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Abstract

I estimate the effects of collaborative and adversarial intergroup contact. I randomly assigned Indian men from different castes to participate in cricket leagues or to serve as a control group. League players faced variation in collaborative contact, through random assignment to homogeneous-caste or mixed-caste teams, and adversarial contact, through random assignment of opponents. Collaborative contact increases cross-caste friendships and efficiency in trade, and reduces own-caste favoritism. In contrast, adversarial contact generally reduces cross-caste interaction and efficiency. League participation reduces intergroup differences, suggesting that the positive aspects of intergroup contact more than offset the negative aspects in this setting.

JEL-Codes: C930, D900, D910, O120.

Keywords: contact hypothesis, caste, social interactions, India, field experiment.

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1 Introduction

Social psychologists have long theorized that the effects of intergroup contact on prejudice should depend on the *type* of contact: in particular, whether the integrated groups have common goals, a lack of intergroup competition, equal status, and the support of authorities (Allport (1954)). This theory is known as the "contact hypothesis". Over six decades since Gordon Allport first formulated the hypothesis, we still lack rigorous evidence on whether, and if so, why, the effects of intergroup contact depend on these four scope conditions (Paluck et al. (2018)).[1] This missing evidence is important: policymakers cannot optimally design integrative policies without an understanding of which conditions matter for positive effects of contact and which conditions do not. Related, naturally occurring integration frequently has negative effects,[2] and whether these negative effects could be prevented by re-structuring the conditions of contact is an open question.

This paper uses a field experiment in caste-segregated, rural India to study the impact of two types of intergroup contact: collaborative, where groups share common goals, and adversarial, where they instead actively compete. I used cricket, the most popular sport in India, to integrate young men from different castes. From a sample of 1,261 men, I randomized 800 to play in eight month-long cricket leagues, and assigned the others to a control group. Of those assigned to play, I assigned 35% to homogeneous-caste teams, and the others to mixed-caste teams. This randomization gave the first type of cross-caste contact: collaborative – those on the same team shared the common goal of winning matches. Once teams formed, I chose opponents randomly to create the second type of cross-caste contact: adversarial – those on opposing teams had opposing goals. I measured intergroup behavioral outcomes one to three weeks after each league ended.

Why should the type of contact matter? Different types of contact provide incentives for different types of intergroup interactions, which may affect outcomes through both belief-based and preference-based channels. Integrated groups with common goals have incentives to cooperate with one another, whereas those with competing goals have incentives to undermine each other. Consistent with this idea, in the leagues cross-caste interactions with opponents are 50 percentage points more likely to be hostile (e.g. arguments or insults) than cross-caste interactions with teammates. These different intergroup behaviors may drive belief updating in opposite directions, especially if participants make attribution

[1] Existing evidence on the important of the conditions of contact primarily uses only cross-study variation. For example, in a meta-analysis of 515 studies, Pettigrew and Tropp (2006) find significantly greater reductions of prejudice in studies satisfying the four Allport conditions than in other studies. This effect is robust to the inclusion of some study-level controls, but may nevertheless be driven by omitted variable bias. Experimental evidence for the positive effects of contact in general is growing, and reviewed in Paluck et al. (2018).

[2] For example, a series of papers find negative political effects of exposure to immigrants and refugees (Enos (2014); Halla et al. (2017); Dustmann et al. (2018); Tabellini (2019); Hangartner et al. (2019)).

errors (Jones and Harris (1967); Ross and Nisbett (2011)): wrongly attributing the pro-social (hostile) behavior of outgroup teammates (opponents) to their caste, rather than to their incentives. Along other dimensions, for example beliefs about cricket ability, both types of contact may give similar information. I use my first set of outcomes to explore this: measures of willingness to interact along two dimensions, as friends and as teammates. Contact may also shift deeper preferences through habit formation (Becker and Murphy (1988)) or participants choosing preferences to rationalize past behavior (Bernheim et al. (2019)). My second set of outcomes sheds light on this preference channel: measures of caste favoritism in the allocation of cricket training. These channels may in turn lead to economic efficiency gains or losses. I capture efficiency effects with my third set of outcomes: measures of trading behavior and trust.

My first set of findings consider players' willingness to interact. Collaborative and adversarial contact have opposite effects on self-reported cross-caste friendships.[3] Having all other-caste teammates instead of none increases the number of other-caste friends by 1.1, while having all other-caste opponents instead of none decreases the number of other-caste friends by 3.4. These friendship effects are not merely driven by players becoming friends with teammates and disliking opponents – collaborative contact also increases cross-caste friendships with non-teammates, and adversarial contact reduces cross-caste friendships with non-opponents.[4] A natural interpretation of these effects, though not the only one, is that the two types of contact have opposite effects on inferences about the cooperativeness of other-caste men.

In contrast with the effects on social interaction, *both* types of contact reduce ability-based statistical discrimination (Arrow (1973), Aigner and Cain (1977), Cornell and Welch (1996)), causing more other-caste men to be chosen as teammates for a future match with monetary stakes. Additional evidence suggests that this result reflects the impact of contact on knowledge about cricket ability. In particular, when players choose teammates for an alternative match *without* a prize for the winner, both types of contact have smaller effects, but the adversarial effect falls significantly further, to zero. Though adversarial contact conveys information about the ability of other-caste players, it also reduces the desire for cross-caste social interaction. When the match has no money at stake, the balance shifts to choosing players on the basis of desired social interaction, fully offsetting the informational effect of adversarial contact.

My second set of findings consider effects on own-caste favoritism in an incentivized voting exercise. Each player voted to determine which representative from each team would receive professional cricket coaching. Collaborative contact reduces own-caste favoritism in voting by up to 33%, while adversarial

[3] I aimed to reduce experimenter demand effects by having participants select friends from a randomly-ordered list of all participants, with caste neither made salient here nor when describing the purpose of the experiment itself.

[4] In addition, these generalized effects are not driven by network effects – players are not becoming friends with the friends of their other-caste teammates, nor are they failing to become friends with the friends of their other-caste opponents.

contact has imprecise effects.[5] Complementary evidence suggests that the collaborative effect comes mainly through effects on preferences rather than through beliefs about ability. In particular, incentivized ability beliefs at baseline are no more likely to be incorrect for other-caste than own-caste players, limiting the scope for belief correction to explain the results.

My third set of findings explore the efficiency effects of contact. Collaborative contact increases cross-caste trade by up to 21% and trade payouts by 18%, as measured in a trading exercise where there were gains from cross-caste trade. This effect corresponds to that of a monetary incentive for cross-caste trade equivalent to one to two hours of wages. The point estimates for adversarial contact are negative, though statistically insignificant. These divergent efficiency effects are similar when considering measures of trust, behavior that is measured in the absence of face-to-face interaction. In particular, adversarial contact reduces levels of trust significantly more than collaborative contact,[6] which has small or somewhat negative effects.

Taken together, my findings demonstrate that the type of contact mediates its impact: collaborative contact increases willingness to interact with men from other castes, reduces own-caste favoritism, and tends to increase efficiency. In contrast, adversarial contact reduces cross-caste social interaction and efficiency. In support of the contact hypothesis of Allport (1954), contact only improves intergroup relations when the groups have common goals. I present some evidence against three mechanisms other than common goals. First, though contact with teammates may be more intensive than that with opponents, differences in intensity alone should not lead to *opposite* effects on the demand for cross-caste social interaction.[7] Second, the two types of contact also differ in duration – contact with each opponent only lasts for one match, whereas contact with each teammate continues for several matches. However, the longer-term nature of collaborative contact does not seem to explain impacts – even the short-term collaborative contact backup players experience has positive effects. Third, neither type of contact affects performance or payouts in the matches, suggesting that the mechanism does not work through sporting success or income effects.

In the final part of the paper, I discuss three additional results with implications for program design and other aspects of the contact hypothesis. First, I show that the cricket league intervention reduced intergroup differences overall, demonstrating that, in this setting, the positive aspects of intergroup contact more than offset any negative aspects.[8] To estimate the impact of the cricket intervention I compare those

[5] While I cannot reject the null hypothesis of no adversarial contact effect, I also cannot reject the hypothesis that the collaborative and adversarial effects are equal.

[6] As measured by an index that combines two standardized variables: the amount given in a trust game (Berg et al. (1995)) and a dummy variable equal to one if the respondent thinks that "most people can be trusted".

[7] More formally, in a learning framework, differences in signal precision should affect the *speed* of learning, but not the *direction*.

[8] Other integrative sports programs exist, but evidence on their impact is scarce. Right to Play reaches one million children

randomly assigned to the leagues with those in the pure control group. Those assigned to mixed teams make more other-caste friends than those in control, choose more other-caste teammates, and engage in more cross-caste trade. Those assigned to homogeneous-caste teams are also positively affected, though much less than those in mixed teams.

Second, the effects of collaborative contact are not affected by the monetary incentive structure randomly assigned to each team. In particular, collaborative contact is no less effective when teams are assigned to a pay structure which increases within-team inequality and competition. A likely explanation for the result is that while the collaborative versus adversarial treatments affected intergroup interactions, the competitive pay structure did not. This result then gives evidence against the importance of one other scope condition emphasized by Allport (1954): the condition of no intergroup competition.

Third, I show that despite evidence for discrimination of lower castes within each team, the positive effects of collaborative contact are mostly similar across castes. Lower castes are measurably worse at cricket on average, and are less likely to be selected as captains, batters or bowlers, even after controlling for ability. Nevertheless, the effects of collaborative contact are mostly not significantly different between upper and lower castes. This shows that collaborative contact is effective even in the absence of equal status between groups within the situation, giving some evidence against one additional scope condition emphasized by Allport (1954).

This paper is the first to systematically test for the effects of different types of contact (Bertrand and Duflo (2017); Paluck et al. (2018); Kremer et al. (2019)), showing both the importance of common goals, and some suggestive evidence against the importance of intergroup cooperation and equal status. Existing empirical tests study one type of contact in isolation (Boisjoly et al. (2006); Barnhardt (2009); Enos (2014); Burns et al. (2015); Schindler and Westcott (2015); Broockman and Kalla (2016); Finseraas et al. (2016); Mo and Conn (2018); Okunogbe (2018); Scacco and Warren (2018); Stegmann (2018); Carrell et al. (2019); Finseraas et al. (2019); Mousa (2019)), or use non-randomized variation in the type of contact (Pettigrew and Tropp (2006); Dustmann et al. (2018); Bazzi et al. (2019)).[9] In a particularly creative example of the former, Rao (2019) shows that integration of rich and poor students in Delhi schools increases the pro-social behavior of rich students. In his case, the contact entails a mix of collaborative

weekly with sports-based programs promoting education, health and peaceful communities, Soccer for Peace uses sport to unite Jews and Arabs in Israel, and cricket programs unite Hutus and Tutsis in Rwanda (Hoult (2016)). Ditlmann and Samii (2016) find mixed effects of an inter-ethnic sports program using a difference-in-differences design. Sport has also been explored as a means of improving intergroup relations through shared national experiences (Depetris-Chauvin et al. (2019)). While Mousa (2019) studies intergroup contact on soccer teams in post-conflict Iraq, the design lacks a pure non-soccer playing control group to use for program evaluation.

[9]Examples from history also suggest that economic structure can drive ethnic conflict – whether trade complementarities reducing Hindu-Muslim violence (Jha (2013)) or increased labor market competition promoting anti-semitic acts (Becker and Pascali (2019)). One possible mechanism for these effects is that economic structure determines the nature of intergroup contact.

and adversarial interactions (e.g. through competing on exams). Mousa (2019) also complements my paper, showing evidence for the positive effects of collaborative contact in soccer leagues in a post-conflict setting, with the added advantage of longer-term outcome measurement. Different to these papers, I investigate the impacts of the two types of contact separately.

The second primary contribution of this paper is to estimate the efficiency effects of contact. A large literature shows that ethnic diversity and ingroup bias affect efficiency and allocation (Alesina and Ferrara (2005); Anderson (2011); Hjort (2014); Burgess et al. (2015); Marx et al. (2016); Fisman et al. (2017)). These papers show that ethnic differences have costs; my paper is the first to show that the efficiency consequences of integration depend on the nature of contact. To do so, I introduce a trading exercise that is cheap to implement, portable, and useful for the incentivized measurement of economic networks in the absence of naturally occurring data on economic links.

More broadly, this paper complements a large psychology and lab-experimental literature on the effects of group membership (Sherif et al. (1961); Tajfel et al. (1971); Chen and Li (2009); Goette et al. (2012)) by showing that team membership can reduce prejudice in a real-world setting. Finally, this paper contributes to a large body of work on caste networks (Munshi (2011); Munshi and Rosenzweig (2016); Banerjee et al. (2013, 2010), reviewed in Munshi (2016)) by exploring not just why these networks matter, but also how they form.

The remainder of this paper is organized as follows. Section 2 describes the basic social psychology and economics of why different types of contact might have different effects. Section 3 provides an overview of India's caste system, and motivates the use of cricket leagues as a tool for the study of contact. Section 4 describes the experiment design and outcomes, while Section 5 explores the effects of both types of contact on willingness to interact, own-caste favoritism, and efficiency. Section 6 considers alternative explanations for why the type of contact matters, and Section 7 considers three program design implications of additional results. Section 8 concludes.

2 Background on the Contact Hypothesis

The Contact Hypothesis. Social psychologists began studying intergroup contact in the 1940s amidst a context of racial conflict in the US (Pettigrew et al. (2011)). On the basis of these studies, Allport (1954) introduced the contact hypothesis. The hypothesis states that interpersonal contact between groups will reduce prejudice only when four conditions are met: (i) equal status of the groups within the situation, (ii) common goals, (iii) a lack of intergroup competition, and (iv) the support of authorities, law or custom.

Existing evidence on the importance of Allport's conditions for the effects of contact relies on cross-study variation (Pettigrew and Tropp (2006)). This evidence concludes that the conditions facilitate, but are not necessary for, prejudice reduction. In particular, studies that satisfy all four conditions show greater reductions in prejudice than those that do not, yet those that do not still find that contact reduces prejudice.[10]

My paper reports results from the first experiment to systematically test the importance of the conditions (Paluck et al. (2018)). I focus primarily on the role of common goals, using the formation of sports teams to create variation.[11] Psychologists consider sports teams to be a leading example of common goals in a natural setting (Pettigrew (1998)). In addition, I give some insight into the importance of equal status and intergroup competition through (i) showing descriptively that team members of each caste do not have equal status, and yet treatment effects are similar across caste, and (ii) randomizing the monetary incentives teams receive to heighten the stakes of intergroup competition between team members.

The contact hypothesis was formulated to rationalize the mixed effects of contact observed in various settings in the US. However, the theory does not say anything about *why* contact has positive effects, or similarly, by what channels contact works through. Pettigrew (1998) builds on the hypothesis by arguing for four primary channels. First, contact leads to *learning* about the outgroup, which can correct negative beliefs. Second, contact leads to *changed behavior*, as individuals reduce dissonance between prejudice and new behavior by revising attitudes. Third, contact generates *affective ties*, and these friendships mediate effects. Fourth, contact leads to *ingroup reappraisal*, for example, through individuals reassessing the norms and customs associated with the ingroup. Social psychologists have found evidence for the importance of these channels using mediation techniques, again with cross-study variation (Pettigrew and Tropp (2008)). That said, these channels do not map directly to economic concepts, nor do they clarify why exactly the condition of common goals might matter.

Economic Channels and Common Goals. The presence of common goals generates incentives for different kinds of interactions. Groups with common goals have incentives to cooperate with and encourage one another. In contrast, groups with opposing goals have incentives to undermine one another. Consistent with this, I find in the cricket leagues that while interactions with other-caste teammates are more frequent than interactions with other-caste opponents (columns 1-5, Table A1), conditional on interact-

[10]For a subset of studies, Pettigrew and Tropp (2006) are able to estimate the effects of each condition separately, with the exception of the support of authorities condition. Here they find that neither condition alone has a significant effect on the contact treatment effect, nor is either condition predictive of effect size when all are included in the same regression. Taking this evidence seriously, the implication is that no single condition alone can enhance the positive effects of contact. My experimental results, albeit in one particular setting, go against this.

[11]An important small-scale precursor of my study is the well-known Robber's Cave Experiment of Sherif et al. (1961). The study randomly assigned 22 boys to two groups at a summer camp, demonstrating both that (i) competition between groups for resources leads to conflict, and (ii) common (or superordinate) goals between the groups reduces conflict. In contrast to my paper, this study involved newly formed groups, rather than existing groups in conflict. The latter is more relevant for policy.

ing, interactions with other-caste teammates are 50 percentage points less likely to be hostile (column 6).

These different types of interactions can generate different treatment effects of contact through both belief-based and preference-based channels. On the beliefs side, participants may make inferences about the nature of the outgroup that depend on the outgroup behavior they observe (Levy and Razin (2018)). These inferences may diverge strongly if participants make attribution errors – participants grow to like other-caste teammates and dislike other-caste opponents since they fail to account for the fact that other-caste teammates have incentives to be cooperative, while other-caste opponents have incentives to be hostile (for a formal model, see Appendix B).[12] In contrast, the two types of contact may give similar information along other outgroup dimensions, like cricket ability. My first set of outcomes on willingness to interact map to these ideas – in particular, I find that common goals are necessary for positive effects on cross-caste social interaction, but not for positive effects on cross-caste team formation.

On the preference side, the interactions that common goals incentivize may mediate effects on preferences if participants develop habits for cooperating with or competing against outgroup members (Becker and Murphy (1988)) or if participants choose their preferences (or "worldviews") to rationalize their cooperative or competitive behavior with the outgroup (Bernheim et al. (2019)). I find evidence for such preference change in Section 5.2 – collaborative contact reduces own-caste favoritism in a voting exercise.

Each of these channels may have implications for the overall effects of different types of intergroup contact on economic efficiency. For example, shifting preferences toward intergroup cooperation could reduce barriers to intergroup trade, allowing groups to exploit gains from trade. Otherwise, positive effects on beliefs about the trustworthiness of outgroup members can increase efficiency in intergroup agreements that require trust to be enforced. I explore these efficiency effects on trade and trust in Section 5.3, while I give a full description of my main outcomes earlier in Section 4.4.

3 Background on Caste and Cricket

3.1 Caste: Past and Present

Caste Origins. The Indian caste system dates back to as far as 1500 BCE. According to the *Manusmriti*, an ancient Hindu legal text, individuals belong to one of four ordered social categories, called *varnas*: Brahmins, Kshatriyas, Vaishyas, and Shudras, with the lowest social group, the untouchables, outside of this class system altogether. Each of these groups contains hundreds of sub-groups, called *jatis*, within

[12] Motivated beliefs that persist after the leagues have ended could cause a similar divergence (Bénabou and Tirole (2016)).

which Hindus historically must marry. In addition to endogamy, the caste system features norms of contact between the groups (e.g. whether food can be shared), residential segregation, and traditional occupations (Ghurye (1932); Oh (2019)).

Though the core of the caste system rests with the endogamous *jatis*, the government categories of General, Other Backwards Castes (OBC), and Scheduled Castes/Scheduled Tribes (SC/ST),[13] are natural groups to consider when studying discrimination in India (Munshi (2016)).[14] These groups follow a traditional hierarchy – with General above OBC, and OBC above SC/ST. In this paper I use "cross-caste" to refer to interactions between these three groups,[15] and unless stated otherwise, all subsequent references to caste refer to one of these three groups.

Discrimination. Despite decades of illegality under the Indian Constitution, discrimination of lower castes (or "untouchability") continues to be widespread. Thirty-seven percent of General and OBC households in Uttar Pradesh (25% in India), the Indian state where I ran the experiment, practice untouchability (Desai et al. (2011)). On the opposite side, 27% of Scheduled Caste households in Uttar Pradesh report experiencing untouchability in the past five years (19% in India). Despite persistent discrimination, there is evidence that affirmative action has improved the economic status of low castes. For example, the median wage premium of non-SC/STs relative to SC/STs fell from 36% to 21% during 1983 to 2004 (Hnatkovska et al. (2012)).

General Segregation. Castes are segregated through marriage, geography, and social networks. Marriage segregates because endogamy is widely practiced – 98% of married women respondents in Uttar Pradesh married within caste (Desai et al. (2011)). Though many castes often reside in the same village, geographical segregation results from castes living in separate hamlets. Reflecting these living arrangements, though each *jati* makes up on average 6% of a village's population across major Indian states, roughly 50% of food transfers and loans come from within the same *jati* (Munshi and Rosenzweig (2015)). Cross-caste interactions that exist are often adversarial – 52% of households in Uttar Pradesh report that there is some or a lot of conflict between *jatis* in their village (Desai et al. (2011)).

Study Segregation. Figure 1 illustrates the social segregation at baseline for two of the eight league locations. Average caste-based homophily (following Jackson (2010)) is 1.92 – study participants are

[13] Erstwhile untouchables, and some others, were classified as Scheduled Castes (SC), with indigenous tribes classified as Scheduled Tribes (ST).

[14] To focus on caste and not religion, I only considered villages with few or no Muslims for the experiment. In practice, only 2.9% of participants were Muslim. These participants could still be assigned a caste given that Muslim communities are also formally classified as General, OBC, or SC/ST.

[15] The grouping of SCs and STs together is reasonable given their similar histories of discrimination and given that only 1.6% of participants in this study are STs.

roughly twice as likely to form friendships with a participant from the same caste than with a participant in general.[16] In addition to homophily, there is hierarchy: General castes have more friends from OBC than SC/ST, while SC/ST have more friends from OBC than General castes.

Salience. Though we might expect caste tension to be weaker among the young than the old, qualitative reports from this study suggest that caste remains highly salient even among the young. Prior to the teams being chosen, one General caste participant requested that he be assigned to a team with players only from his own hamlet, remarking that if he were assigned to a team with a *chamar* (a Scheduled Caste *jati*) he would "beat them a lot." Another General caste participant said "I will assist those from my own caste, and beat the *chamars*. My whole day goes bad when I see face of a *chamar*." On multiple occasions General caste participants requested not to be shown the photos of the "*chamars*" when asked to select their friends. Similarly, one SC/ST participant, upon seeing the photos of General caste participants, asked the surveyor to "scroll through these Tiwaris and Pandits [General castes] quickly."

3.2 An Introduction to Cricket

What is Cricket? The experiment used cricket, a team-based, bat-and-ball sport, as a means of integrating men from different caste groups.[17] Cricket is similar in structure to baseball. Each team usually comprises eleven players, though in the experiment each team consisted of only five players, to maximize statistical power. Each team takes turns to either field or bat. In the experiment, each match lasted 40 minutes on average. When fielding, the team nominates one player to be the bowler and one to be the wicket-keeper (similar to the pitcher and catcher, respectively, in baseball). The bowler throws the ball toward the wickets, which are a set of three wooden stumps (Figure A1). The wicket-keeper stands behind the wickets ready to receive the ball. The three remaining team members play the role of fielders, working together to collect the ball. When batting, only two members of the team play at any one time, both as batsmen. The batsmen attempt to score as many "runs" as possible, which they do by hitting the ball and then running between the wickets, or by hitting the ball sufficiently far (rolling past or flying in the air beyond the "boundary") such that they score a four or a six. The fielding team attempts to minimize the number of runs the batting team scores by, for example, hitting the wickets when bowling (meaning the batsman at that end is "dismissed").

[16] For comparison, Jackson (2010) finds race-based homophily in US high schools to be lower, at 1.4 on average.

[17] Since participation was restricted to men only, a limitation of this particular study is that the findings may well not generalize to women. That said, some evidence suggests that caste discrimination is as common among women as among men – in particular, in household-level data, female-only households are actually around 10 percentage points more likely to practice untouchability than male-only households (Desai et al. (2011)).

Why Cricket? The nature of cricket provides several advantages for this study. First, popularity across castes makes high participation possible, mitigating selection concerns – among study participants, 81% play cricket at least two times per week. Second, cricket tournaments are common in the study area, making the intervention naturalistic – at baseline, 38% of study participants were aware of a local cricket tournament held in the past 12 months. Third, features of cricket make contact treatments natural: teams have to be formed, and teams must face opponents.[18] The collaborative nature of sport in general was apparent to Gordon Allport, who wrote in *The Nature of Prejudice*:

> Only the type of contact that leads people to do things together is likely to result in changed attitudes. The principle is clearly illustrated in the multi-ethnic athletic team. Here the goal is all important: the ethnic composition of the team is irrelevant. It is the cooperative striving for the goal that engenders solidarity. (Allport (1954))

Types of Contact in Cricket. Players on the same team share the common goal of winning the match, and must collaborate to achieve this goal. To succeed when batting, batting partners must communicate, discussing when and how much to run between the wickets. When fielding, all team members are on the field, and to succeed they must cooperate with the bowler and wicket-keeper, who call to receive the ball from where it was hit. At half-time, each team gathers together for a team talk, ostensibly to strategize how to play in the second half of the match. In addition, teams achieve their common goal by playing competitively against their opposition – bowling fast, batting hard, and challenging decisions that the umpire (referee) makes in the other team's favor.

4 Experiment Design

4.1 Recruitment and Baseline Activities

Site Selection. I selected eight gram panchayats[19] (GPs) near Varanasi, Uttar Pradesh, from among 100 GPs visited by the field team. The selected GPs satisfied several desirable criteria, including: the presence of caste-segregated hamlets, a supportive elected GP leader, roughly equal caste proportions,

[18]The idea that such cricket-based contact might unite castes is even present in Indian culture: in the famous Hindi film, *Lagaan* (2001), villagers are persuaded by their desire to win to allow an untouchable to play on their team.

[19]Gram panchayats are local administrative unit comprising several villages.

and an available cricket field.[20] I organized one cricket league per GP, with the matches played from January to July 2017. The experiment design and detailed timeline for a given league is detailed in Figure 2.[21] The subsequent details in this section track this timeline closely.

Recruitment and Baseline. In each GP, surveyors spent the first six days recruiting men aged 14 to 30 to play in the upcoming cricket league. We advertised the basic details of the leagues using posters (Figure A2), and via direct contact from Sarathi Development Foundation (our NGO partner) staff. The information made clear that teams would be chosen randomly by the organizers and not by the participants themselves.[22] By targeting particular hamlets, we kept recruitment roughly equally balanced across the three caste categories.[23] Men who expressed interest completed a baseline survey and were informed that their sign-up was not complete until their cricket ability was tested.

Study Construal. I minimized references to caste in the survey instruments. I did so to avoid priming (as in Hoff and Pandey (2014)), social desirability bias (Paluck and Shafir (2017)), and threats to the experiment's implementation from local resistance. In this spirit, surveyors told participants during baseline that "we are recruiting men interested in playing in cricket tournaments for money. Our aim is to use cricket tournaments to bring the community together, and to study how cooperative and competitive men are in rural India." Similarly, when introducing the trading exercise, surveyors told participants that "the trading game will allow us to study trading and cooperative behavior in Indian villages."

Ability Testing. Following the six days of recruitment, surveyors spent six days testing the cricket ability of each participant. Cricket ability was measured along three dimensions: bowling, batting, and fielding. For bowling, participants bowled six balls towards the wickets, and a surveyor measured the speed using speed guns. For batting, a surveyor bowled six balls towards the wickets, and the participant attempted to hit each ball. The surveyor recorded whether each ball was hit, and if so whether it was hit sufficiently far to score either a four or a six. For fielding, a surveyor threw six balls high in the air towards the participant. The surveyor recorded how many balls were successfully caught. Each team's ability results were made common knowledge within the team by a surveyor who read out the results in the minutes

[20] Secondary criteria included: large population of interested cricketers, few or no Muslims, not used in piloting, and no cricket tournament running at the same time.

[21] I describe differences between the paper and the pre-registered study details in Appendix C. Most notably, the pre-registration emphasizes the variation in the "type of contact" driven by randomization of monetary incentives. Partway into the experiment I realised that an additional useful source of variation existed (teammates vs. opponents), and later that this variation affected the nature of cross-caste interactions whereas the monetary incentives did not. As a result, I report results from both sources of variation in the present paper, and describe and attempt to reconcile the different effects in Section 7.2.

[22] This approach may have screened out those more prejudiced against other castes. Unfortunately I do not have any measures of intergroup behavior for non-participants to be able to test for such selection.

[23] Of the 1,261 participants, 32.7%, 35% and 32.3% were from General, OBC, and SC/ST castes respectively.

immediately prior to the team's first and second matches. Teams could opt-in to hearing the results again from the third match onwards but did so for only 7% of the matches. The ability measures are strongly predictive of league performance, as shown in Section 7.3. For some of the analysis, I create an individual-level ability index as the average across three standardized measures: maximum bowling speed, number of fours or sixes when batting, and number of catches when fielding. 1,261 participants completed both the baseline and the ability testing.

Social Networks. Once the participants were finalized, I administered a short social network survey. Each participant was shown a list of the full names and photos of all other participants and asked which they considered to be friends. Though caste cannot be visibly discerned, it is usually signalled strongly by the last name a person uses. When participants are asked to guess the caste category of a hypothetical name at endline, they correctly identify the name as belonging to the same or a different caste 80% of the time. This figure represents a lower bound on caste recognition during the experiment itself, since beyond observing last names, participants may recognize the photo and correctly infer caste through knowing what hamlet the individual lives in. Due to time constraints, 93% of the 1,261 participants completed the social network survey prior to treatment assignment.

4.2 Randomization

League Assignment. In each of the eight GPs, I randomly assigned 100 participants to play in the cricket league. I stratified this randomization on caste and selected a well-balanced draw from among 100 re-randomizations to avoid other chance imbalances (following Banerjee et al. (2017)).[24] I assigned the remaining participants to the control group.

Backup Protocol. Cricket matches are difficult to play without a full roster of players. Since 100% match attendance could not be guaranteed, control participants served as backup players. To preserve a comparison group with very few matches actually played, I followed a strict backup protocol. I assigned a priority number randomly to each backup, within each caste. If a particular player could not attend one of his matches, surveyors called a backup player from the same caste in priority order. This protocol ensured that only high-priority backups played frequently – while the three highest-priority backups played six to eight matches on average, the remaining backups played far fewer (Figure A3). This protocol has three advantages. First, since I chose the priority order randomly conditional on caste, the low-priority backups serve as a valid control group within each caste. Second, by replacing absent players with someone of

[24] Further details of the randomization are in Appendix D.

the same caste[25] I kept the caste composition of each team constant, preserving the collaborative contact treatment. Third, the nature of the treatment for high-priority backups (in particular, the fact that they cycle in and out of different teams) helps me distinguish between different explanations for the results (see Section 6.2).

Team Assignment. For each of the eight leagues, I randomly assigned the 100 league players to 20 teams of five players each. 35% of the players were randomly assigned to homogeneous-caste teams, making seven out of 20 teams homogeneous-caste. I pooled and randomly ordered the remaining players. Each sequence of five then formed a mixed-caste team.[26]

Incentives. The survey team paid each player a cash incentive based on his cricket performance following each match. The exact type of monetary incentive was randomized. Of the 20 teams participating in each league, I randomized 10 teams to receive Individual Pay and the remaining 10 to receive Team Pay. Surveyors paid players on Individual Pay teams according to individual performance (giving on-team inequality) while players on Team Pay teams were paid based on team performance (giving on-team equality).[27] The variation in incentives allows a test of an additional Allport (1954) condition: that of a lack of intergroup competition. Team Pay reduces intergroup competition for pay on each team, by ensuring pay equality. Individual Pay increases intergroup competition for pay, giving incentives to "jockey for position" to ensure enough play-time to make money. If intergroup competition matters in the way that Allport (1954) hypothesized, we would expect the positive effects of collaborative contact to be greater for teams that receive Team Pay.

Match Schedule. I scheduled each team to play eight matches, never playing the same team more than once. This scheduling problem is identical to the network problem of choosing a random simple regular graph. In this case, each of the teams represents a node in a network. A k-regular graph is a graph where each node is connected to exactly k others. If this graph is simple, no node is connected to itself (no "loops") or to another node more than once (no "parallel edges"). A match schedule in which 20

[25] Of all cases of absent players, 99.9% were replaced with a backup player of the same caste.

[26] There were 104 mixed-caste teams in total. In principle, homogeneous-caste teams could have occurred here by chance, but none did, leaving the total number of homogeneous-caste teams at 7*8 = 56.

[27] More specifically, the Individual Pay incentive scheme was as follows: when batting, if a player scored one run, he earned Rs. 2.5 (~$0.04). When bowling, if a player got a wicket, he earned Rs. 35 (~$0.50). In this way, individuals on the same team were paid based on their *own* performance, creating some incentive to compete with one another (e.g. by vying for the first slot in the batting order, or for the chance to bowl, in order to make more money). In contrast, players on Team Pay teams were paid equally: if a player scored one run when batting, each player on his team earned Rs. 0.5 (~$0.01). If a player got a wicket when bowling, each player earned Rs. 7 (~$0.10). Conditional on the same performance, a Team Pay team earned the same aggregate payout as an Individual Pay team, but the distribution across players within the team was equalized. As expected, Individual Pay players had much more dispersed payouts (Figure A4).

teams each play eight matches (never playing themselves and never playing against a given team more than once) can be represented by a simple 8-regular graph with 20 nodes. I randomly chose a graph for each league using an existing algorithm, Bollobás' "pairing method" (see Appendix D for details). The algorithm generated an adjacency matrix for each league, representing which teams were to play which. With these matrices I scheduled 80 matches per league, with the matches randomly ordered. The randomness of the match schedule ensured that a given player's exposure to other castes as opponents was also random.[28] Together, the assignment to teams and random match schedule created significant variation in collaborative and, albeit less so, adversarial cross-caste contact (Figure A5).[29]

4.3 Implementation Period

Treatment Announcement. During the two-day period following treatment assignment, surveyors called each participant to explain their treatment assignment over the phone. If the participant was assigned to play in the league, the surveyor read out the full name and father's name of each teammate. Surveyors also elicited cricket ability priors by asking league-assigned participants to predict the eventual ranking of themselves and their teammates according to batting strike rate, a commonly used measure of batting ability. The prediction was incentivized – at the second endline we paid Rs. 50 (~$0.80) to those that guessed the ranking correctly. I use the predictions to explore ability belief updating as a possible mechanism for the effects of contact on favoritism.

League Logistics. Each league ran for roughly three and half weeks following the phone calls. Match attendance averaged 75.6%.[30] Each match also required an umpire to make final decisions. I allowed men to sign up to be players, umpires, or both. Seven signed up to be umpires exclusively. I used these men as umpires, but not as part of the sample of 1,261 for which I measured outcomes. Of the 1,261 that completed their sign-up as players, 281 also signed up to be umpires, of which 156 umpired at least one

[28]More precisely, it is random conditional on the caste composition of his own team. For example, if a player has four other-caste men on his team, he is less likely to be exposed to other-caste opponents than a player with only one other-caste man on his team. All analysis of adversarial contact effects below controls for on-team cross-caste exposure.

[29]I faced a tradeoff between naturalism and statistical power when creating variation in adversarial contact. For example, I could have increased variation in adversarial contact (and statistical power) by having each team play the same opposing team eight times. Instead, I erred on the side of naturalism by having each team play a given opposing team only once. This design choice bears a cost: I have less statistical power to test for the effects of adversarial contact than the effects of collaborative contact.

[30]To address concerns of low attendance: (i) surveyors gave a Rs. 10 (~$0.15) show-up fee to each player for each match attended; (ii) I held a lottery for a cricket bat following the league for all those who attended at least six matches; (iii) I accommodated weather conditions and conflicting schedules by adjusting match times; and (iv) I required participants to have a phone number in order to sign up, and surveyors called these phone numbers the day before each match and on the day itself to remind players to attend.

match. In addition to the umpire, one surveyor observed each match and recorded interactions between players using the *Match Observation* survey (as referenced already in Table A1). After each league ended I held an awards ceremony at which the best three teams (according to the final league table, see Figure A6) and players (based on number of times voted man-of-the-match[31]) were given trophies and cash prizes.

Recognizing Caste. To avoid explicit references to caste, participants were not directly told the caste group of their teammates and opponents. However, several features of the experiment enable caste to be identified implicitly. First, the full names and father's names of teammates conveyed over the phone strongly signal the caste of each teammate. Second, close interaction with teammates on the pitch, including mandatory team talks, gives opportunities for teammates to learn each other's caste. Third, the catchment area for each league is sufficiently small that players can recognize their teammates and opponents, even if they are not friends – indeed when players are asked on the phone whether they know of their randomly assigned teammates, 39% say yes when the teammate is from the same caste, while still 27% say yes when the teammate is from a different caste. The corresponding figures for baseline friendships are 15% and 4%, suggesting that even though participants are far more likely to be friends with members of their own caste, they are not that much more likely to know them than participants from other castes. Fourth, during the matches the full names of bowlers are called out whenever the bowler is to be changed. Fifth, spectators at the matches[32] frequently call out the names of players, and sometimes refer to players using caste slurs.

4.4 Main Outcomes

I measured three classes of outcomes during two endline surveys (*Endline-1* and *Endline-2*) one to three weeks after the completion of each cricket league: (i) willingness to interact; (ii) caste favoritism; and (iii) efficiency. These outcomes were measured for all participants, except own-caste favoritism, which was not measured for the control group due to time constraints.

(i) Willingness to Interact

Social Interaction. During *Endline-2* participants scrolled through a randomly-ordered list of all other participants in their location, seeing each participant's photo and full name. Surveyors asked them to select the participants they would like to spend more time with in the future (*Want to Interact w/*). Restricting responses to the people they listed, surveyors then asked them to select those they considered

[31] This voting occurred immediately after each match.

[32] On average, 17 spectators attend each match.

friends (*Friends*). By matching selections to the caste of each person, I calculated the total number of other-caste men selected for each question. By matching selections to the full network data on which players were teammates and which were opponents, I am able to distinguish between effects of contact on individuals played with versus those other-caste men not directly met.

Team Formation. To capture willingness to interact on the cricket field, a decision which depends more on beliefs about ability, during *Endline-1* surveyors told participants in each league that there would be two additional matches played two to three weeks later. One match would have stakes: there would be Rs. 500 (~$8) awarded to the winning team. The other match would not have stakes: both teams would receive Rs. 250 (~$4) regardless of their performance. Surveyors asked participants to select their team twice: once for the match with stakes, and once for the match without. They selected their team by scrolling through the entire list of participants,[33] again seeing their full names and photos. I then randomly selected four players per league (~1.25% probability) to have one of their two team choices implemented, making them the captain of their chosen team for one of the additional matches.[34] I used the team choice data to calculate the number of other-caste teammates chosen by each participant. By having participants choose a team for matches both with and without stakes, I varied the strength of the main feature of team formation that is distinct from social interaction: that participants had an incentive to select those who will play the best cricket.

(ii) Favoritism

Voting. Beyond willingness to interact, caste differences may affect welfare and allocation through ingroup favoritism (Burgess et al. (2015)). I measured own-caste favoritism with a voting exercise during *Endline-2*. Surveyors informed league participants that one member of each team would be selected to go on a field trip for professional cricket coaching.[35] The field trip was popular: 96% said they would go if they were selected and were available. The selection was decided by vote. Each participant ranked players on four other randomly-chosen teams (two opposition teams and two non-opposition teams) from one to five, based on his preferences as to who should go on the field trip. I randomized the order in which they ranked the four teams. Surveyors explained to participants in basic terms that a Condorcet winner would be selected if one existed, and otherwise the winner would be decided by Borda count. The survey team encouraged participants to vote honestly regardless of their understanding of the voting rule, and

[33] In this case, the list was alphabetically ordered to help participants find their four favorite teammates quickly.

[34] This approach is related to the "random-lottery incentive system," popular among experimental economists (see Sprenger (2015) for an example). Though critiqued by Holt (1986), the method has been defended since by Starmer and Sugden (1991) and Cubitt et al. (1998), who find empirically that subjects treat decisions in isolation, alleviating Holt's concern that subjects would treat the choices as a grand meta-lottery.

[35] All field trips were held in September 2017, roughly one month after the final *Endline-2* survey was taken.

explicitly told them that cricket ability need not factor into their decision – they should just rank higher the players they most prefer. I designed this voting exercise to give a naturalistic measure (given the cricket intervention) of caste favoritism in the allocation of a desirable prize.

(iii) Efficiency

Trading. I designed a new trading exercise to measure efficiency impacts through contact changing barriers to cross-caste interaction. For this exercise, surveyors visited all participants at their homes for *Endline-1*, and gave them each two goods: a pair of gloves and a pair of flip-flops, each worth roughly Rs. 100 (~$1.50). The pairs were intentionally mis-matched – the participant either received two left-hand or two right-hand gloves, and two left-foot or two right-foot flip-flops. The mis-matching created gains from trade. To provide further gains from trade, surveyors gave participants monetary incentives. Half of the participants earned Rs. 10 (~$0.16) for each successful trade, while the rest earned Rs. 20 (~$0.32).

In addition, surveyors gave incentives for "color-switching" to create obfuscated *cross-caste* gains from trade. Each good had a sticker of one of three colors affixed to it. The three colors were assigned to very strongly, though not perfectly, correlate with caste. We informed participants that different colors would be more difficult to find, but not that colors correlated with caste.[36] I randomly selected half of the participants to receive this color-switching bonus, with half of these promised Rs. 50 (~$0.80) and half promised Rs. 100 (~$1.60) per good. The color-switching bonus incentivized cross-caste trade without requiring explicit references to caste. This incentive serves two purposes: (i) it can be used to "price" the effects of treatments, and (ii) by creating gains from specifically *cross-caste* trade, providing this incentive permits a test of the efficiency effects of contact. Surveyors logged successful trades during *Endline-2*.[37] If any of the IDs on the final gloves/flip-flops were initially assigned to a participant of a different caste, I classified this participant as having made a cross-caste glove/flip-flop trade.

Trust. To explore efficiency effects in the absence of face-to-face interaction I included in *Endline-2*: (i) a standard trust game (as created by Berg et al. (1995) and used more recently in India by Castilla (2015)), and (ii) a World Values Survey question on whether the participant thinks that most people can be trusted, or that you need to be very careful in dealing with people (as used in Alesina and La Ferrara (2002), Algan and Cahuc (2010)). For the trust game, I partnered each participant with three men from another village

[36] Participants may have been able to infer the caste-color correlation, though debriefs with surveyors suggest that this rarely happened.

[37] I took two steps to reduce the possibility of fraudulent reporting. First, surveyors took photos of the sticker with the ID on the final gloves and flip-flops. This approach reduced the possibility of collusion between surveyors and participants since surveyors could later be audited if the photo did not match the code entered. Second, after a trade was catalogued, the surveyor removed and destroyed the sticker so that it could not be used again. In practice, there were no reported cases of fraudulent trades.

– one General caste, one OBC, and one SC/ST. Participants played the role of the Sender. Senders were allocated Rs. 50 (~$0.80) (only with some probability, explained below) and decided how much of the Rs. 50 to transfer to another person, the Recipient. Any money transferred was to be tripled. After the transfer took place, the Recipient decided how much money to return. The money returned would not be tripled. The amount of money that participants send to their partners proxies for trust of own and other castes, and given that the partners are strangers from another village, this measure immediately answers the question of whether or not contact effects extend to the caste group. Furthermore, since the social optimum would require the full amount to be transferred, we can interpret positive treatment effects as increases in efficiency.

Surveyors told Senders and Recipients the age and full name of the other, though a different first name was substituted to keep the exact identity of each player secret. This secrecy was common knowledge to both players. I chose as Recipients men with last names that both strongly signalled caste and that were relatively common among the participants in the Senders' league. Surveyors did not give the Senders Rs. 50 up front, but rather asked them to state how much of the Rs. 50 they would transfer, should they be given it, to each of the three Recipients (in random order). I randomly chose 20% of the participants to have one of their three trust choices implemented. Participants were informed that their transfer would happen for at most one of the three Recipients they had been assigned. Given the complexity of the task, participants also answered several comprehension questions before reporting their choices.

4.5 Empirical Specification

To test for the effects of the two types of contact in Section 5, I focus on the subsample of participants randomly assigned to play in the leagues ($N = 800$), and primarily use the following empirical specification:

$$y_{icl} = \alpha_{cl} + \beta \text{Prop. Oth. Caste on Team}_{icl} + \gamma \text{Prop. Oth. Caste of Opponents}_{icl} + \eta \text{X}_{icl} + \varepsilon_{icl} \qquad (1)$$

where y_{icl} denotes outcome y for participant i from caste $c \in \{\text{General, OBC, SC/ST}\}$ playing in league l, α_{cl} are caste-by-league fixed effects since these were used as strata for the randomization to teams, and ε_{icl} is the error term. $\mathbf{X}_{icl}$ are a vector of baseline covariates, detailed in the next subsection. To allow for correlated shocks within teams, I cluster standard errors at the team-level.

The collaborative contact treatment is Prop. Oth. Caste on Team$_{icl} \in \{0, 0.25, 0.5, 0.75, 1\}$, which is the proportion of player i's four teammates that belong to a different caste. β gives the causal effect of a player having all other-caste teammates instead of none. The adversarial contact treatment is Prop.

Oth. Caste of Opponents$_{icl}$, which ranges from 0.35 to 0.975. In this case, given the linearity assumption and extrapolation beyond the support of the variable, γ identifies the causal effect of a player having all other-caste opponents instead of none.

To test for the effects of league participation in Section 7.1, I use the full participant sample ($N = 1,261$) and the following empirical specification:

$$y_{icl} = a_{cl} + \phi_1 \text{Homog. Team}_{icl} + \phi_2 \text{Mixed Team}_{icl} + \phi_3 \text{High Backup}_{icl} + \theta \mathbf{X}_{icl} + e_{icl} \quad (2)$$

which compares the low-priority backups which played few matches (the omitted group) with the high-priority backups (High Backup$_{icl} = 1 \Leftrightarrow$Backup Priority Number$_{icl} \leq 3$), those assigned to homogeneous-caste teams (Homog. Team$_{icl} = 1 \Leftrightarrow$Prop. Oth. Caste on Team$_{icl} = 0$), and those assigned to mixed-caste teams (Mixed Team$_{icl} = 1 \Leftrightarrow$Prop. Oth. Caste on Team$_{icl} > 0$). I cluster standard errors at the team-level for those assigned to play in the leagues, and at the participant-level otherwise.

4.6 Randomization and Implementation Checks

Balance checks suggest that the randomization was successful. For the effects of contact (Table A2), two of twelve coefficients not affected by re-randomization (for age and whether in school) are statistically significant at the 10% level or more for the checks on the full sample (panel A), and likewise for the checks with the most restrictive analysis sample – participants with complete data for all endline outcomes (panel B). There are no statistically significant effects for column 1, the most important baseline variable to test for balance: the number of other-caste friends listed in the social network survey. That said, the signs go in the direction of the hypothesized effects, with a positive coefficient for collaborative contact and a negative coefficient for adversarial contact.

The balance checks for the program participation specification are similar (Table A3), with the only statistically significant difference being that high-priority backups have around one more other-caste friend at baseline (column 1). Given this imbalance, and the similar concern in Table A2, I control for the number of other-caste friendships at baseline throughout. In particular, since 7% of the 1,261 participants did not complete the social network survey prior to treatment assignment, I control for two baseline covariates: a dummy variable equal to one if the social network survey was not completed, and the number of other-caste friendships at baseline, set to -99 if missing. Given that baseline other-caste friendships are predictive of endline intergroup behaviors, this approach also increases precision.[38]

[38] The results are in any case very similar if I instead (i) control additionally for age and whether in school, (ii) control for all eight variables used for the balance checks, or (iii) include no controls.

Attrition is low at 6.8%, and not statistically significantly affected by either collaborative or adversarial contact. This lack of selective attrition holds for the full sample and for each caste separately (columns 1 to 4, Table A4). Similarly, there are no statistically significant effects on the number of matches attended, for the full sample or caste-wise (columns 5 to 8). Having other-caste teammates is not a deterrent to playing.

5 The Effects of Collaborative and Adversarial Contact

5.1 Willingness to Interact

Social Interaction. Collaborative and adversarial contact have opposite effects on cross-caste friendships (Figure 3, panel A of Table 1). Collaborative contact has a positive effect on desired future interaction with participants from other castes and cross-caste friendships. On average, those in homogeneous-caste teams want to interact with 7.1 other-caste participants in future, and are friends with 3.1. Moving from a homogeneous-caste team to a team with four other-caste men increases desired cross-caste interactions by 2.2 (31%), and cross-caste friendships by 1.1 (35%). In contrast, adversarial contact has a negative effect on these outcomes, larger in magnitude than the effect of collaborative contact. An increase in adversarial exposure from the least (35%) to the most (97.5%) leads to 2.1 fewer other-caste friends.[39] For each outcome, the equivalence of the effects of collaborative and adversarial contact can be rejected at at least the 95% significance level.

While the effects of collaborative contact are not mediated by the ability of players exposed to (in contrast to Carrell et al. (2019)), higher-ability other-caste opponents significantly reduce the negative effects of adversarial contact (panel A of Table 1, column 4). Given that teams with higher ability players also win more matches (as I show in Section 6.3), this suggests firstly that shared victories do not enhance the effects of collaborative contact. Second, losing matches to other-caste opponents is not likely driving the negative adversarial effects. Instead, it appears that high-ability other-caste opponents earn respect. Together, these results suggest that the nature of collaboration and competition *during* the matches determines the effects of contact, rather than the *outcomes* of those matches. Put another way, collaborative contact is about working together, not winning together.

The effects of contact are different when I consider instead exposure to people from the *same* caste[40]

[39] Both types of contact have relatively linear effects, with the exception of the negative effect of having one other-caste teammate instead of none (Figure 3, $p = 0.19$ for top panel, $p = 0.04$ for bottom panel). While this may give some suggestive evidence of a negative effect of collaborative contact when the contact is with a small minority, I do not find this non-monotonicity for any other outcomes.

[40] Where $\text{Prop. Own Caste on Team}_{icl} = 1 - \text{Prop. Oth. Caste on Team}_{icl}$ and $\text{Prop. Own Caste of Opponents}_{icl} = 1 -$

(panel B of Table 1). Collaborative contact with own-caste participants has a small positive effect on own-caste desired future interaction and friendships – the magnitudes are roughly one-half of the size of the cross-caste collaborative contact effects. This result is consistent with diminishing returns to contact: social networks are caste segregated to begin with, giving less scope for forming new network links with members of the same caste.

Own-caste adversarial contact has marginally significant *positive* effects – the opposite of the cross-caste effect. The point estimate of 4.5 for desired future interaction implies that for every 10 additional own-caste opponents faced, a participant wants to spend time with 1.1 more own-caste men in future. In this context, adversarial contact alone does not create friction, but *intergroup* adversarial contact does. Competing against ingroup members has a fundamentally different effect than competing against out-group members.

Individuals vs. Groups. To test whether the effects of contact extend beyond those played with, I explore effects of collaborative contact on friendships with non-teammates, and effects of adversarial contact on friendships with non-opponents.

For the effects of collaborative contact, I define the outcome as the percentage of other-caste friends among those assigned to play on other teams. This definition excludes all backup players, since some backup players will play as substitutes on the participant's team. No one in this set of people played in a match with the respondent. Effects of collaborative contact on friendships with these people are then not driven by direct contact as teammates.[41]

For the effects of adversarial contact, I define the outcome as the percentage of other-caste friends among very-low priority backups – those with a priority number of seven or above. There are 173 of these backups across the eight leagues, and they played an average of only 0.8 matches each. Since they played so little, they would only rarely have been played against as opponents. Any effects of adversarial contact on desired interaction with these people are unlikely to be driven by direct contact as opponents.[42]

Both collaborative and adversarial contact effects extend to the outgroup as a whole. Collaborative contact has a positive and statistically significant effect on desired future interaction and friendships with other-caste men in other teams (Figure 4). Adversarial contact again has negative effects ($p = 0.14$

Prop. Oth. Caste of Opponents$_{icl}$.

[41]Furthermore, by defining the outcome as the *percentage* of this set of other-caste participants listed as friends, I adjust for the fact that the size of this set differs systematically by treatment. For example, those with four other-caste teammates have four fewer other-caste participants in the relevant set than those with zero other-caste teammates. If collaborative contact does not matter, these two types of treated players will select on average the same percentage (but a different level) of other-caste other-team participants as friends.

[42]The results are similar if I instead define the outcome as the percentage of other-caste friends from among backups that played zero matches. This set of people is a select sample, but has the advantage of containing no opponent players at all.

for desired interaction, $p < 0.01$ for friendships). These effects are large: full collaborative exposure increases non-teammate cross-caste friendships by 0.15 of a standard deviation of the outcome. The effect is stronger for adversarial contact: an increase in adversarial exposure from the least to the most reduces non-opponent cross-caste friendships by 0.42-0.85 of a standard deviation.

In addition, the effect of collaborative contact on non-teammate friendships is much stronger for non-opponents than opponents (Table A5). This fact suggests that the generalized effects do not come through other-caste teammates introducing players to other-caste opponents.

Network Access. Another type of introduction might matter – players may get introduced to the other-caste friends of their other-caste teammates, causing positive effects of collaborative contact beyond direct interactions. Related, players may lose access to the other-caste friends of their other-caste opponents after facing them in a match. In each case, generalized effects of contact may come through network effects, rather than through players updating their beliefs about the outgroup. I use outcomes at the dyad-level to test directly for the network access mechanism, with the following specification for collaborative contact:

$$y_{ij} = \left(\alpha_{jcl} \times \text{Prop. Oth. Caste on Team}_{icl}\right) + \beta_1 \text{Teammate}_{ij} + \beta_2 \text{Friend of Oth. Caste Teammate}_{ij} + \zeta \mathbf{X}_{ij} + \varepsilon_{ij} \quad (3)$$

where y_{ij} is a dummy variable equal to one if participant i listed j as a friend, α_{jcl} are caste-by-league (of participant i) fixed effects fully interacted with participant j fixed effects, and these fixed effects are fully interacted with the categories of Prop. Oth. Caste on Team_{icl}. In parallel with equation 1, the vector $\mathbf{X}_{ij}$ contains a dummy variable equal to one if i listed j as a friend at baseline, and a dummy variable equal to one if i did not complete the social network survey prior to treatment assignment.

The two remaining regressors are dummy variables: Teammate_{ij} is equal to one if j is a teammate of i's (a direct link), and Friend of Oth. Caste Teammate_{ij} equals one if j is a friend (using baseline data) of any of i's other-caste teammates (an indirect link). β_1 gives the causal effect on friendship of being directly linked (as a teammate) with a member of a different caste. β_2 gives the causal effect on friendship of being indirectly linked (through a teammate's existing friendships) with a member of a different caste. Standard errors are dyadic-robust, allowing residuals to be correlated between any two dyads with a team in common.

For this specification, I restrict only to observations where i and j belong to different castes, and where i is in a mixed team (with Prop. Oth. Caste on $\text{Team}_{icl} > 0$). The intuition behind the specification is shown visually in Figure A7. In brief, β_1 is identified by comparing two players that share the same caste, league, and collaborative exposure, but belong to different teams. Suppose these players are i and i', and that player k is a teammate of i's, but not i''s. The effect is estimated by asking "how much more likely is it that i is friends with k after the league is over than i' is?" Similarly, suppose that there is some player j who is an other-caste friend (at baseline) of one of i's other-caste teammates, but not linked to any of i''s other-caste teammates. β_2 is estimated by asking "how much more likely is it that i is friends with j after the league is over than i' is?" Each of these effects is causal since the randomization to teams ensures random assignment of both direct and indirect links (conditional on fixed effects).

I use a parallel specification to test for network effects of adversarial contact:

$$y_{ij} = \left(\alpha_{jcl} \times \text{Prop. Oth. Caste of Opponents}_{icl}\right) + \theta_1 \text{Opponent}_{ij} + \theta_2 \text{Friend of Oth. Caste Opponents}_{ij} + \mu \mathbf{X}_{ij} + u_{ij} \quad (4)$$

where for this specification, I restrict to observations where i and j belong to different castes, and where i is assigned to league participation, since Prop. Oth. Caste of Opponents$_{icl} > 0$ for all league participants.

Turning first to the collaborative network effect, assignment to be teammates with a player increases the probability of wanting to interact with that player in future by 24 percentage points (column 1, Table A6) and friendship by 13 percentage points (column 2). In contrast, the effect of being indirectly linked to other-caste players through other-caste teammates is a precise zero for both outcomes. Friendship effects beyond teammates do not come through network access – players are not getting introduced to the friends of their other-caste teammates. Instead, the effects are more consistent with players changing their general beliefs about the other caste groups. For opponents, both the direct and indirect network effects are insignificant and close to zero (columns 3 and 4). This suggests that the entire negative effect of adversarial contact on demand for social interaction is driven by generalization about those not interacted with.

Taken together, the results on social interaction suggest that the two types of contact have opposite effects on inferences about the cooperativeness of other castes.[43] In turn, these effects are likely driven by the experiences of collaboration and competition, and less so by the outcomes of the matches themselves.

Team Formation. While the type of contact may matter for belief updating along some dimensions, like cooperativeness, it may matter much less for others. I explore this possibility by looking at effects on team formation. Here the choices are payoff-relevant and depend much more on knowledge about the cricket ability of other-caste men.

Both collaborative and adversarial contact have positive effects on cross-caste team formation for the match with stakes, with a similar estimated effect: $\hat{\beta} = 0.71$ for collaborative contact ($p < 0.01$), and $\hat{\gamma} = 0.90$ ($p = 0.05$) for adversarial contact (Figure 5, Table A7). These effects are 47 to 60% of the mean

[43] To the extent that these inferences are unrelated to actual "productivity", we could think of the two types of contact as having opposite effects on taste-based discrimination (Becker (1957); Rao (2019)).

of 1.5 other-caste men chosen, and are somewhat positively mediated by the ability of teammates and opponents (columns 3, 4).

To benchmark the collaborative effect, if players chose teammates randomly, they would choose other-caste players 67% of the time. In contrast, homogeneous-caste teams choose other-caste players 29% of the time, whereas those with four other-caste teammates choose other-caste players 47% of the time. Full collaborative exposure closes roughly half of the gap between the choices of homogeneous-caste team players and the random benchmark.

Unlike the effects on friendships, the adversarial contact effect on other-caste teammate choice is positive. Though adversarial contact creates animus, it still conveys knowledge about the ability of outgroup members. The net effect of the animus and knowledge is a greater willingness to work together with men from other castes. Given the social interaction outcomes, this suggests that the type of contact may have different implications for effects on future *social* integration versus future *economic* integration (e.g. as in Miller (2017)).

A natural interpretation of this set of results is that while the two types of contact have opposite effects inferences about cooperativeness, they both reduce ability-based statistical discrimination (Arrow (1973); Aigner and Cain (1977); Cornell and Welch (1996)). Given this interpretation, we might expect that removing the stakes for the bonus match should weaken the adversarial effect much more than the collaborative effect – without stakes, motives should shift away from picking the best cricketers, and toward picking those that are fun to play with, reducing the adversarial contact effect more. This pattern is borne out in the data. Both types of contact have significantly weaker effects when stakes are removed. However, while collaborative contact continues to have a positive significant effect, the adversarial contact effect falls significantly more ($p = 0.06$), to 0.13 (bottom panel of Figure 5, Table A7).[44]

The results on willingness to interact show that the type of contact mediates some belief-based channels, but not others. In particular, while the type of contact matters for future social integration, it matters much less for future economic integration, where incentives for interaction are motivated primarily by beliefs about ability.

[44] Contact is less likely to affect non-teammate and non-opponent other-castes being chosen as teammates given the structure of the exercise. Participants can only select a maximum of four players, meaning that a participant who selects several players from their own team may have no slots left for other-caste players from other teams, even if they have become less biased. Despite this limitation, those with more other-caste players on their team also pick more other-caste players from *other* teams for a future team for the match with stakes (Table A8), despite not having played with them, and despite not knowing more about their ability. In contrast, those with more other-caste opponents are not more likely to pick other-caste non-opponents for a future team. This is consistent with adversarial contact revealing the ability of individual players, such that they get selected. Collaborative contact reveals ability, but it also shifts willingness to cooperate with other castes more generally.

5.2 Favoritism

Voting. To explore whether contact molds preferences, I estimate own-caste favoritism using data from the incentivized voting exercise. General castes show the most own-caste favoritism in voting, followed by OBCs – even conditional on age and three ability measures (all of which are predictive), General castes and OBCs rank players from their own caste significantly higher (columns 1 and 2, Table 2). General castes on average rank someone from their own caste 0.78 positions higher – this favoritism is larger than the effect of the votee being a full two standard deviations better in bowling, batting, and fielding ability. The own-caste favoritism of SC/STs is small and statistically insignificant (column 3), though this may signal that non-caste-related unobservables remain even after controlling for ability, motivating specifications that include fixed effects for the player voted on (columns 4 to 6).

Pooling all castes, and including votee fixed effects, the favoritism amounts to ranking own-castes 0.4 positions higher (column 4). Furthermore, this favoritism is not merely driven by players having more friends from their own caste, and showing favoritism toward them – the coefficient is similar when controlling for pairwise friendship links at baseline (column 1, Table A9).

Collaborative contact reduces own-caste favoritism by up to one-third of the mean (column 5, Table 2, $p = 0.08$). This effect is stronger when considering only votes for non-opponents, consistent with effects on the demand for social interaction (column 6).[45] This result complements the results on social interaction and team formation. For both measures, collaborative contact leads to effects on other castes not interacted with, but in the voting exercise, the effect is more likely to imply a shift in social preferences. In contrast, the adversarial contact effect is not statistically significant (columns 5, 6), but given imprecision, equality between the collaborative and adversarial effects cannot be rejected ($p = 0.57, 0.74$).

Ability Beliefs. The effect of collaborative contact on caste favoritism in voting could be explained by shifting social preferences or ability-based statistical discrimination. In particular, the latter might predominate if contact corrects incorrect beliefs about the cricket ability of other-caste players. This belief correction could cause participants to rank other-caste players relatively higher. Though I cannot completely rule out this explanation, it seems unlikely for two reasons.

First, ability beliefs are not more incorrect for other-caste players than own-caste players at baseline (Table A10). In particular, players are confident in their own abilities – predicting their ranking (from 1 to 5) to be 0.57 ranks better than their teammates on average (column 1). Among their four other teammates, they predict that the other-caste players will be 0.15 ranks worse on average. These baseline rankings are

[45] The reduction in favoritism is not driven only by a reduction in favoritism towards friends (columns 2 and 3, Table A9) – the treatment effects on "friendship favoritism" are insignificant and of inconsistent sign. The magnitudes of the caste-favoritism effects remain relatively unchanged, though become marginally insignificant.

predictive of actual rankings. An increase in predicted rank of one is associated with an increase in actual rank of 0.14 (column 2). Taking the difference between the actual and the predicted rank, the prediction error is large and significant for the player themselves – consistent with over-confidence (column 3). In contrast, for teammates there is no statistically significant difference in prediction error between own and other castes – inconsistent with the idea that players have systematically biased beliefs about the relative ability of other castes to begin with. There is also no statistically significant difference in the *absolute* prediction error between own and other castes, showing that beliefs about other castes are not noisier either (column 4).

Second, note that a full standard deviation increase in one of the baseline ability measures improves the vote by 0.07 to 0.14 ranks (columns 1-3, Table 2). Given these estimates, the magnitude of the collaborative effect on non-opponents in column 6 seems too large to be explained only by effects on statistical discrimination. For this to be the case, we would need full collaborative exposure to lead players to treat other-caste players that they have not seen play *as if* they are 0.45 to 0.9 of a standard deviation higher on each of the ability measures. This extent of generalized belief updating seems implausible given that baseline beliefs are anyway no more likely to be incorrect for other-caste players than own-caste players, and that even those with no collaborative exposure see signals of the ability of the other-caste players they play against.

These results together support the claim that collaborative contact reduces own-caste favoritism in voting primarily through its impact on preferences.[46]

5.3 Efficiency

Trade. To test for effects of contact on efficiency, I first explore behavior in the trading exercise. Here there can be efficiency gains on both the extensive margin (making more trades) and the intensive margin (making more valuable cross-caste trades). Most participants trade successfully – 88% of goods received by those in homogeneous-caste teams are traded, with no statistically significant effect of collaborative or adversarial contact (top panel Figure 6, column 1, Table 3).[47] It follows that any efficiency effects must come through the intensive margin.

The color-switch incentives have large effects, increasing cross-caste trade by 22 to 25 percentage points relative to a mean in homogeneous-caste teams of 52% (column 2). Collaborative contact has a

[46]In addition, the results are not likely explained by collaborative contact shifting perceived or actual social norms, given that the voting is done privately and anonymously (Bursztyn et al. (2017, 2018)). The voting behavior is then more likely to reflect private preferences than social concerns.

[47]According to participant self-reports, almost all trades were made directly without the use of a middleman (95%), and only a handful involved the transfer of any money (0.3%), suggesting that participants tended not to enter into any surplus-sharing agreements.

small and positive, though marginally insignificant, effect – full collaborative exposure increases cross-caste trade by six percentage points ($p = 0.14$).[48] While adversarial contact has a large negative coefficient (-0.16), this estimate is not significant given the large standard errors, and the equality of effects of collaborative and adversarial contact cannot quite be rejected at conventional levels ($p = 0.2$).

These results for the full sample include those without efficiency gains from cross-caste trade – those without the color-switch incentives. For the half of the sample with potential efficiency gains, full collaborative exposure significantly increases cross-caste trade by 11 percentage points (column 3 and bottom panel, Figure 6) and trade payouts by Rs. 15 or 18% of the homogeneous-caste team mean (column 4). This effect on cross-caste trade is roughly one-half of the effect of the color-switch incentives. Given a local daily wage of Rs. 200, and assuming a linear effect of incentives, full collaborative contact increases cross-caste trade as much as a direct incentive equal to one or two hours of wages. This benchmarking suggests that collaborative contact substantially reduces barriers to cross-caste economic interaction.

These results suggest that collaborative contact is a complement of, not a substitute for, incentives for intergroup interaction – the effect of collaborative contact is 9 percentage points higher (though not significantly so, with $p = 0.24$) in the presence of incentives. Collaborative contact then facilitates intergroup cooperation in a context where there are incentives for cooperation, but it does not create cooperation on its own. This result makes sense in the trading exercise – castes are segregated geographically, making the ingroup the easiest to trade with. Though collaborative contact leads to cross-caste friendships, those friends live further away. These friendships are unlikely to be strong enough to supersede all existing own-caste friends. The sensible conclusion is to trade nearby where possible, but to consider trading across caste when the incentives exist.

The effect is notable for three more reasons. First, unlike the effects on willingness to interact, here there is clear evidence that collaborative cross-caste contact leads to further verifiable cross-caste contact. Second, the effect is likely a lower bound given spillovers in the trading network – if a mixed-team player decides to trade across caste (because of treatment) with a homogeneous-team player, both will be recorded as having engaged in cross-caste trade, obscuring the actual treatment effect. Third, though this is an effect on trading low-cost goods, cross-caste trading is an important issue in the region. Anderson (2011), for example, argues that cross-caste trade breakdowns in irrigation markets have led to low incomes for low-castes in the same region of this study.

Two pieces of evidence suggest that, as with the results on social interaction, these collaborative effects generalize beyond teammates. First, the key coefficient falls only from 0.11 to 0.06 when looking at

[48] Nevertheless, cross-caste trade is lower for those in homogeneous-caste teams than those with any level of collaborative contact (middle-left panel, Figure 6). As a result, if I estimate equation 1 with an indicator for mixed-caste team instead of Prop. Oth. Caste on Team_{icl}, $\hat{\beta} = 6.4$ with a p-value of 0.03.

effects only on cross-caste trades with those assigned to *other* teams (column 5).[49] Second, the causal effect of being assigned a teammate on trading with that teammate is small and insignificant, as is the effect of indirect links through teammates (Table A11).[50] These results show that collaborative contact does not merely increase cross-caste trading via information (about who to trade with) or network (through who you know) channels. Instead, complementing the evidence on willingness to interact, collaborative contact changes the willingness to cooperate with other-caste men in general.

Trust. While collaborative contact increases efficiency in trading by reducing barriers to cross-caste interaction, a separate question is whether contact has efficiency effects in the absence of interaction. To test for this, I explore effects on trust, which depends on both altruism and beliefs about trustworthiness. In the trust game, efficiency is maximized when the Sender transfers the full Rs. 50 to the Recipient.

Though a 6% own-caste trust advantage exists (column 1, Table 4), there is no evidence that either type of contact significantly affects this cross-caste trust gap (column 2). This result reflects some combination of the effect of contact on the altruism and trustworthiness beliefs inherent in trust game behavior. If we take the voting results as evidence for collaborative contact shifting altruism toward other-castes, we might interpret these trust results as ruling out meaningful positive belief updating about the trustworthiness of other-caste men.[51]

I next consider effects on overall levels of trust. Here the contact hypothesis gives less guidance – while contact with outgroups may, under certain conditions, mold beliefs and preferences toward outgroups, it is less clear whether contact should shift the general levels of beliefs or preferences.[52] Despite this, ethnic diversity is typically negatively associated with generalized trust (Dinesen et al. (2020)), and researchers have proposed interethnic contact as a potential causal channel.

Both types of contact somewhat reduce the overall amount sent in the trust game (column 3), while adversarial contact also significantly reduces levels of stated trust in others (column 4). After standardizing these two measures and combining them into an index, there is clear evidence only for adversarial contact reducing trust, with the collaborative and adversarial effects significantly different ($p = 0.01$). Adversarial contact then harms efficiency by reducing generalized trust, and not only trust toward the

[49]As discussed in Section 5.1, this is a conservative test given that those with more collaborative exposure have *fewer* potential other-caste trading partners among those assigned to other teams. In Section 5.1 I dealt with this by normalizing the outcome by the set of possible other-caste other-team friendship links. With the trading outcome it makes less sense to normalize given that each participant can only trade each good once.

[50]To the extent that there are negative effects of adversarial contact, there is also some limited evidence of generalization (column 6).

[51]In contrast, Finseraas et al. (2019) find that Norwegian soldiers randomly assigned to ethnic minority roommates send more money in a trust game to the minority member, but no more or less to a majority member.

[52]Though related evidence exists on social preferences – Rao (2019) finds that contact with poor students increases the pro-sociality of rich students *in general*, as measured by dictator games played with both rich and poor students.

outgroup.

The suggestive evidence that collaborative contact also reduces trust (present in column 3, but not column 4) is perhaps surprising – we might instead expect that positive contact with the outgroup would lead to positive belief updating about trustworthiness of the outgroup, no updating about the ingroup, and a net positive effect on the levels of trust. An alternative hypothesis is that how trusting individuals are depends on the extent of trustworthiness experienced in the recent past, with a failure to attribute variation in past trustworthiness to the characteristics (e.g. partner's caste) of past interactions. This hypothesis predicts that outgroup interaction in general depresses future trust, merely because interactions with outgroups tend to be less trusting and cooperative than interactions with ingroups. Consistent with this, holding collaborative and adversarial contact constant, men randomly assigned to have more baseline friends (another type of ingroup) on their team report higher trust at endline (Table A12). Together these results suggest that outgroup interaction in general may reduce efficiency by decreasing trust. Nevertheless, adversarial contact reduces efficiency significantly more than collaborative contact.

6 Alternative Explanations

I argue that the divergent effects of collaborative and adversarial contact derive from the fact that collaborative contact involves common goals between groups, whereas with adversarial contact goals are opposing. But there may be other explanations for why the two types of contact have different effects. I consider three alternative explanations in this section.

6.1 Intensity of Contact

Though players observe the behaviors of both teammates and opponents during each match, contact with teammates is more intensive than contact with opponents – for one thing, teammates interact more often, whether these interactions are friendly or hostile (Table A1). We might model this in a learning framework (as in Appendix B) by assuming that signals from teammates are more precise than signals from opponents. This signal precision argument is unlikely to explain the main results for two reasons. First, in general the signal precision should affect the speed of learning, but not the direction, inconsistent with the opposite effects on tastes for social interaction (Table 1). Second, even if signals from opponents are less precise, I still find some evidence of learning about their ability – since adversarial contact leads to more other-caste players chosen in the team formation task (Table A7). This result rules out the extreme possibility that players do not learn about their opponents at all.

6.2 Duration of Contact

In the experiment, collaborative contact entails interaction with the same other-caste players many times. If a team had full attendance of the matches, then each player in that team experiences collaborative contact with only four other people. In contrast, each player experiences adversarial contact with forty other people (eight matches multiplied by five players). The two types of contact differ not only in whether they are collaborative, but also in their duration. A competing hypothesis is that it is not the adversarial nature of cross-caste contact that hurts friendships, but rather the short-term nature of the contact. This could be the case if participants tend to get a bad impression from someone the first time they meet them, regardless of the context in which they meet. If this is true, then short-term *collaborative* contact would itself have negative effects.

I test this hypothesis by exploiting another feature of the experiment design: control participants served as backup players, and these backup players played not just on one team, but on whichever team they were asked to play as a substitute. Backup players then experienced cross-caste collaborative contact, but it was short-term in nature relative to that of the players assigned to mixed teams. Players in mixed teams played on average 3.7 matches with each of the other-caste men they were exposed to as teammates. In contrast, backup players played on average only 1.5 matches with each of the other-caste men they were exposed to. A natural test is to compare high-priority backups with low-priority backups. High-priority backups have more collaborative cross-caste contact than low-priority backups, but this contact is more short-term in nature than that experienced by non-backups. If the negative effects of cross-caste contact with opponents is driven by the duration of interactions, then there would likely be negative effects here too.[53]

In the top panel of Figure A8 I verify that high-priority backups experience more cross-caste exposure than low-priority backups. High-priority backups play on teams with more other-caste players in total (top-left panel), and with more unique other-caste players (top-right panel). High-priority backups also list more other-caste participants that they would like to spend more time with, more other-caste participants that are friends (middle panel), and are more likely to engage in cross-caste trade (bottom panel). This suggests that the duration of contact is not the reason for the negative effects of adversarial contact.

[53]High-priority backups also have more exposure to other-caste opponents than low-priority backups, but since other-caste opponent exposure has a negative effect on cross-caste friendships, this makes the test more conservative.

6.3 Winning and Income

A large body of work finds that ethnic diversity affects productivity and efficiency (Alesina and Ferrara (2005), Hjort (2014), Marx et al. (2016), Hoogendoorn et al. (2018)). In this experiment, it is possible that caste composition affects team performance, and that performance in turn affects outcomes. The warm glow or income effects from winning, for example, could lead respondents to list more other-caste friends when asked.[54] I rule this out in Table A13 – effects of either type of contact on the number of matches won are small and insignificant (column 1). At the team-level, there is also no evidence that mixed teams perform better or worse, conditional on the mean ability of their players (column 2). Related, there are no effects of contact on total match payouts (column 3).

7 Policy Implications

In this final section I discuss three additional findings with implications for policy. In particular, I ask: (i) what is the overall effect of league participation, (ii) does intergroup competition within teams blunt the effects of collaborative contact, and (iii) do castes need to be of equal status for collaborative contact to work?

7.1 Program Evaluation

Since league participation includes both collaborative and adversarial contact, does the integrative cricket intervention have positive effects overall? I address this question by comparing outcomes for the backup players with those that played in the leagues. Since higher priority backups played on average as many, or more, matches than the league players (Figure A3), I split the backup players into two categories: high-priority backups (with priority numbers from one to three) and low-priority backups (with all remaining priority numbers). The latter are closer to a pure control group given that they played on average only 1.6 matches each, compared with 6.1 matches for league players. Since low-priority backup players still played some matches, I consider these treatment effects to be a lower bound on the overall effects of the intervention.

The intervention had positive effects overall. Relative to low-priority backups, those assigned to mixed-caste teams have 1.13 (0.25 s.d.) more other-caste friends, choose 0.42 (0.37 s.d.) more other-caste players for their team for a match with stakes, and engage in 8.4 percentage points (0.17 s.d.) more cross-caste trade (Figure 7). The only insignificant comparison is for trust – those assigned to mixed-caste

[54] For example, Depetris-Chauvin et al. (2019) find that shared experiences (football matches) only build national identity when the team wins, and not when the team draws or loses.

teams send a similar amount in the trust game to the low-priority backups.[55] Effects of assignment to homogeneous-caste teams tend to be positive but weaker. The sole significant positive effect is on trust, consistent with the earlier interpretation (Section 5.3) that ingroup interaction increases generalized trust. Finally, treatment effects for high-priority backups tend to lie in between those for those in homogeneous-caste and mixed-caste teams, consistent with high-priority backups facing collaborative exposure that, on average, also lies in between the two.

These comparisons suggest firstly that integration should be very local to be effective – teams must be integrated for positive effects, not just the leagues themselves.[56] Second, since there are no negative effects of being on a homogeneous-caste team relative to control, there are likely other aspects of the league (beyond collaborative contact) that somewhat offset the negative effects of adversarial contact. Whilst speculative, these effects may come through intergroup interactions outside of teams and opponents, e.g. with umpires and spectators, or income effects from the participation and performance incentives.

7.2 Incentive Structure

Collaborative contact may be effective, but solely collaborative contact is rare in the wild – even members of sports teams compete for positions in the starting line up, co-authors in economics compete on the job market, and colleagues at work compete for promotion. Organizations may face a tension – efficiency requires meritocracy, but does meritocracy undo the collaborative forces that promote cohesion? Allport (1954) would conceivably argue yes: for contact to work, groups should not only have common goals, they should not face intergroup competition between them.

I explore this tension directly by exploiting the random assignment of teams to monetary incentives. Half of the teams were randomly assigned to receive Team Pay and the rest to receive Individual Pay. Individual Pay creates competition for payouts *within* teams, conditional on the same on-team contact. As a result, players on Individual Pay teams receive much more dispersed payouts (Figure A4). These incentives create the same tension inherent in organizations – players (from different castes) remain on the same team, but for half the teams there are much stronger incentives for competition.

Even with these competitive incentives, the collaborative effects do not unravel (Table A14). The

[55] The voting data is not shown in the figure since, as explained in Section 4.4, this data was not collected for the control group.

[56] I can calculate a rough estimate of counterfactual program effects if participants were allowed to choose their own teams. Low-priority backups chose 1.2 other-caste teammates (second-left panel, Figure 7). Let us assume this would be the level of cross-caste exposure in the case where participants choose their own teams. In contrast, simple random assignment to teams would ensure players have 2.7 other-caste teammates on average. Assuming that treatment effects are linear in the number of other-caste teammates (whether self-selected or not), my estimates imply that the effect of the program on cross-caste trade would be roughly 40% smaller if participants were able to choose their own teams.

effects of collaborative contact are similar on Individual Pay and Team Pay teams, and never statistically significantly different. Furthermore, in contrast to the effects of being a teammate vs. opponent on interactions (Table A1), cross-caste interactions on Individual Pay teams are no more likely to be hostile (Table A15). This pattern of results supports the idea that the type of contact matters only to the extent that it affects the nature of intergroup interactions. When the nature of interaction is unaffected, so are the resultant intergroup behaviors. In this case, the common goal that the team shares is sufficient for the positive effects of contact – the effects are resilient even to the introduction of additional intra-team competition.

7.3 Status Differences and Caste Heterogeneity

Beyond common goals and a lack of intergroup competition, the contact hypothesis makes the claim that integrated groups should have equal status within the situation for contact to work. In this final section I describe the unequal status across castes within each team, and show nevertheless that the effects of collaborative contact are similar across caste groups.

Ability and Discrimination. SC/ST players are 0.19 to 0.34 of a standard deviation worse than General castes at cricket according to baseline measures, conditional on age (columns 1 to 3, Table A16), while OBC players do not differ significantly from General castes for any ability measure. The SC/ST difference affects payouts – hypothetical performance-related pay is 25% lower for SC/STs than General castes (column 4). Fifty-eight percent of these payout differences remain after controlling for ability (column 5), suggesting that these differences are not just due to ability, but also on-team discrimination.

Consistent with this, favoritism of upper castes exists for three types of within-team allocation: SC/STs are significantly less likely to be chosen as captains, and less favored in the batting and bowling orders (Table A17).[57] This effect changes little when ability controls are added (columns 2, 4, and 6). Since ability measures are made common knowledge prior to the first match, the evidence suggests that teams actively discriminate against lower castes. Considering the coefficient on age, SC/STs are effectively treated like a General caste four or five years their junior. OBCs also appear to be less favored than General castes, but the effect is much smaller and significant only for batting order choices.

Together the data suggest that different castes do not enjoy equal status on each team, but rather reflect the status hierarchy of the caste system itself.

[57]Players prioritized for batting and bowling can make more money if on Individual Pay teams, and get more play time regardless of the incentive structure. On average, 14% of players in a given match didn't get the chance to bat, and 44% didn't get the chance to bowl.

Contact Effects by Caste. Given the evidence of status differences and discrimination, it seems plausible that the integration in this experiment could have different treatment effects by caste. Such heterogeneity is in fact rarely the case – the only outcome for which the collaborative contact effect is significantly less positive for SC/STs than other castes is cross-caste trade (top panel of Tables A18 and A19).[58] The general lack of heterogeneity suggests that even unequal status contact, as might be a result of some affirmative action policies, may nevertheless lead to improved intergroup relations.[59]

8 Conclusion

This paper provides evidence that the effects of integration depend on the type of contact. While collaborative contact reduces barriers to cross-caste interaction and tends to increase economic efficiency, adversarial contact has the opposite effects. In this setting, Allport (1954) was correct in arguing that common goals are necessary for intergroup contact to be effective. On the other hand, the claim that equal status and a lack of intergroup competition are necessary conditions receives less support – collaborative contact has positive effects despite the fact that the caste hierarchy is replicated within teams, and has positive effects even in the presence of monetary incentives that promote within-team competition.[60]

Why do some conditions matter and not others? One tentative idea is that the type of contact only matters to the extent that it changes the type of intergroup interactions. Consistent with this, players have more conflictual interactions with opponents than teammates, but interactions with teammates are not affected by competitive monetary incentives. In turn, common goals mediate the effects of contact while the incentive structure on teams does not.

Beyond conceptual contributions, this paper has two main implications for policy. First, the program evaluation results suggest that short-term sports programs can be effective in reducing intergroup differences. Second, the effects of intergroup contact interventions may be increased if the contact within these interventions is made more collaborative – through smaller, integrated groups, with common, and desirable goals.

Finally, limitations of the current paper suggest interesting avenues for future research. First, to systematically test for the importance of equal status, researchers could randomize the positions (e.g.

[58] This difference is despite the fact that SC/STs are not significantly less responsive to monetary incentives for cross-caste trade (Table A20), suggesting that the difference is not just driven by caste heterogeneity in the elasticity of the cost of effort (DellaVigna et al. (2019)).

[59] No reliable pattern is observed for caste heterogeneity of adversarial contact effects (bottom panel of Tables A18 and A19).

[60] My results also contrast with the findings of a more recent meta-analysis (Pettigrew and Tropp (2006)) since I find that common goals are necessary for prejudice reduction, not merely a facilitating factor. That said, the present paper is just one study, with external validity an open question.

captains vs. players) held by participants. One hypothesis motivated by this paper is that what actually matters is whether status differences are consistent with prevailing norms (i.e. high castes in leadership positions) or not.[61] Second, studies with greater statistical power could test for the role of relative group size, which may imply non-linear and possibly non-monotonic effects of contact. For example, contact between several small groups may lead to quite different group dynamics than contact between two large groups (Bazzi et al. (2019)). Third, general equilibrium effects may be important, but are not captured here given the individual-level randomization in my experiment. Future work could randomize integrated leagues at the village-level, and estimate effects on the prevalence of caste-based norms which reflect village-level equilibria, rather than individual-level beliefs and preferences.

[61] Somewhat like theories of expectations-based reference-dependence, e.g. Kőszegi and Rabin (2006).

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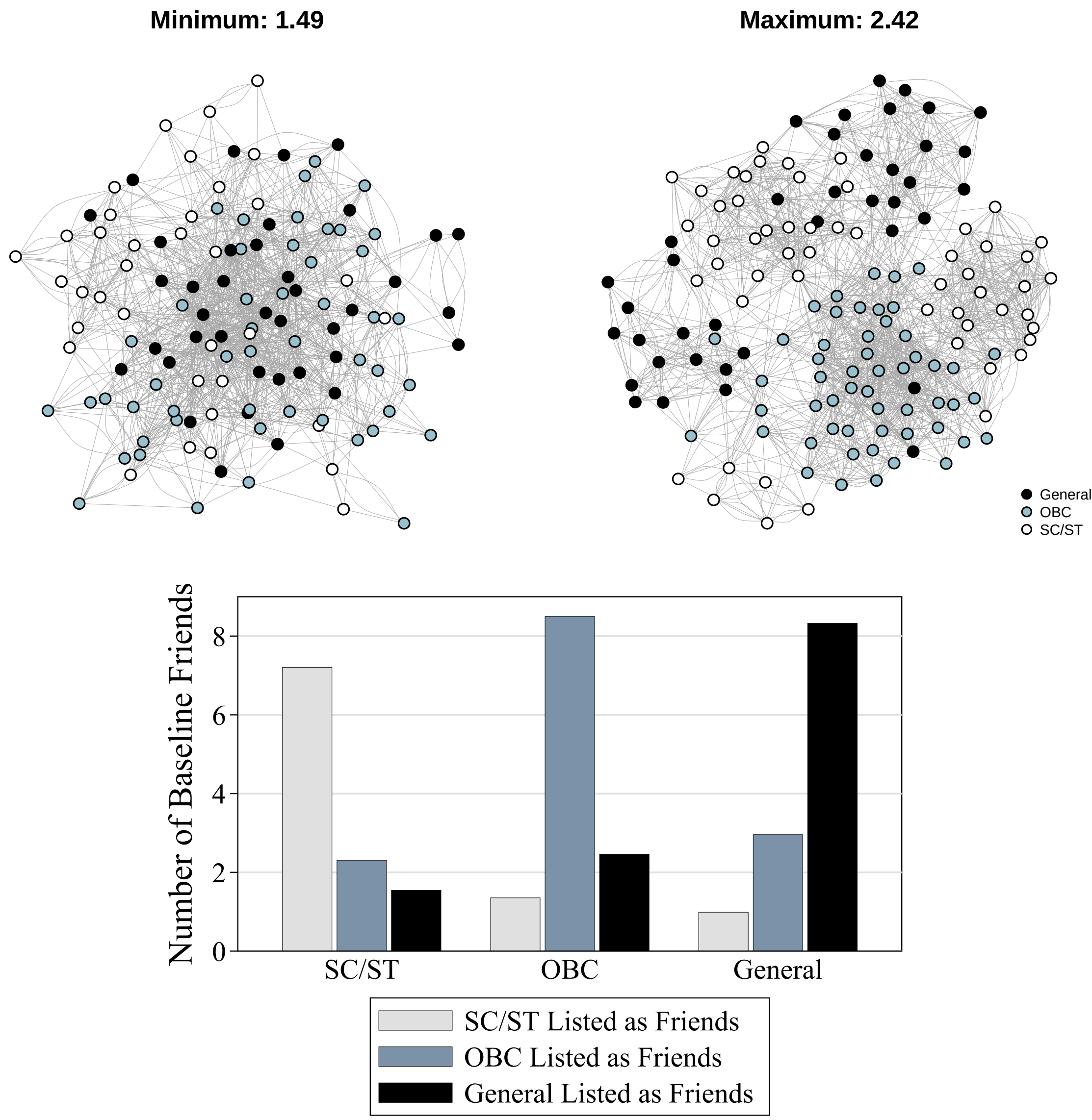
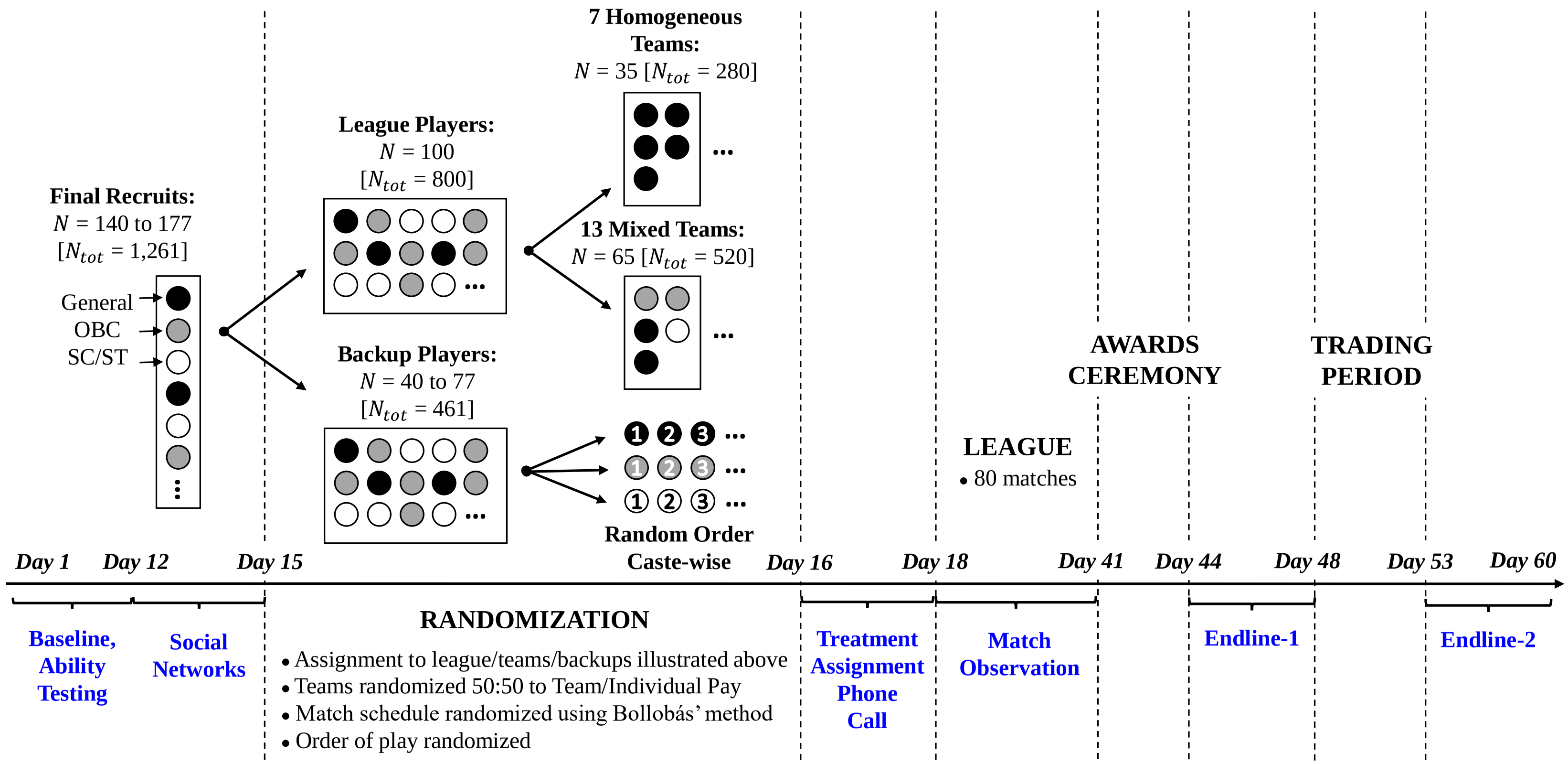
Figures

Figure 1: Social Networks Are Segregated by Caste at Baseline



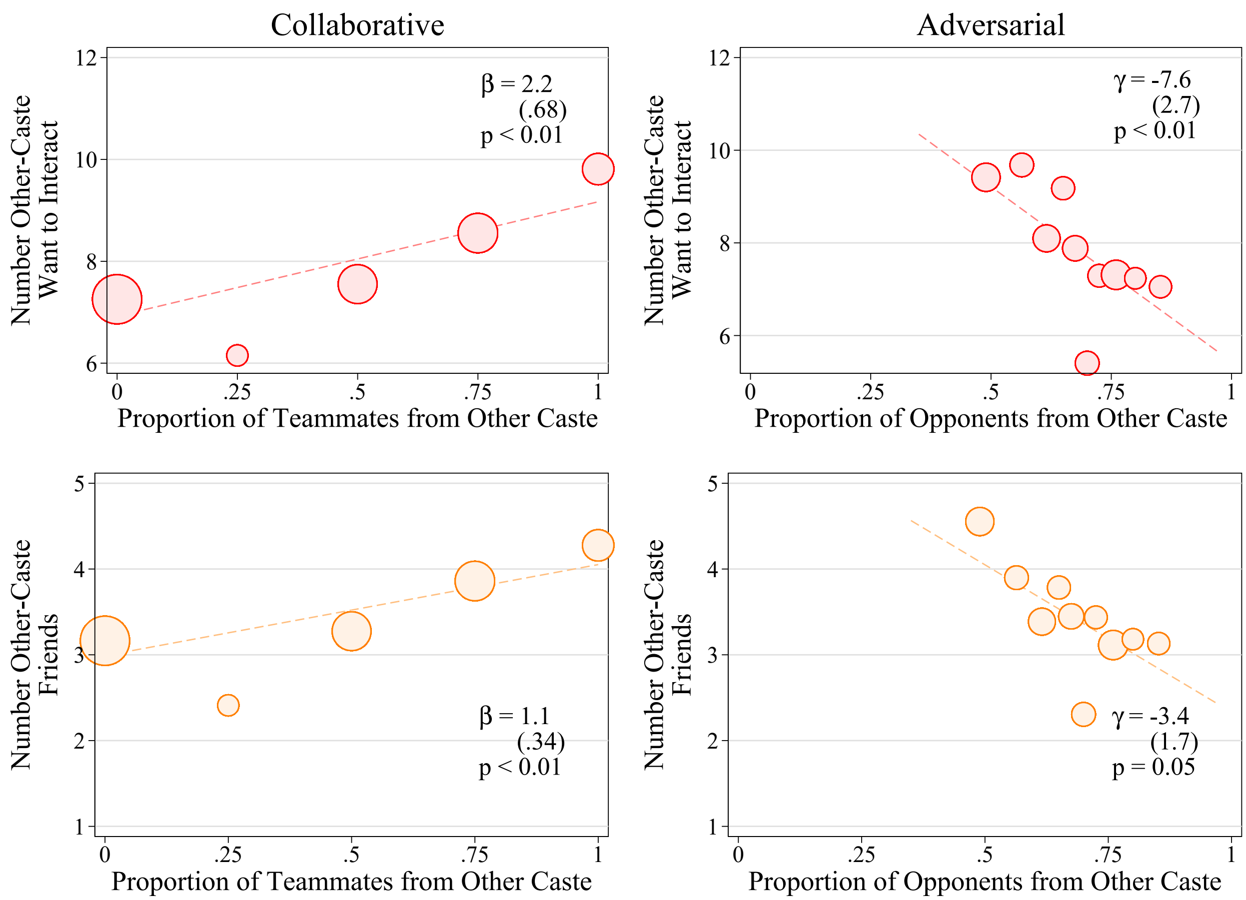
Notes: Top panel: the social network is visualized for the two locations (out of eight) with the minimum and maximum caste-based homophily. The homophily measure reflects how prevalent baseline friendships are between same-caste pairs compared to pairs in the network overall, with a measure above one reflecting that friendships are more likely to be formed within groups than across groups. The lowest caste-based homophily measure is 1.49, the maximum is 2.42, and the mean is 1.92. Each network displays only nodes that completed the baseline social network survey (1,174 of 1,261 participants). The question asked was: "Now we have finalized the sign-up for all players in the league, I would like to ask one more question. Here are the photos of all the sign-ups. As I scroll through slowly, please can you tell me which of these people you consider to be friends?" Each network is undirected, with an edge between two nodes if either (or both) participant(s) listed the other as a friend. Each network is drawn using a force-directed algorithm (Kamada Kawai) that puts nodes closer together that have a smaller length of the shortest path between them. Bottom panel: for those that completed the baseline social network survey the bars show the mean number of friends listed of a given caste, averaging across all participants from a given caste. For example, the far-left bar shows that SC/ST caste participants report having 7.2 SC/ST friends on average.

Figure 2: Experiment Design and Timeline



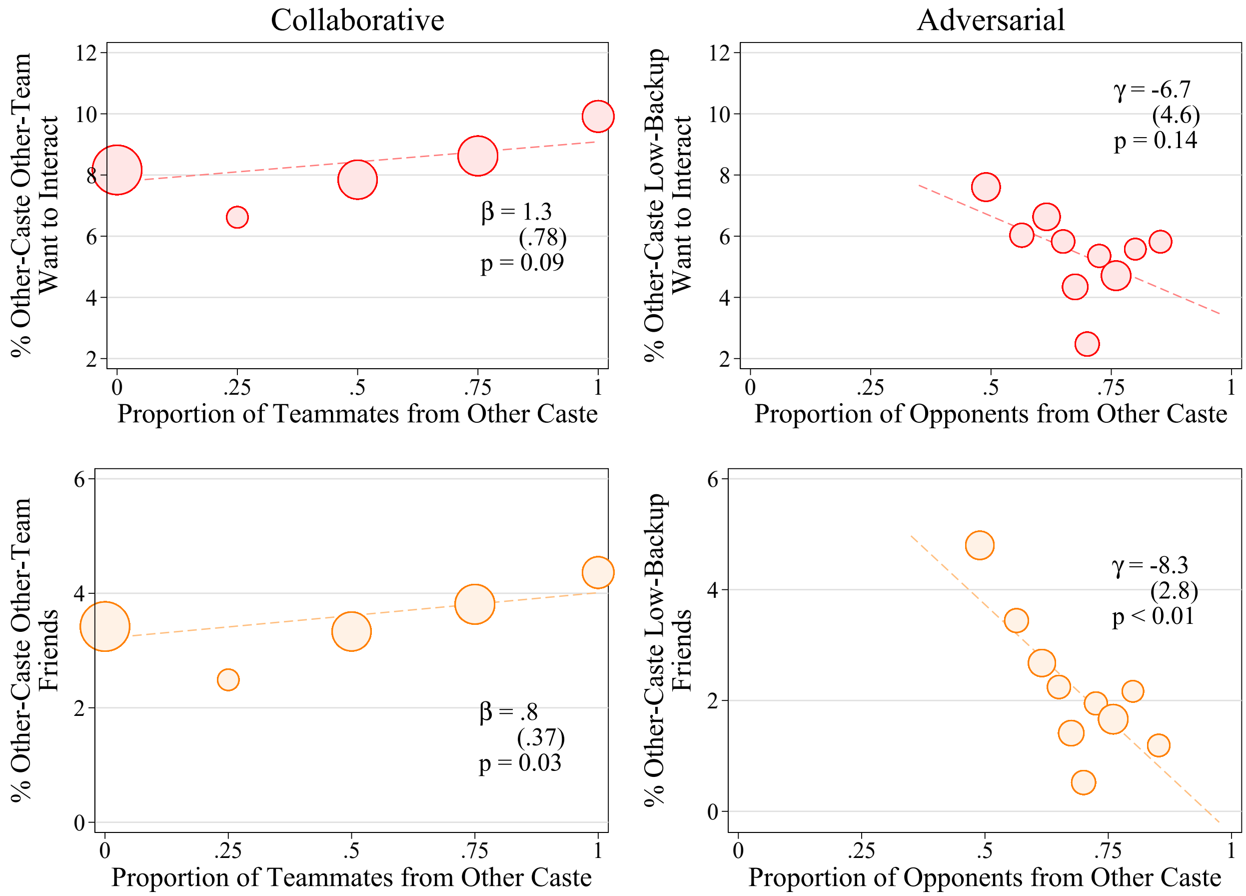
Notes: The figure shows the roughly sixty-day timeline for each league. The experiment ran in three phases with two leagues running in parallel during the first phase and three during each of the latter phases. N is the sample size per league, N_{tot} is the overall sample size summed across all eight leagues. **Blue** text refers to survey instruments. The order of events in the figure reflects exactly the order of events in each league, while the day numbers for each event are approximate given some slight variation between leagues.

Figure 3: Opposite Effects on Demand for Cross-Caste Social Interaction



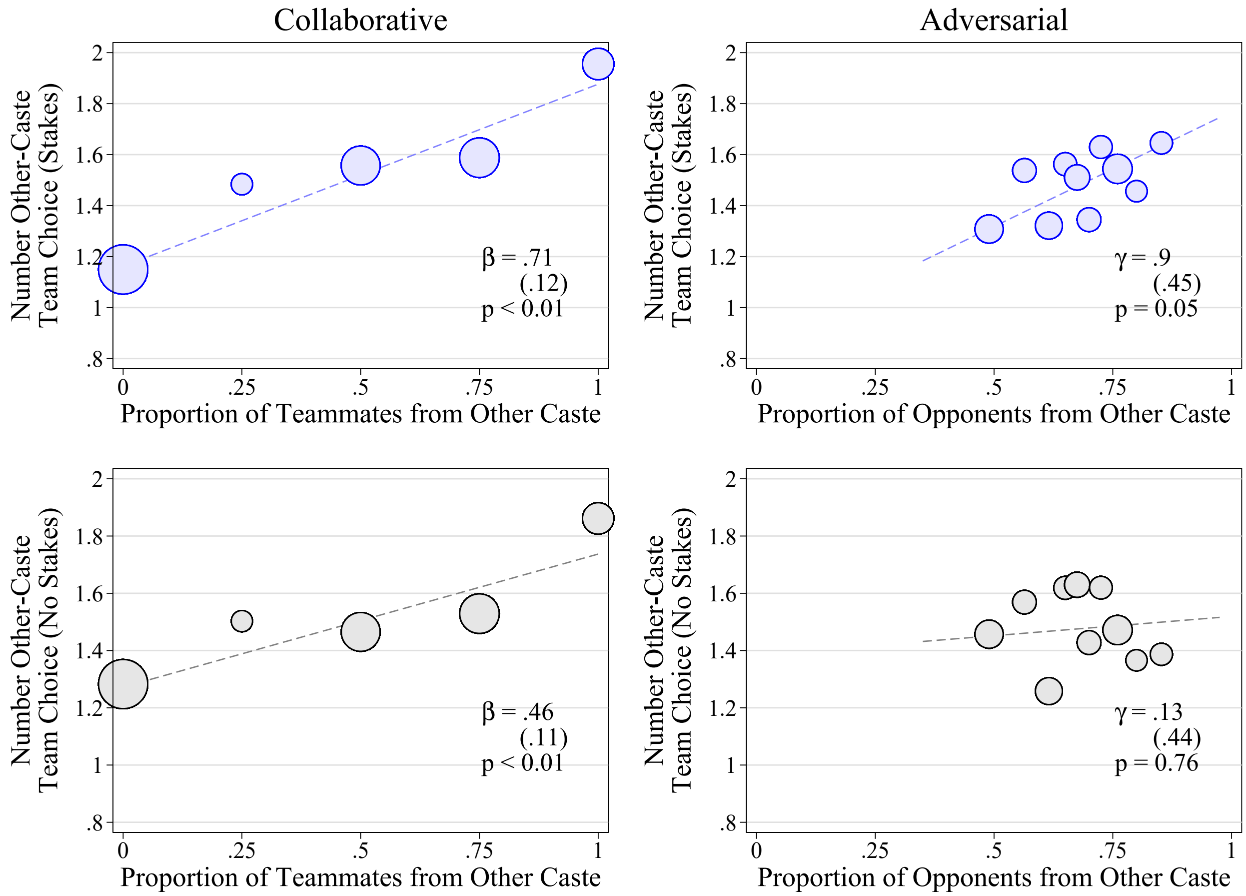
Notes: The two [left-hand-side/right-hand-side] panels show the effects of [collaborative/adversarial] contact. The estimated β and γ from equation 1 along with each standard error and p-value is shown, as well as a dashed line showing this linear fit. The bubbles in each panel plot fitted values from a semi-linear specification that parallels equation 1, but replaces the relevant contact variable with a set of dummy variables for the contact variable belonging to different bins (Cattaneo et al. (2019)). The panels visualize the fitted value for each bin, holding all other variables at their mean values. The figure then controls for the same covariates and strata fixed effects as in the regression tables. The bins for collaborative contact are the five possible values (0, 0.25, 0.5, 0.75, and 1). The bins for adversarial contact are ten quantile bins. The bubble size reflects the sample size in each bin. The top panel outcome is the number of other-caste men the participant wants to spend more time with. The bottom panel outcome is the number of other-caste men the participant considers friends.

Figure 4: Contact Effects Extend Beyond Immediate Interactions



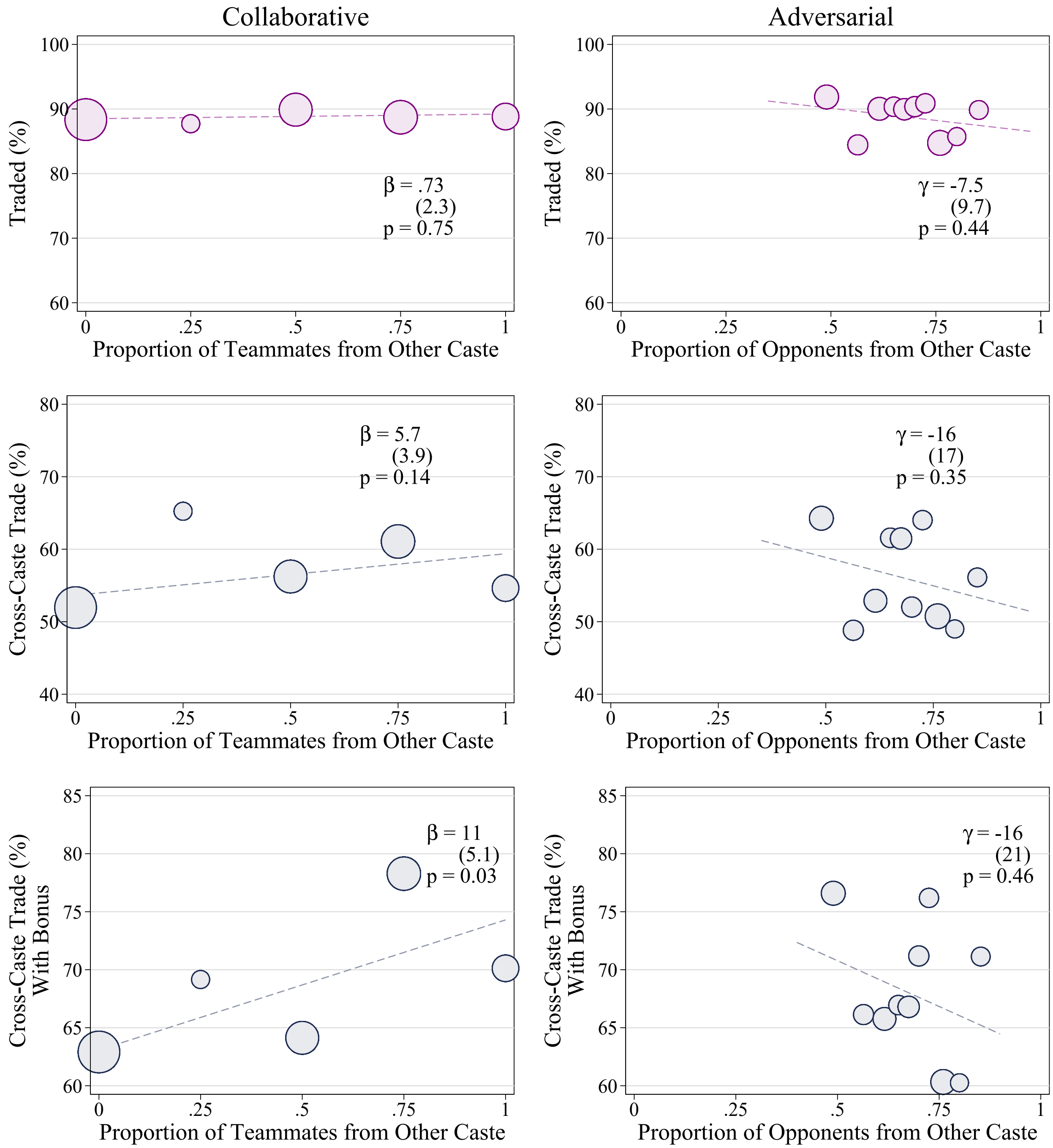
Notes: The figure is created based on equation 1, and as described in Figure 3. The [top-left/bottom-left] panel outcome is the percentage of other-caste men from among the other teams (in the same league) that the participant [wants to spend more time with/considers friends]. The [top-right/bottom-right] panel outcome is the percentage of other-caste men from among backups with priority number seven or above that the participant [wants to spend more time with/considers friends]. The left panel shows whether collaborative contact affects cross-caste friendships other than with teammates. The right panel shows whether adversarial contact affects cross-caste friendships other than with opponents.

Figure 5: Type of Contact Matters for Team Formation Only When Stakes Are Removed



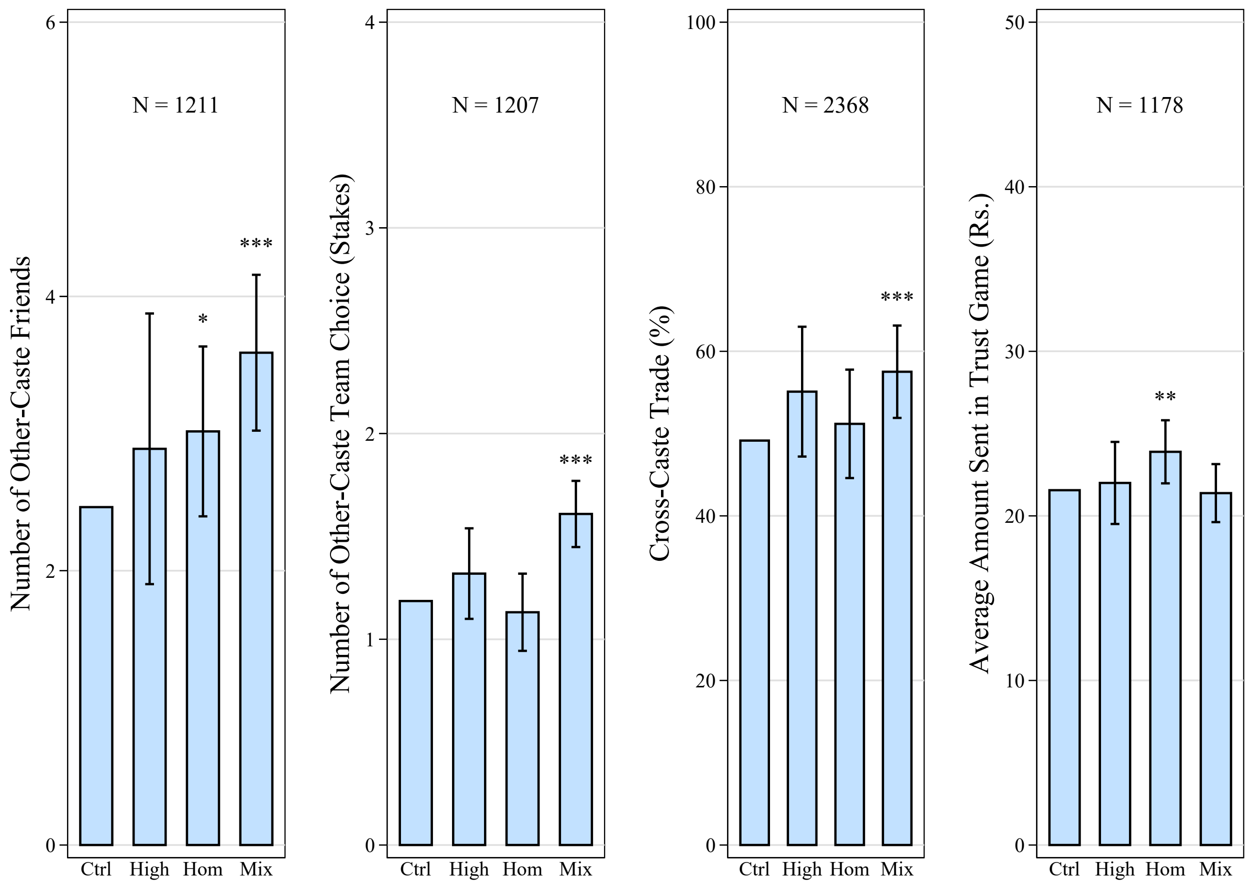
Notes: The figure is created based on equation 1, and as described in Figure 3. The top panel outcome is the number of other-caste men (from zero to four) chosen as teammates for the future match with stakes. The bottom panel outcome is the same, for the future match without stakes.

Figure 6: Collaborative Contact Increases Cross-Caste Trade



Notes: The figure is created based on equation 1, and as described in Figure 3, with the addition of the Trade and Color-Switch Bonus dummy variables. For this trading figure, the unit of observation is the participant-good, meaning there are two observations per participant. The top panel outcome is the percentage of goods successfully traded. The middle panel outcome is the percentage of goods successfully traded with someone from a different caste. The bottom panel outcome is the same, but includes only those assigned to the league that were also assigned a positive monetary incentive to switch the sticker color of their goods.

Figure 7: League Participation Reduces Intergroup Differences



Notes: The figure shows treatment effects and significance levels of Homog. Team (Hom), High Backup (High), and Mixed Team (Mix) relative to the low-priority backups (Ctrl), drawing on estimates from equation 2. From left-to-right the outcomes are: (1) number of other-caste men participant considers friends, (2) number of other-caste men chosen as teammates for future match with stakes, (3) percentage of cross-caste trade, and (4) average amount sent in the trust game to the three Recipients. For the cross-caste trade outcome, the regression additionally includes the Trade and Color-Switch Bonus dummy variables. *** p<0.01, ** p<0.05, * p<0.1.

Tables

Table 1: Ability Mediates Adversarial, But Not Collaborative, Contact

	Want to Interact w/	Friends	
	(1)	(2)	(3)
Panel A:	Number of Other-Caste Participants		
Prop. Oth. Caste on Team	2.25***	1.06***	1.25***
	(0.68)	(0.34)	(0.35)
Prop. Oth. Caste of Opponents	-7.55***	-3.44**	-2.88*
	(2.73)	(1.71)	(1.70)
Prop. Oth. Caste on Team*Oth. Caste Team Ability			0.28
			(0.66)
Prop. Oth. Caste of Opp.*Oth. Caste Opp. Ability			6.64***
			(2.48)
Caste*League FE	Yes	Yes	Yes
Outcome Mean	7.9	3.5	3.5
p(Collaborative = Adversarial)	.00074	.012	
Panel B:	Number of Own-Caste Participants		
Prop. Own Caste on Team	0.95*	0.47	
	(0.53)	(0.39)	
Prop. Own Caste of Opponents	4.53*	2.49	
	(2.38)	(1.84)	
Caste*League FE	Yes	Yes	
Outcome Mean	12	6.6	
p(Collaborative = Adversarial)	.14	.28	
Observations	770	770	770

Notes: Standard errors clustered at team-level. Column (1) outcome is number of other/own-caste participants the respondent wants to spend more time with. Column (2) and (3) outcome is number of other/own-caste participants the respondent considers friends. Oth. Caste Team Ability is the average ability index across all other-caste players in a given player's team (set equal to zero in the case of no other-caste players), where the ability index is the average across three standardized baseline ability measures: maximum bowling speed, number of 4s/6s when batting, and number of catches when fielding. Oth. Caste Opp. Ability is the average ability index across all other-caste opponents. Panel A regressions control for number of other-caste friends at baseline (and dummy for missing). Panel B is the same, except number of own-caste friends instead of number of other-caste friends. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Collaborative Contact Reduces Own-Caste Favoritism in Voting

	Vote Rank = 1 to 5, where 5 is best (reverse-coded)					
	Gen (1)	OBC (2)	SC/ST (3)	All (4)	All (5)	Non-Opp (6)
Own Caste Voted On	0.78***	0.32***	0.06	0.40***		
	(0.06)	(0.05)	(0.06)	(0.03)		
Age of Votee	0.06***	0.07***	0.07***			
	(0.01)	(0.01)	(0.01)			
Bowl Ability of Votee	0.10***	0.14***	0.13***			
	(0.03)	(0.03)	(0.03)			
Bat Ability of Votee	0.12***	0.14***	0.09***			
	(0.02)	(0.02)	(0.03)			
Field Ability of Votee	0.08***	0.09***	0.07***			
	(0.03)	(0.02)	(0.03)			
Own Caste Voted On*Prop. Oth. Caste on Team					-0.13*	-0.19*
					(0.07)	(0.11)
Own Caste Voted On*Prop. Oth. Caste of Opp.					0.09	-0.35
					(0.39)	(0.48)
Observations	3035	3200	2945	9180	9180	4570
p(Collaborative = Adversarial)					.57	.74
Votee FE	No	No	No	Yes	Yes	Yes
Prop. Oth. Caste on Team	No	No	No	No	Yes	Yes
Prop. Oth. Caste of Opponents	No	No	No	No	Yes	Yes
Caste*League*Own Caste Voted On FE	No	No	No	No	Yes	Yes

Notes: The unit of observation is a voter-votee pair. Voter-clustered standard errors for columns (1) to (4). Team of voter-clustered standard errors for columns (5) and (6). All columns exclude votes for teams with players only of the same caste of the voter or players only of other castes. Votee fixed effects can be included because the same person can be voted on by multiple voters. Columns (1) to (3) only include the votes made by General, OBC, and SC/ST caste players respectively. Column (6) only includes votes made on teams that were not faced as opponents during the league. Each ability measure of the person voted on is from baseline ability testing. Bowl Ability is maximum bowling speed (standardized), Bat Ability is number of 4s/6s out of 6 (standardized), and Field Ability is number of catches out of 6 (standardized). Columns (5) and (6) also control for number of other-caste friends at baseline (and dummy for missing), as well as each interacted with Own Caste Voted On. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Collaborative Contact Increases Cross-Caste Trade

	Traded	Cross-Caste Trade		Trade Payout	Cross-Caste Trade	
					Other Team	Low Backup
	(1)	(2)	(3)	(4)	(5)	(6)
Prop. Oth. Caste on Team	0.01	0.06				
	(0.02)	(0.04)				
Prop. Oth. Caste of Opponents	-0.08	-0.16				
	(0.10)	(0.17)				
Color Switch Bonus = 50	-0.00	0.22***				
	(0.02)	(0.04)				
Color Switch Bonus = 100	0.02	0.25***	0.03	85.97***	-0.03	0.05**
	(0.02)	(0.04)	(0.04)	(6.51)	(0.04)	(0.02)
Prop. Oth. Caste on Team*(Bonus>0)			0.11**	14.93**	0.06	-0.00
			(0.05)	(7.14)	(0.05)	(0.02)
Prop. Oth. Caste on Team*(Bonus=0)			0.02	-1.66	-0.01	0.00
			(0.06)	(1.34)	(0.05)	(0.02)
Prop. Oth. Caste of Opponents*(Bonus>0)			-0.16	-22.23	0.05	-0.11
			(0.21)	(30.48)	(0.22)	(0.12)
Prop. Oth. Caste of Opponents*(Bonus=0)			-0.16	-5.28	0.03	-0.11
			(0.23)	(5.32)	(0.23)	(0.10)
Observations	1510	1510	1510	1510	1510	1510
Homog. Team Mean	.88	.52	.52	83	.36	.055
Caste*League FE	Yes	Yes	Yes	Yes	Yes	Yes
Caste*League*(Bonus>0) FE	No	No	Yes	Yes	Yes	Yes
Trade Bonus Dummy	Yes	Yes	Yes	Yes	Yes	Yes
p(Collaborative = Adversarial)	.39	.2				
p(Coll.*Bonus = Coll.*No Bonus)			.24	.025	.35	.86
p(Adv.*Bonus = Adv.*No Bonus)			1	.58	.93	1

Notes: Standard errors clustered at team-level. The unit of observation is the participant-good, meaning there are two observations per participant. The outcome for column (1) is a dummy variable equal to one if the good was successfully traded. The outcome for columns (2) and (3) is a dummy variable equal to one if the good was successfully traded with someone from a different caste. The outcome for column (4) is the total payouts received for trading that good, including any successful trade or color-switching incentive. The outcome for column (5) is a dummy variable equal to one if the good was traded with a different caste from one of the other teams in the same league (for generalization of collaborative contact). The outcome for column (6) is a dummy variable equal to one if the good is traded with a different caste from backups with priority number seven or higher (for generalization of adversarial contact). Trade Bonus Dummy is equal to one if the participant was assigned Rs. 20 for each successful trade, and zero if the participant was assigned Rs. 10 for each successful trade. Each regression controls for number of other-caste friends at baseline (and dummy for missing). *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Adversarial Contact Reduces Trust Levels

	Amount Sent in Trust Game (Rs. 0 to 50)			Stated Trust	Trust Index
	(1)	(2)	(3)	(4)	(5)
Own Caste Recipient	1.31**				
	(0.58)				
Own Caste Recip.*Prop. Oth. Caste on Team		-0.26			
		(1.49)			
Own Caste Recip.*Prop. Oth. Caste of Opp.		0.70			
		(5.54)			
Prop. Oth. Caste on Team			-2.95**	0.02	-0.09
			(1.18)	(0.04)	(0.07)
Prop. Oth. Caste of Opponents			-8.17	-0.43**	-0.89***
			(5.68)	(0.17)	(0.32)
Observations	2253	2253	2253	770	751
Outcome Mean	22.2	22.2	22.2	.21	0.03
p(Collaborative = Adversarial)		.86	.35	.011	.013
Sender FE	Yes	Yes	No	No	No
Age of Recipient	Yes	Yes	No	No	No
Caste*League*Own Caste Recipient FE	No	Yes	No	No	No
Caste*League FE	No	No	Yes	Yes	Yes

Notes: The unit of observation is a Sender-Recipient pair in columns (1) to (3), and an individual in columns (4) and (5). Senders are partnered with one General, one OBC, and one SC/ST Recipient, such that there are three observations per Sender. Standard errors clustered at individual-level in column (1), team-level otherwise. Outcome in columns (1) to (3) is amount sent by Sender to Recipient in trust game. Outcome in column (4), Stated Trust, is a dummy variable coming from the question "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?". Stated Trust equals one if the respondent answers "Most people can be trusted" and equals zero if the respondent answers "Need to be very careful". Outcome in column (5), Trust Index, is the average of two variables: the standardized individual-level mean amount sent in the trust game and standardized Stated Trust. Column (2) also includes the interaction of Own Caste Recipient with number of other-caste friends at baseline (and dummy for missing). Columns (3) to (5) include number of other-caste friends at baseline (and dummy for missing) as controls. *** p<0.01, ** p<0.05, * p<0.1.

A Appendix [For Online Publication]

Figures

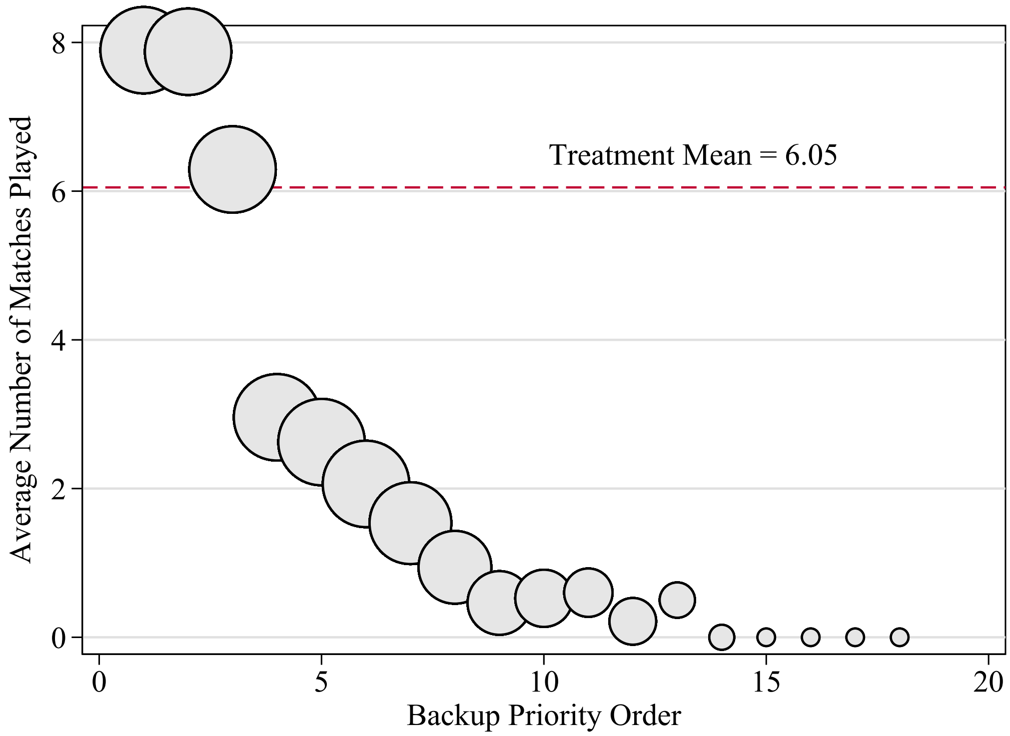
Figure A1: Cricket Explained

Notes: This photo was taken during one of the experimental matches. The fielding team comprises one bowler, one wicket-keeper, and three fielders. The batting team comprises two batsmen currently playing, and three sat together waiting their turn to bat.

Figure A2: Recruitment Poster

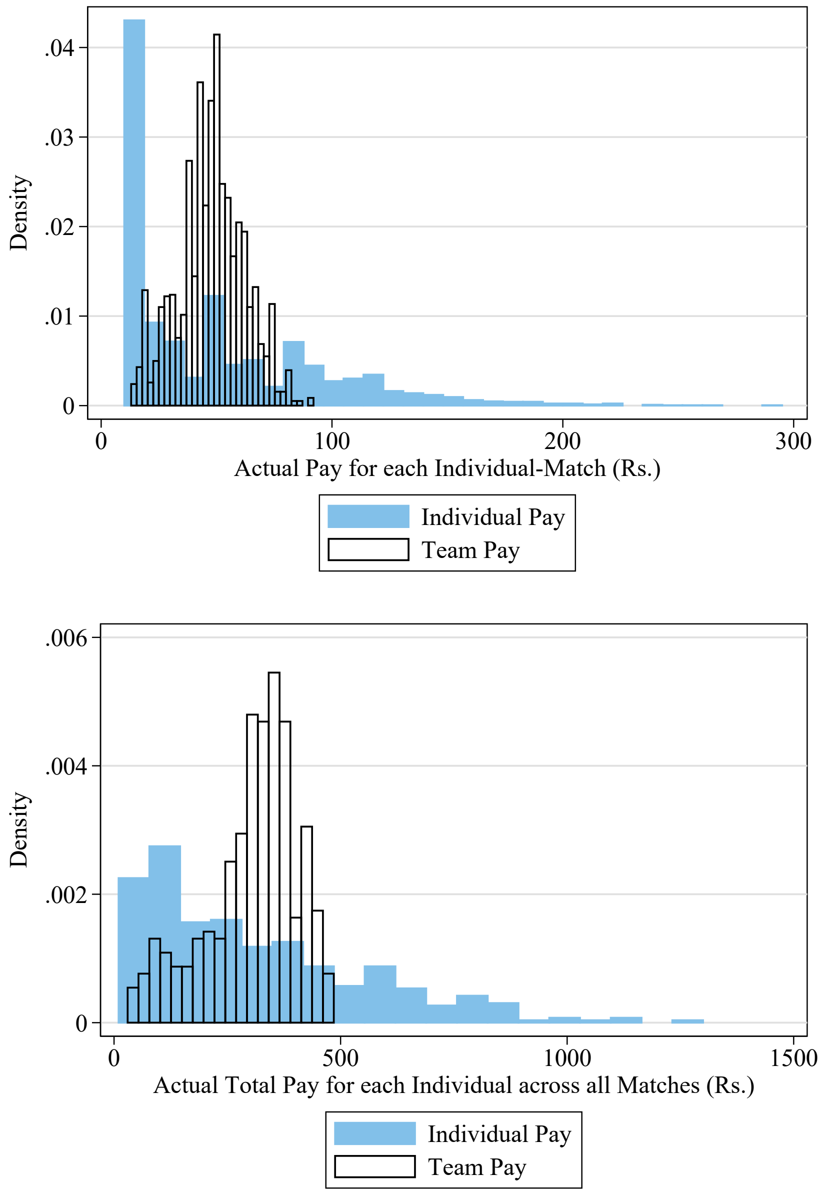
Notes: English and Hindi version of recruitment poster (only the Hindi version was used). The phone numbers and location (for the Hindi version) are blurred out for confidentiality reasons.

Figure A3: Number of Matches Played by Backups



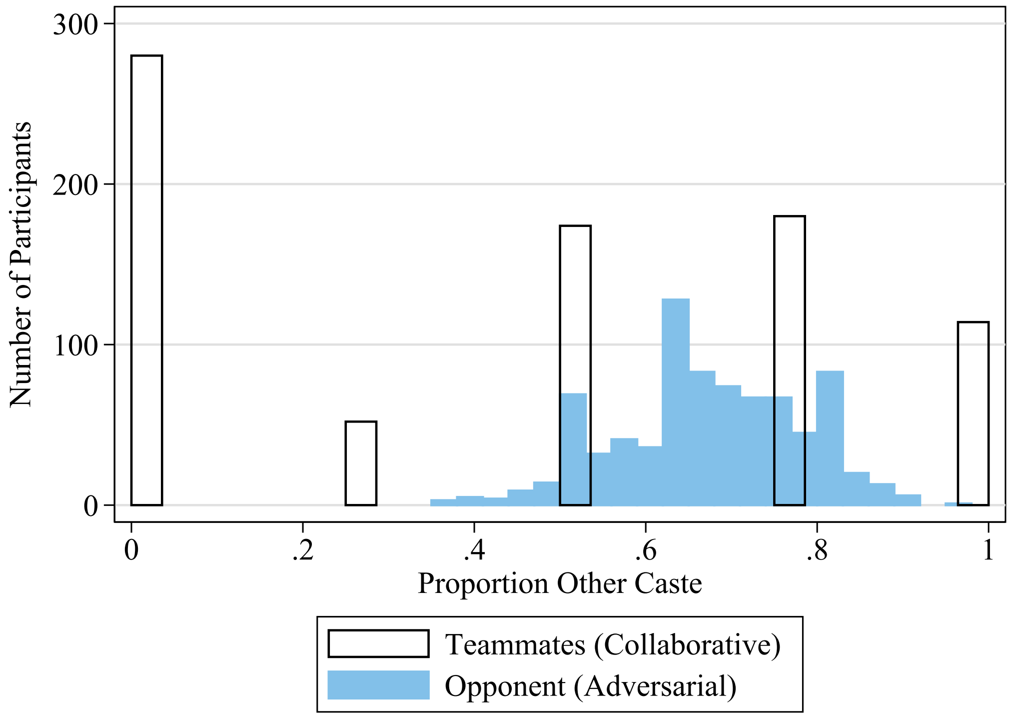
Notes: This is a scatter plot of the average number of matches played against the backup priority number, with the size of the bubbles reflecting the sample size for each priority number. The dashed line is the average number of matches played by the 800 participants assigned to play in the leagues.

Figure A4: Payout Distributions



Notes: The top panel shows histograms of actual incentive pay at the individual-match level, including a Rs. 10 show-up incentive received by everyone for each match. The blue histogram is for those in teams assigned to Individual Pay and the transparent histogram is for those in teams assigned to Team Pay. The bottom panel shows the two histograms for actual incentive pay aggregated across all matches to the individual-level.

Figure A5: Variation in Collaborative and Adversarial Contact

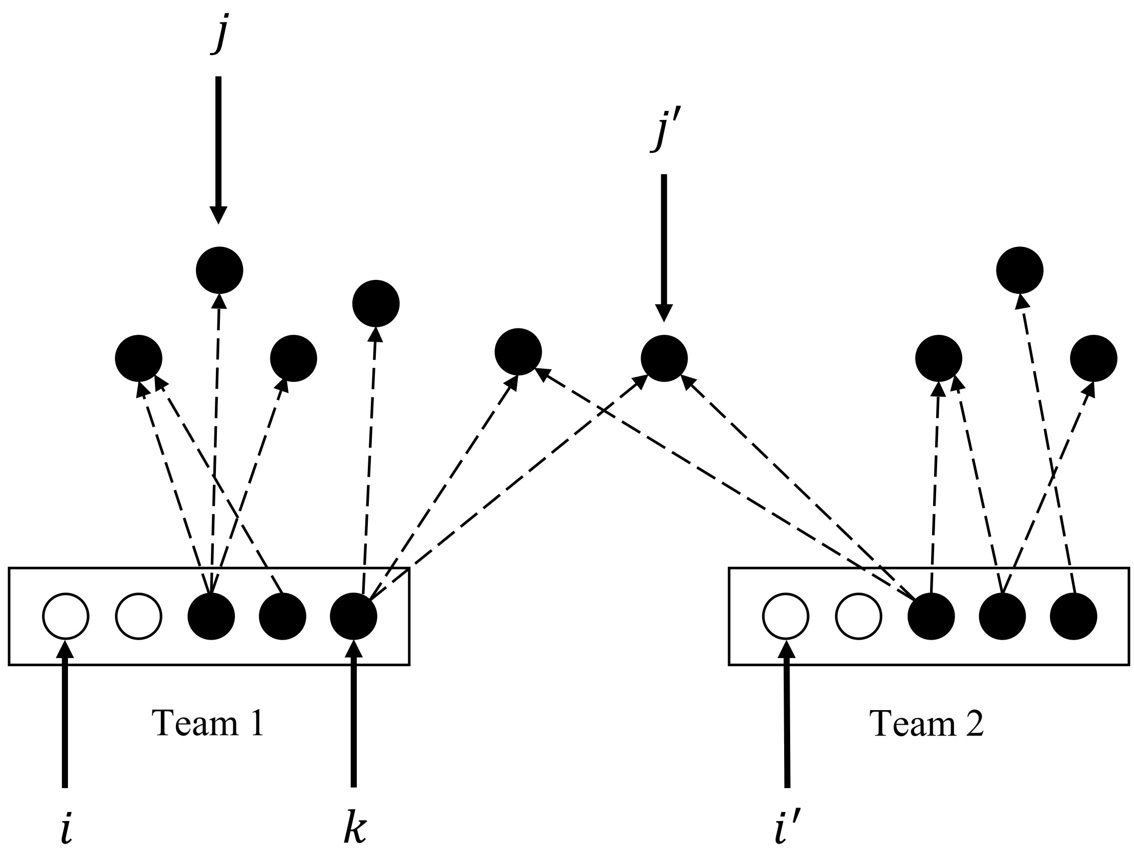


Notes: The white histogram shows the variation in the proportion of a player's teammates that belong to a different caste (collaborative contact). The blue histogram shows the variation in the proportion of a player's opponents that belong to a different caste (adversarial contact).

Figure A6: League Table

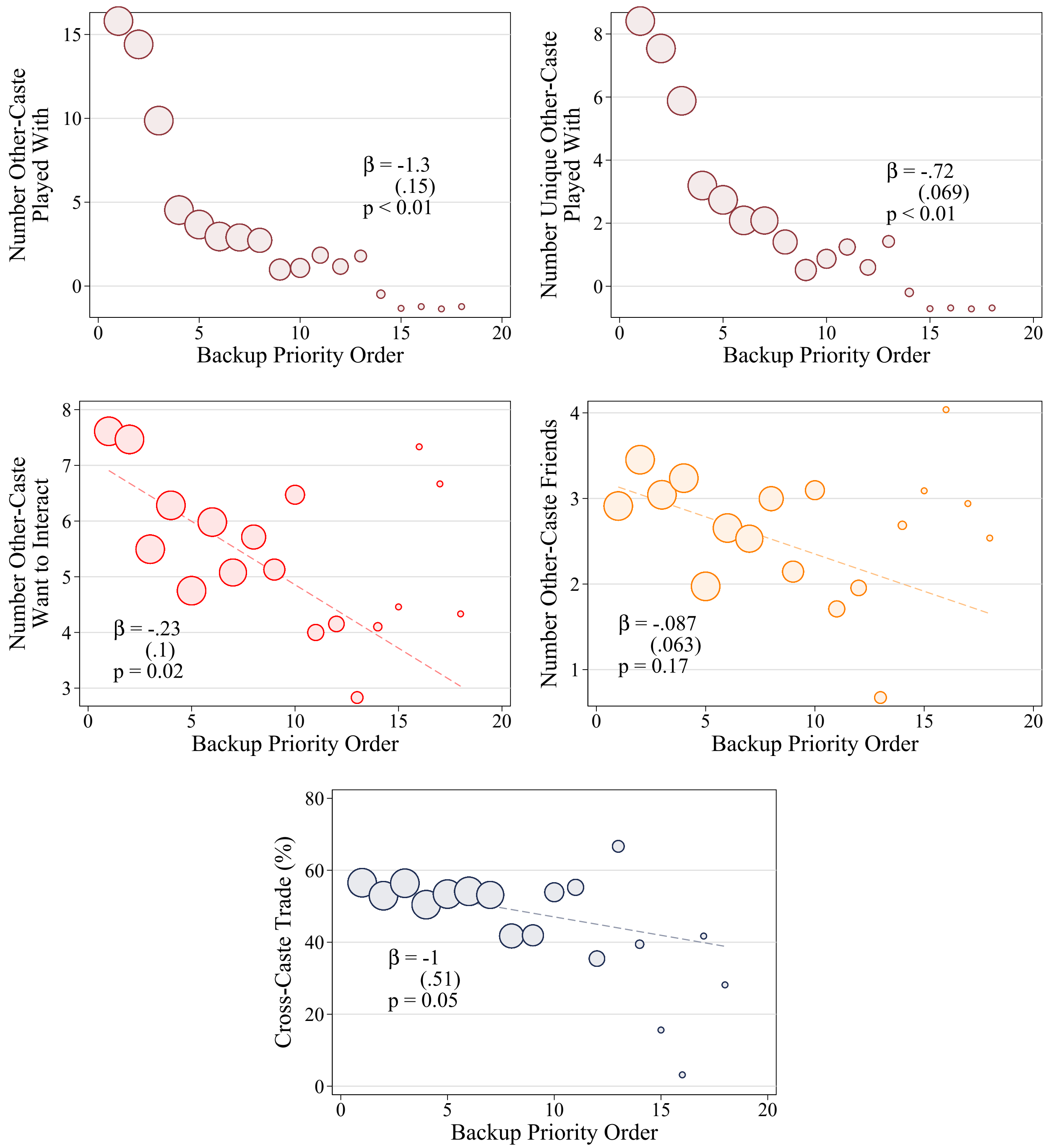
Notes: Example league table after 36 of 80 matches had been played. NRR is net run rate (used to settle ties between two teams with the same number of points). The location is blurred out for confidentiality reasons. Each team chose their own team name – for example, team 2T17, made up of five SC/ST players, chose to be called "Ambedkar Sporting Club". B. R. Ambedkar, a lower caste himself, was a champion of human rights for lower castes, an author of the Indian constitution, and an economist (with PhDs from both Columbia University and the London School of Economics).

Figure A7: Friends of Friends of Teammates – Identification Intuition



Notes: This figure demonstrates the intuition behind the identification of the effect on friendship of being indirectly linked with another participant. The dashed lines reflect baseline friendship links reported by the other-caste members of Teams 1 and 2. Players i and i' belong to the same caste (same color) and have the same collaborative contact (Prop. Oth. Caste on Team_{icl} = Prop. Oth. Caste on $\text{Team}_{i'cl} = 0.75$). Player k is a teammate of i's, but not a teammate of i''s. I find the effects of direct links on friendship by asking "is the probability that i and k are friends after the league is over greater than the probability that i' and k are friends?". Similarly, player j is an other-caste friend of an other-caste teammate of i's (an indirect link), but he is not an indirect link of i''s. I find the effects of indirect links on friendship by asking "is the probability that i and j are friends after the league is over greater than the probability that i' and j are friends?" Each of these comparisons is an example of one within-cell comparison that contributes to identification – the actual estimates come from pooling many such comparisons. Finally, j' is an example of an other-caste player indirectly linked to both i and i'. This player does not contribute to identification of the indirect link effect since there is no variation within this cell.

Figure A8: Short-Term Collaborative Contact Also Has Positive Effects



Notes: The figure is constructed similarly to Figure 3, though with the estimates coming from the specification:

$$y_{icl} = \alpha_{cl} + \beta \text{Backup Priority}_{icl} + \theta \mathbf{X}_{icl} + \varepsilon_{icl}$$

where Backup Priority$_{icl} \in \{1, 2, ..., 18\}$ and X_{icl} is a vector of control variables: number of other-caste friends at baseline (and dummy variable for missing) for all five figures, and color-switch and trade bonus dummy variables for the Cross-Caste Trade figure (bottom-right). Robust standard errors are used. The outcomes are: (1) the total number of other-caste men played on a team with (including double-counting when the same other-caste is played with multiple times) (top-left), (2) the total number of unique other-caste men played on a team with (with no double-counting) (top-right), (3) the number of other-caste men the participant wants to spend more time with (middle-left), (4) the number of other-caste men the participant considers friends (middle-right), and (5) the percentage of goods traded with someone from a different caste (bottom).

Tables

Table A1: Interactions with Cross-Caste Opponents Are More Hostile

	Friendly			Hostile		Proportion Hostile
	High-Fives (1)	Congrats (2)	Hugs (3)	Arguments (4)	Insults (5)	(6)
Teammates	0.52***	0.22***	0.88***	0.08***	0.02***	-0.50***
	(0.07)	(0.04)	(0.10)	(0.02)	(0.01)	(0.06)
Observations	9300	9300	9300	9300	9300	2260
Opponents Mean	.0052	.0026	.0086	.033	.0042	.68
Match FE	Yes	Yes	Yes	Yes	Yes	Yes
Caste i*Caste j FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Surveyors recorded all instances of friendly and hostile behavior between players during each match. The table uses the following dyadic specification to test for whether the type of contact affected the nature of actual in-match interactions:

$$y_{ijt} = \alpha_t + \alpha_{c(i)c(j)} + \phi \text{Teammate}_{ijt} + \xi_{ijt}$$

where y_{ijt} is the number of interactions (e.g. number of high-fives) that took place between players i and j during match t. α_t are a set of match fixed effects, and $\alpha_{c(i)c(j)}$ are a set of caste of player i-by-caste of player j fixed effects. Teammate$_{ijt}$ is the key regressor: a dummy variable equal to one if i and j are assigned to the same team, and equal to zero if they are instead opponents during match t. This regressor is random conditional on the caste-by-caste fixed effects given that random assignment to teams within each league was stratified only on caste (see Section 4.2).

I include only dyad-match observations where (1) neither i or j is a backup player, and (2) i and j are members of different castes. Standard errors are dyadic-robust at team-level. Opponents Mean is the mean of the outcome for all dyad-matches in which i and j are playing on opposing teams. The outcomes for columns (1) to (5) are the counts of interactions that i and j were involved in during match t, where the interactions are: (1) high-fives, (2) hugs/taps on back, (3) one player complimenting/congratulating another player, (4) arguments, and (5) one player insulting (sledging) another player. The sample in column (6) is further restricted to those dyad-matches involved in at least one interaction. The outcome is the total number of hostile interactions ((4)+(5)) divided by the total number of interactions ((1)+(2)+(3)+(4)+(5)). *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Randomization Checks – Effects of Contact

	N. Oth. Caste Friends (1)	Worked Last Year (2)	Played Last Tournament (3)	N. Catches (4)	Age (5)	Max. Bowling Speed (6)	Would Volunteer (7)	In School (8)
				Panel A: Full Sample				
Prop. Oth. Caste on Team	0.30	-0.01	0.02	0.15	0.10	0.84	0.04	0.02
	(0.44)	(0.03)	(0.04)	(0.11)	(0.33)	(0.82)	(0.04)	(0.04)
Prop. Oth. Caste of Opponents	-3.44	0.04	0.18	-0.23	2.60**	3.16	-0.00	-0.28*
	(2.83)	(0.14)	(0.15)	(0.50)	(1.31)	(4.77)	(0.20)	(0.16)
Caste*League FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	746	800	800	800	800	800	800	800
				Panel B: Analysis Sample - Completed All Outcomes				
Prop. Oth. Caste on Team	0.31	-0.02	0.03	0.12	0.14	0.87	0.03	0.01
	(0.48)	(0.03)	(0.04)	(0.12)	(0.34)	(0.84)	(0.05)	(0.04)
Prop. Oth. Caste of Opponents	-3.66	0.07	0.21	-0.30	2.82**	1.64	0.03	-0.34**
	(2.97)	(0.14)	(0.14)	(0.52)	(1.34)	(4.86)	(0.19)	(0.16)
Caste*League FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	701	746	746	746	746	746	746	746
Full Sample Outcome Mean	3.8	.17	.18	5	18	87	.43	.77
Used for re-randomization			No				Yes	

Notes: Standard errors clustered at team-level. Outcome variables are: (1) number of other-caste friends listed at baseline, (2) dummy variable equal to one if worked for income in the past year, (3) dummy variable equal to one if played in a cricket tournament in the area in the past year, (4) number of catches (from 0 to 6) in the fielding ability test, (5) age, (6) maximum bowling speed from 6 attempts in the bowling ability test, (7) dummy variable equal to one if said willing to volunteer to help with league organization, and (8) dummy variable equal to one if currently attending school or college. Re-randomization is relevant for collaborative contact treatment only. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Randomization Checks – Effects of Program Participation

	N. Oth. Caste Friends (1)	Worked Last Year (2)	Played Last Tournament (3)	N. Catches (4)	Age (5)	Max. Bowling Speed (6)	Would Volunteer (7)	In School (8)
				Panel A: Full Sample				
Mixed Team	0.54	-0.01	0.01	0.07	-0.17	0.50	-0.04	0.03
	(0.35)	(0.03)	(0.03)	(0.08)	(0.26)	(0.74)	(0.03)	(0.03)
Homog. Team	0.15	0.03	-0.02	-0.03	-0.18	-0.66	-0.04	-0.00
	(0.35)	(0.03)	(0.03)	(0.08)	(0.30)	(0.80)	(0.04)	(0.03)
High Backup	1.08**	0.01	0.02	0.15	-0.06	1.12	0.01	0.00
	(0.49)	(0.04)	(0.04)	(0.11)	(0.38)	(1.07)	(0.05)	(0.04)
Caste*League FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1174	1261	1261	1261	1261	1261	1261	1261
				Panel B: Analysis Sample - Completed All Outcomes				
Mixed Team	0.56	-0.01	0.02	0.07	-0.14	0.88	-0.06	0.04
	(0.38)	(0.03)	(0.03)	(0.09)	(0.27)	(0.77)	(0.04)	(0.03)
Homog. Team	0.12	0.03	-0.02	-0.02	-0.14	-0.34	-0.04	0.01
	(0.38)	(0.03)	(0.03)	(0.09)	(0.32)	(0.83)	(0.04)	(0.04)
High Backup	1.16**	0.01	0.01	0.15	-0.15	1.70	0.02	0.04
	(0.52)	(0.04)	(0.04)	(0.12)	(0.37)	(1.08)	(0.05)	(0.04)
Caste*League FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1099	1167	1167	1167	1167	1167	1167	1167
Full Sample Outcome Mean	3.9	.17	.18	5	18	87	.44	.77
Used for re-randomization			No				Yes	

Notes: Standard errors clustered at team-level for those assigned to play in the leagues, otherwise participant-level. Homog. Team is equal to one if the participant is assigned to a homogeneous-caste team, and zero otherwise. Mixed Team is equal to one if the participant is assigned to a team with at least one other-caste teammate, and zero otherwise. High Backup is equal to one if the participant is assigned to the control group and has a priority number of 1 to 3. Backups with a priority number of 4 and above are the omitted category. Outcome variables are: (1) number of other-caste friends listed at baseline, (2) dummy variable equal to one if worked for income in the past year, (3) dummy variable equal to one if played in a cricket tournament in the area in the past year, (4) number of catches (from 0 to 6) in the fielding ability test, (5) age, (6) maximum bowling speed from 6 attempts in the bowling ability test, (7) dummy variable equal to one if said willing to volunteer to help with league organization, and (8) dummy variable equal to one if currently attending school or college. Re-randomization is relevant for the Mixed Team and Homog. Team coefficients only. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Attrition and Attendance

	Attrited				N. Matches Attended			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prop. Oth. Caste on Team	-0.00	-0.01	-0.01	0.01	-0.01	-0.39	0.42	-0.08
	(0.02)	(0.04)	(0.04)	(0.04)	(0.21)	(0.34)	(0.29)	(0.42)
Prop. Oth. Caste of Opponents	-0.10	-0.14	-0.07	-0.10	0.49	2.66	-1.89	0.57
	(0.08)	(0.17)	(0.15)	(0.17)	(0.93)	(1.65)	(1.17)	(1.85)
Caste*League FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	.068	.068	.058	.077	6	6.1	6.3	5.6
Caste Sample	ALL	General	OBC	SC/ST	ALL	General	OBC	SC/ST
Observations	800	263	278	259	800	263	278	259

Notes: Standard errors clustered at team-level. Attrited is a dummy variable equal to one if the participant did not complete all endline outcomes. N. Matches Attended is the number of matches the participant played in, ranging from zero to eight. *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Collaborative Contact Effects on Opponents vs. Non-Opponents

	Want to Interact w/ (1)	Friends (2)
Panel A:	% of Oth. Caste Opponents	
Prop. Oth. Caste on Team	0.59	0.28
	(0.90)	(0.44)
Prop. Oth. Caste of Opponents	Yes	Yes
Caste*League FE	Yes	Yes
Outcome Mean	8.3	3.6
Panel B:	% of Oth. Caste Non-Opponents	
Prop. Oth. Caste on Team	1.87**	1.18***
	(0.85)	(0.44)
Prop. Oth. Caste of Opponents	Yes	Yes
Caste*League FE	Yes	Yes
Outcome Mean	8.4	3.6
Observations	770	770

Notes: Standard errors clustered at team-level. Panel A outcomes are the percentage of other-caste men among opponent teams selected for each question. Panel B outcomes are the percentage of other-caste men among non-opponent teams selected for each question. Each regression controls for number of other-caste friends at baseline (and dummy for missing). *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Indirect Links Do Not Explain Generalized Effects of Contact

	Whether i lists j as... (=0/1)			
	Want to Interact w/	Friend	Want to Interact w/	Friend
	(1)	(2)	(3)	(4)
Teammate	0.239***	0.126***		
	(0.019)	(0.016)		
Friend of Other-Caste Teammate	0.0019	0.0015		
	(0.004)	(0.003)		
Opponent			-0.004	-0.001
			(0.005)	(0.003)
Friend of Other-Caste Opponent			-0.0036	0.0018
			(0.006)	(0.003)
Observations	52171	52171	79858	79858
Outcome Mean	.08	.035	.076	.033
α_{jcl}*Prop. Oth. Caste on Team FE	Yes	Yes	No	No
α_{jcl}*Prop. Oth. Caste of Opponents FE	No	No	Yes	Yes

Notes: Unit of observation is i-j dyad (pair of individuals). Sample includes only dyads where j is not the same caste as i. Additionally, columns (1) and (2) include only dyads where i is assigned to a mixed team. Columns (3) and (4) include only dyads where i is assigned to league participation. All columns control for a dummy variable equal to one if i listed j as a friend at baseline, and a dummy variable equal to one if this baseline link data is missing. Standard errors are dyadic-robust at team-level. Teammate (Opponent) is a dummy variable equal to one if i and j are teammates (opponents). Friend of Other-Caste Teammate (Opponent) is a dummy variable equal to one if j is listed at baseline as a friend of any of i's other-caste teammates (opponents). α_{jcl} is a set of Caste*League (of i) fixed effects fully interacted with person j fixed effects. α_{jcl} is then fully interacted with the categories of Prop. Oth. Caste on Team in columns (1) and (2), and with the categories of Prop. Oth. Caste of Opponents in columns (3) and (4). *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Type of Contact Matters for Team Formation Only When Stakes Are Removed

	Num. Other-Castes for Team for Match with			
	Stakes (1)	No Stakes (2)	Stakes (3)	No Stakes (4)
Prop. Oth. Caste on Team	0.71***	0.46***	0.72***	0.48***
	(0.12)	(0.11)	(0.12)	(0.11)
Prop. Oth. Caste of Opponents	0.90**	0.13	0.93**	0.21
	(0.45)	(0.44)	(0.45)	(0.44)
Prop. Oth. Caste on Team*Oth. Caste Team Ability			0.30*	0.32*
			(0.18)	(0.18)
Prop. Oth. Caste of Opponents*Oth. Caste Opp. Ability			0.26	0.87
			(0.69)	(0.66)
Observations	768	768	768	768
Outcome Mean	1.5	1.5	1.5	1.5
Caste*League FE	Yes	Yes	Yes	Yes
p(Collaborative = Adversarial)	.68	.46		
Collaborative: p(Stakes = No Stakes)	.00049			
Adversarial: p(Stakes = No Stakes)	.0063			
p(Stakes has same effect on Collaborative and Adversarial)	.057			

Notes: Standard errors clustered at team-level. Column (1) and (3) outcome is number of other castes (from zero to four) chosen as teammates for future match with stakes (monetary prize only for winning team). Column (2) and (4) outcome is number of other castes chosen for a match without stakes (monetary prizes for both teams). Oth. Caste Team Ability is the average ability index across all other caste players in a given player's team (set equal to zero in the case of no other-caste players), where the ability index is the average across three standardized baseline ability measures: maximum bowling speed, number of 4s/6s when batting, and number of catches when fielding. Oth. Caste Opp. Ability is the average ability index across all other caste opponents. Each regression controls for number of other-caste friends at baseline (and dummy for missing). Tests for equality of the coefficients in columns (1) and (2) come from a pooled regression with an interaction term between each contact variable and whether the choice was for the match with stakes or not. The bottom row gives the p-value from a test that the effect of removing Stakes on the collaborative contact effect is equal to its effect on the adversarial contact effect. *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Effects of Contact on Team Formation Beyond Immediate Interactions

	Team Choice for Match with	
	Stakes	No Stakes
	(1)	(2)
Panel A:	% of Other Castes from Other Teams	
Prop. Oth. Caste on Team	0.35**	0.09
	(0.16)	(0.15)
Prop. Oth. Caste of Opponents	Yes	Yes
Outcome Mean	1.5	1.5
Panel B:	% of Other Castes among Low Backups	
Prop. Oth. Caste of Opponents	-0.28	-0.18
	(1.37)	(2.20)
	No	No
Prop. Oth. Caste on Team	Yes	Yes
Outcome Mean	.88	1.1
Caste*League FE	Yes	Yes
Observations	768	

Notes: Standard errors clustered at team-level. Panel A outcomes are for generalization of collaborative contact effects. They are the percentage of other-caste men among other teams (in the same league) listed as future teammates for either the future match with stakes (column (1)) or the one without (column (2)). Panel B outcomes are for generalization of adversarial contact effects. They are the percentage of other-caste men among backups with priority seven or above selected as teammates for each match. Each regression controls for number of other-caste friends at baseline (and dummy for missing). *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Voting for Friends vs. Same Caste

	Vote Rank = 1 to 5		
	All	All	Non-Opp
	(1)	(2)	(3)
Own Caste Voted On	0.31***		
	(0.03)		
Baseline Friend	0.84***	0.85***	0.54
	(0.05)	(0.32)	(0.46)
Own Caste Voted On*Prop. Oth. Caste on Team		-0.13	-0.13
		(0.08)	(0.12)
Own Caste Voted On*Prop. Oth. Caste of Opp.		0.13	-0.25
		(0.38)	(0.49)
Baseline Friend*Prop. Oth. Caste on Team		0.05	-0.09
		(0.14)	(0.18)
Baseline Friend*Prop. Oth. Caste of Opp.		-0.05	0.53
		(0.46)	(0.65)
Observations	9180	9180	4570
Votee FE	Yes	Yes	Yes
Prop. Oth. Caste on Team	No	Yes	Yes
Prop. Oth. Caste of Opponents	No	Yes	Yes
Caste*League*Own Caste Voted On FE	No	Yes	Yes

Notes: The unit of observation is a voter-votee pair. Voter-clustered standard errors for column (1). Team of voter-clustered standard errors for columns (2) and (3). The outcome was reverse-coded such that a higher number is better. All columns exclude votes for teams with players only of the same caste of the voter or players only of other castes. Baseline Friend is a dummy variable equal to one if the voter listed the votee as a friend at baseline. Each column also includes a dummy variable equal to one if this baseline data is missing. Columns (2) and (3) additionally include an interaction between this dummy variable for missing and each of Prop. Oth. Caste on Team and Prop. Oth. Caste of Opp. Votee fixed effects can be included because the same person can be voted on by multiple voters. Column (3) only includes votes made on teams that were not faced as opponents during the league. Columns (2) and (3) also control for number of other-caste friends at baseline (and dummy for missing), as well as each interacted with Own Caste Voted On. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Ability Priors and Mistakes

	Predicted Rank (1)	Actual Rank (2)	Error (3)	Absolute Error (4)
Predict Self	-0.57***		0.59***	-0.18***
	(0.07)		(0.08)	(0.05)
Predict Other-Caste	0.15*		-0.03	0.03
	(0.09)		(0.11)	(0.07)
Predicted Rank		0.14***		
		(0.02)		
Observations	3686	3686	3686	3686
Number of Predictors	764	764	764	764
Predictor FE	Yes	Yes	Yes	Yes

Notes: Unit of observation is i-j pair, where i predicted the batting performance rank (from 1 to 5) of teammate j, within i's team. Standard errors clustered at team-level. Regressors Predict Self and Other-Caste are dummy variables for whether the prediction was made about i himself, or other-caste (omitted category is prediction about own-caste other than self). Predicted Rank is the prediction i made about j, whereas Actual Rank is the realized rank given actual performance in the league. Error is Actual Rank - Predicted Rank, which is positive if the player batted worse than predicted. Absolute Error is the absolute value of Error. All columns exclude predictions made after a player had already played his first match, and predictions for players that played zero matches. *** p<0.01, ** p<0.05, * p<0.1.

Table A11: Trading with Teammates and Friends of Teammates

	Whether i trades with j (=0/1)
Teammate	-0.001
	(0.009)
Friend of Oth. Caste Teammate	0.002
	(0.003)
Observations	25482
Outcome Mean	.013
α_{jcl}*Prop. Oth. Caste on Team FE	Yes

Notes: Unit of observation is i-j dyad (pair of individuals). Sample includes only dyads where (1) i is assigned to a mixed team, (2) i is given a positive monetary incentive to switch the sticker color of his gifts, and (3) j is not the same caste as i. Regression includes control for a dummy variable equal to one if i listed j as a friend at baseline, and a dummy variable equal to one if this baseline link data is missing. Standard errors are dyadic-robust at team-level. Teammate is a dummy variable equal to one if i and j are teammates. Friend of Oth. Caste Teammate is a dummy variable equal to one if j is listed at baseline as a friend of any of i's other caste teammates. α_{jcl} is set of Caste*League (of i) fixed effects fully interacted with person j fixed effects. α_{jcl} is then fully interacted with the categories of Prop. Oth. Caste on Team (0.25, 0.5, 0.75, and 1, since the sample is only those i's assigned to mixed teams). *** p<0.01, ** p<0.05, * p<0.1.

Table A12: Contact with Friends Increases Trust

	Amount Sent (1)	Stated Trust (2)	Trust Index (3)
Number of Friends on Team	1.23	0.04	0.11**
	(0.75)	(0.03)	(0.05)
Observations	2253	770	751
Outcome Mean	22.2	.21	0.03
Outcome Mean	.1	.12	.015
Prop. Oth. Caste on Team	Yes	Yes	Yes
Prop. Oth. Caste of Opp.	Yes	Yes	Yes
Caste*League FE	Yes	Yes	Yes

Notes: The unit of observation is a Sender-Recipient pair in column (1), and an individual in columns (2) and (3). Senders are partnered with one General, one OBC, and one SC/ST Recipient, such that there are three observations per Sender in column (1). Standard errors clustered at team-level. Outcome in column (1) is amount sent (Rs. 0 to 50) by Sender to Recipient in trust game. Outcome in column (2), Stated Trust, is a dummy variable coming from the question "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?". Stated Trust equals one if the respondent answers "Most people can be trusted" and equals zero if the respondent answers "Need to be very careful". Outcome in column (3), Trust Index, is the average of two variables: the standardized individual-level mean amount sent in the trust game and standardized Stated Trust. All columns include number of own-caste friends and number of other-caste friends at baseline (and dummy for missing) as controls. *** p<0.01, ** p<0.05, * p<0.1.

Table A13: Effects of Contact on Winning

	Number of Matches Won		
	Participant (1)	Team (2)	Total Pay (3)
Prop. Oth. Caste on Team	-0.03		-0.35
	(0.34)		(15.96)
Prop. Oth. Caste of Opponents	0.21		24.94
	(1.00)		(74.38)
Mixed Team		-0.17	
		(0.29)	
Team Ability Index		2.52***	
		(0.47)	
Outcome Mean	3	4	294
Caste*League FE	Yes	Yes	Yes
Observations	800	160	800

Notes: The unit of observation in columns (1) and (3) is the participant. In column (2) it is the team. Standard errors clustered at team-level. The outcome for column (1) is the number of matches played and won by each participant. The outcome for column (2) is the number of matches played and won by each team. The outcome for column (3) is the total payout in Rs. earned by each participant, including a Rs. 10 show-up fee for each match the participant played in. Mixed Team is equal to zero if all players in the team are from the same caste, and one otherwise. Team Ability Index is the average ability index across the five players in a team, where the ability index is the average across three standardized baseline ability measures: maximum bowling speed, number of 4s/6s when batting, and number of catches when fielding. Columns (1) and (3) also control for number of other-caste friends at baseline (and dummy for missing). *** p<0.01, ** p<0.05, * p<0.1.

Table A14: Collaborative Contact Effects Do Not Depend on Incentive Structure

	N. Oth. Caste		Voting	Trade	Trust
	Friends	Team Choice Stakes	Rank (1-5)	Cross-Caste	Amount Sent
	(1)	(2)	(3)	(4)	(5)
Ind. Pay*Prop. Oth. Caste on Team	1.63***	0.73***		0.06	
	(0.50)	(0.16)		(0.06)	
Team Pay*Prop. Oth. Caste on Team	0.80	0.72***		0.07	
	(0.50)	(0.17)		(0.05)	
Ind. Pay*Prop Oth. Caste on Team*Own Caste			-0.03		-0.88
			(0.14)		(2.08)
Team Pay*Prop Oth. Caste on Team*Own Caste			-0.14		0.11
			(0.10)		(1.79)
Observations	770	768	9180	1510	2253
Outcome Mean	3.5	1.5	3	.56	22
p(Team = Individual)	.24	.99	.5	.98	.72

Notes: Standard errors clustered at team-level. Each regression estimates the effect of collaborative contact separately for those on Team Pay and Individual Pay teams. To do this, each column includes a set of covariates (used previously) fully interacted with a dummy for Individual Pay. For the set of covariates for column (1), see Panel A, column (2) of Table 1. For column (2), see column (1) of Table 3. For column (3), see column (5) of Table 4. For column (4), see column (2) of Table 5. For column (5), see column (3) of Table 6. The unit of observation is the participant for columns (1) and (2). The unit of observation is a voter-votee pair in column (3), a participant-good in column (4), and a sender-recipient pair in column (5). Ind. (Team) Pay is a dummy variable equal to one if the participant's team receives Individual (Team) Pay incentives. Own Caste is a dummy variable equal to one if the votee/recipient is the same caste as the voter/sender for the voting and trust outcomes. The outcome for each column is: (1) number of other-caste men participant considers friends, (2) number of other-caste men participant chose as teammates for match with stakes, (3) vote rank given for field trip (5 is best), (4) dummy variable equal to one if good was traded with someone from a different caste, and (5) amount sent in trust game (Rs. 0 to 50). *** p<0.01, ** p<0.05, * p<0.1.

Table A15: Cross-Caste Interactions with Teammates Do Not Depend on Incentive Structure

	Friendly			Hostile		Proportion Hostile
	High-Fives	Congrats	Hugs	Arguments	Insults	
	(1)	(2)	(3)	(4)	(5)	(6)
Individual Pay	-0.11	0.06	-0.12	-0.06	-0.01	-0.02
	(0.10)	(0.04)	(0.12)	(0.04)	(0.01)	(0.03)
Observations	3153	3153	3153	3153	3153	1967
Team Pay Mean	.53	.2	.91	.12	.029	.083
Match FE	Yes	Yes	Yes	Yes	Yes	Yes
Caste i*Caste j FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Unit of observation is individual dyad-match (ijt). Sample only includes dyad-match observations where (1) neither i or j is a backup player, (2) i and j belong to the same team, and (3) i and j are members of different castes. Standard errors are dyadic-robust at team-level. Individual Pay is a dummy variable equal to one if i and j are playing on a team assigned to Individual Pay incentives. Team Pay Mean is the mean of the outcome for all dyad-matches in which i and j are on a team assigned to Team Pay incentives. The outcomes for columns (1) to (5) are the counts of interactions that i and j were involved in during match t, where the interactions are: (1) high-fives, (2) hugs/taps on back, (3) one player complimenting/congratulating another player, (4) arguments, and (5) one player insulting (sledging) another player. The sample in column (6) is further restricted to those dyad-matches involved in at least one interaction. The outcome is the total number of hostile interactions ((4)+(5)) divided by the total number of interactions ((1)+(2)+(3)+(4)+(5)). *** p<0.01, ** p<0.05, * p<0.1.

Table A16: Lower Castes Are Lower Ability

	Baseline Ability			Individual Pay Per Match	
	Bowl	Bat	Field		
	(1)	(2)	(3)	(4)	(5)
OBC	-1.11	-0.10	0.05	0.01	1.82
	(0.90)	(0.11)	(0.10)	(2.75)	(2.50)
SC/ST	-3.45***	-0.31***	-0.22**	-12.92***	-7.45***
	(0.84)	(0.11)	(0.10)	(2.64)	(2.40)
Age	0.61***	0.04***	0.05***	3.72***	2.94***
	(0.09)	(0.01)	(0.01)	(0.35)	(0.34)
Bowl Ability					6.69***
					(1.00)
Bat Ability					8.18***
					(1.16)
Field Ability					4.46***
					(0.86)
Observations	800	800	800	769	769
General Caste Outcome Mean	88.1	1.94	5.02	50.7	
Outcome Standard Deviation	10.1	1.29	1.16	33	
League FE	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors. Ability measures are from baseline ability testing. Bat Ability is number of 4s/6s (out of 6), standardized when used as regressor (in column (5)) such that one unit corresponds to one standard deviation. Bowl Ability is maximum bowling speed and Field Ability is number of catches (out of 6). Both are also standardized when used as regressors. Individual Pay Per Match is the average payout the player would have received per match, based on his performance, if he received Individual Pay incentives (for those with Team Pay, this is counterfactual pay, for those with Individual Pay, it is actual pay). *** p<0.01, ** p<0.05, * p<0.1.

Table A17: Discrimination in Within-Team Allocation

	Captain Choice		Batting Order		Bowling Order	
	(1)	(2)	(3)	(4)	(5)	(6)
OBC	-0.32	-0.26	-0.24**	-0.23**	-0.04	-0.04
	(0.28)	(0.29)	(0.10)	(0.10)	(0.12)	(0.12)
SC/ST	-1.16***	-0.96***	-0.69***	-0.59***	-0.54***	-0.43***
	(0.26)	(0.26)	(0.10)	(0.09)	(0.12)	(0.12)
Age	0.28***	0.26***	0.14***	0.12***	0.11***	0.08***
	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
Bowl Ability		0.28***		0.18***		0.33***
		(0.10)		(0.03)		(0.04)
Bat Ability		0.33***		0.27***		0.19***
		(0.09)		(0.03)		(0.04)
Field Ability		0.21*		0.22***		0.19***
		(0.12)		(0.04)		(0.04)
Observations	4814		6400			
Estimation	Conditional Logit		Rank-ordered Logit			

Notes: Standard errors clustered at team-level. The unit of observation is the player-match. Columns (1) and (2) exclude backup players since they could not be selected as captains. Captain Choice is equal to one if the player was chosen as the captain of his team for a given match, and zero otherwise. Batting and Bowling Order range from 1 to 5, giving the order within a team for a given match. These two outcomes are reverse-coded so that a higher number is better. Bowlers are not explicitly ordered from 1 to 5 – I use the number of balls actually bowled to rank each team member in each match, yielding a bowling order (in which there may be ties). Coefficients reflect effects on the latent utility from choosing a player as a captain, batsman, or bowler. Bat Ability is number of 4s/6s (out of 6), standardized such that one unit corresponds to one standard deviation. Bowl Ability is maximum bowling speed and Field Ability is number of catches (out of 6). Both are also standardized. *** p<0.01, ** p<0.05, * p<0.1.

Table A18: Caste Heterogeneity of Contact Effects (I)

	N. Oth. Caste		Trade
	Friends	Team Choice Stakes	Cross-Caste
	(1)	(2)	(3)
Prop. Oth. Caste on Team: General	1.29**	0.65***	0.19***
	(0.53)	(0.17)	(0.07)
.... OBC	0.54	0.61***	-0.03
	(0.69)	(0.18)	(0.06)
.... SC/ST	1.32***	0.88***	0.03
	(0.51)	(0.22)	(0.07)
p(General = OBC)	.39	.86	.018
p(OBC = SC/ST)	.36	.34	.56
p(General = SC/ST)	.96	.39	.099
Prop. Oth. Caste of Opponents: General	-5.94*	0.39	-0.02
	(3.06)	(0.82)	(0.27)
.... OBC	0.20	0.69	-0.02
	(2.99)	(0.79)	(0.28)
.... SC/ST	-3.92*	1.26	-0.52*
	(2.26)	(0.89)	(0.29)
p(General = OBC)	.14	.8	.99
p(OBC = SC/ST)	.27	.63	.21
p(General = SC/ST)	.61	.49	.19
Observations	770	768	1510
Caste*League FE	Yes	Yes	Yes
Trade and Color-Switch Bonus FE	No	No	Yes

Notes: Standard errors clustered at team-level. Each column corresponds to one regression, with the outcome regressed on the two contact regressors fully interacted with the three caste group dummy variables. The unit of observation is the participant for columns (1) and (2), and the participant-good for column (3). The outcomes for each column are: (1) number of other-caste men participant considers friends, (2) number of other-caste men participant chose as teammates for future match with stakes, (3) dummy variable equal to one if good was traded with someone from a different caste. Trade and Color-Switch Bonus FE are dummy variables for the participant's trading and color-switching incentives. All regressions include number of other-caste friends at baseline (and dummy for missing), with each interacted with the three caste group dummy variables. *** p<0.01, ** p<0.05, * p<0.1.

Table A19: Caste Heterogeneity of Contact Effects (II)

	Voting	Trust
	Rank (1-5)	Amount Sent
	(1)	(2)
Own Caste*Prop. Oth. Caste on Team: General	0.01	-2.20
	(0.14)	(2.03)
.... OBC	-0.28**	-0.38
	(0.12)	(2.54)
.... SC/ST	-0.10	1.66
	(0.13)	(2.78)
p(General = OBC)	.15	.57
p(OBC = SC/ST)	.33	.58
p(General = SC/ST)	.58	.26
Own Caste*Prop. Oth. Caste of Opponents: General	0.29	-4.63
	(0.63)	(9.39)
.... OBC	1.02**	-3.16
	(0.49)	(9.63)
.... SC/ST	-1.11*	10.02
	(0.64)	(11.16)
p(General = OBC)	.28	.91
p(OBC = SC/ST)	.011	.38
p(General = SC/ST)	.11	.33
Observations	9180	2253
Caste*League*Own Caste Recipient FE	Yes	Yes
Prop. Oth. Caste on Team*Caste FE	Yes	No
Prop. Oth. Caste of Opponents*Caste FE	Yes	No
Votee FE	Yes	No
Sender FE	No	Yes
Age of Recipient	No	Yes

Notes: Standard errors clustered at team-level. Each column corresponds to one regression, and shows the effect of each type of contact on own-caste favoritism (in voting and trust) separately for each caste. The unit of observation is a voter-votee pair for column (1), and a sender-recipient pair in column (2). The outcome in column (1) is the vote rank given for the field trip (5 is best). The outcome in column (2) is the amount sent in the trust game (from Rs. 0 to 50). Both regressions include number of other-caste friends at baseline (and dummy for missing), with each interacted with the three caste group dummy variables. *** p<0.01, ** p<0.05, * p<0.1.

Table A20: Each Caste Responds Similarly to Trading Incentives

	Cross-Caste Trade (1)
Color Switch Bonus = 50: General	0.23***
	(0.06)
.... OBC	0.24***
	(0.06)
.... SC/ST	0.18***
	(0.06)
Color Switch Bonus = 100: General	0.27***
	(0.06)
.... OBC	0.25***
	(0.06)
.... SC/ST	0.21***
	(0.06)
Observations	1510
Prop. Oth. Caste on Team	Yes
Prop. Oth. Caste of Opponents	Yes
Caste*League FE	Yes
Trade Bonus Dummy	Yes
p(General 50 = OBC 50)	.92
p(OBC 50 = SC/ST 50)	.42
p(General 50 = SC/ST 50)	.53
p(General 100 = OBC 100)	.84
p(OBC 100 = SC/ST 100)	.6
p(General 100 = SC/ST 100)	.48

Notes: Standard errors clustered at team-level. The regression shows the effect of each Color-Switch Bonus on cross-caste trade, separately for each caste. The unit of observation is the participant-good, meaning there are two observations per participant. The outcome is a dummy variable equal to one if the good was successfully traded with someone from a different caste. Trade Bonus Dummy is equal to one if the participant was assigned Rs. 20 for each successful trade, and zero if the participant was assigned Rs. 10 for each successful trade. The regression controls for number of other-caste friends at baseline (and dummy for missing). *** p<0.01, ** p<0.05, * p<0.1.

B Learning Model

In this appendix section I develop a simple model to formalize how the type of contact can mediate impacts on future intergroup behaviors through a learning channel. The starting point is that integration leads to learning about the underlying "types" of other-caste players. The type of integration affects the nature of this learning by changing the structure of signals observed about others.

B.1 Bayesian Information Processing

Each participant is either a good (friendly) or bad (hostile) type, denoted by $\beta_i \in \{\beta_G, \beta_B\}$. I assume that each participant knows the types of players from their own caste[62] (due to more frequent interaction), but learns about the types of other-caste players through observing signals of their types during cricket matches.

For simplicity, assume that two players i and j play together for one match. They face two possible types of contact: they either belong to the same team ($m = 1$) or they are opponents ($m = 0$). During the match, each player can either be friendly to the other ($y = 1$) or be hostile ($y = 0$). A friendly action could be to encourage the other verbally, while a hostile action could be to argue with the other player. Players i and j each observe one signal (y) from the other about their type.

I assume the net utility of player i being friendly with player j to be

$$u_{ij} = \alpha + \phi_1 \mathbb{1}[\beta_i = \beta_G] + \phi_2 m_{ij} + \varepsilon_{ij} \tag{5}$$

where $\varepsilon_{ij} \sim \text{Logistic}(0,1)$. Good types have greater net utility from being friendly with others than bad types ($\phi_1 > 0$). In addition, since teammates have common goals and opponents do not, players receive greater net utility from being friendly with teammates than opponents ($\phi_2 > 0$).[63]

This underlying utility micro-founds the signal structure. Defining π_m^β as $P(y = 1 \mid \beta, m)$, the probability of seeing the other player be friendly given their type and the type of contact, it follows that

$$\pi_m^\beta = P(u_{ij} \geq 0) = \frac{e^{\alpha + \phi_1 \mathbb{1}[\beta_i = \beta_G] + \phi_2 m}}{1 + e^{\alpha + \phi_1 \mathbb{1}[\beta_i = \beta_G] + \phi_2 m}} \tag{6}$$

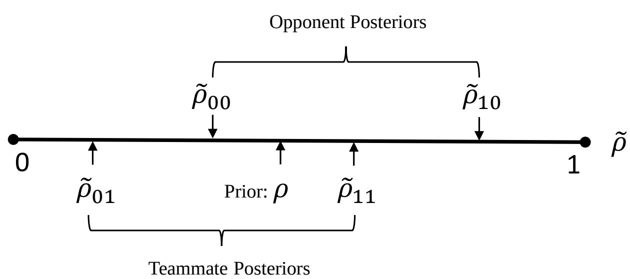
This signal structure has the following features: (i) $\pi_m^G > \pi_m^B \,\forall m$: good types are more likely to be friendly

[62] Empirical results are consistent with this – for example, own-caste contact has only weak effects on own-caste friendships (panel B, Table 1).

[63] This could be further micro-founded by assuming that players receive utility from winning matches and that being friendly with teammates increases the probability of winning more than being friendly with opponents.

than bad types, whether they are teammates or opponents; (ii) $\pi_1^\beta > \pi_0^\beta \; \forall \beta$: teammates are more likely to be friendly than opponents, whether good or bad types; and (iii) $\frac{\pi_0^G}{\pi_0^B} > \frac{\pi_1^G}{\pi_1^B}$, $\frac{1-\pi_0^G}{1-\pi_0^B} > \frac{1-\pi_1^G}{1-\pi_1^B}$: a monotone likelihood ratio property ensuring that posteriors have an intuitive ordering[64] (see Online Appendix B.5 for all proofs).

Players hold the common and correct prior ρ that others are good types.[65] Suppose now that players i and j are randomly assigned to be teammates or opponents – i.e. as in the experiment, the type of contact is random. After playing the match, each player updates as a rational (Bayesian) information processor. I first consider the case where i rationally conditions on m. Here i recognizes the fact that opponents should be more hostile, and correspondingly discounts hostile behavior when $m = 0$. More generally, rational information processors should condition on the type of contact (the "situation") when forming inferences about others. In this case, posteriors $\tilde{\rho}_{sm}$ (where $s = 1$ if the friendly signal is observed) can be summarized as:



since $\tilde{\rho}_{10} > \tilde{\rho}_{11} > \rho > \tilde{\rho}_{00} > \tilde{\rho}_{01}$. The type of contact affects the distribution of posteriors – in particular, the highest possible posterior occurs when opponents are friendly, since given the incentives they have, a friendly opponent sends a strong signal that they are a good type. In contrast, the type of contact does not affect the expected posterior, i.e.

$$E_\rho\left[\tilde{\rho} \mid m = 0\right] = E_\rho\left[\tilde{\rho} \mid m = 1\right] = \rho \tag{7}$$

This result follows from the well-known martingale property of Bayesian models. This feature of the fully rational model suggests that the type of contact should have limited impact on inferences about the

[64] For example, if instead $\frac{1-\pi_0^G}{1-\pi_0^B} < \frac{1-\pi_1^G}{1-\pi_1^B}$, it can be the case that players update *less* negatively after observing hostile behavior ($y = 0$) from a teammate than after observing hostile behavior from an opponent. This result is counterintuitive given that hostile behavior should to some extent be expected of opponents.

[65] As explained below, the most important implications of the model are similar if I instead assume that players hold incorrect priors.

type of others. The intuition is clear: though players randomly assigned to be opponents are more hostile, the fully rational Bayesian does not conclude from this that these opponents are more likely to be bad types – this agent properly accounts for how the situation drove the behavior, not the person.

B.2 Fundamental Attribution Error

A large literature in social psychology challenges the claim that individuals properly account for the situation when making inferences about others. Evidence from many settings shows that individuals commit the so-called "fundamental attribution error", over-inferring character traits of individuals from behavior relative to situational effects (Jones and Harris (1967); Jones and Nisbett (1971); Nisbett et al. (1973); Ross (1977); Gilbert and Malone (1995); Ross and Nisbett (2011)). This evidence suggests that a more natural model in this setting is one in which players over-attribute behavior to underlying types.

To model these attribution errors, I assume that players continue to use Bayes' rule to update beliefs, but fail to condition on m (similar to the approaches of Jehiel (2005); Eyster and Rabin (2005); Furukawa (2017); Chauvin (2018))[66] – treating signals from teammates and opponents identically.[67] It now follows that

$$E^b_\rho[\tilde{\rho} \mid m=0] < \rho < E^b_\rho[\tilde{\rho} \mid m=1] \tag{8}$$

where the b superscript references the bias. With attribution bias, the type of contact systematically affects the expected posterior, with the two types of contact moving the expected posterior in *opposite* directions from the prior. In expectation, players infer that randomly chosen opponents are less likely to be good types than randomly chosen teammates. Players do so because, conditional on the type of behavior observed, players have the same posterior belief regardless of whether the observed behavior was from an opponent or from a teammate. Since friendly signals are more likely to be observed from teammates, this attribution bias leads the expected posteriors to diverge.

[66] I am agnostic as to the source of the lack of conditioning, though one possibility is that conditioning takes cognitive effort. In support of this explanation, evidence exists that individuals are more likely to commit the fundamental attribution error when under cognitive load (Gilbert (1989)). An alternative explanation is that individuals' motivated "belief in a just world" leads them to attribute behaviors to internal factors rather than external causes, such that people "get what they deserve" (Bénabou and Tirole (2006)).

[67] Haggag et al. (2018) study intrapersonal (as opposed to interpersonal in this paper) attribution bias in the context of consumer choice: when individuals decide their value of drinking a new drink, they fail to properly condition on the (random) state in which they consumed it last time. Their model of attribution bias does not explicitly map to Bayesian learning, but has the advantage of allowing attribution bias to range from zero to one, nesting the extreme cases of perfect and no conditioning. Other papers in economics study intrapersonal attribution errors through the lens of motivated forgetting, e.g. through recalling past successes more than past failures (Bénabou and Tirole (2002), Bénabou and Tirole (2006)).

B.3 Decisions to Interact

I do not observe $\tilde{\rho}$ directly in the data, and consequently cannot test the theory directly by comparing the sample expectation of posteriors across treatments. Instead, I observe each player's choices of whom to interact with. Focusing on the case of social interaction, suppose that players select others as friends only when $\tilde{\rho} > c$. Without attribution bias, it follows that

$$P(\tilde{\rho} > c \mid m = 0) \lesseqgtr P(\tilde{\rho} > c \mid m = 1) \tag{9}$$

meaning that, without bias, the type of contact has an ambiguous effect on the likelihood of friendship, with the ambiguity depending on the exact cutoff c. For some cutoffs it is even possible for opponents to be *more* likely to become friends than teammates. This result holds because an instance of opponent friendliness is particularly informative of their type.

The model with attribution bias does not have the same ambiguity, since regardless of c it implies that

$$P^b(\tilde{\rho} > c \mid m = 0) \leq P^b(\tilde{\rho} > c \mid m = 1) \tag{10}$$

i.e. players are weakly more likely to become friends with teammates than opponents regardless of the cutoff. In this sense, the results most naturally fit with a model of belief updating with attribution bias.

B.4 Discussion

Friendliness vs. Ability. In the model, players update only about the friendliness of other-caste players. In the experiment, there is an important second dimension of updating: players learn about the cricket ability of other-caste players. Along this dimension, it is plausible that the type of contact should not affect updating. Though participants observe very different signals of friendliness from teammates vs. opponents, the signals of cricket ability observed are likely to be similar. In this sense, the type of contact might systematically affect learning along some dimensions but not others.

Incorrect Priors. To simplify the exposition, I assume that priors are correct. A more plausible assumption may be that priors are incorrect, such that $\rho \neq \rho^t$, where ρ^t is the true proportion of other-castes that are good types. In this case, the type of contact can affect the speed of learning ($| \rho^t - E_{\rho^t}[\tilde{\rho} \mid m = x] |$) even in the absence of attribution bias.[68] But only with attribution bias can the learning (in expectation) go in opposite directions from the prior, depending on the type of contact. In this sense, even with

[68]However, the predicted effect of the type of contact on the speed of learning is of ambiguous sign.

incorrect priors the model with attribution bias is a more natural model through which to interpret the results.

Individuals vs. Groups. The model focuses on inferences about the types of individuals. Similar updating can occur about the caste group as a whole if we assume a second level of uncertainty, regarding the proportion of types in the caste group. Signals of behavior from individuals are then used to also update about the group. In the empirics I explore effects of contact on behaviors toward individuals directly interacted with, as well as the broader caste group.

B.5 Model Proofs

Endogenous Signal Structure

Utility structure implies that $\pi_m^G > \pi_m^B \ \forall m$:

$$\pi_m^G = \frac{e^{\alpha+\phi_1+\phi_2 m}}{1+e^{\alpha+\phi_1+\phi_2 m}} \gtrless \frac{e^{\alpha+\phi_2 m}}{1+e^{\alpha+\phi_2 m}} = \pi_m^B$$
$$e^{\alpha+\phi_1+\phi_2 m} + e^{2\alpha+\phi_1+2\phi_2 m} \gtrless e^{\alpha+\phi_2 m} + e^{2\alpha+\phi_1+2\phi_2 m}$$
$$e^{\alpha+\phi_1+\phi_2 m} > e^{\alpha+\phi_2 m}$$

since $\phi_1 > 0$. It follows that $\pi_1^\beta > \pi_0^\beta \ \forall \beta$ given the symmetry in the problem, and that $\phi_2 > 0$.

Utility structure implies that $\frac{\pi_0^G}{\pi_0^B} > \frac{\pi_1^G}{\pi_1^B}$:

$$\frac{\pi_0^G}{\pi_0^B} = \frac{\frac{e^{\alpha+\phi_1}}{1+e^{\alpha+\phi_1}}}{\frac{e^{\alpha}}{1+e^{\alpha}}} \gtrless \frac{\frac{e^{\alpha+\phi_1+\phi_2}}{1+e^{\alpha+\phi_1+\phi_2}}}{\frac{e^{\alpha+\phi_2}}{1+e^{\alpha+\phi_2}}} = \frac{\pi_1^G}{\pi_1^B}$$
$$\frac{e^{2\alpha+\phi_1+\phi_2}}{(1+e^{\alpha+\phi_1})(1+e^{\alpha+\phi_2})} \gtrless \frac{e^{2\alpha+\phi_1+\phi_2}}{(1+e^{\alpha})(1+e^{\alpha+\phi_1+\phi_2})}$$
$$1+e^{\alpha}+e^{\alpha+\phi_1+\phi_2}+e^{2\alpha+\phi_1+\phi_2} \gtrless 1+e^{\alpha+\phi_1}+e^{\alpha+\phi_2}+e^{2\alpha+\phi_1+\phi_2}$$
$$e^{\alpha+\phi_1+\phi_2} - e^{\alpha+\phi_1} \gtrless e^{\alpha+\phi_2} - e^{\alpha}$$
$$e^{\phi_1}e^{\phi_2} - e^{\phi_1} \gtrless e^{\phi_2} - 1$$
$$e^{\phi_1} > 1$$

since $\phi_1 > 0$.

Utility structure implies that $\frac{1-\pi_0^G}{1-\pi_0^B} > \frac{1-\pi_1^G}{1-\pi_1^B}$:

$$\begin{aligned}
\frac{1-\pi_0^G}{1-\pi_0^B} = \frac{\frac{1}{1+e^{\alpha+\phi_1}}}{\frac{1}{1+e^{\alpha}}} &\gtrless \frac{\frac{1}{1+e^{\alpha+\phi_1+\phi_2}}}{\frac{1}{1+e^{\alpha+\phi_2}}} = \frac{1-\pi_1^G}{1-\pi_1^B} \\
\frac{1+e^{\alpha}}{1+e^{\alpha+\phi_1}} &\gtrless \frac{1+e^{\alpha+\phi_2}}{1+e^{\alpha+\phi_1+\phi_2}} \\
1+e^{\alpha}+e^{\alpha+\phi_1+\phi_2}+e^{2\alpha+\phi_1+\phi_2} &\gtrless 1+e^{\alpha+\phi_2}+e^{\alpha+\phi_1}+e^{2\alpha+\phi_1+\phi_2} \\
e^{\alpha+\phi_1+\phi_2}-e^{\alpha+\phi_1} &> e^{\alpha+\phi_2}-e^{\alpha}
\end{aligned}$$

from the working above.

Posteriors – No Attribution Bias

Posteriors follow from the application of Bayes' Rule, i.e. that $P(\beta_i=\beta_G \mid y,m) = \frac{P(y|\beta_i=\beta_G,m)\cdot P(\beta_i=\beta_G,m)}{P(y,m)}$. It follows that posteriors are:

	Teammate: $m=1$	Opponent: $m=0$
$y=1$	$\tilde{\rho}_{11} = \frac{\rho\pi_1^G}{\rho\pi_1^G+(1-\rho)\pi_1^B}$	$\tilde{\rho}_{10} = \frac{\rho\pi_0^G}{\rho\pi_0^G+(1-\rho)\pi_0^B}$
$y=0$	$\tilde{\rho}_{01} = \frac{\rho(1-\pi_1^G)}{\rho(1-\pi_1^G)+(1-\rho)(1-\pi_1^B)}$	$\tilde{\rho}_{00} = \frac{\rho(1-\pi_0^G)}{\rho(1-\pi_0^G)+(1-\rho)(1-\pi_0^B)}$

Posteriors – Attribution Bias

In this case, $P(\beta_i=\beta_G \mid y=a,m=1) = P(\beta_i=\beta_G \mid y=a,m=0) = P(\beta_i=\beta_G \mid y=a)$. Posteriors are now the same for teammates and opponents, conditional on the signal observed:

	Teammate: $m=1$, Opponent: $m=0$
$y=1$	$\tilde{\rho}_1^b = \frac{\rho(\pi_1^G+\pi_0^G)}{\rho(\pi_1^G+\pi_0^G)+(1-\rho)(\pi_1^B+\pi_0^B)}$
$y=0$	$\tilde{\rho}_0^b = \frac{\rho(2-\pi_1^G-\pi_0^G)}{\rho(2-\pi_1^G-\pi_0^G)+(1-\rho)(2-\pi_1^B-\pi_0^B)}$

Expected posteriors now depend on the type of contact (equation 8):

$$E_\rho^b[\tilde{\rho} \mid m=1] = \rho\pi_1^G\tilde{\rho}_1^b + \rho\left(1-\pi_1^G\right)\tilde{\rho}_0^b + (1-\rho)\pi_1^B\tilde{\rho}_1^b + (1-\rho)\left(1-\pi_1^B\right)\tilde{\rho}_0^b > \rho$$

since $\tilde{\rho}_1^b > \tilde{\rho}_{11}$ and $\tilde{\rho}_0^b > \tilde{\rho}_{01}$ (the posteriors in each state are greater than when conditioning on the situation, but the probability with which each state occurs is unchanged). Similarly:

$$E_\rho^b[\tilde{\rho} \mid m=0] = \rho\pi_0^G\tilde{\rho}_1^b + \rho\left(1-\pi_0^G\right)\tilde{\rho}_0^b + (1-\rho)\pi_0^B\tilde{\rho}_1^b + (1-\rho)\left(1-\pi_0^B\right)\tilde{\rho}_0^b < \rho$$

since $\tilde{\rho}_1^b < \tilde{\rho}_{10}$ and $\tilde{\rho}_0^b < \tilde{\rho}_{00}$.

Probability of Selecting Friends – No Attribution Bias

To see equation 9, note that the cutoff c can fall into five relevant regions:

1. $0 \leq c < \tilde{\rho}_{01}$: $P[\tilde{\rho} > c \mid m=0] = P[\tilde{\rho} > c \mid m=1] = 1$
2. $\tilde{\rho}_{01} \leq c < \tilde{\rho}_{00}$: $P[\tilde{\rho} > c \mid m=0] > P[\tilde{\rho} > c \mid m=1]$
3. $\tilde{\rho}_{00} \leq c < \tilde{\rho}_{11}$: $P[\tilde{\rho} > c \mid m=0] = \rho\pi_0^G + (1-\rho)\pi_0^B < \rho\pi_1^G + (1-\rho)\pi_1^B = P[\tilde{\rho} > c \mid m=1]$
4. $\tilde{\rho}_{11} \leq c < \tilde{\rho}_{10}$: $P[\tilde{\rho} > c \mid m=0] > P[\tilde{\rho} > c \mid m=1]$
5. $\tilde{\rho}_{10} \leq c \leq 1$: $P[\tilde{\rho} > c \mid m=0] = P[\tilde{\rho} > c \mid m=1] = 0$

Teammates are more likely to become friends than opponents in Region 3, but in Regions 2 and 4 the opposite is true.

Probability of Selecting Friends – Attribution Bias

To see equation 10, note that there are now only three relevant regions for the cutoff:

1. $0 \leq c < \tilde{\rho}_0^b$: $P[\tilde{\rho} > c \mid m=0] = P[\tilde{\rho} > c \mid m=1] = 1$
2. $\tilde{\rho}_0^b \leq c < \tilde{\rho}_1^b$: $P[\tilde{\rho} > c \mid m=0] = \rho\pi_0^G + (1-\rho)\pi_0^B < \rho\pi_1^G + (1-\rho)\pi_1^B = P[\tilde{\rho} > c \mid m=1]$
3. $\tilde{\rho}_1^b \leq c \leq 1$: $P[\tilde{\rho} > c \mid m=0] = P[\tilde{\rho} > c \mid m=1] = 0$

There are now no regions where opponents are more likely to become friends than teammates. In this sense, the attribution bias model maps more easily to the stylized facts of the empirics. In particular, the attribution bias model is consistent with the results when $\tilde{\rho}_0^b \leq c < \tilde{\rho}_1^b$, whereas the Bayesian model is only consistent with the results when $\tilde{\rho}_{00}^b \leq c < \tilde{\rho}_{11}^b$, a smaller region.

C Pre-registration Differences

This study was pre-registered (without a formal pre-analysis plan) in the AEA registry with ID #0001856. The key differences between the paper and the pre-registration are:

- The pre-registration describes the experimental variation in monetary incentives designed to test whether the type of contact mediates treatments effects. In the paper, I estimate these effects (Section 7.2), but devote more attention to the variation in contact induced by random assignment to teammates vs. opponents. Partway through the experiment it became clear that the latter source of variation could be exploited and would be informative, and later it became clear that while the latter variation affected the nature of interactions (Table A1), the former did not (Table A15). As a result I describe both sets of results in the paper, but focus my attention on the "stronger" treatment.

- The pre-registration describes 11 primary and secondary outcomes. I measured all of these outcomes, but do not report effects on three of them in the present paper:
  - First, I do not estimate effects on collective field trip voting (votes jointly agreed on by each team) since this paper focuses on how *individual*-level beliefs, preferences, and behaviors, respond to variation in intergroup contact.
  - Second, the caste IAT data I collected measures associations between General/Scheduled Caste and Good/Bad. This outcome turns out to not fit naturally with the set of outcomes I report in this paper given that the implicit preferences here relate to General Castes vs. Scheduled Castes rather than Ingroup Castes vs. Outgroup Castes. As a result, the IAT data has nothing to say about preferences towards OBCs, and unclear predictions for when OBCs have contact with both General and Scheduled Castes.
  - Third, participants answered vignettes that aimed to capture their willingness to cooperate with other-caste members (as signalled by names) immediately after each match. I omit this data in the paper due to two doubts: first, there is no baseline evidence of ingroup bias in the answers, and second, these measures are much more short-term than those I ultimately focus on in the paper.

- As detailed in the pre-registration, I held income lotteries after each match. I held these lotteries to allow me to explore income effects as a channel for the effects of contact in case contact also affected income. In practice, neither collaborative nor adversarial contact affected match earnings (column 3, Table A13), and so I don't exploit the variation given by the income lotteries.

D Randomization Details

Re-randomization

Only caste was used for stratification when randomly assigning participants to the leagues and teams. To avoid other chance imbalances, I re-randomized, following Banerjee et al. (2017). I ran the full randomization 100 times, selecting the run with the minimum maximum t-statistic from a series of balance checks on age, maximum bowling speed, total 4s/6s during the batting test, whether would volunteer, and whether attend school. Ideally I would also have used the social network data for re-randomization. Unfortunately timing constraints made this infeasible, given that the social network survey was done separately after the baseline survey.

This re-randomization approach aimed to improve balance between league vs. control participants, and between mixed vs. homogeneous-caste teams. Since the network-based randomization of the match schedule was not re-randomized, balance for adversarial contact was not affected by this approach.

Match Schedule Generation

In principle, to randomly determine the match schedule we need only consider the set of all possible simple 8-regular graphs, and randomly choose one. In practice, this set of graphs is too large for this approach to be feasible. I instead used an existing algorithm, Bollobás' "pairing method", to choose a random simple 8-regular graph. This algorithm works as follows:

1. Start with a set of 20 nodes. Create a set of 20*8 = 160 points, associating each set of 8 points with one of the nodes (teams).
2. Choose two points randomly and pair them.
3. If these two points are associated with the same team (= team playing itself) or are already connected (= teams already assigned to play one another), go back to 2.
4. Add an edge (fixture) between the two teams these points are associated with.
5. Remove the two points that have now been successfully paired.
6. If any points are left, go back to 2. and continue pairing.
7. If no points are left, exit. Create adjacency matrix from the resultant team pairings.

Stage three ensures that the resulting graph is simple. In practice, the algorithm may not complete successfully (this is more likely as k grows). In these cases, the algorithm is re-started.