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Types of Contact: A Field Experiment on Collaborative and Adversarial Caste Integration†

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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* C93, D83, D91, J15, O15, Z13, Z21)

Social psychologists have long theorized that the effects of intergroup contact on prejudice should depend on the *type* of contact: in particular, contact should only reduce prejudice when the integrated groups have common goals, intergroup cooperation, equal status, and the support of authorities (Allport 1954, Pettigrew 1998). 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, Green, and Green 2018).[1] This missing evidence is important: policymakers cannot optimally design integrative policies without an understanding

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[1] Existing evidence on the importance 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 that the conditions facilitate, but are not necessary for, prejudice reduction. Experimental evidence for the positive effects of contact in general is growing, and reviewed in Paluck, Green, and Green (2018). For more background on the contact hypothesis, see online Appendix Section B.

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 restructuring 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 8 one-month-long cricket leagues, and assigned the others to a control group. Of those assigned to play, I assigned 35 percent 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: these cooperative groups might be coworkers on political campaigns or company cofounders. In contrast, groups with competing goals have incentives to undermine each other, as evidenced in police-protester interactions and the perceived competition with outgroups for places in elite colleges. Consistent with these ideas, in the leagues cross-caste interactions with opponents are 50 percentage points more likely to be hostile than cross-caste interactions with teammates. These different intergroup behaviors may drive belief updating in opposite directions, especially if participants make attribution errors (Ross and Nisbett 2011, model in online Appendix Section C): wrongly attributing the prosocial (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. 2021). 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.

While I preregistered each of these outcomes, my preregistration emphasizes a different source of variation in the type of contact: the variation driven by randomization to more versus less collaborative monetary incentives. Partway

[2]For example, a series of papers find negative political effects of exposure to immigrants and refugees (Dahlberg, Edmark, and Lundqvist 2012; Enos 2014; Halla, Wagner, and Zweimüller 2017; Dustmann, Vasiljeva, and Piil Damm 2018; Tabellini 2020), with one exception being the lack of correlation between local immigration and the rise of the Sweden Democrats (Dal Bó et al. 2018).

into the experiment I realized that an additional useful source of variation existed (teammates versus 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, though I focus more on the "stronger" variation of teammates versus opponents.[3]

My first set of findings considers players' willingness to interact. Collaborative and adversarial contact have opposite effects on self-reported cross-caste friendships. Having all other-caste teammates instead of none increases the number of other-caste friends by 1, while having all other-caste opponents instead of none decreases the number of other-caste friends by 3.5. These friendship effects are not merely driven by direct contact, players becoming friends with teammates and disliking opponents, or network introductions to the preexisting friends of teammates and opponents. Furthermore, collaborative contact is no more potent when other-caste teammates are high-ability cricketers: this contact reduces division through working together, not winning together. A natural interpretation of this first set of findings, 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 (Cornell and Welch 1996; Bohren, Imas, and Rosenberg 2019), 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. 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 towards choosing players on the basis of desired social interaction, 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 percent, while adversarial contact has imprecise effects. An accounting exercise suggests that the collaborative effect is unlikely to be fully explained by generalized ability belief updating, and rather may reflect a shift in social preferences.

My third set of findings explore the efficiency effects of contact. Collaborative contact increases cross-caste trade by up to 21 percent and trade payouts by 18 percent, 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

[3] I describe differences between the paper and the preregistration in detail in online Appendix Section D.

significantly more than collaborative contact, which has small or somewhat negative effects.

Summarizing the three sets of findings, full collaborative contact increases an omnibus index of cross-caste behavior by 0.23σ ($p < 0.01$) while full adversarial contact reduces such behaviors by 0.38σ ($p = 0.07$, p-value of the difference < 0.01).

In support of the contact hypothesis, 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 cross-caste behaviors. 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.[4] Those assigned to mixed teams score 0.22σ higher on the cross-caste behavior index than those assigned to the control group. Those assigned to homogeneous-caste teams are also positively affected, though less so, scoring 0.09σ higher.

Second, 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 this result is that while the collaborative versus adversarial treatments affected intergroup interactions, the competitive pay structure did not. This result suggests that Allport's intergroup cooperation scope condition need not facilitate positive effects of contact, at least in a setting where groups already have common goals.

Third, I show that despite evidence of discrimination of lower castes within each team, the positive effects of collaborative contact are 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 similar for 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).

[4] Other integrative sports programs exist, but evidence on their impact is scarce. Right to Play reaches one million children weekly with sports-based programs promoting education, health, and peaceful communities, and Soccer for Peace uses sport to unite Jews and Arabs in Israel. Ditlmann and Samii (2016) find mixed effects of an interethnic sports program using a difference-in-differences design, and while Mousa (2020) 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. Otherwise, sport has also been explored as a means of improving intergroup relations through shared national experiences (Depetris-Chauvin, Durante, and Campante 2020).

This paper is the first to systematically test for the effects of different types of contact (Paluck, Green, and Green 2018), 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; Enos 2014; Dahl, Kotsadam, and Rooth 2021; Scacco and Warren 2018; Corno, La Ferrara, and Burns 2019; Schindler and Westcott 2021), or use nonrandomized variation in the type of contact (Pettigrew and Tropp 2006; Dustmann, Vasiljeva, and Piil Damm 2018; Bazzi et al. 2019).[5] In a particularly creative example of the former, Rao (2019) shows that integration of rich and poor students in Delhi schools increases the prosocial behavior of rich students. In his case, the contact entails a mix of collaborative and adversarial interactions (e.g., through competing on exams). Mousa (2020) also complements my paper, showing evidence for the positive effects of collaborative contact in soccer leagues in a postconflict 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 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; Marx, Pons, and Suri 2016). 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 (Tajfel et al. 1971, Goette et al. 2012) by showing that team membership can reduce prejudice in a real-world setting. Most relevant, the Robber's Cave Experiment of Sherif et al. (1961) randomly assigned 22 boys to 2 groups at a summer camp, demonstrating both that (i) competition between groups for resources leads to conflict, and (ii) common 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. Finally, this paper contributes to a large body of work on caste networks (reviewed in Munshi 2019) by exploring not just why these networks matter, but also how they form.

The remainder of this paper is organized as follows. Section I provides an overview of India's caste system, and motivates the use of cricket leagues as a tool for the study of contact. Section II describes the experiment design and outcomes, while Section III explores the effects of both types of contact on willingness to interact, own-caste favoritism, and efficiency. Section IV considers alternative explanations

[5] 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. Otherwise, there is evidence that the type of organizational contact (e.g., horizontal versus vertical) affects productivity (Karaca-Mandic, Maestas, and Powell 2013; Marx, Pons, and Suri 2016).

for why the type of contact matters, and Section V considers the three program design implications of additional results. Section VI concludes.

I. Background on Caste and Cricket

A. *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 subgroups, called *jatis*, within 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),[6] are natural groups to consider when studying discrimination in India (Munshi 2019).[7] 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, 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 continues to be widespread. Thirty-nine percent of General and OBC households in Uttar Pradesh (24 percent in India), the Indian state where I ran the experiment, practice untouchability (Desai, Vanneman, and National Council of Applied Economic Research 2011), limiting their physical interactions with lower castes with the aim of remaining "unpolluted." Despite persistent discrimination, there is evidence that the relative economic status of low castes has improved in recent decades (Hnatkovska, Lahiri, and Paul 2012).

General Segregation.—Castes are segregated through marriage, geography, and social networks. 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 percent of a village's population across major Indian states, roughly 50 percent of food transfers and loans come from within the same *jati* (Munshi and Rosenzweig 2015). Cross-caste interactions that exist are often adversarial: 50 percent of households in Uttar Pradesh report that there is some or a lot of conflict between *jatis* in their village (Desai, Vanneman, and National Council of Applied Economic Research 2011).

[6] Erstwhile untouchables, and some others, were classified as Scheduled Castes (SC), with indigenous tribes classified as Scheduled Tribes (ST). Only 1.6 percent of participants in this study are STs.

[7] To focus on caste and not religion, I only considered villages with few or no Muslims for the experiment. In practice, only 2.9 percent 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.

Study Segregation.—Online Appendix Figure A1 illustrates the social segregation at baseline for two of the eight league locations. Average caste-based homophily (following Jackson 2011) is 1.92: study participants are roughly twice as likely to form friendships with a participant from the same caste than with a participant in general.[8] 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 (online Appendix Section E). Among many such examples, one General caste participant said "I will assist those from my own caste, and beat the *chamars* [a Scheduled Caste *jati*]. My whole day goes bad when I see face of a *chamar*." 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."

B. *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. Cricket is similar in structure to baseball. Each team usually comprises 11 players, though in the experiment each team consisted of only 5 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 (online Appendix Figure A2). 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 4 or a 6. 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").

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 percent play cricket at least two times per week. Second, cricket tournaments are common in the study area, making the intervention naturalistic: at baseline, 38 percent 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

[8] For comparison, Jackson (2011) finds race-based homophily in US high schools to be lower, at 1.4 on average.

opponents.[9] The collaborative nature of sport in general was apparent to Gordon Allport, who wrote in *The Nature of Prejudice* (1954, p. 276):

> *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.*

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. These types of contact generate incentives for different types of interactions: while interactions with other-caste teammates are more frequent than interactions with other-caste opponents (columns 1–5, online Appendix Table A1), conditional on interacting, interactions with other-caste teammates are 50 percentage points less likely to be hostile (column 6).

II. Experiment Design

A. *Recruitment and Baseline Activities*

Site Selection.—I selected 8 gram panchayats[10] (GPs) near Varanasi, Uttar Pradesh, from among 100 GPs visited by the field team.[11] The selected GPs satisfied several desirable criteria, including: the presence of caste-segregated hamlets, a supportive elected GP leader, roughly equal caste proportions, and an available cricket field. 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 1. 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 (online Appendix Figure A3), 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. By targeting particular hamlets, we kept

[9] 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.

[10] Gram panchayats are local administrative units comprising several villages.

[11] See online Appendix Section F for additional, but less essential, experiment design details.

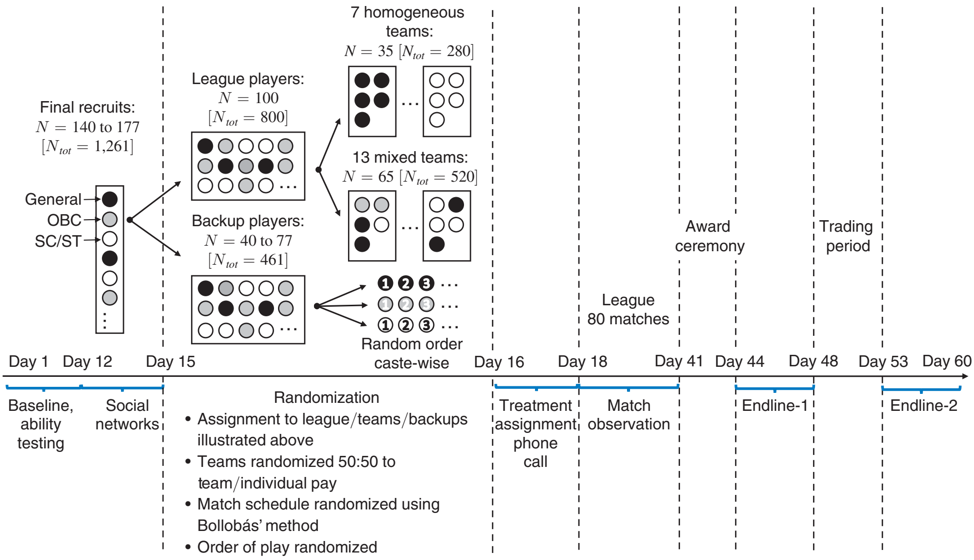

Figure 1. Experiment Design and Timeline

Notes: The figure shows the roughly 60-day time line 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. Text under blue brackets 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.

recruitment roughly equally balanced across the three caste categories.[12] Men who expressed interest completed a baseline survey (for summary statistics, see online Appendix Table A2) and were informed that their sign-up was not complete until their cricket ability was tested. 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 for which I measured outcomes. Of the 1,261 who completed both the baseline and ability testing as players, 281 also signed up to be umpires, of which 156 umpired at least one match.

Study Construal.—I minimized references to caste in the survey instruments to avoid priming and social desirability bias. 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." This framing appears to have been successful: according to daily surveyor debriefs, participants very rarely voiced suspicions that the study was designed to understand caste relations.

Ability Testing.—Following the six days of recruitment, surveyors spent six days testing the cricket ability of each participant. Cricket ability was measured along

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

three dimensions: bowling, batting, and fielding. For bowling, participants bowled six balls toward the wickets, and a surveyor measured the speed using speed guns. For batting, a surveyor bowled six balls toward 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 4 or a 6. For fielding, a surveyor threw six balls high in the air toward the participant. The surveyor recorded how many balls were successfully caught. The ability measures are strongly predictive of league performance, as shown in Section VC. For some of the analysis, I create an individual-level ability index as the average across three standardized measures: maximum bowling speed, number of 4s or 6s when batting, and number of catches when fielding.

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 signaled strongly by the last name a person uses. When participants are asked to guess the caste category of a hypothetical name at end-line, they correctly identify the name as belonging to the same or a different caste 80 percent 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 percent of the 1,261 participants completed the social network survey prior to treatment assignment.

B. *Randomization*

League Assignment.—In each of the 8 locations, 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, Chassang, and Snowberg 2017, further details in online Appendix Section G). 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 percent 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: the three highest-priority backups played 7.4 matches on average, while the remaining backups played far fewer (online Appendix Figure A4). This protocol has an additional advantage: 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 in Section IVB.

Team Assignment.—For each of the 8 leagues, I randomly assigned the 100 league players to 20 teams of 5 players each. Thirty-five percent of the players were randomly assigned to homogeneous-caste teams, making 7 out of 20 teams homogeneous-caste. I pooled and randomly ordered the remaining players. Each sequence of 5 then formed a mixed-caste team.

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). The variation in incentives allows a test of an additional Allport (1954) condition: that of intergroup cooperation. Team Pay increases intergroup cooperation on each team, by making own pay depend positively on the performance of teammates. In contrast, Individual Pay increases competition, giving incentives to "jockey for position" to ensure enough play-time to make money. If intergroup cooperation 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. I randomly chose a graph for each league using an existing algorithm, Bollobás' "pairing method" (see online Appendix Section G 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.[13] Together, the assignment to teams and random match schedule created significant variation in collaborative and, albeit less so, adversarial cross-caste contact (online Appendix Figure A5). This difference in variation results in much lower powered statistical tests for adversarial contact than for collaborative contact.[14] I counter this limitation somewhat by also estimating overall effects on an omnibus index of cross-caste behavior (Section IIID).

C. *Implementation Period*

Treatment Announcement and League.—During the two-day period following randomization, surveyors phoned each participant to explain their treatment assignment. Following these phone calls, each league ran for roughly three-and-a-half weeks, with match attendance averaging 76 percent. One surveyor observed each

[13] 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.

[14] I faced a trade-off 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.

match and recorded interactions between players using the *Match Observation* survey (as referenced already in online Appendix Table A1).

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 were conveyed over the phone as part of the treatment announcement, strongly signaling 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 percent say yes when the teammate is from the same caste, while still 27 percent say yes when the teammate is from a different caste. The corresponding figures for baseline friendships are 15 percent and 4 percent, 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 (17 on average) frequently call out the names of players, and sometimes refer to players using caste slurs.

Outcome Measurement.—I measured three classes of outcomes during two end-line 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. The relatively short-run outcome measurement is a key limitation of this study. These outcomes were measured for all participants, except own-caste favoritism, which was not measured for the control group due to time constraints. I summarize the measurement in Table 1, and in full, together with the results, in Section III.

D. *Empirical Specification*

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

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

where y_{icl} denotes outcome y for participant i from caste $c \in \{\text{General}, \text{OBC}, \text{SC/ST}\}$ 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. The term $\mathbf{X}_{icl}$ is a vector of baseline covariates, detailed in the next subsection. The sample size varies slightly across outcomes given differing attrition for *Endline-1* and *Endline-2*, and logistical constraints preventing us from measuring all outcomes for the final

Table 1—Description of Main Outcomes

Exercise	Brief Description	Main Outcome	Concept
Social interaction	Participants scroll through photos and full names of all other participants, first selecting those who are friends or with whom they would like to spend time, second selecting those who are friends.	Number of other-caste men selected	Willingness to interact socially
Team formation	Participants again scroll through all other participants, this time making incentivized choices of four future teammates, first for a match with monetary stakes, second for a match without monetary stakes.	Number of other-caste men selected	Willingness to interact productively
Voting	League players vote on which players from other teams get to go on a field trip for professional coaching, by ranking the players of these other teams from 1 to 5.	Vote ranking given to other players matched to caste	Caste favoritism
Trading	Participants receive two mismatched goods (e.g., two left-handed gloves) and have several days to find other participants to trade with. One-half of the participants receive obfuscated monetary incentives to trade with someone from a different caste.	Whether traded with someone from a different caste	Efficiency
Trust	Participants play a standard trust game with three partners, one from each caste of General, OBC, and SC/ST. Participants can send up to Rs. 50 to partners, any amount sent is tripled, and partners can decide how much to return. Participants also self-report whether they think that most people can be trusted.	Amount transferred in trust game; Whether think that most people can be trusted	Efficiency

few respondents. 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. The parameter 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, I use the full participant sample ($N = 1{,}261$) and the following empirical specification:

$$y_{icl} = a_{cl} + \phi_1 Homog.\ Team_{icl} + \phi_2 Mixed\ Team_{icl} + \phi_3 High\ Backup_{icl} + \theta \mathbf{X}_{icl} + e_{icl}, \tag{2}$$

which compares the low-priority backups which played few matches (the omitted group) with the high-priority backups ($Backup\ Priority\ Number_{icl} \leq 3$), those assigned to homogeneous-caste teams ($Prop.\ Oth.\ Caste\ on\ Team_{icl} = 0$), and those assigned to mixed-caste teams (*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.

E. *Randomization and Implementation Checks*

Balance checks suggest that the randomization was successful. For the effects of contact (online Appendix Table A3), 2 of 17 coefficients not affected by re-randomization (for age and whether in school) are statistically significant at the 10 percent level 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 end-line outcomes (panel B). There are no statistically significant effects for the most important variable: the number of other-caste friends listed in the social network survey (column 1). 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 (online Appendix Table A4), though high-priority backups have significantly more other-caste friends (column 1). Given this imbalance, and the similar concern in online Appendix Table A3, I control for the number of other-caste friendships at baseline throughout. In particular, since 7 percent 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 end-line intergroup behaviors, this approach also increases precision. In addition, I control throughout for the five variables used for re-randomization (following Bruhn and McKenzie 2009): age, whether in school, whether would volunteer, number of 4s or 6s in the batting test, and maximum speed in the bowling test.

Attrition is low at 6.8 percent, 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, online Appendix Table A5). 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.

III. The Effects of Collaborative and Adversarial Contact

A. *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 that they either consider friends or would like to spend time with in the future (*Want to Interact*). Restricting responses to the people they listed, surveyors then asked them to select those they considered friends (*Friends*). By matching selections to the caste of each person, I calculated the total number of other-caste men selected for each question.

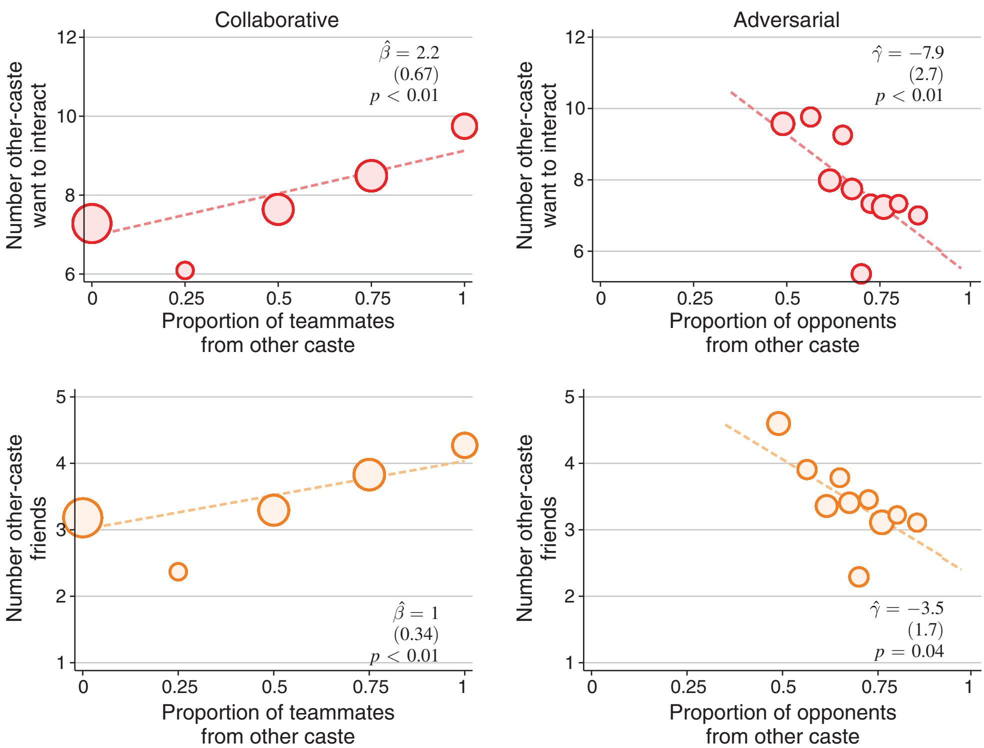

Figure 2. Collaborative and Adversarial Contact Have Opposite Effects on Demand for Cross-Caste Social Interaction

Notes: The left-hand-side panels show the effects of collaborative contact, while the right-hand-side panels show the effects of adversarial contact. The top panel outcome is the number of other-caste men who the participant considers friends or wants to spend time with. The bottom panel outcome is the number of other-caste men who the participant considers friends (a subset of the top panel outcome). 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.

Though self-reported, selections are highly predictive of subsequent actual trading behavior (online Appendix Table A6).

Analysis.—Collaborative and adversarial contact have opposite effects on cross-caste friendships (Figure 2; panel A of Table 2).[15] 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 (12.1 own-caste) participants in future, and are friends

[15] These malleable friendships are consistent with evidence that people, and especially the below-median age, form social attachments easily (Sherif et al. 1961, Baumeister and Leary 1995). Consistent with this, below-median age participants are more likely to form friendships as a result of collaborative contact, and less likely to lose them as a result of adversarial contact (online Appendix Table A7).

Table 2—Ability Mediates Adversarial, but Not Collaborative, Contact

	Want to interact	Friends	
	(1)	(2)	(3)
Panel A. Number of other-caste participants			
Prop. Oth. Caste on Team	2.17	1.03	1.22
	(0.67)	(0.34)	(0.35)
Prop. Oth. Caste of Opponents	−7.90	−3.49	−2.93
	(2.75)	(1.71)	(1.71)
Prop. Oth. Caste on Team × *Oth. Caste Team Ability*			0.29
			(0.65)
Prop. Oth. Caste of Opponents × *Oth. Caste Opponent Ability*			6.71
			(2.46)
Caste × league fixed effects	Yes	Yes	Yes
Outcome mean	7.9	3.5	3.5
p-value (collaborative = adversarial)	0.00058	0.012	
Panel B. Number of own-caste participants			
Prop. Own Caste on Team	0.97	0.46	
	(0.53)	(0.39)	
Prop. Own Caste of Opponents	4.52	2.38	
	(2.36)	(1.85)	
Caste × league fixed effects	Yes	Yes	
Outcome mean	12	6.6	
p-value (collaborative = adversarial)	0.14	0.31	
Observations	770	770	770

Notes: Standard errors clustered at team-level. Column 1 outcome is number of other/own-caste participants who the respondent considers friends or wants to spend time with. Column 2 and 3 outcome is number of other/own-caste participants who 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 Opponent Ability* is the average ability index across all other-caste opponents. *Prop. Own Caste on Team* is equal to 1 minus *Prop. Oth. Caste on Team*. *Prop. Own Caste of Opponents* is equal to 1 minus *Prop. Oth. Caste of Opponents*. Panel A regressions control for number of other-caste friends at baseline (and dummy for missing), and the five variables used for re-randomization. Panel B is the same, except number of own-caste friends instead of number of other-caste friends.

with 3.1 (7 own-caste). Moving from a homogeneous-caste team to a team with four other-caste men increases desired cross-caste interactions by 2.2 (31 percent), and cross-caste friendships by 1 (32 percent). 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 percent) to the most (97.5 percent) leads to 2.2 fewer other-caste friends. For each outcome, the equivalence of the effects of collaborative and adversarial contact can be rejected at least at the 95 percent significance level.

While the effects of collaborative contact are not mediated by the ability of players exposed to (in contrast to Carrell, Hoekstra, and West 2019), higher-ability other-caste opponents significantly reduce the negative effects of adversarial contact (panel A of Table 2, column 3). Given that teams with higher ability players also win more matches (as I show in Section IVC), 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 (panel B of Table 2). 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 outgroup members.

Individuals versus 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.[16]

For the effects of adversarial contact, I define the outcome as the percentage of other-caste friends among the 205 backups who played zero matches. Since they did not play, any effects of adversarial contact on desired interaction with these people cannot be driven by direct contact as opponents.[17]

Both collaborative and adversarial contact effects extend to the outgroup as a whole. Collaborative contact has a positive effect on desired future interaction and friendships with other-caste men in other teams ($p = 0.11, 0.04$ respectively, Figure 3). Adversarial contact again has negative effects ($p = 0.03$, 0.26 respectively).

[16] 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.

[17] The results are similar if I instead define the outcome as the percentage of other-caste friends from among very-low priority backups: those with a priority number of seven or above (online Appendix Figure A7). This set of 173 backups has the advantage of being randomly selected, but the drawback of having played some matches (0.8 on average), meaning there could have been some on-pitch contact.

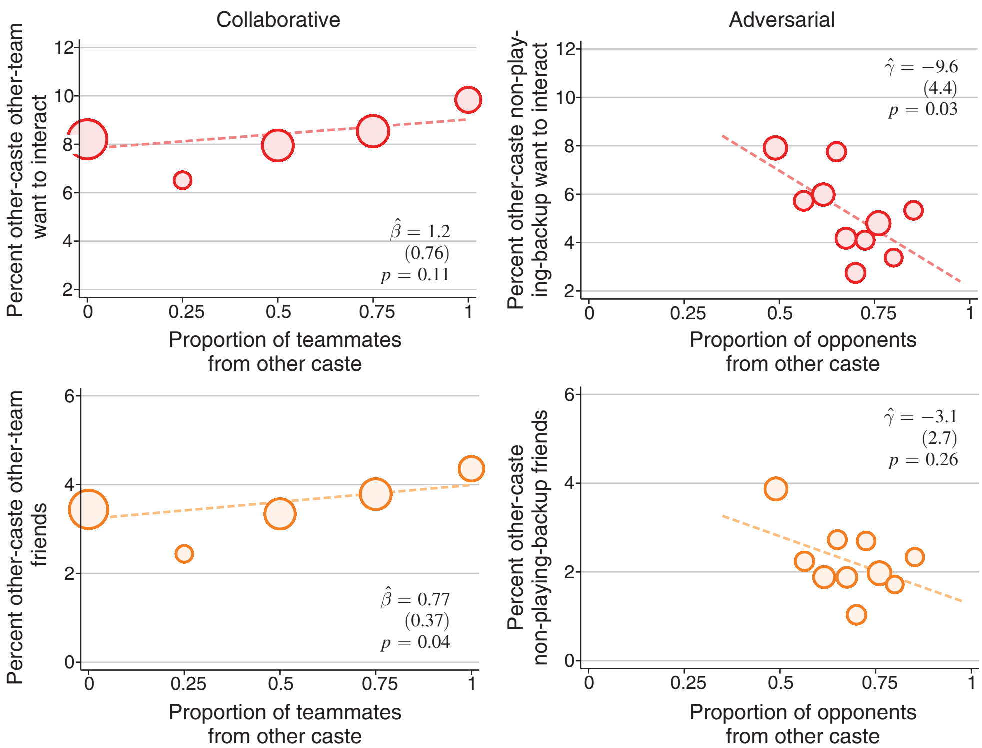

Figure 3. Contact Affects Cross-Caste Friendships with Non-Teammates and Non-Opponents

Notes: The figure is created based on equation (1), and as described in Figure 2. The left panel outcome is the percentage of other-caste men from among the other teams (in the same league) who the participant considers friends or wants to spend time with (top) or considers friends (bottom). The right panel outcome is the percentage of other-caste men from among non-playing backups who the participant considers friends or wants to spend time with (top) or considers friends (bottom). 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.

These effects are meaningful: full collaborative exposure increases non-teammate cross-caste friendships by 0.15σ. 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 0.56σ.

In addition, the effect of collaborative contact on non-teammate friendships is much stronger for non-opponents than opponents (online Appendix Table A8). 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:

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

where y_{ij} is a dummy variable equal to 1 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 1 if i listed j as a friend at baseline, and a dummy variable equal to 1 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 1 if j is a teammate of i's (a direct link), and *Friend of Oth. Caste Teammate*$_{ij}$ equals 1 if j is a friend (using baseline data) of any of i's other-caste teammates (an indirect link). The parameter 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 Team*$_{icl} > 0$). The intuition behind the specification is shown visually in online Appendix Figure A8. 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. Parameter 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:

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

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, online Appendix Table A9) 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.[18] 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. 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.—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 ($\sim$\$8) awarded to the winning team. The other match would not have stakes: both teams would receive Rs. 250 ($\sim$\$4) regardless of their performance. Surveyors asked participants to select their team twice: first for the match with stakes, and then immediately after for the match without. They selected their team by scrolling through the entire list of participants,[19] again seeing their full names and photos. I then randomly selected four players per league ($\sim$1.25 percent probability) to have one of their two team choices implemented, making them the captain of their chosen team for one of the additional matches. 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.

Analysis.—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 $\hat{\gamma} = 0.88$ ($p = 0.06$) for adversarial contact (Table 3, online Appendix Figure A10). These effects are 47 to 59 percent of the mean 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 percent of the time. In contrast, homogeneous-caste teams choose other-caste players 29 percent of the time, whereas those with four other-caste teammates choose other-caste players 47 percent of the

[18] There is some suggestive evidence that generalized effects come through friendships made with match spectators: in particular, collaborative contact leads to more non-team cross-caste friendships in the leagues with more spectators (online Appendix Figure A9; Table A10), and participants that watch more matches make more friends (online Appendix Table A11). These findings are consistent with a different type of network access effect, or with post-match spectator friendships being a consequence of belief updating about other castes.

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

TABLE 3—BOTH TYPES OF CONTACT REDUCE ABILITY-BASED STATISTICAL DISCRIMINATION

	Number other-castes for team for match with...			
	Stakes (1)	No stakes (2)	Stakes (3)	No stakes (4)
Prop. Oth. Caste on Team	0.71	0.45	0.71	0.47
	(0.12)	(0.11)	(0.12)	(0.11)
Prop. Oth. Caste of Opponents	0.88	0.09	0.90	0.17
	(0.46)	(0.44)	(0.46)	(0.44)
Prop. Oth. Caste on Team × *Oth. Caste Team Ability*			0.31	0.33
			(0.18)	(0.18)
Prop. Oth. Caste of Opponents × *Oth. Caste Opponent Ability*			0.27	0.89
			(0.70)	(0.66)
Observations	768	768	768	768
Outcome mean	1.5	1.5	1.5	1.5
Caste × league fixed effects	Yes	Yes	Yes	Yes
p-value (collaborative = adversarial)	0.71	0.43		
Collaborative: p-value (stakes = no stakes)	0.00048			
Adversarial: p-value (stakes = no stakes)	0.0055			
p-value (stakes has same effect on collaborative and adversarial)	0.051			

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 0 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 Opponent 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), and the five variables used for re-randomization. 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.

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.

A natural interpretation of this set of results is that while the two types of contact have opposite effects on inferences about cooperativeness, they both reduce ability-based statistical discrimination, by revealing the ability of teammates and opponents. 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 (Bandiera, Barankay, and Rasul 2009), 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 ($p = 0.05$), to 0.09 (Table 3, bottom panel of online Appendix Figure A10,).[20,21]

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.[22]

B. *Favoritism*

Voting.—Beyond willingness to interact, caste differences may affect welfare and allocation through ingroup favoritism. I measured own-caste favoritism of the league participants 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. The field trip was popular: 96 percent said they would go if they were selected and were available. The selection was decided by vote. Each participant privately and anonymously ranked players on four other randomly chosen teams (two opposition teams and two non-opposition teams), in random order, from 1 to 5. 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 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.

Analysis.—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 4). 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).

[20] 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 (online Appendix Table A12), despite not having played with them. Given that the effect for the match without stakes is smaller and insignificant, this result suggests at least some *generalized* updating of ability beliefs (updating about those not played with).

[21] The lack of a *negative* effect of adversarial contact for the match without monetary stakes may be because players get intrinsic value from winning matches, giving them an incentive to pick high ability players even in the absence of a monetary return.

[22] This said, it is possible that some of the difference in effects is due to the fact that the social interaction measures are self-reported while the team formation measures are incentivized.

TABLE 4—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	−0.20
					(0.07)	(0.11)
Own caste voted on × *Prop. Oth. Caste of Opponents*					0.08	−0.40
					(0.39)	(0.47)
Observations	3,035	3,200	2,945	9,180	9,180	4,570
Individuals	250	257	244	751	751	633
Votee fixed effects	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 fixed effects	No	No	No	No	Yes	Yes
p-value (collaborative = adversarial)					0.6	0.68

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), and the five variables used for re-randomization, as well as each interacted with *Own caste voted on.*

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, online Appendix Table A13).

Collaborative contact reduces own-caste favoritism by up to one-third of the mean (column 5, Table 4, $p = 0.08$). This effect is stronger when considering only votes for non-opponents, consistent with effects on the demand for social interaction (column 6).[23] 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

[23] The reduction in favoritism is not driven only by a reduction in favoritism towards friends (columns 2 and 3, online Appendix Table A13): 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.

shift in social preferences. In contrast, the adversarial contact effect is not statistically significant (columns 5, 6),[24] but given imprecision, equality between the collaborative and adversarial effects cannot be rejected ($p = 0.6, 0.68$).

Ability Beliefs.—The effect of collaborative contact on caste favoritism in voting could be explained by shifting social preferences or generalized ability belief updating. The latter might predominate if contact corrects or tightens belief distributions about the cricket ability of other-caste players not played with. There is already some suggestive evidence of generalized belief updating: collaborative contact increases other-caste choices among players not played with for the future match with stakes, but not for the one without (online Appendix Table A12). The question then is whether such generalized belief updating can account for the effect on voting.

To explore this, 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 4). Given these estimates, for the collaborative effect on non-opponents (column 6) to be fully explained by effects on statistical discrimination 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 0.9σ higher on each of the ability measures. This extent of generalized belief updating seems relatively unlikely given that even those with no collaborative exposure see signals of the ability of the other-caste players they play against. This exercise suggests that collaborative contact reduces own-caste favoritism in voting at least partially through its impact on preferences, though I cannot rule out the possibility that the effect is driven by generalized belief updating.

C. *Efficiency*

Trading.—I designed a new trading exercise to measure efficiency impacts through contact changing barriers to cross-caste interaction.[25] 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 mismatched: the participant either received two left-hand or two right-hand gloves, and two left-foot or two right-foot flip-flops. The mismatching created gains from trade. Surveyors told participants that all league sign-ups would be receiving the same two mismatched goods, and that they would have the next four or five days of regular life to find people to trade with (in practice, surveyors revisited for *Endline-2* a median of 10 days later). To provide further gains from trade, surveyors gave participants monetary incentives. One-half of the participants earned Rs. 10 (~\$0.16) for each successful trade, while the rest earned Rs. 20 (~\$0.32).

[24] More concretely, the ex post minimum detectable effect size with 80 percent power and a 5 percent significance level is 1.09: larger than the overall bias of the most biased caste group. In this sense, the test is clearly underpowered.

[25] I am grateful for conversations with Frank Schilbach that inspired aspects of this design. The design here complements a larger literature that uses trading games to understand the efficiency of markets, with Bulte et al. (2017) being a recent example in a developing country setting. For the full survey script, see online Appendix Section H.

In addition, the survey team stuck colored stickers on each mismatched pair, and we offered incentives for trades made with identical goods that had a *different* color sticker. This served to create, and obfuscate, *cross-caste* gains from trade, since the three possible sticker 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.[26] I randomly selected one-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*. 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.

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

Analysis.—Most participants trade successfully: 88 percent of goods received by those in homogeneous-caste teams are traded, with no statistically significant effect of collaborative or adversarial contact (column 1 of Table 5; top panel of online Appendix Figure A11). 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 percent (column 2). Collaborative contact has a small and positive, though marginally insignificant, effect: full collaborative exposure increases cross-caste trade by 6 percentage points ($p = 0.15$).[27] While adversarial contact has a large negative coefficient (-0.15), 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 ($p = 0.22$).

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 of online Appendix Figure A11) and trade payouts by Rs. 15 or 18 percent 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

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

[27] Nevertheless, cross-caste trade is lower for those in homogeneous-caste teams than those with any level of collaborative contact (middle-left panel, online Appendix Figure A11). 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.

Table 5—Collaborative Contact Increases Incentivized Cross-Caste Trade

	Traded	Cross-caste trade		Trade payout	Cross-caste trade: Other team	Cross-caste trade: Non-playing 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	−0.06	−0.15				
	(0.10)	(0.17)				
Color switch bonus = 50	−0.00	0.22				
	(0.02)	(0.04)				
Color switch bonus = 100	0.01	0.25	0.02	85.97	−0.03	0.05
	(0.02)	(0.04)	(0.04)	(6.52)	(0.04)	(0.02)
Prop. Oth. Caste on Team × (bonus > 0)			0.11	14.71	0.06	0.01
			(0.05)	(7.19)	(0.05)	(0.03)
Prop. Oth. Caste on Team × (bonus = 0)			0.01	−1.86	−0.00	−0.00
			(0.06)	(1.48)	(0.05)	(0.03)
Prop. Oth. Caste of Opponents × (bonus > 0)			−0.15	−21.62	0.06	−0.09
			(0.21)	(30.78)	(0.22)	(0.13)
Prop. Oth. Caste of Opponents × (bonus = 0)			−0.17	−6.01	0.03	−0.12
			(0.23)	(6.38)	(0.23)	(0.12)
Observations	1,510	1,510	1,510	1,510	1,510	1,510
Individuals	755	755	755	755	755	755
Homog. team mean	0.88	0.52	0.52	83	0.36	.064
Caste × league fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Caste × league × (bonus > 0) fixed effects	No	No	Yes	Yes	Yes	Yes
Trade bonus dummy	Yes	Yes	Yes	Yes	Yes	Yes
p-value (collaborative = adversarial)	0.47	0.22				
p-value (coll. × bonus = coll. × no bonus)			0.23	0.027	0.39	0.75
p-value (adv. × bonus = adv. × no bonus)			0.94	0.63	0.94	0.84

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 1 if the good was successfully traded. The outcome for columns 2 and 3 is a dummy variable equal to 1 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 1 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 1 if the good is traded with a different caste non-playing backup (for generalization of adversarial contact). *Trade bonus dummy* is equal to 1 if the participant was assigned Rs. 20 for each successful trade, and 0 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), and the five variables used for re-randomization.

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 10 percentage points higher (though not significantly so, with $p = 0.23$) 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 effects only on cross-caste trades with those assigned to *other* teams (column 5).[28] 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 (online Appendix Table A14). 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 appears to change the willingness to cooperate with other-caste men in general.

Trust.—To explore efficiency effects in the absence of face-to-face interaction I included in *Endline-2*: a standard trust game (as created by Berg, Dickhaut, and McCabe 1995), and 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. For the trust game, I partnered each participant with three men from another village: one General caste, one OBC, and one SC/ST. Participants played the role of the Sender. Senders were allocated Rs. 50 ($\sim$\$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

[28] As discussed in Section IIIA, 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 IIIA 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.

with last names that both strongly signaled 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 percent 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.

Analysis.—Though a 6 percent own-caste trust advantage exists (column 1 of Table 6), there is no evidence that either type of contact significantly affects this cross-caste trust gap on average (column 2).[29] This result masks some heterogeneity: general castes show the largest trust gap ex ante, and collaborative contact nearly fully closes this gap, though the effect for this subgroup is not statistically significant ($p = 0.29$, see online Appendix Table A15 and Figure A12). These effects may then be consistent with collaborative contact positively updating beliefs about the trustworthiness of other-caste men, though only when a meaningful trust gap exists to begin with.

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 general levels of beliefs or preferences.[30] Despite this, ethnic diversity is typically negatively associated with generalized trust (Dinesen, Schaeffer, and Sønderskov 2020), and researchers have proposed interethnic contact as a potential causal channel.

Both types of contact reduce the overall amount sent in the trust game (column 3, Table 6), 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 outgroup.

The suggestive evidence that collaborative contact also reduces trust (only in column 3) 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

[29] The adversarial test is again hindered by a lack of power—in this case the ex-post minimum detectable effect size is Rs. 15.5.

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

Table 6—Contact Reduces Trust Levels, More So When Adversarial

	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			
		(1.48)			
Own caste recip. × *Prop. Oth. Caste of Opponents*		−0.11			
		(5.52)			
Prop. Oth. Caste on Team			−3.04	0.02	−0.09
			(1.16)	(0.04)	(0.07)
Prop. Oth. Caste of Opponents			−9.54	−0.42	−0.95
			(5.55)	(0.18)	(0.32)
Observations	2,253	2,253	2,253	770	751
Individuals	751	751	751	770	751
Outcome mean	22.2	22.2	22.2	0.21	0.03
Sender fixed effects	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 fixed effects	No	No	Yes	Yes	Yes
p-value (collaborative = adversarial)		0.98	0.24	0.012	0.0073

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 1 if the respondent answers "Most people can be trusted" and equals 0 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), and the five variables used for re-randomization. Columns 3 to 5 include number of other-caste friends at baseline (and dummy for missing), and the five variables used for re-randomization as controls.

cooperative than interactions with in-groups. Consistent with this hypothesis, 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 end-line (online Appendix Table A16). 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.

D. *Summary*

To estimate the overall effect of each type of contact on outcomes I follow Kling, Liebman, and Katz (2007) and create a cross-caste behavior index equal to the mean of seven standardized individual-level outcomes. This approach has two key advantages: first, it increases power to detect effects, with the ex-post minimum detectable effect size for collaborative and adversarial contact now 0.14σ and 0.59σ, respectively. Second, it yields effect sizes that usefully summarize the overall effects of

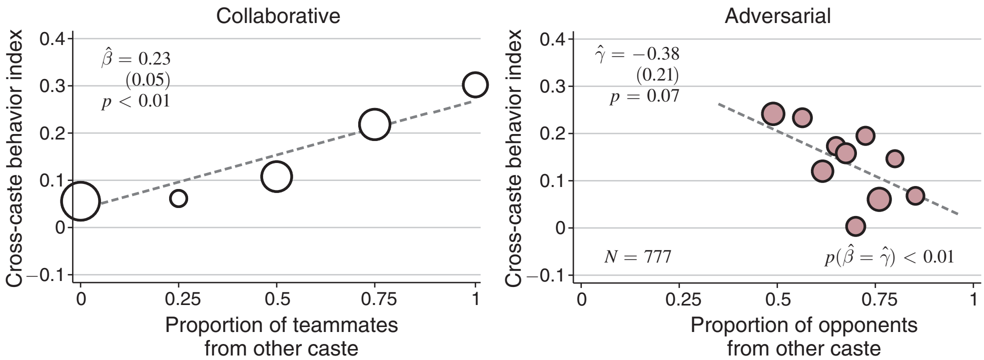

Figure 4. Collaborative and Adversarial Contact Have Opposite Effects on Overall Cross-Caste Behaviors

Notes: The figure is created based on equation (1), and as described in Figure 2. The cross-caste behavior index outcome is the mean of seven standardized outcomes: (i) number of other-caste men who the participant considers friends or wants to spend time with, (ii) number of other-caste men who the participant considers friends, (iii) number of other-caste men chosen as teammates for the future match with stakes, (iv) same, but for the match without stakes, (v) the fraction of goods traded across caste (equal to 0, 0.5, or 1), (vi) the trust index, and (vii) the mean trust amount sent to other-caste partners less the amount sent to the one own-caste partner. I standardize each outcome by subtracting the control group (backup player) mean, and then dividing by the control group standard deviation. For participants with missing data for at least one of these seven outcomes the outcome is the mean of the non-missing outcomes. The one important excluded outcome is voting, given the difficulty in defining an individual-level outcome that would parallel the analysis in Table 4.

each type of contact. Full collaborative contact increases cross-caste behaviors by 0.23σ ($p < 0.01$), while full adversarial contact reduces cross-caste behaviors by 0.38σ ($p = 0.07$) (Figure 4). The equality of the two effects is rejected ($p < 0.01$).

To put these effect sizes in context, I can compare them to the 27 experimental estimates of the effects of contact identified in a recent meta-analysis (online Appendix Figure A13). First, my estimated effect of collaborative contact is somewhat smaller than the pooled meta-analytic estimate of 0.39σ, though it is also more precisely estimated than any other estimate. Second, my estimated effect of adversarial contact is substantially more negative than any other estimate; in fact, only one other estimate is negative, in this case with a 95 percent confidence interval spanning -0.39σ to 0.28σ.

IV. 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. I consider three alternative explanations in this section.

A. *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 (online Appendix Table A1). We might model this in a learning framework (as in online Appendix C) 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 demand for social interaction (Table 2). 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 3). This result rules out the extreme possibility that players do not learn about their opponents at all.

B. *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 4 other people. In contrast, each player experiences adversarial contact with 40 other people (8 matches multiplied by 5 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. This fact motivates two complementary tests. First, I can 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.[31] An issue with this first test is that high-priority backups also play more matches, which might independently affect outcomes. I address this using the second test: by comparing backups that experienced more versus less collaborative contact, controlling for the number of matches played.

In the top panels of online Appendix Figure A14, 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

[31] 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.

with more unique other-caste players (top-right panel). High-priority backups also list more other-caste participants that they would like to spend time with, more other-caste participants that are friends (middle panels), and are more likely to engage in cross-caste trade (bottom panel). Findings from the second test are mostly complementary, with the caveat that the confidence intervals are large. Variation in collaborative contact conditional on matches played appears to be somewhat idiosyncratic (columns 1 and 2, online Appendix Table A17), and backups that experienced more collaborative contact have more other-caste friends at end-line (columns 3 to 6), with more mixed effects on cross-caste trade (columns 7 and 8). Taken together, the two tests suggest that the differing duration of contact does not explain the contrasting effects of collaborative and adversarial contact.

C. *Winning and Income*

A large body of work finds that ethnic diversity affects productivity and efficiency (Alesina and Ferrara 2005; Hjort 2014; Marx, Pons, and Suri 2016). 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 (Depetris-Chauvin, Durante, and Campante 2020). I rule this out in online Appendix Table A18: 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).

V. Policy Implications

In this 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?

A. *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 low-priority backup players (those with priority numbers above three) with those that played in the leagues. The low-priority backups are close to a pure control group given that they played on average only 1.6 matches each, compared with 6.1 matches for league players (online Appendix Figure A4). 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 1.14 (0.24σ) more other-caste friends, choose 0.43 (0.35σ) more other-caste players for their team for a match with stakes, and engage in 8.4 percentage points (0.17σ) more cross-caste trade (Figure 5). The only

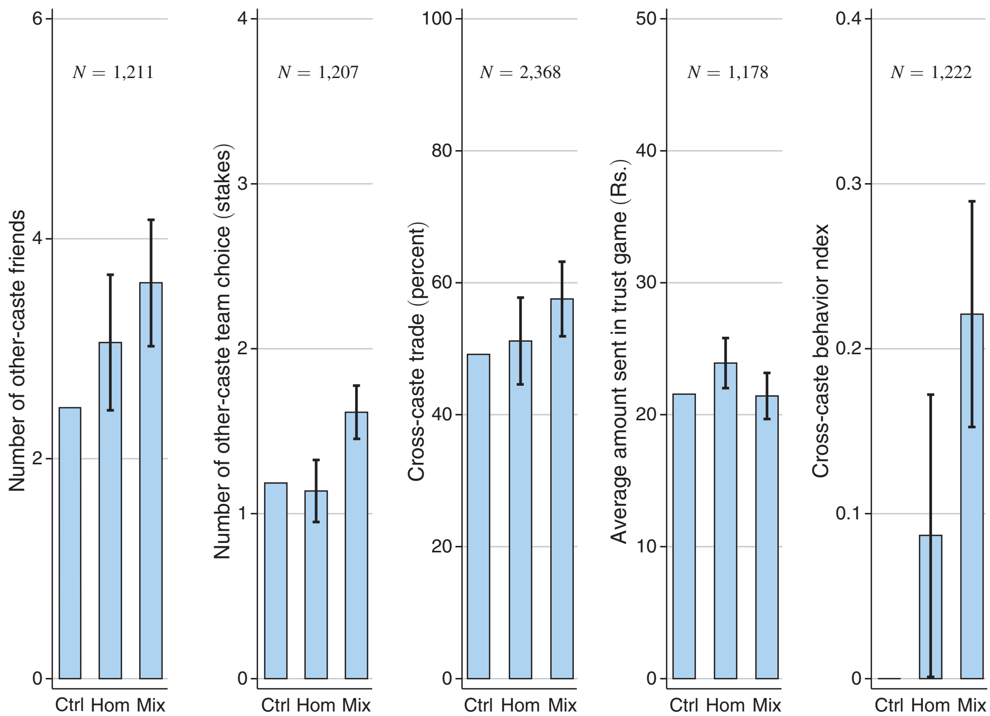

Figure 5. League Participation Increases Cross-Caste Behavior

Notes: The figure shows treatment effects and significance levels of Homog. Team (Hom), and Mixed Team (Mix) relative to the low-priority backups (Ctrl), drawing on estimates from equation (2) (for parsimony, the treatment effects for high-priority backups are not displayed). From left-to-right the outcomes are (i) number of other-caste men who participant considers friends, (ii) number of other-caste men chosen as teammates for future match with stakes, (iii) percentage of cross-caste trade, (iv) average amount sent in the trust game to the three Recipients, and (v) the cross-caste behavior index formed as the mean of seven standardized cross-caste outcomes (described in Figure 4), with the Ctrl group normalized to zero in this case. For the cross-caste trade outcome, the regression additionally includes the trade and color-switch bonus dummy variables.

insignificant comparison is for trust: those assigned to mixed-caste teams send a similar amount in the trust game to the low-priority backups. Effects of assignment to homogeneous-caste teams tend to be positive but weaker. Overall, assignment to homogeneous-caste teams increases cross-caste behaviors by 0.09σ, while mixed teams increase these behaviors by 0.22σ (far-right panel). Finally, I can use control group team choices to estimate counterfactual program effects where participants are allowed to choose their own teams. Under some assumptions, the effect on overall cross-caste behaviors would be 34 percent smaller than the effect of a league with simple random assignment to teams.[32]

These comparisons suggest firstly that integration should be very local to be effective: teams should be integrated to maximize positive effects, not just the leagues themselves. Second, since there are no negative effects of being on a

[32] Low-priority backups chose 1.2 other-caste teammates (second-left panel, Figure 5). 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. I then calculate counterfactual treatment effects by assuming that treatment effects are linear in the number of other-caste teammates (whether self-selected or not).

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.

B. *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, coauthors 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 attain those goals through "an interdependent effort without intergroup competition" (Pettigrew 1998).

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.[33] 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 (online Appendix Figure A15). 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. That said, these incentives do not provide a pure test of Allport's scope condition, given that they create intra-team competition in general, rather than only cross-caste competition.

Even with these competitive incentives, the collaborative effects do not unravel (online Appendix Table A19). The 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 versus opponent on interactions (online Appendix Table A1), cross-caste interactions on Individual Pay teams are no more likely to be hostile (online Appendix Table A20). 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.[34]

C. *Status Differences and Caste Heterogeneity*

Beyond common goals and intergroup cooperation, the contact hypothesis makes the claim that integrated groups should have equal status within the situation for

[33] Similar to naturally occurring variation used by Hjort (2014).

[34] One related interpretation is that adversarial contact is less damaging when individuals have at least some experience of collaborative cross-caste interactions. Consistent with this, there is suggestive evidence that the negative effects of adversarial contact with opponents are smaller when other-caste opponents have more other-caste friendships ex ante (online Appendix Table A21).

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 0.19σ to 0.34σ worse than General castes at cricket according to baseline measures, conditional on age (columns 1 to 3 of online Appendix Table A22), 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 percent 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 (online Appendix Table A23).[35] 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.

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 online Appendix Tables 24 and 25).[36] The general lack of heterogeneity suggests that even unequal status contact, as might be a result of some affirmative action policies, may lead to improved intergroup relations.

VI. 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 tends to have 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

[35] 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 percent of players in a given match didn't get the chance to bat, and 44 percent didn't get the chance to bowl.

[36] This difference is despite the fact that SC/STs are no less responsive to monetary incentives for cross-caste trade (online Appendix Table A26), suggesting that the difference is not just driven by caste heterogeneity in the elasticity of the cost of effort (DellaVigna et al. 2020). Otherwise, there is no reliable pattern of caste heterogeneity of the effects of adversarial contact (bottom panel of online Appendix Tables A24 and A25).

equal status and intergroup cooperation are necessary, or even facilitating, conditions receives less support: collaborative contact has positive effects despite the fact that the caste hierarchy is replicated within teams, and these positive effects are no larger in the presence of monetary incentives that promote intergroup cooperation.

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.

A natural question follows: why do some types of contact change intergroup interactions, but not others? One idea, consistent with the evidence in this paper, is that different types of contact also have *interaction* effects. In particular, in the presence of *some* collaborative contact (e.g., being on the same team), *additional* adversarial contact (e.g., competitive monetary incentives) may have muted effects. This would be the case if the baseline collaborative contact provides people with incentives to find ways of adapting to, or reinterpreting, the adversarial interactions to preserve group cohesion.

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., captains versus 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). Second, studies with greater statistical power could test for the role of relative group size, which may imply nonlinear 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. 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.

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