslim mayor			-1.208	-0.506		
			(0.959)	(0.798)		
Beliefs about average hours worked by leader of the group					0.35	0.577
					(0.199)	(0.144)
Observations	246	246	246	246	246	246
R-squared	0.488	0.439	0.484	0.436	0.492	0.490
Town FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Religious Controls	Yes	Yes	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. "Fraction believe that Muslim leaders are less competent" are those that agree with the statement “Do you think Muslim leaders are less capable relative to Hindu leaders?” in our post-experiment survey (27% of Hindus and 10% of Muslims answered yes). "Town has Muslim mayor" is a dummy that equals one if the town elected a Muslim mayor in the most recent urban local council elections of 2012 (17 out of 44 towns). Beliefs about hours worked by their leader was elicited by direct questions in the post-experiment survey.

Table A8
Leader Identity and Social Appropriateness of High Effort Choices

Panel A: Views of Hindu subjects

	Hindu leaders	Muslim leaders	p-value of difference
Hindu employees	2.76	2.72	0.38
Muslim employees	2.66	2.80	0.00

Panel B: Views of Muslim subjects

	Hindu leaders	Muslim leaders	p-value of difference
Hindu employees	2.75	2.70	0.18
Muslim employees	2.75	2.76	0.82

Numbers are the mean values of answers to the question "How socially appropriate would others consider it to be for a Hindu/Muslim employee to choose the 20 hours effort under a Hindu/Muslim leader?" Answers were on a scale of 1 to 4, where 1 represents "not at all socially appropriate" and 4 represents "very socially appropriate."

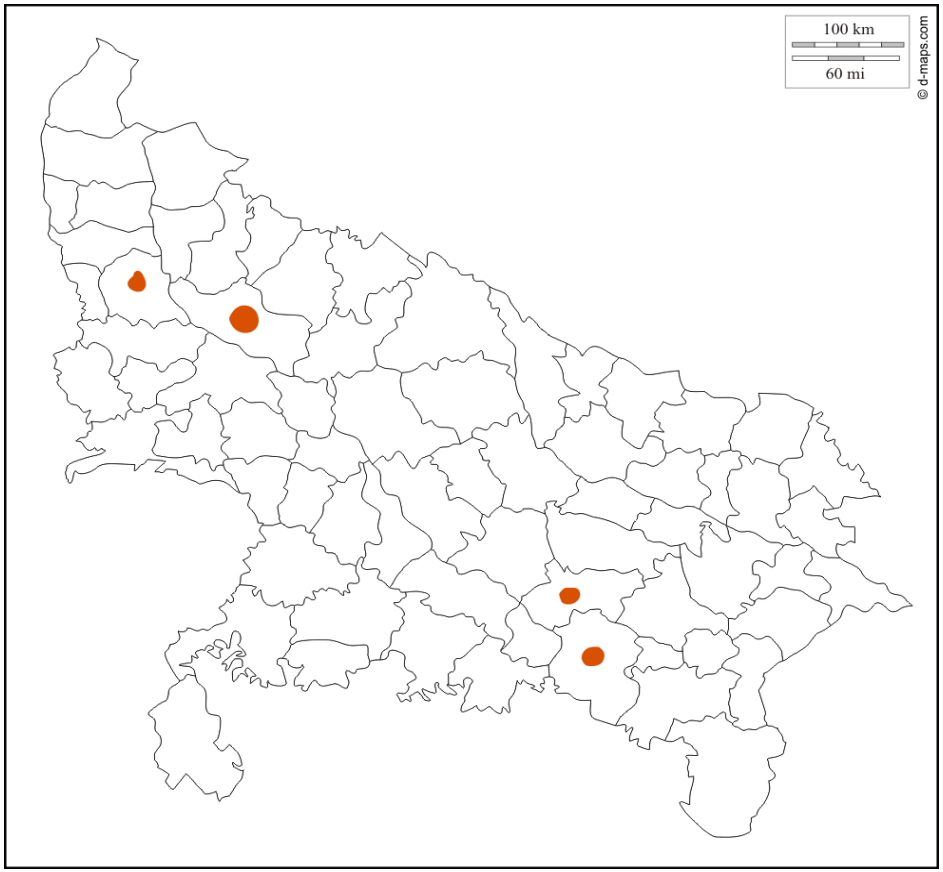
Table A9
Leader Identity and Individual Effort in Different Policy Environments

Dependent variable: Effort Choice of Participant

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Muslim Leaders		Hindu Leaders		Muslim Leaders		Hindu Leaders	
	Muslim Employees	Hindu Employees	Muslim Employees	Hindu Employees	Muslim Employees	Hindu Employees	Muslim Employees	Hindu Employees
Leader (Period>4) f_1	1.157	0.377	0.015	-1.156				
	(0.427)	(0.522)	(0.509)	(0.401)				
Leader (Period>4)*AA f_2	-0.628	-0.420	0.088	2.524	-0.622	-1.549	1.098	2.454
	(0.615)	(0.730)	(0.652)	(0.666)	(0.796)	(0.742)	(0.809)	(0.686)
Leader (Period>4)*Contact f_3	-0.163	0.712	0.602	2.392	-0.146	-0.248	1.821	2.587
	(0.672)	(0.670)	(0.670)	(0.680)	(0.677)	(0.655)	(0.829)	(0.596)
Observations	1494	1524	1506	1506	498	508	502	502
R-squared	0.197	0.153	0.178	0.231	0.223	0.215	0.087	0.347
Town FE	Yes	Yes	Yes	Yes	No	No	No	No
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Religious Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experimental Controls	No	No	No	No	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; experimental controls include the leader's proposal and the group minimum effort in period 4. Data includes effort choices of both leaders and employees.

Figure A1
Districts Chosen for Experimental Sites



Notes: Map of districts in Uttar Pradesh state, India. Sites chosen are Aligarh (high-conflict) and Budaun (low-conflict) in the western part of the state and Allahabad (high-conflict) and Pratapgarh (low-conflict) in the central part of the state.

Figure A2
Experimental Setting and Layout of Player Locations

Four players were seated on each mat. Participants were told that the people on the mat were not part of their firm. Participants were provided folders so that their effort choices were not visible to other players. They were also instructed not to look at other players' choices. Each mat also had a research assistant to explain the experimental procedures to the participants.

Figure A3
Puzzle Game in Different Groups

A. Solve Individually in Control and AA Groups

B. Solve with a Partner in the Contact Group

Figure A4
Leader Identity and Leader Proposals (Histogram)

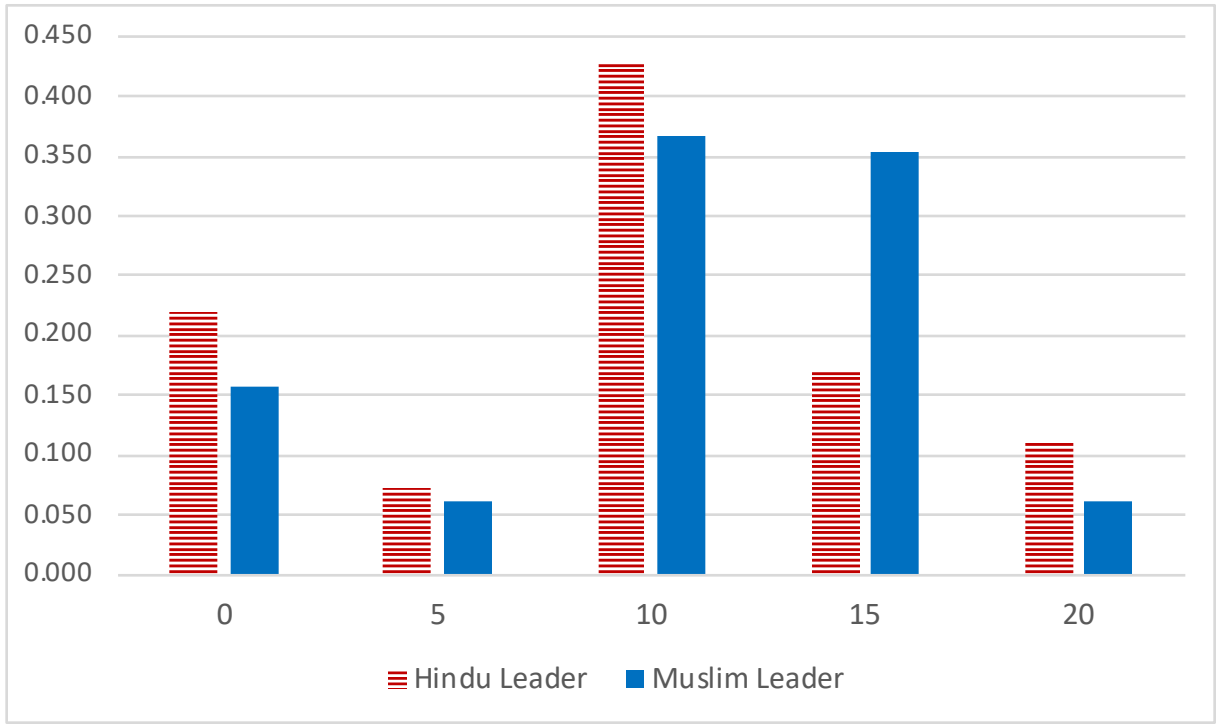
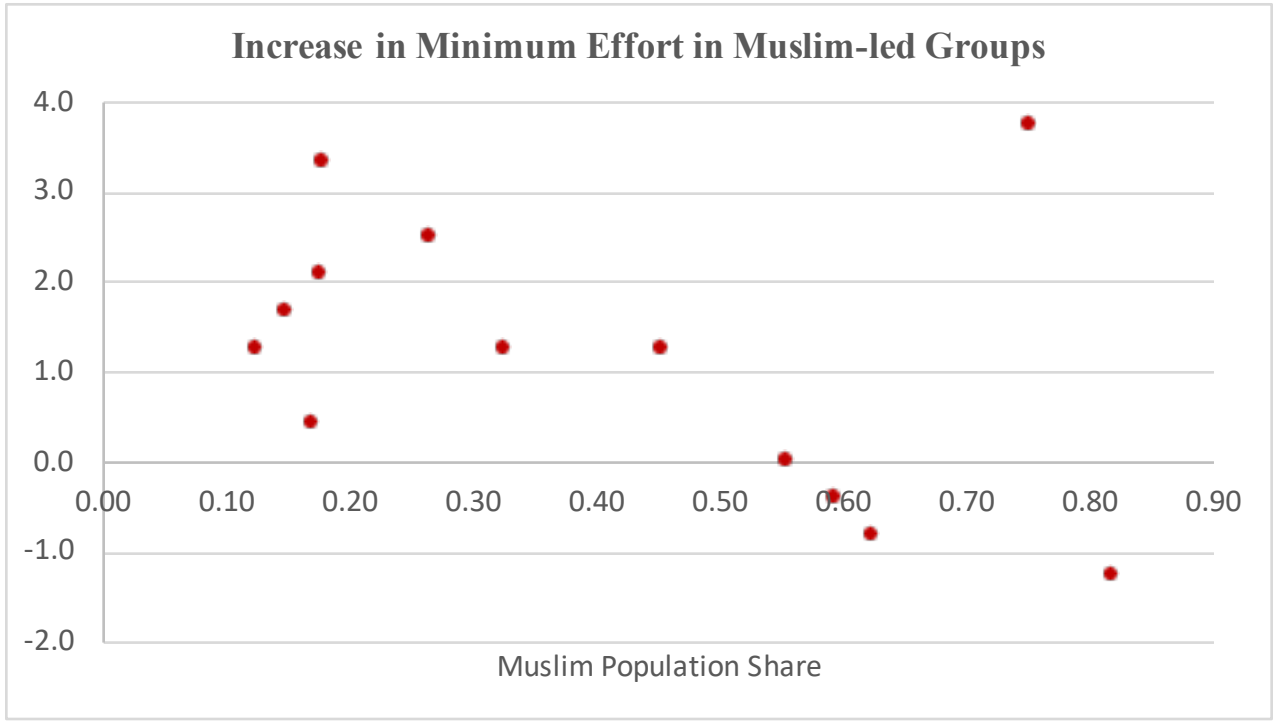


Figure A5
Muslim Leader Impact and Muslim Population Share (Control Group Towns)



Leader Identity and Coordination

Sonia Bhalotra
University of Essex

Irma Clots-Figueras
University of Kent

Lakshmi Iyer
University of Notre Dame

Joseph Vecci
University of Gothenburg

Appendix B
Details of Field Experiment

B1. Site Selection, Subject Recruitment and Experimental Procedures

B1.1. Site Selection and Balance Across Treatment Arms

Based on data on the incidence of religious violence over 1980-2010 (Kaysser et al., 2015), we selected two pairs of neighboring districts in Uttar Pradesh, India's most populous state. Each district pair was composed of one high conflict district and one low conflict district (see map in Appendix Figure A1). One pair was in the western part of the state (Aligarh and Budaun) while the other was in the central part of the state (Allahabad and Pratapgarh).

The experiment was conducted in 44 sites in July 2017 with 1028 subjects. Within each district, we selected sites based on the following criteria: (i) they were officially listed as towns in the census of 2011,[1] (ii) their population was below 50,000, (iii) they had a relatively high population share of Muslims, and (iv) selected towns were comparable in their demographics across high conflict and low conflict districts. The average town in our experimental sample had 40% Muslims in the population, compared to the overall state proportion of 19%.[2] 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 town level sessions consisted of 24 participants each,[3] resulting in a sample of 1028 subjects. The 44 sites were randomly assigned to three different treatment arms: 14 sites are retained

[1] Towns in India are defined as (a) statutory towns i.e. all places with a municipality, corporation, cantonment board or notified town area committee or (b) all other places which satisfied the following criteria: (i) a minimum population of 5,000; (ii) at least 75 per cent of the male main working population engaged in non-agricultural pursuits; and (iii) a density of population of at least 400 persons per sq. km.

[2] According to the census of 2011, Hindus and Muslims account for more than 99% of the population of Uttar Pradesh.

[3] There were a few exceptions due to recruitment difficulties: sessions consisted of 20 participants in three towns, and 16 participants in two towns. Our results control for town fixed effects and are robust to the exclusion of the towns with recruitment difficulties.

as control, and 15 each are assigned to the intergroup contact treatment and to the affirmative action treatment (see Appendix Table A1). The assignment was performed within each district (i.e. implicitly stratifying by high/low conflict status) and by further stratifying by Muslim population proportion and total population. We verify that census 2011 and respondent demographic characteristics are balanced across the different treatment arms (see Appendix Table A2, Panels A and B). In particular, we find that only 8 out of 56 comparisons in this table show statistically significant differences, a small proportion relative to the overall number of tests. Nevertheless, we control for all these characteristics in our regressions to ensure that these small differences in participant characteristics do not bias our results; results without these controls are remarkably similar to those with controls.

B1.2. Subject Recruitment

To recruit participants for the experiment, our research assistants visited both the Hindu and the Muslim sections of each town, and distributed flyers containing information about participant requirements (age and numeracy), remuneration (between 200-1000 rupees), time and location of the experimental sessions. Flyers were also posted at prominent landmarks and distributed at shops, temples and mosques. Average renumeration was relatively high (about 2.5 days wage for the average labourer) to ensure a varied distribution of the population. Upon arrival, participants were screened for eligibility (over the age of 18 and could read numbers) and their names were recorded on a participant list that was not visible to other participants. Upon commencement of the experiment, an equal number of Hindus and Muslims were selected from the list. Once seated, participants were given a number tag representing their randomly assigned identification number.

In India, it is relatively easy to identity Muslims from their names. After recording participant names, research assistants identified religion from name. The accuracy of this coding of religion was cross-checked once subjects completed the pre-experiment questionnaire. There were only 2

misclassifications of religion based on name (these participants were compensated and replaced with others prior to the experiment). Subject ID numbers were never matched with participant names, and the list of participant names were destroyed at the end of each session.

B1.3. Experimental Procedures

Effort choices in the coordination task were made individually using pen and paper and within a large binder. To ensure privacy we implemented a number of measures:, subjects were required to write all answers within a large plastic folder that easily hid their responses; the mats were quite large--each subject was over 2 meters from others on the mat, a research assistant sat on each mat to ensure privacy and that subjects did not talk to one another; finally, participants were instructed not to look at other players' choices. The research assistant was also tasked with answering questions and ensuring privacy.

Each session was run with six research assistants including one experimenter. At the start of a task, instructions were read aloud by the experimenter to establish common knowledge. To determine whether subjects understood the instructions, each participant answered a set of control questions in private both before the first period and at the start of the fifth period (when the leader is introduced). Control questions mainly focused on understanding the payoff table. The experimenter and research assistants cross-checked the answers and started the experiment once satisfied that subjects understood the task.

To ensure the anonymity of the leader, in all sessions, the leader's proposed effort was elicited after the control questions were answered, but before collecting the control question answer sheets i.e. it was not possible to identify a leader by seeing who was writing a suggested effort, since everyone was answering control questions. At the end of the session, each subject was privately paid in cash for one of the tasks (excluding the pre-experiment survey).

B2. Experiment Instructions

Overview

As part of today's experiment, you will be participating in three games. You will be given Rs 200 for participating and you can earn more money based on your decisions in the games. You will be paid for only one of the three games. Only you will know the amount you have earned. The payment will be made in cash. At the *end* of the session today, we will pick a number out of a bag, this will determine which game you will be paid for. So, which game you will be paid will be decided by chance. Since you do not know which game you will be paid for, you should ensure you understand the rules in all the games. Do ask if you do not understand something. After we have completed all the games, I would like you to answer some questions about yourself. Before we begin, please turn off your phone, we are about to read aloud the consent form which will briefly explain the basic activities, and the rules to follow.

Consent for Participation in Research

Introduction

I will now inform you about the study so you can decide whether or not to participate. Please listen carefully and ask any questions you might have before deciding whether or not to take part.

Purpose of the Study

You have been asked to participate in a research study about understanding how individuals make decisions in different group settings.

What will you be asked to do?

You will be asked to participate in a game, where you will make decisions corresponding to your assigned role, according to the rules of the game.

After participation in the game, you will complete a survey questionnaire. You can skip any question that makes you uncomfortable and you can stop the survey at any time. This study will take approximately 4 hours to complete. If you are not free for this time, please let us know. In your location, there will be approximately 24 people participating in the study.

What are the risks involved in this study?

There are no foreseeable risks from participating in this study.

What are the possible benefits of this study?

If you participate, you will be paid Rs. 200 as a participation fee. In addition, you could earn more money throughout the study depending on your decisions. You can earn up to Rs. 1000. You cannot lose any money by playing these games. Payments will be made after the game and the survey have been completed.

There are no other direct benefits to you of this study, beyond the compensation you will receive for your participation and the decisions you make in the games.

Do you have to participate?

Your participation is voluntary. You may decide not to participate at all or, if you start the study, you may withdraw at any time. If you want to withdraw from the study, raise your hand and say so, turn in your materials, and leave the study area. If you leave this experiment incomplete, you will only receive the participation fee and not the money earned on the basis of your decisions.

How will your privacy and confidentiality be protected if you participate in this research study?

Your data will remain confidential. All records of your participation will be assigned a code number. We will not retain any information connected to your identity such as name, address or phone number. All other information will only be used by the researchers involved in the project.

Participation

If you agree to participate, please say so to the researcher reading this form to you. Also, let this person know if you would like a copy of this form.

<Do not read below>

Whom to contact with questions about the study?

If you would like to contact the researchers involved in the project please let us know.

Prior to, during or after your participation you can contact the researcher Lakshmi Iyer at +1-617-319-0955 or send an email to liyer@nd.edu for any questions or if you feel that you have been harmed. You may also contact Professor Debayan Pakrashi at IIT Kanpur +91-40-2318-7267

This study has been reviewed and approved by The University's Institutional Review Board and the study number is 17-05-3869.

Whom to contact with questions concerning your rights as a research participant?

For questions about your rights or any dissatisfaction with any part of this study, you can contact, anonymously if you wish, the Notre Dame Research Compliance Office, at 574-631-1461 or by email at compliance@nd.edu .

Instructions

Before we begin the games, we are going to ask you a few questions. Please take your time and answer as honestly and as accurately as possible. You will not be identified and your answers will only be used for this experiment and will only be used by the researchers involved in this project.

<Hand out questionnaire>

I'll first read each question aloud and then you will have time for your response. Please do not say your answer aloud, only mark your response on your paper.

[*Say the question number first, read the question and the possible responses (and the number of the response) and give the subjects time to answer.]*

Initial Questionnaire

	Question	**Response**
1	What is your height?	1.☐ 0-4 feet 2.☐ 4.1-7 feet
2	Is your hair colour black?	1.☐ Yes 2.☐ No
3	What is your religion?	1. ☐ Muslim 2. ☐ Hindu 3. ☐ Christian 4. ☐ Buddhist
4	Is your eye colour blue?	1. ☐ Yes 2. ☐ No

Game 1

You are about to begin the first game. In this game, you will be given a puzzle. Your aim is to complete the puzzle. You will be given 12 minutes to solve it. If you complete the puzzle and the game is chosen for payment you will receive 400 rupees. Along with the puzzle you must answer one question, this question can be found in your puzzle pack. Please do not talk at any time while completing this puzzle. Once you have completed the puzzle please wait patiently until the time limit has been exhausted.
We will collect all puzzle packs at the end of the allotted time.

Contact treatment: In this game you with be paired with someone in the room. Together you will receive a puzzle. Your groups aim is to complete the puzzle together. You will be given 12 minutes to solve the puzzle. If you complete the puzzle and the game is chosen for payment you and your group member will each receive 400 rupees. Along with the puzzle you must answer one questions, this question can be found in your puzzle pack. You can talk to your group member, but no one else in the room. Once your group has completed the puzzle please wait patiently until the time limit has been exhausted.

We will collect all puzzle packs at the end of the allotted time.

<Hand out the puzzle packs.>

Question at the end of the puzzle is:

Q) What is the picture in the puzzle

1. Mowgli and Motu-Patlu

2. Spider-Man and Ninja Hathodi

3. Krrish and Ben Ten

4. Angry Birds and Doraemon

Game 2

We are about to begin the second game. All the money that you earn from this game is yours to keep and will be given to you at the end of this experiment if this game is chosen as the one that you will be paid for.

Please listen carefully. It is important that you understand the instructions of the game properly. If you do not understand, you will not be able to participate effectively. We will explain the game and go through some examples together. In this game, you are not allowed to talk or discuss the game with people around you. There will be opportunities to ask us questions to be sure that you understand how to perform the game. Also, do not look at other's responses at any time during this game and **please keep your sheets in your folder at all times.**

In this game, you will be grouped with three other participants from those in this room so the four of you form a group. In this game, your group of four will form a company. Everyone in the company will play as a company employee. (emphasis)_*You will never know which of the other participants are in your company and* your other company members will never know your identity.

Decision: In this game, you need to decide how many hours you would like to spend working (notionally) for the company. The available choices are 0, 5, 10, 15 or 20 hours. You won't be working actual hours, this is hypothetical, but your payoffs from your decisions are real. That is, you will be paid cash if this game is chosen for payment.

Payoffs: Your reward depends on the number of hours you work at the company <u>and</u> the number of hours the other members of your company decide to work. In particular, it depends on the **minimum** number of hours the others in your company work.

Please take a look at the table below, to identify how your payment will be calculated. This table shows your total earnings excluding show up fee. In this table, real payoffs are given; That is,

the table represents the amount you can receive in cash at the end of the experiment. *Explain that columns represent the minimum number of hours spent by other employees in the company*. *Rows indicate the number of hours you spend working. Give subjects 2 minutes to study the table.*

Payoffs can be seen in the Table below:

		Minimum of the Hours Spent by Other Employees				
		0	**5**	**10**	**15**	**20**
My Hours working	**0**	₹ 500	₹ 500	₹ 500	₹ 500	₹ 500
	5	₹ 375	₹ 575	₹ 575	₹ 575	₹ 575
	10	₹ 250	₹ 450	₹ 650	₹ 650	₹ 650
	15	₹ 125	₹ 325	₹ 525	₹ 725	₹ 725
	20	₹ 0	₹ 200	₹ 400	₹ 600	₹ 800

- If you don't spend any hours working you will receive 500 rupees.

- Every hour you spend working reduces your reward by 25 rupees. We do this to represent that working requires effort which is costly; since you won't actually be working, we just impose a cost of Rs 25 per hour.

- If you work more hours and someone else (even just one person) in your company works less hours, then you will earn less. That is, if you work more hours and others in the company do not work, then the company wont be able to function effectively so you will earn less.

- If you work more hours and everyone else works more hours, you will earn more. That is, if you work more hours and others also work, the company will be able to function effectively so you will earn more.

Here are 3 examples of what could happen, you can follow along on the diagrams at the front:

Example 1:

You spend 15 hours working at the company and two of the people in the company work 20 hours,

but the fourth works 5 hours. Your payoff depends on only two things: the hours you work and the hours worked by the person in the company who works the least hours, in this case 5 hours.

Looking at the table, you can see that your total income from this game is: ₹ 325. Again, this represents real cash.

Example 2:

You spend 0 hours working at the company. The other three people in the company work 10, 5 and 15 hours respectively.

Looking at the table, you can see that your total payoff from this game is: ₹ 500.

Example 3:

You spend 15 hours working. The others in the company work 15, 20 and 20 hours each.

Looking at the table, you can see that your total income from this game is: ₹ 725.

This game will be played for six rounds. Your company will stay the same. You will be paid for one randomly selected round, if this game is chosen for payment. At the end of each round you will be informed of the minimum hours worked in your company.

In summary :

1) Everyone is assigned to a group of four. This is your company

2) Each person must decide how many hours to work for the company. This is only a decision in this game. Your decisions impact your payoffs.

3) This game repeats itself six times, with the same employees.

Confidentiality and Payoff:

At no point in time will we identify the identity of an employee in the company. In other words, the actions you take will remain confidential. At the end of the experiment today you will be informed of your payoff and paid this amount in cash in private along with your show up fee. You will be paid privately and we will not disclose your payoffs to the other participants in the experiment.

<Hand out Information sheet>

<Explain that this sheets contains information about the game. Have subjects turn to the second page, and explain that they are employees and their group consists of 4 employees in total. Move to the third page and explain the control questions>

Quality Control Questions

Before we begin the game, we will ask you some questions to ensure that you understand the game. Using the payoff Table in front of you please answer the following questions

Payoffs can be seen in the Table below:

		Minimum Hours Spent by Other Employees working at the company				
		0	**5**	**10**	**15**	**20**
	0	₹ 500	₹ 500	₹ 500	₹ 500	₹ 500
My Hours working	**5**	₹ 375	₹ 575	₹ 575	₹ 575	₹ 575
	10	₹ 250	₹ 450	₹ 650	₹ 650	₹ 650
	15	₹ 125	₹ 325	₹ 525	₹ 725	₹ 725
	20	₹ 0	₹ 200	₹ 400	₹ 600	₹ 800

Suppose you chose to dedicate 10 hours working at the company. The rest of the employees choose to dedicate 15, 0 and 20 hours to working at the company.

Q1) What is the minimum number of hours worked by the other employees at the company?

Q2) What is your payoff from the above example?

Q2) In this game will your actions and your payoff be confidential?

Q3) In this game how many rounds will you take part in?

<Check all control questions. Collect information sheets and hand out round 1 decision sheets >

Information at the end of Round 1

You will now play this game again with the same company employees. You will also be informed of the minimum hours worked in the company in round 1. You can use this and the payoff table to identify your possible payoffs. Again, the numbers in your payoff table represent real money you can earn. Please raise your hand if you have any questions.

<Hand out round 2 decision sheets>

<Collect round 2 decision sheets>

Information at the end of Round 2

You will now play this game again with the same company employees. You will also be informed of the minimum hours worked in the company in round 2. You can use this and the payoff table to identify your possible payoffs. Again, the numbers in your payoff table represent real money you can earn. Please raise your hand if you have any questions.

<Hand out round 3 decision sheets>

<Collect round 3 decision sheets>

Information at the end of Round 3

You will now play this game again with the same company employees. You will also be informed of the minimum hours worked in the company in round 3. You can use this and the payoff table to identify your possible payoffs. Again, the numbers in your payoff table represent real money you can earn. Please raise your hand if you have any questions.

<Hand out round 4 decision sheets>

<Collect round 4 decision sheets>

Information at the end of Round 4

The game will repeat for another two rounds. Your company will stay the same but please listen to the following additional information.

Further Information *(Treatment 1: Control + Contact)*:

1. The company has decided to introduce a leader. Within each company there will be one leader and 3 employees. You will either be the leader of the company or you will stay as an employee. You will not be informed of the identity of the leader i.e you will not know who the leader is. Instead, employees will be informed of some characteristics of the leader taken from the questionnaire they filled out at the beginning of today's session.

2. The leader's task is to propose the number of hours employees should devote to working at the company. This could be either 0, 5,10,15 or 20 hours. The leader can only make one suggestion for the whole company. The leader's suggestion is only a suggestion.

3. All employees will be informed of the leader's proposal. After this, all employees including the leader must decide how many hours to work at the company.

Further Information *(Treatment 2: Affirmative Action)*:

1. The company has decided to introduce a leader. Within each company there will be one leader and 3 employees. You will either be the leader of the company or you will stay as an employee. You will not be informed of the identity of the leader i.e you will not know who the leader is. Instead, employees will be informed of some characteristics of the leader taken from the questionnaire they filled out at the beginning of today's session

2. The leader's task is to propose the number of hours employees should devote to working at the company. This could be either 0, 5,10,15 or 20 hours. The leader can only make one suggestion for the whole company. The leader's suggestion is only a suggestion.

3. All employees will be informed of the leader's proposal. After this, all employees including the leader must decide how many hours to work at the company.

4. Similar to many government positions, 50% of the leadership positions in this game will be reserved. Reservation is based on some important characteristic of the leader taken from the questionnaire they filled out at the beginning of today's session. Employees will be informed if the leader of their company is in a reserved or an unreserved position.

In summary:

1) A company leader is chosen, all other participations stay as company employees.

2) *AA only-Within this game, some of the leadership positions will be reserved. Leadership positions are reserved for leaders with certain characteristics.*

3) To inform employees of the leader's characteristics, employees will be given the leaders response to the initial questionnaire.

4) The company leader must suggest to employees the number of hours to spend working at the company.

5) Company employees are then informed of the leader's suggestion.

6) All company employees including the leader must decide how many hours to devote to working at the company.

7) The game repeats for two rounds then ends.

<Hand out round 5 information sheets>

<Refer subjects to the second page, where they will be informed of their role as either the leader or employee. Inform them that if they are leaders, in their sheets the word would be highlighted in yellow; and if they are employees, in their sheets the word would be highlighted in blue. The third page contains a control question, while the fourth page contains the leaders suggested proposal. Read this out and check responses.>

<Collect all information sheets>

<Hand out round 5 decision sheets>

<Explain that the first page of the decision sheet contains information on the minimum number of hours worked in the last round, the second page contains information on the leaders proposal and the leaders characteristics. On the third page subjects must input the number of hours they will work this round. The leaders have an additional page, where they must suggest the number of hours in the final round. Mention these pages simultaneously. >

Extra for AA- Remind people that on the second page, they are told if their leaders position is reserved or not.

<Collect round 5 decision sheets>

Information at the end of Round 5

You will now play this game again with the same company employees and the same company leader. You will also be informed of the leaders new proposed number of hours, the leaders characteristics and the minimum hours worked in the company in round 5. You can look at the payoff table to identify your possible payoffs. Please raise your hand if you have any questions

Extra for AA- Remind people that on the second page, they are told if their leaders position is reserved or not.

<Hand out round 6 decision sheets>

<Explain that the first page of the decision sheet contains information on the minimum number of hours worked in the last round, the second page contains information on the leaders proposal and the leaders characteristics. On the

third page subjects must input the number of hours they will work this round. >

<Collect round 6 decision sheets>

Information at the end of Round 6

The game is now complete. We will now move to game 3.

Game 3

We will now play Game 3. In playing this game, you must decide, for each possible action, whether taking that action is socially appropriate (i.e. consistent with proper social behavior) or not. By "socially appropriate", we mean behaviour that most people agree is the correct thing to do. Another way to think about this is that if an individual were to act socially inappropriately, then this might make many people angry or uncomfortable with them.

To give you an idea of how the game will proceed, we will go through an example.

Example:

A person is at a local store. While there, she notices that someone has left a wallet on the table. She must decide what to do. You must decide whether choosing that option is very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, or very socially appropriate

	1. Very Socially Inappropriate	*2. Somewhat socially inappropriate*	*3. Somewhat socially appropriate*	*4. Very socially appropriate*
Leave the wallet where it is				

Payment: At the end of the experiment today, for each question, we will determine which response *most people* selected in today's session. If your response is the same then you will receive Rs 200. In other words, **if you give the same response as that most frequently given by people in today's session, then you will receive an additional Rs 200**. This amount will be paid to you privately, in cash, at the conclusion of the experiment if this game is chosen for payment.

For instance, if we were to select the example situation above and the option "Leave the wallet where it is," and if your response had been "somewhat socially inappropriate," then you would receive Rs 200, in addition to any other earnings you might receive, **if this was the response selected by most people in today's session**. If you selected very socially inappropriate, somewhat socially appropriate, or very socially appropriate then you would not be paid. It is important you remember in this game you will be paid if your decision is the same decisions as most people in today's session, rather than what *you* think is socially appropriate.

<Have subjects flip to their question sheet. >

Please note that each row is a separate question, we will go through each question together. You will notice there are 21 questions. In front of each question there are four boxes. You should tick one of these four boxes for each question. If your answer is "Very socially inappropriate", tick the first box in front of the question; if it is "Somewhat socially inappropriate", tick the second box; if it is "Somewhat socially appropriate", tick the third box; if it is "Very socially appropriate", tick the fourth box in front of the question. We will randomly select three of the questions to make payment. For each selected question you will be paid if your response is the same as the majority of people in todays session. Since you do not know which questions we will pay, you should ensure you understand all questions. Don't forget we are playing with real money.

Questions:

No.	**Question**	***1****. Very Socially Inappropriate*	***2.*** *Somewhat socially inappropriate*	***3****. Somewhat socially appropriate*	***4.*** *Very socially appropriate*
	If a ***Hindu leader*** was assigned to a group, **how do you think people in the session** would rate each of the following decisions?				
1	A Hindu employee spending 0 hours working at the company				
2	A Hindu employee spending 10 hours working				

3	A Hindu employee spending 20 hours working				
	If a ***Muslim leader*** was assigned to a group, **how do you think people in the session** would rate each of the following decisions?				
4	A Hindu employee spending 0 hours working				
5	A Hindu employee spending 10 hours working				
6	A Hindu employee spending 20 hours working				
	Reminder: Remind people that they will be paid in rupees if their response is the same as the majority of people in todays session rather than what ***you*** think is socially appropriate				
	If a ***Hindu leader*** was assigned to a group, **how do you think people in the session** would rate each of the following decisions?				
7	A Muslim employee spending 0 hours working				
8	A Muslim employee spending 10 hours working				
9	A Muslim employee spending 20 hours working				

	If a ***Muslim leader*** was assigned to a group, **how do you think people in the session** would rate each of the following decisions?				
10	A Muslim employee spending 0 hours working				
11	A Muslim employee spending 10 hours working				
12	A Muslim employee spending 20 hours working				

The following questions do not refer to game 2.

No	**Question**	***1****. Very Socially Inappropriate*	***2.*** *Somewhat socially inappropriate*	***3****. Somewhat socially appropriate*	***4.*** *Very socially appropriate*
	Reminder: Remind people that they will be paid if their response is the same as the majority of people in todays session rather than what *you* think is socially appropriate				
13	Do you think other believe it is socially appropriate for Muslims *to become town leader?*				
14	Do you think other people believe it is socially appropriate for Muslims *to become*				

	leaders (other than the town leader)				
15	Do you think other people believe it is socially appropriate to undertake an abortion if one does not want a son				
16	Do you think other people believe it is socially appropriate to undertake an abortion if one does not want a daughter				
17	Do you think other people believe it is socially appropriate for a Hindu and Muslim to be friends				
18	Do you think other people believe it is socially appropriate for a Hindu and Muslim to marry each other				
19	Do you think other people believe it is socially appropriate for a Hindu and Muslim to eat at each-others' houses				
20	Do you think other people believe it is socially appropriate to express your anger when you are not happy with something/someone				
21	Do you think other people believe it is socially appropriate to act on your anger in				

	such circumstances				

Experiment Ends– Payment and Survey

You have now completed all games. Now we would like you to answer some questions about yourself. You will be placed with one assistant who will go through the survey with you. Please take your time and answer honestly and as accurately as possible. You will not be identified and the assistant will not disclose any information about you. Your survey answers will only be used for this experiment and will only be used by the researcher(s) involved in this project.

After the survey is complete we will we will pay everyone individually and in private.

Leader Identity and Coordination

Sonia Bhalotra
University of Essex

Irma Clots-Figueras
University of Kent

Lakshmi Iyer
University of Notre Dame

Joseph Vecci
University of Gothenburg

Appendix C: Robustness Checks

Table C1
Leader Identity and Effort Choices in the Control Group: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Town random effects		Ordered probit		Control for dislike of other religion		Control for town*mat fixed effects			
	Muslim Leaders	Hindu Leaders	Muslim Leaders	Hindu Leaders	Muslim Leaders	Hindu Leaders	Muslim Leaders Muslim Employees	Muslim Leaders Hindu Employees	Hindu Leaders Muslim Employees	Hindu Leaders Hindu Employees
Leader (Period>4)	1.067	-0.488	0.328	-0.168	1.067	-0.488	1.120	0.380	-0.144	-1.020
	(0.494)	(0.381)	(0.136)	(0.121)	(0.509)	(0.393)	(0.485)	(0.590)	(0.555)	(0.432)
Observations	246	246	246	246	246	246	462	474	468	456
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Religious Controls	Yes	Yes	Yes	Yes	Yes	Yes	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. Columns (7)-(10) exclude one town where we could not recruit a full sample of 24 participants.

Table C2
Policy Environments and Leader Effectiveness, Controlling for Gender of Puzzle Partner

Dependent variable: Minimum Effort in the Group

	(1) Muslim Leaders	(2) Hindu Leaders
Leader (Period>4)	0.456	-0.535
	(0.693)	(0.617)
Leader (Period>4)*AA	-0.940	2.42
	(0.879)	(0.690)
Leader (Period>4)*Contact	3.301	2.956
	(1.191)	(1.063)
Leader (Period>4)*Contact*FemalePuzzle Partners	-4.803	-0.132
	(2.206)	(2.032)
Leader (Period>4)*Female PuzzlePartners	1.669	-0.029
	(1.512)	(1.232)
Female Puzzle Partners	-2.021	0.132
	(1.083)	(0.933)
Observations	702	702
R-squared	0.265	0.349
Town FE	Yes	Yes
Demographic Controls	Yes	Yes
Religious Controls	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. Results exclude five towns where we could not recruit a full sample of 24 participants each.

Table C3
Conflict History and Policy Environments, Controlling for Leader Proposals

Dependent variable: Minimum Effort in the Group

	(1) Muslim Leaders Low Conflict Areas	(2) Muslim Leaders High Conflict Areas	(3) Hindu Leaders Low Conflict Areas	(4) Hindu Leaders High Conflict Areas
Leader (Period>4)*AA	0.499	-2.502	1.778	2.77
	(1.341)	(1.082)	(0.897)	(0.828)
Leader (Period>4)*Contact	2.460	1.409	1.429	3.812
	(1.097)	(1.150)	(0.788)	(1.176)
Observations	112	146	110	146
R-squared	0.418	0.416	0.428	0.452
Town FE	No	No	No	No
Demographic Controls	Yes	Yes	Yes	Yes
Religious Controls	Yes	Yes	Yes	Yes
Experimental 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; experimental controls include the leader's proposal and the group minimum effort in period 4. Results restricted to periods 5 and 6 only, in order to include experimental controls.