Types of Contact:

A Field Experiment on Collaborative and Adversarial Caste Integration

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Abstract

Integration is a common policy used to reduce discrimination, but different types of integration may have different effects. This paper estimates the effects of two types of integration: collaborative and adversarial. I recruited 1,261 young Indian men from different castes and randomly assigned them either to participate in month-long cricket leagues or to serve as a control group. 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 reduces discrimination, leading to more cross-caste friendships and 33% less own-caste favoritism when voting to allocate cricket rewards. These effects have efficiency consequences, increasing both the quality of teammates chosen for a future match, and cross-caste trade and payouts in a real-stakes trading exercise. In contrast, adversarial contact generally has no, or even harmful, effects. Together these findings show that the economic effects of integration depend on the type of contact.

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1 Introduction

Discrimination is costly and persistent. Racial discrimination in the United States contributes to the black-white wage gap, anti-immigrant sentiment prevents efficiency-improving labor migration, and caste prejudice in India creates barriers to intra-village trade. One possible reason for the persistence of discrimination is the de facto segregation of groups. This segregation can perpetuate statistical discrimination, by impeding information diffusion between groups, and arguably taste-based discrimination, by hindering intergroup social interaction. Integration could reduce discrimination, though evidence exists of both positive and negative effects of contact with outgroups.[1] One explanation for these divergent effects is that the *type* of integration affects the shaping of outgroup knowledge and attitudes, a claim which is yet to be investigated in detail.[2]

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

Why should the type of contact matter? I argue that different types of contact provide incentives for different types of intergroup interactions, and that these interactions affect inferences about the friendliness of outgroups. The Bayesian information processor should fully condition on the type of contact, updating less when teammates are friendly than when opponents are, and updating more when teammates are hostile than when opponents are. In this case, the type of contact does not systematically affect inferences about outgroups – the expected posterior is equal to the prior regardless of whether the contact is collaborative or adversarial. In contrast, agents may commit the "fundamental attribution error" (Ross and Nisbett (2011)), over-attributing the behavior of other-caste players to their types rather than their

[1] For example, exposure to immigrants increased anti-immigrant voting and attitudes in the US and Europe (Enos (2014), Halla et al. (2017), Tabellini (2018)), yet several papers find that intergroup roommate contact *reduces* prejudice (Boisjoly et al. (2006), Burns et al. (2015)).

[2] Allport (1954) hypothesized that intergroup contact would lower prejudice, but only under certain conditions: equal status between groups, support of authority, intergroup cooperation, and common goals. Pettigrew and Tropp (2006) conclude that these conditions are not necessary for prejudice reduction, though the observational studies underlying their meta-analysis suffer from selection concerns and a lack of behavioral outcomes. Laboratory experiments (e.g. work on cooperation in the classroom in the 1970s, culminating in the development of the "jigsaw learning" method, reviewed in Aronson (2011)) have addressed selection problems, though at the expense of some realism.

incentives. With these errors, collaborative and adversarial contact have opposite effects on inferences about the friendliness of other-caste players – collaborative contact improves outgroup inferences, while adversarial contact worsens them. These inferences can in turn have efficiency consequences, through affecting the willingness to engage in cross-caste economic exchange. I designed survey and field exercises to map to these channels by measuring participants' willingness to interact with other-castes (as friends and as teammates), own-caste favoritism, and efficiency in economic exchange. Participants completed these exercises in the one to three weeks following each league.

My first set of findings consider players' willingness to interact and own-caste favoritism. Collaborative and adversarial contact have opposite effects on self-reported cross-caste friendships.[3] Having all other-caste teammates instead of none increases the number of other-caste friends by 1.2, while having all other-caste opponents instead of none decreases the number of other-caste friends by 5.5. The negative effect is not due to contact with opponents in general – exposure to *own*-caste opponents has a small *positive* effect on the number of own-caste friends.

These friendship effects are not merely driven by players becoming friends with teammates and disliking opponents – collaborative contact also increases cross-caste friendships with non-teammates (particularly non-opponents), and adversarial contact reduces cross-caste friendships with non-opponents. In turn, this collaborative effect on non-teammates does not come through players becoming friends with the friends of their other-caste teammates. Instead, these effects together suggest that the two types of contact have opposite effects on inferences about the friendliness of other-caste men.

To explore the mechanism for these opposing effects, I exploit data on interactions observed during matches. Cross-caste interactions with opponents are 50 percentage points more likely to be hostile (arguments or insults), as opposed to friendly (high-fives, compliments, and hugs), than cross-caste interactions with teammates. To the extent that players attribute such behavior of other-caste players to their caste, rather than the situation created by the experiment, these interactions naturally lead to tastes shifting in opposite directions.

In contrast with the effects on tastes for social interaction, both types of contact reduce statistical discrimination (Arrow (1973), Aigner and Cain (1977), Cornell and Welch (1996)), causing more other-caste men to be chosen as teammates for a future match with monetary stakes. Two pieces of evidence suggest that this result reflects the impact of contact on knowledge about cricket ability. First, the collaborative effect on other-caste teammate choice is larger for those randomly assigned to have contact with higher-ability other-caste players. However, the collaborative effect on other-caste friendships is not mediated by teammate ability. Second, when players choose teammates for an alternative match *without*

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

a prize for the winner, both types of contact have smaller effects, but the adversarial effect falls significantly further, to zero. Though adversarial contact conveys information about the ability of other-caste players, it also reduces the desire for cross-caste social interaction. When the match has no money at stake, the balance shifts to choosing players on the basis of desired social interaction, fully offsetting the informational effect of adversarial contact.

To explore the effects of contact on own-caste favoritism, I designed an incentivized voting exercise. Each player voted to determine which representative from each team would receive professional cricket coaching. In the voting, taste-based and statistical discrimination jointly determine favoritism – players vote partly based on social preferences (taste-based), and partly based on beliefs about cricket ability (statistical), ranking more talented players higher. I find that collaborative contact reduces own-caste favoritism in voting by up to 33%, while adversarial contact has no effect. Complementary evidence suggests that the collaborative effect comes mainly through effects on taste-based discrimination. In particular, incentivized ability beliefs at baseline are no more likely to be incorrect for other-caste than own-caste players, limiting the scope for belief correction to explain the results.

My second set of findings explore the efficiency effects of contact. Collaborative contact enhances efficiency in two ways. First, it increases the quality of teammates chosen for the future match with a cash prize for the winner, as measured by their predicted probability of winning the match. Though both types of contact increase the number of other-caste teammates chosen, only collaborative contact affects team quality. This finding suggests that collaborative contact could reduce one important cost of discrimination emphasized in labor economics: efficiency losses in hiring that occur when employers overlook talented outgroup candidates in favor of less-talented ingroup candidates (Hsieh et al. (2013)).

Second, collaborative contact increases cross-caste trade by up to 11 percentage points and trade payouts by 11%, as measured in a trading exercise in which gains from cross-caste trade were introduced. Consistent with the results on team quality, the effect for adversarial contact on cross-caste trade and payouts is statistically insignificant. The collaborative contact effect is driven by the behavior of the highest castes, with contact increasing their cross-caste trade by up to 30 percentage points. This result is not driven by a pure information channel of knowing other-caste players to trade with – collaborative contact increases cross-caste trade with *non*-teammates by a similar amount. These results show that collaborative contact asymmetrically reduces social barriers to trade: the highest castes can now trade with those below them in the caste hierarchy, but the opposite is not true, despite relatively symmetric effects on willingness to interact socially across caste.

Taken together, my findings demonstrate that the type of contact mediates its impact: collaborative contact increases willingness to interact with men from other castes, reduces own-caste favoritism, and increases efficiency. In contrast, adversarial contact has no positive impacts, and can even have negative effects. In support of the original "contact hypothesis" of Allport (1954), contact only improves intergroup relations when the groups have common goals. I rule out three alternative explanations that challenge the role of 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 tastes for cross-caste social interaction.[4] 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 explain impacts – even the short-term collaborative contact backup players experience has positive effects. Third, neither type of contact affects performance in the matches, showing that the mechanism does not work through income effects or sporting success.

My findings have implications for policy questions such as how to integrate refugees into society, reconcile groups in the aftermath of conflict, and reduce long-running intergroup prejudices. Integrative sports programs exist for these purposes, but evidence on their impact is scarce.[5] To estimate the impact of the cricket intervention I compare those randomly assigned to the leagues with those in the pure control group. Despite comprising multiple types of contact, the intervention has positive effects overall. Those assigned to mixed teams make more other-caste friends than those in control, choose more other-caste teammates, choose higher quality teams, and engage in more cross-caste trade. Those assigned to homogeneous-caste teams are also positively affected, though much less than those in mixed teams.

This paper is the first to systematically test for the effects of different types of contact (Paluck et al. (2017), Bertrand and Duflo (2017), Ashraf and Bandiera (2017)). Social psychologists have long speculated that the effects of contact should depend on its type (Allport (1954)), but existing empirical tests merely study one type of contact in isolation (Pettigrew and Tropp (2006), Carrell et al. (2015), Broockman and Kalla (2016), Finseraas et al. (2016), Scacco and Warren (2016), Schroeder and Risen (2016), Bazzi et al. (2017)).[6] For example, Rao (2014) shows that integration of rich and poor students in Delhi schools increases the pro-social behavior of rich students. In his case, the contact entails a mix of collaborative and adversarial interactions (e.g. through competing on exams) in a school setting. I instead

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

[5] Right to Play reaches one million children weekly with sports-based programs promoting education, health and peaceful communities, Soccer for Peace uses sport to unite Jews and Arabs in Israel, and cricket programs unite Hutus and Tutsis in Rwanda (Hoult (2016)). Ditlmann and Samii (2016) find mixed effects of an inter-ethnic sports program using a difference-in-differences design. Sport has also been explored as a means of improving intergroup relations through shared national experiences (Depetris-Chauvin and Durante (2017)).

[6] 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 (2016)). One possible mechanism for these effects is that economic structure determines the nature of intergroup contact.

investigate the impacts of these two different types of contact separately.

The second primary contribution of this paper is to estimate the efficiency effects of contact. A large literature shows that ethnic diversity and ingroup bias affect efficiency and allocation (Alesina and Ferrara (2005), Anderson (2011), Hjort (2014), Burgess et al. (2015), Marx et al. (2016), Fisman et al. (2017)). These papers show that ethnic differences have costs; my paper is the first to show that efficiency consequences of integration depend on the nature of contact.

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

The remainder of this paper is organized as follows. Section 2 develops a learning model to explain why different types of contact might have different effects. Section 3 provides an overview of India's caste system, and motivates the use of cricket leagues as a tool for the study of contact. Section 4 describes the experimental design and outcomes, while Section 5 explores the effects of both types of contact on willingness to interact, own-caste favoritism, and efficiency. Section 6 considers alternative explanations for why the type of contact matters, and Section 7 considers whether the effects of contact differ by caste. Section 8 investigates the overall effect of the cricket program, and Section 9 concludes.

2 Conceptual Framework

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

2.1 Bayesian Information Processing

Each participant is either a good (friendly) or bad (hostile) type, denoted by $\beta_i \in \{\beta_G, \beta_B\}$. I assume that each participant knows the types of players from their own caste[7] (due to more frequent interaction),

[7] Subsequent empirical results are consistent with this – for example, own-caste contact has only weak effects on own-caste friendships (Panel B, Table 1).

but learns about the types of other-caste players through observing signals of their types during cricket matches.

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

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

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

where $\varepsilon_{ij} \sim \text{Logistic}(0,1)$. Good types have greater net utility from being friendly with others than bad types ($\phi_1 > 0$). In addition, since teammates have common goals and opponents do not, players receive greater net utility from being friendly with teammates than opponents ($\phi_2 > 0$).[8]

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

$$\pi_m^\beta = P\left(u_{ij} \geq 0\right) = \frac{e^{\alpha + \phi_1 \mathbb{1}[\beta_i = \beta_G] + \phi_2 m}}{1 + e^{\alpha + \phi_1 \mathbb{1}[\beta_i = \beta_G] + \phi_2 m}} \tag{2}$$

This signal structure has the following features: (i) $\pi_m^G > \pi_m^B \; \forall m$: good types are more likely to be friendly than bad types, whether they are teammates or opponents; (ii) $\pi_1^\beta > \pi_0^\beta \; \forall \beta$: teammates are more likely to be friendly than opponents, whether good or bad types; and (iii) $\frac{\pi_0^G}{\pi_0^B} > \frac{\pi_1^G}{\pi_1^B}$, $\frac{1-\pi_0^G}{1-\pi_0^B} > \frac{1-\pi_1^G}{1-\pi_1^B}$: a monotone likelihood ratio property ensuring that posteriors have an intuitive ordering[9] (see Supplementary Appendix C.1 for all proofs).

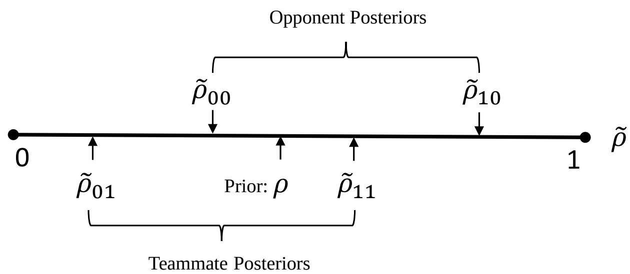
Players hold the common and correct prior ρ that others are good types.[10] Suppose now that players i and j are randomly assigned to be teammates or opponents – i.e. as in the experiment, the type of contact is random. After playing the match, each player updates as a rational (Bayesian) information processor. I first consider the case where i rationally conditions on m. Here i recognizes the fact that opponents

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

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

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

should be more hostile, and correspondingly discounts hostile behavior when $m = 0$. More generally, rational information processors should condition on the type of contact (the "situation") when forming inferences about others. In this case, posteriors $\tilde{\rho}_{sm}$ (where $s = 1$ if the friendly signal is observed) can be summarized as:



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

$$E_{\rho}\left[\tilde{\rho} \mid m = 0\right] = E_{\rho}\left[\tilde{\rho} \mid m = 1\right] = \rho \tag{3}$$

This result follows from the well-known martingale property of Bayesian models. This feature of the fully rational model suggests that the type of contact should have limited impact on inferences about the type of others. The intuition is clear: though players randomly assigned to be opponents are more hostile, the fully rational Bayesian does not conclude from this that these opponents are more likely to be bad types – this agent properly accounts for how the situation drove the behavior, not the person.

2.2 Fundamental Attribution Error

A large literature in social psychology challenges the claim that individuals properly account for the situation when making inferences about others. Evidence from many settings shows that individuals commit the so-called "fundamental attribution error", over-inferring character traits of individuals from behavior relative to situational effects (Jones and Harris (1967), Jones and Nisbett (1971), Nisbett et al. (1973), Ross (1977), Humphrey (1985), Gilbert and Malone (1995), Ross and Nisbett (2011)). This evidence suggests that a more natural model in this setting is one in which players over-attribute behavior to underlying types.

To model these attribution errors, I assume that players continue to use Bayes' rule to update beliefs, but fail to condition on m (similar to the approaches of Jehiel (2005), Eyster and Rabin (2005), and most recently, Furukawa (2017))[11] – treating signals from teammates and opponents identically.[12] It now follows that

$$E^b_\rho[\tilde{\rho} \mid m=0] < \rho < E^b_\rho[\tilde{\rho} \mid m=1] \tag{4}$$

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

2.3 Decisions to Interact

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

$$P(\tilde{\rho} > c \mid m=0) \lesseqgtr P(\tilde{\rho} > c \mid m=1) \tag{5}$$

meaning that, without bias, the type of contact has an ambiguous effect on the likelihood of friendship, with the ambiguity depending on the exact cutoff c. For some cutoffs it is even possible for opponents to be *more* likely to become friends than teammates. This result holds because an instance of opponent friendliness is particularly informative of their type. The model with attribution bias does not have the same ambiguity, since regardless of c it implies that

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

[12] Haggag and Pope (2016) study intrapersonal (as opposed to interpersonal in this paper) attribution bias in the context of consumer choice: when individuals decide their value of drinking a new drink, they fail to properly condition on the (random) state in which they consumed it last time. Their model of attribution bias does not explicitly map to Bayesian learning, but has the advantage of allowing attribution bias to range from zero to one, nesting the extreme cases of perfect and no conditioning. Other papers in economics study intrapersonal attribution errors through the lens of motivated forgetting, e.g. through recalling past successes more than past failures (Benabou and Tirole (2002), Benabou and Tirole (2006)).

$$P^b(\tilde{\rho} > c \mid m = 0) \leq P^b(\tilde{\rho} > c \mid m = 1) \tag{6}$$

i.e. players are weakly more likely to become friends with teammates than opponents.

2.4 Discussion

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

Discrimination and Stereotyping. The idea of updating along two dimensions (friendliness and ability) has parallels in both economics and psychology. In economics, models of discrimination largely fall into two categories: taste-based (Becker (1957)) and statistical discrimination (Arrow (1973)). Taste-based discrimination concerns the differential treatment of groups conditional on known ability (or productivity) being equivalent. Though usually thought of as a preference-based form of discrimination, we might also think of taste-based discrimination as being related to beliefs about whether others are friendly or not. Put differently, distaste for outgroups is quite closely related to the lack of desired social interaction with outgroups, in that both are independent of beliefs about ability. Statistical discrimination on the other hand regards discrimination of groups due to beliefs about ability, even in the absence of any distaste for outgroups. This concept maps well to the possibility of players segregating by caste (in teams) not because they have a distaste for other castes, but because they have less information about the ability of other-caste players. Similarly, psychologists have argued that most stereotypes regarding outgroups fall naturally among two dimensions: warmth and competence (Fiske et al. (2002), Fiske et al. (2007)). These dimensions map to friendliness and ability respectively.

Incorrect Priors. To simplify the exposition, I assume that priors are correct. A more plausible assumption may be that priors are incorrect, such that $\rho \neq \rho^t$, where ρ^t is the true proportion of other-castes that are good types. In this case, the type of contact can affect the speed of learning ($\mid \rho^t - E_{\rho^t}[\tilde{\rho} \mid m = x] \mid$) even in the absence of attribution bias.[13] But only with attribution bias can the learning (in expectation) go in opposite directions from the prior, depending on the type of contact. In this sense, even with

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

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

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

3 Background on Caste and Cricket

3.1 A Brief History of Caste

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

Two theories about the Indian caste system predominate. One is that the caste system has ideological origins which are based on notions of purity and impurity, and which naturally lead to a hierarchy in which the pure and impure are opposed (Dumont (1970)). The other theory argues that caste is not merely Indian tradition, but rather a modern phenomenon, shaped by economic and political interests. Dirks (2011) in particular argues that colonialism molded the caste system – through the British strengthening caste affiliation through censuses, branding some castes as "criminal", and showing preferential treatment to others in hiring.

In the postcolonial era, caste continues to be a central feature of Indian society. The caste-based reservation of jobs, begun by the British, was formalized. Erstwhile untouchables, and some others, were classified as Scheduled Castes (SC), with indigenous tribes classified as Scheduled Tribes (ST). In attempts to correct past discrimination, the government imposed quotas for these groups in government jobs, in higher education, and in politics. After the Mandal Commission in 1979, the government extended quotas to include another group of historically disadvantaged castes, the Other Backwards Castes

(OBC).

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

3.2 Caste in Modern India

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

Segregation. Castes are segregated through marriage, geography, and social networks. Marriage segregates because endogamy is widely practiced – 98% of married women respondents in Uttar Pradesh married within caste (Desai et al. (2011)). Though many castes often reside in the same village, geographical segregation results from castes living in separate hamlets. Reflecting these living arrangements, though each *jati* makes up on average 6% of a village's population, roughly 50% of food transfers and loans come from within the same *jati* (Munshi and Rosenzweig (2015)). Figure C1 illustrates the social segregation at baseline for three of the eight study locations where I organized cricket leagues. Formally, following Jackson (2010) I measure caste-based homophily as how prevalent friendships are between same-caste pairs compared to pairs in the network overall. A measure above one reflects that friendships are more likely to be formed within groups than across groups. The average homophily across the eight locations is 1.93 – study participants are roughly twice as likely to form friendships with a participant from the same caste than with a participant in general. For comparison, Jackson (2010) finds race-based homophily in US high schools to be lower, at 1.4 on average.

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

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

In addition to caste-based homophily, OBC men often lie between General and SC/ST men. On average, General caste participants have 8.4 General caste friends, but only 3 OBC and 1 SC/ST friends, with similar but opposite patterns for OBC and SC/ST participants (Figure 1). As predicted by the hierarchy of caste, 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 demonstrate that caste remains highly salient even among the young. Prior to the teams being chosen, one General caste participant requested that he be assigned to a team with players only from his own hamlet, remarking that if he were assigned to a team with a *chamar* (a Scheduled Caste *jati*) he would "beat them a lot." Another General caste participant said "I will assist those from my own caste, and beat the *chamars*. My whole day goes bad when I see face of a *chamar*." On multiple occasions General caste participants requested not to be shown the photos of the "*chamars*" when asked to select their friends. Similarly, one SC/ST participant, upon seeing the photos of General caste participants, asked the surveyor to "scroll through these Tiwaris and Pandits [General castes] quickly."

3.3 Why Cricket?

The experiment used cricket, a team-based, bat-and-ball sport, as a means of integrating men from different caste groups.[16] There are several advantages of this approach:

1. Cricket is popular in India, across all castes. Cricket's popularity makes high participation possible, mitigating selection concerns. Among study participants, 47% play cricket daily and 34% play two or three times a week.

2. Cricket tournaments are common in the area of the study, making the intervention naturalistic. At baseline, 38% of study participants were aware of a cricket tournament being held in their area in the past 12 months, and of those, 48% played in one.

3. Features of cricket make contact treatments natural: teams have to be formed, players must collaborate together to win, and teams must face opponents.[17] The collaborative nature of sport in

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

[17]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.

general was apparent to Allport, the originator of the contact hypothesis, who wrote in *The Nature of Prejudice*:

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

4. Features of cricket resemble other economic settings. Players exert individual effort for economic incentives. Players must cooperate with team members to maximize performance – with a tension between what is optimal for the team and what is optimal for the individual (as with team production). Teams defer authority to captains (bosses), and teams compete with others (as with competition for promotions in firms).

A final motivation for using cricket leagues is the measurement advantages they provide. First, features of cricket give natural within-match measures of discrimination, since teams must prioritize some players over others, as well as pick team captains. These measures are unobtrusive since they must be collected to score the matches. Second, individual-level ability can be accurately measured given that the main aims of each player are straightforward and structured. Batters must hit the ball far and bowlers must throw the ball fast toward the wickets. Baseline ability measures can then be used as controls when testing for within-team discrimination. This feature is an advantage relative to other sports. For example, in soccer, ability is more multi-dimensional, with players specialized to their position (e.g. defender), making ability comparisons between players difficult.

3.4 Cricket: A Primer

Cricket is similar in structure to baseball. Each team usually comprises eleven players, though in the experiment each team consisted of only five players, to maximize statistical power (by maximizing the number of clusters). Each team takes turns to either field or bat, with each period of play called an "innings." In the experiment, each match lasted 40 minutes on average.

When fielding, the team nominates one player to be the bowler (similar to a pitcher in baseball). The bowler throws the ball toward the wickets, which are a set of three wooden stumps (Figure 2). The team also nominates a wicket-keeper, to stand behind the wickets and receive the ball (similar to a catcher in baseball). The remaining team members play the role of fielders, working together to collect the ball and return it to the bowler and the wicket-keeper. 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 (toward or beyond the "boundary") such that they score a four or a six. The fielding team attempts to minimize the number of runs the batting team scores. They can do this, for example, by hitting the wickets when bowling (meaning the batsman at that end is "dismissed" and must be replaced by the next person in the batting order), or by catching the ball when it is hit into the air.

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. They must coordinate their running to avoid being dismissed, in the same way that baseball players must coordinate when running between bases. 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 innings. The collaborative contact in cricket entails both learning about the ability of teammates and experiencing cooperative interaction with them. Each of these channels could potentially affect cross-caste interaction and attitudes.

In addition, I exploit the interaction between opposing teams to identify the effect of adversarial contact. 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. As with collaborative contact, players learn about the ability of opponents. In contrast, players experience competitive interaction with them. Adversarial and collaborative contact may then have different effects – in particular, their effects on inferences about friendliness are more likely to diverge than their effects on inferences about ability (or statistical discrimination).

4 Experimental Design

4.1 Recruitment and Baseline Activities

Site Selection. Participants were recruited from eight gram panchayats[18] (GPs) near Varanasi (a city in Uttar Pradesh), with one cricket league organized per GP.[19] The experiment ran in three phases, with two leagues during the first phase, and three during each of the latter phases. The matches were played from

[18] Local administrative unit comprising several villages.

[19] In some cases the catchment area for a league included an additional neighboring GP, but for simplicity I will refer to each catchment area as being a single GP.

January to July 2017. GPs were selected according to several criteria, including that the GP must have: caste-segregated hamlets, a supportive pradhan (elected village head), roughly equal caste proportions, and an available cricket field.[20] From among 100 GPs visited by the field team, eight were chosen that met these criteria.

Participant Recruitment. In each GP, surveyors spent the first five days recruiting males aged 14 to 30 to play in the upcoming cricket league (see Timeline in Figure 3). We advertised the basic details of the leagues using posters (Figure C2), and via direct contact from Sarathi Development Foundation (our NGO partner) staff. The information made clear that teams would be chosen by the organizers and not by the participants themselves. This approach may have screened out those more prejudiced against other castes, making measures of prejudice and effects of collaborative contact potentially underestimates. By targeting particular hamlets, we kept recruitment roughly equally balanced across the three caste categories.[21] Men who expressed interested completed a baseline survey and were informed that their sign-up was not complete until their cricket ability was tested.

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

Ability Testing. Following the five days of recruitment, surveyors spent five days testing the cricket ability of each participant. Holding the ability testing separately from the baseline survey served a screening purpose: by increasing the time cost of signing up, the less enthusiastic participants (who might have attended fewer matches) were screened out.[22] Cricket ability was measured along three dimensions: bowling, batting, and fielding. For bowling, participants bowled six balls towards the wickets, and we measured both the speed (using speed guns) and accuracy (by recording the number of times the wickets were hit, and whether the ball was valid, wide, or a no ball). For batting, a surveyor bowled six balls towards the wickets, and the participant attempted to hit each ball. We recorded whether each ball was

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

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

[22] There is evidence of selection: those that did not complete ability testing (308 of 1,569) were less likely to say that they play cricket daily (31% vs. 47%).

hit, and if so whether it was hit sufficiently far to score either a four or a six. For fielding, a surveyor threw six balls high in the air towards the participant. We recorded how many balls were successfully caught, and how many times the participant hit the wickets with their return throws. Each team's ability results were made common knowledge within the team by a surveyor who read out the results prior to the team's first and second matches. Teams could opt-in to hearing the results again from the third match onwards but did so for only 7% of the matches. The ability measures are strongly predictive of league performance, as shown in Section 7.

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

4.2 Treatments

League Assignment. In each GP, 100 participants were randomly assigned to play in the cricket league. This randomization was stratified on caste.[23] I assigned the remaining participants to the control group.

Team Assignment. I assigned the 100 league players to 20 teams of five players each. 35% of the players were randomly assigned to homogeneous-caste teams, making seven out of 20 teams homogeneous-caste. I pooled and randomly ordered the remaining players. Each sequence of five then formed a mixed-caste team.[24]

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

[23] Further details of the randomization are in Appendix C.2.

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

a given team more than once) can be represented by a simple 8-regular graph with 20 nodes. I randomly chose a graph for each league using an existing algorithm, Bollobás' "pairing method" (see Appendix C.2 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.[25] Together, the assignment to teams and random match schedule created significant variation in collaborative and adversarial cross-caste contact (Figure 4).

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, when players are informed of their team assignment by phone, they are told the full names and father's names of their teammates, with these names strongly signalling caste. Second, close interaction with teammates on the pitch, including mandatory team talks, gives opportunities for teammates to learn each other's caste. Third, the catchment area for each league is sufficiently small that players can recognize their teammates and opponents, even if they are not friends – indeed when players are asked on the phone whether they know of their randomly assigned teammates, 39% say yes when the teammate is from the same caste, and 27% still say yes when the teammate is from a different caste. The corresponding figures for baseline friendships are 15% and 4%, suggesting that even though participants are far more likely to be friends with members of their own caste, they are not that much more likely to know them than participants from other castes. Fourth, during the matches the full names of bowlers are called out whenever the bowler is to be changed. Fifth, spectators at the matches[26] frequently call out the names of players, and sometimes even refer to players using caste slurs.

4.3 Additional Program Features

Ability Priors. Upon learning their team assignment, we elicited cricket ability priors by asking participants to predict the eventual ranking of themselves and their teammates according to batting strike rate, a commonly used measure of batting ability. The prediction was incentivized – at endline we paid Rs. 50 (~$0.80) to those that guessed the ranking correctly. I use the predictions to explore belief correction as a possible mechanism for the effects of contact.

[25] 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.

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

Incentives. To track the progress of teams in the league, a league table was updated daily and displayed at the cricket field (Figure C3), making the competition between teams salient. At the end of the league, the best three teams according to this league table won trophies and cash prizes (Rs. 1500/1000/500 or ~$23/16/8). Similarly, the best three players (based on number of times voted man-of-the-match[27]) won trophies and cash prizes (Rs. 500/350/200 or ~$8/5/3).

We also 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. We 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).[28] The incentive structure affected the competitiveness and payout inequality among teammates, providing a test of whether effects of collaborative contact are sensitive to the heightening of within-team competition.

Backup Protocol. Ideally the control-group players would play no matches at all (preserving their "control-ness" for program evaluation), but 100% attendance among players could not be guaranteed, and cricket matches are difficult to play without a full roster of players. As a result, control participants served as backup players.

To preserve a control group with very few matches actually played, we followed a strict backup protocol. I assigned a priority number randomly to each backup, within each caste. If a particular player could not attend one of his matches, surveyors called a backup player from the same caste in priority order. This protocol ensured that only high-priority backups played frequently – while the three highest-priority backups played six to eight matches on average, the remaining backups played far fewer (Figure C5). Since I chose the priority order randomly conditional on caste, the low-priority backups serve as a valid control group within each caste. In addition, by replacing absent players with someone of the same caste[29] I kept the caste composition of each team constant, preserving the collaborative contact treatment.

Attendance. I took several steps to address concerns of low attendance: (i) we gave a Rs. 10 (~$0.15) show-up fee to each player for each match attended; (ii) we held a lottery for a cricket bat following the

[27] This voting occurred immediately after each match.

[28] More specifically, the Individual Pay incentive scheme was as follows: when batting, if a player scored one run, he earned Rs. 2.5 (~$0.04). When bowling, if a player got a wicket, he earned Rs. 35 (~$0.50). In this way, individuals on the same team were paid based on their *own* performance, creating some incentive to compete with one another (e.g. by vying for the first slot in the batting order, or for the chance to bowl, in order to make more money). In contrast, players on Team Pay teams were paid equally: if a player scored one run when batting, each player on his team earned Rs. 0.5 (~$0.01). If a player got a wicket when bowling, each player earned Rs. 7 (~$0.10). Conditional on the same performance, a Team Pay team earned the same aggregate payout as an Individual Pay team, but the distribution across players within the team was equalized. As expected, Individual Pay players had much more dispersed payouts (Figure C4).

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

league for all those who attended at least six matches; (iii) we accommodated weather conditions and conflicting schedules by adjusting match times; and (iv) we required participants to have a phone number in order to sign up, and we called these phone numbers the day before each match and on the day itself to remind players to attend. Match attendance averaged 75.6%.

Umpires. Each match required an umpire to make final decisions. I allowed men to sign up to be players, umpires, or both. Seven signed up to be umpires exclusively. We used these men as umpires, but not as part of the sample of 1,261 for which we measured outcomes. Of the 1,261 that completed their sign-up as players, 281 also signed up to be umpires, of which 156 umpired at least one match.

4.4 Main Outcomes

I measured three sets of outcomes one to three weeks after the completion of each cricket league (see Phase 5 in Figure 3): (i) tastes for social interaction and team formation; (ii) voting and trust; and (iii) trading behavior. These outcomes were measured for all participants, except voting, which was not collected for the control group.

Social Interaction. Participants scrolled through a randomly-ordered list of all other participants in their location, seeing each participant's photo and full name. Surveyors asked them to select the participants they would like to spend more time with in the future (*Want to Interact w/*). Restricting responses to the people they listed, we then asked them to select those they considered friends (*Friends*). For leagues three to eight, surveyors additionally asked participants which people they would specifically *not* like to spend more time with (*Enemies*). By matching selections to the caste of each person, I calculated the total number of other-caste men selected for each question. By matching selections to the full network data on which players were teammates and which were opponents, I was able to distinguish between effects of contact on individuals played with versus those other-caste men not directly met.

Team Formation. While the social network questions capture tastes for social interaction (or taste-based discrimination), economies feature other important types of network formation – firms decide whom to hire, workers decide whom to work for, and workers decide with whom to work alongside. These decisions depend less on tastes for social interaction and more on beliefs about ability (or statistical discrimination). Though not in a firm context, I mimicked this type of choice by asking participants to select which players they would like on their team for future matches.

Specifically, we told participants in each league that there would be two additional matches played in one week. One match would have stakes: there would be Rs. 500 (~$8) awarded to the winning team. The other match would not have stakes: both teams would receive Rs. 250 (~$4) regardless of their performance. We asked participants to select their team twice: once for the match with stakes, and once for the match without. They selected their team by scrolling through the entire list of participants, again seeing their full names and photos. I then randomly selected four players per league (~1.25% probability) to have one of their two team choices implemented, making them the captain of their chosen team for one of the additional matches.[30] I used the team choice data to generate two main outcomes: the number of other-caste teammates chosen and the quality of the overall team, as measured by the predicted probability that the chosen team would win the future match.

By having participants choose a team twice, for matches with and without stakes, I varied 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. This incentive was present for both types of matches, but was stronger for the match with stakes.

Beyond willingness to interact (and its corollary, segregation), caste differences may affect welfare and allocation through caste favoritism and mistrust of other castes. In particular, evidence exists that group-based favoritism affects the allocation of public goods (Burgess et al. (2015)), and that trust promotes economic growth (Algan and Cahuc (2010)). I measured caste favoritism with a voting exercise, and trust with a behavioral game and a survey question.

Voting. 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% said they would go if they were selected and were available. The selection was decided by vote. Each participant ranked players on four other randomly-chosen teams from one to five, based on his preferences as to who should go on the field trip. I randomized the order in which they ranked the four teams. We 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. We encouraged participants to vote honestly regardless of their understanding of the voting rule, and explicitly told them that cricketing 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. Furthermore, the exercise has a parallel with another caste-based issue of importance: caste-based voting in elections. Interventions have been studied elsewhere to reduce the prevalence of caste-based or ethnic voting (Banerjee et al. (2010), Casey (2015)),

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

with the hope of having a positive impact on the competence of politicians ultimately elected. Here I test whether contact has any impact on caste-based voting in an apolitical context.

Trust. I measured cross-caste trust in two ways: (i) a standard trust game (as created by Berg et al. (1995) and used more recently in India by Castilla (2015)); and (ii) a World Values Survey question on whether the participant thinks that most people can be trusted, or that you need to be very careful in dealing with people (as used in Alesina and La Ferrara (2002), Algan and Cahuc (2010)).

For the trust game, I partnered each participant with three men from another village – one General caste, one OBC, and one SC/ST. Participants played the role of the Sender. Senders were allocated Rs. 50 (~$0.80) (only with some probability, explained below) and decided how much of the Rs. 50 to transfer to another person, the Recipient. Any money transferred was to be tripled. After the transfer took place, the Recipient decided how much money to return. The money returned would not be tripled.

The amount of money that participants send to their partners proxies for trust of own and other castes,[31] 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 effects of treatments as moving participants closer to the social optimum, increasing efficiency.

We 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. We chose as Recipients men with last names that both strongly signalled caste and that were relatively common among the participants in the Senders' league.

We 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). We randomly chose 20% of the participants to have one of their three trust choices implemented. We informed participants 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.

Trading. I designed the trading exercise to measure potential efficiency gains through contact reducing barriers to cross-caste interaction. For this exercise, in the two to three days following a league's final match, surveyors visited all participants at their homes, 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 mis-matched – the participant either received two left-hand or two right-hand gloves, and two left-foot or two right-foot

[31] Though there can be other interpretations – for example, sending more in the trust game might reflect greater altruism, not trust. The World Values Survey question suffers less from this critique.

flip-flops. Because the goods were mis-matched, participants had an incentive to trade with one another – the mis-matching created gains from trade.

To provide further gains from trade, we gave participants monetary incentives. Half of the participants earned Rs. 10 (~$0.16) for each successful trade, while the rest earned Rs. 20 (~$0.32). In addition, we gave incentives for "color-switching" to create specifically *cross-caste* gains from trade. Each good had a sticker of one of three colors affixed to it. The three colors were assigned to very strongly, though not perfectly, correlate with caste. We informed participants that different colors would be more difficult to find, but not that colors correlated with caste.[32] I randomly selected half of the participants to receive this color-switching bonus, with half of these promised Rs. 50 (~$0.80) and half promised Rs. 100 (~$1.60) per good. The color-switching bonus incentivized cross-caste trade without making caste salient. This incentive serves three purposes: (i) it can be used to "price" the effects of treatments; (ii) it ensured that a reasonable amount of cross-caste trade actually occurred (increasing power to detect effects); and (iii) by creating gains from specifically *cross-caste* trade, providing this incentive permits a test of the efficiency effects of contact.

After four or five days passed, surveyors returned to participants to log successful trades (by recording the unique ID written on any item they acquired), and to administer the final endline.[33] At endline, if any of the IDs on the gloves were initially assigned to a participant of a different caste, I classified this participant as having made a cross-caste trade. I did the same for the flip-flops.

4.5 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, and primarily use the following empirical specification:

$$y_{icl} = \alpha_{cl} + \beta \text{Prop. Oth. Caste on Team}_{icl} + \gamma \text{Prop. Oth. Caste of Opponents}_{icl} + \varepsilon_{icl} \quad (7)$$

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

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

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

team-level.

The collaborative contact treatment is Prop. Oth. Caste on Team$_{icl} \in \{0, 0.25, 0.5, 0.75, 1\}$, which is the proportion of player i's four teammates that belong to a different caste. β gives the causal effect of a player having all other-caste teammates instead of none. The adversarial contact treatment is Prop. Oth. Caste of Opponents$_{icl}$, which ranges from 0.35 to 0.975. In this case, given the linearity assumption and extrapolation beyond the support of the variable, γ identifies the causal effect of a player having all other-caste opponents instead of none.

To distinguish between effects that are due to cross-caste contact and those due to contact with others in general, it is sometimes appropriate to use the alternative specification:

$$y_{icl} = \lambda_{cl} + \eta \text{Prop. Own Caste on Team}_{icl} + \theta \text{Prop. Own Caste of Opponents}_{icl} + u_{icl} \quad (8)$$

where Prop. Own Caste on Team$_{icl} = 1 -$ Prop. Oth. Caste on Team$_{icl}$, and similar for opponent exposure. The contact hypothesis (Allport (1954)) claims that, under certain conditions, contact with outgroups should reduce prejudice, not that marginal contact with ingroups should change attitudes toward ingroups. This specification tests for this alternative ingroup effect.

4.6 Randomization and Implementation Checks

Balance checks suggest that the randomization was successful (Table C1). Two of sixteen coefficients (for age and whether in school) are statistically significant at the 10% 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 endline outcomes (Panel B). Most notably, there are no statistically significant effects for column 1, the most important baseline variable to test for balance: the number of other-caste friends listed in the social network survey.[34]

Attrition is low at 6.8%, and not statistically significantly affected by either collaborative or adversarial contact. This lack of selective attrition holds for the full sample and for each caste separately. Similarly, there are no statistically significant effects on the number of matches attended, for the full sample or caste-wise (Table C2). Having other-caste teammates is not a deterrent to playing.

[34] All results are similar if I control for the number of other-caste friends at baseline, or for age and whether in school, or for all eight variables used for the balance checks.

5 The Effects of Collaborative and Adversarial Contact

5.1 Willingness to Interact

Tastes for Social Interaction. Collaborative and adversarial contact have opposite effects on cross-caste friendships (top and middle-panel of Figure 5, Panel A of Table 1). Collaborative contact has a positive effect on desired future interaction with participants from other castes and cross-caste friendships. Those in homogeneous-caste teams want to interact with 7.1 other-caste participants in future, and are friends with 3.1. The average effect of moving from a homogeneous team to a team with four other-caste men ("full exposure" from now) is to want to interact with 2.4 more other-caste men in future, and to have 1.2 more friends. In contrast, adversarial contact has a negative effect on these outcomes, larger in magnitude than the effect of collaborative contact. An increase in adversarial exposure from the least (35%) to the most (97.5%) implies 3.4 fewer other-caste friends. In contrast with other evidence (Carrell et al. (2015)), the effects of contact are not mediated by the ability of players exposed to (column 4).

The collaborative effect is not due to participants recognizing more faces – if this was the case, participants would also select more people that they *don't* like, but this does not happen (bottom-panel of Figure 5, column 3 Panel A of Table 1). Collaborative contact has no statistically significant effect on the number of other-caste enemies – those that they would specifically not like to spend time with. This insignificant effect also rules out another possibility – that collaborative contact effects come through learning about teammates in general, including both that some are friendly, and others are hostile. In this world, beliefs about other castes don't improve on average, they just become more precise. Adversarial contact also has no significant effect on enemies – the point estimate is negative, and small relative to the outcome mean.

These effects are different when I consider instead exposure to people from the same caste, using empirical specification 8 (Panel B of Table 1). Collaborative contact with own-caste participants has a positive but insignificant effect on own-caste desired future interaction and friendships – the magnitudes are roughly one third 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 positive and significant effects – the opposite of the cross-caste effect. The point estimate of 6.3 for desired future interaction implies that for every 10 additional own-caste opponents faced, a participant wants to spend time with 1.6 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 vs. Groups. To test whether the effects of contact extend beyond those played with, I explore effects of collaborative contact on friendships with non-teammates, and effects of adversarial contact on friendships with non-opponents.

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

For the effects of adversarial contact, I define the outcome as the percentage of other-caste friends among low priority backups – those with a priority number of seven or above. There are 173 of these backups, and they played an average of only 0.8 matches each. Since they played so little, they would only rarely have been played against as opponents. Any effects of adversarial contact on desired interaction with these people are unlikely to be driven by direct contact as opponents.[35]

Both collaborative and adversarial contact effects extend to the outgroup as a whole. Collaborative contact has a positive and statistically significant effect on desired future interaction and friendships with other-caste men in other teams. Adversarial contact again has significant negative effects (Figure 6, Table 2). These effects are large: full collaborative exposure increases non-teammate cross-caste friendships by 0.17-0.18 standard deviations of the outcome. The effect is stronger for adversarial contact: an increase in adversarial exposure from the least to the most reduces non-opponent cross-caste friendships by 0.54-1.1 standard deviations.

Consistent with the negative adversarial effect on cross-caste friendships, the effect of collaborative contact on non-teammate friendships is statistically significant for non-opponents but insignificant for opponents (Table C3). Collaborative contact increases friendliness to other castes in general, but this effect is counteracted when the other castes are faced as opponents. Furthermore, the fact that the effect is driven by non-opponents rules out another possibility – that the effects 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. In this scenario, players may not change their general tastes towards interaction with

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

other castes – they may merely get access to a broader network of other-caste men. I use outcome data at the dyadic-level to test directly for this mechanism, with the following specification:

$$y_{ij} = \left(\alpha_{jcl} \times \text{Prop. Oth. Caste on Team}_{icl}\right) + \beta_1 \text{Teammate}_{ij} + \beta_2 \text{Friend of Oth. Caste Teammate}_{ij} + \varepsilon_{ij} \tag{9}$$

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

For this specification, I restrict only to observations where i and j belong to different castes, and where i is in a mixed team (with Prop. Oth. Caste on $\text{Team}_{icl} > 0$). The intuition behind the specification is shown visually in Figure C6. In brief, β_1 is identified by comparing two players that share the same caste, league, and collaborative exposure, but belong to different teams. Suppose these players are i and i', and that player k is a teammate of i's, but not i''s. The effect is estimated by asking "how much more likely is it that i is friends with k after the league is over than i' is?" Similarly, suppose that there is some player j who is an other-caste friend (at baseline) of one of i's other-caste teammates, but not linked to any of i''s other-caste teammates. β_2 is estimated by asking "how much more likely is it that i is friends with j after the league is over than i' is?" Each of these effects is causal since the randomization to teams ensures random assignment of both direct and indirect links (conditional on fixed effects).

Verifying the causal claim, there are no significant differences at baseline (column 1 of Table 3). Players are not more likely to be friends at baseline with future direct or indirect links, when compared with other players in the same cell. Assignment to be teammates with a player increases the probability of wanting to interact with that player in future by 24 percentage points (column 2) and friendship by 13 percentage points (column 3). 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 most consistent with players changing their general attitudes towards the other caste groups.

Taken together, these results on desire for social interaction suggest that the two types of contact have opposite effects on inferences about the friendliness of other-caste players. To the extent that these inferences are unrelated to actual "productivity", we can similarly think of the two types of contact as having opposite effects on taste-based discrimination (Becker (1957)). Rao (2014) finds similarly that contact (with poor students) can reduce taste-based discrimination, as measured by the willingness to interact socially with a poor student for two hours following a relay race, and the willingness to go on a play date. In contrast, I show here that contact can also *increase* taste-based discrimination, when the contact is of an adversarial nature.

Match Interactions. Why do collaborative and adversarial contact have opposite effects on inferences about others? The learning model proposes one explanation: the two types of contact incentivize different types of behaviors. Groups with common goals have incentives to cooperate with and encourage one another. In contrast, groups with opposing goals have incentives to undermine one another. These behaviors may in turn drive opposite inferences about the friendliness of outgroups if players over-attribute the behavior they observe to individuals (and the caste group) as opposed to the incentives. To test for effects on behaviors, surveyors recorded all instances of friendly and hostile behavior between players during each match. Friendly events include high-fives, hugs/taps on backs, and compliments/congratulations. Hostile events include arguments and insults. I use the following dyadic specification to test for whether the type of contact affected the nature of actual in-match interactions:

$$y_{ijt} = \alpha_t + \alpha_{c(i)c(j)} + \phi \text{Teammate}_{ijt} + \xi_{ijt} \tag{10}$$

where y_{ijt} is the number of interactions (e.g. number of high-fives) that took place between players i and j during match t. α_t are a set of match fixed effects, and $\alpha_{c(i)c(j)}$ are a set of caste of player i-by-caste of player j fixed effects. Teammate_{ijt} is the key regressor: a dummy variable equal to one if i and j are assigned to the same team, and equal to zero if they are instead opponents during match t. Standard errors are dyadic-robust at the team-level.

I include only observations where i and j belong to different castes, and exclude any backup players. The dyads are then formed by pairing any two different-caste non-backup players participating in the same match. It follows that ϕ is the causal effect on match interactions of a player from a different-caste being a teammate instead of an opponent.

Interactions with teammates are more frequent, regardless of whether the interaction is friendly or hostile (columns 1-5, Table C4). These effects are intuitive for friendly behavior – sporting norms dictate that teams should high-five and congratulate their teammates, but not their opponents. For hostile behavior, the effects likely reflect the greater communication necessary with teammates in general.

Conditional on interacting, interactions with teammates are 50 percentage points less likely to be hostile (column 6). Simply put, the most common interaction with an other-caste opponent is an argument, whereas the most common interaction with an other-caste teammate is a hug. The effects on social interaction can then be explained through these interactions leading to corresponding inferences about outgroups.

Team Formation. Apart from distaste for social interaction, the social segregation of castes may provide a supportive environment for statistical discrimination (Arrow (1973), Aigner and Cain (1977), Cornell and Welch (1996)). Men may discriminate against other-caste men as a result of imprecise information (relative to own-castes) about their underlying ability or productivity. Furthermore, the effects of contact on statistical discrimination, which depend on the information conveyed about other-caste ability, may be different to effects on tastes. I explore this possibility by looking at effects on team formation. Here the choices are payoff-relevant and depend heavily on knowledge about cricketing ability of other-caste men.

Both collaborative and adversarial contact have positive effects on cross-caste team formation for the match with stakes, with a similar estimated effect: $\hat{\beta} = 0.72$ for collaborative contact ($p < 0.01$), and $\hat{\gamma} = 0.75$ ($p = 0.1$) for adversarial contact (Figure 7, Table 4). These effects are roughly 50% of the mean of 1.5 other-caste men chosen. To benchmark the collaborative effect, if players chose teammates randomly, they would choose other-caste players 67% of the time. In contrast, homogeneous-caste teams choose other-caste players 29% of the time, whereas those with four other-caste teammates choose other-caste players 51% of the time. Full on-team exposure closes over 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 – knowledge which is especially important when interaction is infrequent to begin with. The net effect of the animus and knowledge is a greater willingness to work together with men from other castes. This result has parallels with Miller (2017). Miller finds persistent effects of affirmative action in the US – the black share of employees at establishments continues to grow after establishments are no longer subject to affirmative action regulation. Experience with blacks leads to increased willingness to work with blacks. Also consistent, Beaman et al. (2009) find that exposure to female leaders in India leads to improved perceptions of female leader effectiveness and more votes for women in future. In contrast, they find no effects on stated and implicit tastes for female leaders. Their interpretation is that deep preferences are less malleable than beliefs regarding effectiveness. The result in this paper is stronger: adversarial contact increases willingness to play with other-caste men in future, even with *negative* effects on friendships, which reflect tastes more directly.

A natural interpretation of this set of results is that while the two types of contact have opposite effects on taste-based discrimination (through inferences about friendliness), they both reduce statistical discrimination. Two additional results support this interpretation. First, the effect of collaborative contact on other-caste team choice is significantly greater when a player's randomly assigned other-caste teammates have higher measured cricket ability at baseline (columns 3 and 4, Table 4),[36] unlike the effect of collaborative contact on cross-caste friendships, which does not differ by teammate ability (column 4, Table 1). Tastes for social interaction are then not driven by information about other-caste cricketing ability, whereas future team formation is.

Second, if collaborative contact reduces both taste-based and statistical discrimination, but adversarial contact only reduces the latter, then removing the stakes for the bonus match should weaken the adversarial effect much more than the collaborative effect. Without stakes, motives should shift away from picking the best cricketers, and toward picking those that are fun to play with, reducing the adversarial contact effect more. This pattern is borne out in the data. Both types of contact have significantly weaker effects when stakes are removed. However, while collaborative contact continues to have a positive significant effect, the adversarial contact effect falls significantly more, to -0.04 (bottom-panel of Figure 7, Table 4).

Contact is less likely to affect non-teammate and non-opponent other-castes being chosen as teammates given the structure of the exercise. Participants can only select a maximum of four players, meaning that a participant who selects several players from their own team may have no slots left for other-caste players from other teams, even if they have become less biased. Despite this limitation, those with more other-caste players on their team also pick more other-caste players from *other* teams for a future team for the match with stakes (Table C5), despite not having played with them, and despite not knowing more about their ability. In contrast, those with more other-caste opponents are not more likely to pick other-caste non-opponents for a future team. This is consistent with adversarial contact revealing the ability of individual players, such that they get selected. Collaborative contact reveals ability, but it also shifts tastes toward cooperation with other castes more generally.

5.2 Caste Favoritism and Trust

Beyond willingness to interact, caste preferences may affect village economies through ingroup favoritism in allocation (Fisman et al. (2017)). Most notably in the Indian context, local elected village heads may show favoritism in distributing public goods to their own group. Related, caste preferences may also have impacts through distrust, impeding intergroup cooperation and the enforcement of contracts (Bubb et al. (2016)). I explore these effects using the voting exercise and trust measures.

[36] The interaction with other-caste opponent ability is also positive, but noisily estimated given the smaller variation in average ability of opponents.

Voting. The voting exercise sheds light on caste favoritism when the stakes are high. General castes show the most ingroup 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 other castes significantly lower (columns 1 and 2, Table 5). General castes on average rank someone from a different caste 0.78 positions lower – this outgroup bias is larger than the effect of the votee being a full two standard deviations worse in bowling, batting, *and* fielding ability. The own-caste favoritism of SC/STs is small and statistically insignificant (column 3), reflecting the asymmetry in caste discrimination. Pooling all castes, and including fixed effects for the player voted on, the outgroup bias amounts to ranking other castes 0.4 positions lower (column 4). Furthermore, this outgroup bias is not merely driven by players having more friends from their own caste, and showing favoritism toward them – the estimated bias is similar when controlling for pairwise friendship links at baseline (column 1, Table C6).

Collaborative contact reduces the outgroup bias by up to one-third (column 5, Table 5, p = 0.08). The reduction in bias is not merely driven by a reduction in favoritism towards friends (column 2, Table C6). 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 stakes are higher. In contrast, the adversarial contact effect is not statistically significant, whether considering votes for all other teams (column 5), or just those for non-opponent teams (column 6).

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

First, ability beliefs are not more incorrect for other-caste players than own-caste players at baseline (Table C7). In particular, players are confident in their own abilities – predicting their ranking (from 1 to 5) to be 0.57 ranks better than their teammates on average (column 1). Among their four other teammates, they predict that the other-caste players will be 0.15 ranks worse on average. These baseline rankings are predictive of actual rankings. An increase in predicted rank of one is associated with an increase in actual rank of 0.14 (column 2). Taking the difference between the actual and the predicted rank, the prediction error is large and significant for the player themselves – consistent with over-confidence (column 3). In contrast, for teammates there is no statistically significant difference in prediction error between own and other castes – inconsistent with the idea that players have systematically biased beliefs about the relative ability of other castes to begin with. There is also no statistically significant difference in the *absolute* prediction error between own and other castes, showing that beliefs about other castes are not noisier either.

Second, note that a full standard deviation increase in one of the baseline ability measures improves the vote by 0.07 to 0.14 ranks (columns 1-3, Table C6). Given these estimates, the magnitude of the collaborative effect seems too large to be explained only by effects on statistical discrimination. For this to be the case, we would need full collaborative exposure to lead players to treat other-caste players *as if* they are one-third to two-thirds of a standard deviation higher on each of the ability measures. This extent of generalized belief updating seems implausible given that baseline beliefs are anyway no more likely to be incorrect for other-caste players than own-caste players, and that even those with no collaborative exposure see signals of the ability of the other-caste players they play against.

These results together support the claim that collaborative contact affects caste favoritism in voting primarily through its impact on taste-based, not statistical, discrimination.

Trust. Consistent with the patterns of caste favoritism in the voting exercise, General and OBC castes show the most distrust of other castes in the trust game, while SC/STs show none (columns 1 to 3, Table 6). General and OBC castes send roughly 10% less to other-caste partners than same-caste partners. There is no evidence that contact affects trust of other castes in other villages (column 4) – the interactions with each type of contact are small and statistically insignificant. However, both types of contact *reduce* the amount sent in the trust game overall, though the effect is statistically insignificant (p = 0.16) for adversarial contact (column 5). These negative effects suggest that cross-caste contact might affect trust, but uniformly across castes, rather than for one caste in particular. For stated trust, adversarial contact also has a negative effect, with no effect of collaborative contact (column 6). The negative effect of adversarial contact is large in magnitude – going from the least to most adversarial contact in the sample implies a reduction in stated trust of 26 percentage points. Together this evidence suggests that while adversarial contact may reduce trust in general, neither type of contact affects cross-caste trust specifically.

5.3 Efficiency

Team Quality. Motivated by evidence of discrimination in hiring (Bertrand and Mullainathan (2004)), and its implications for allocation and efficiency (Hsieh et al. (2013)), I first explore efficiency effects of contact by estimating effects on the quality of the team chosen for the future match with stakes. If collaborative contact reduces discrimination against other castes, it should follow that participants choose more other-caste participants as teammates. Moreover, the team itself should be of higher quality overall, since discrimination can cause talented outgroup candidates to be overlooked, sacrificing efficiency.

I use machine learning techniques to measure the quality of chosen teams. First, I develop a linear predictive model of whether a team wins a given match. Specifically, I divide the sample of 640 match results into a training sample (the first 60 matches in each league) and a hold-out sample (the last 20 matches in each league) following Mullainathan and Spiess (2017). I consider four sets of "predictors" – pre-determined variables matched to each team – of the outcome of each match. The first set contains only baseline variables and their squares, including each ability measure for the first to fifth best player in each team (see Appendix C.3 for full details). The second set additionally includes all possible interaction terms. The third set includes baseline and actual previous league performance variables, and their squares. The fourth is the same as the third, plus all possible interaction terms. I use LASSO for variable selection among each of these sets of predictors.[37] By running a linear regression of the win dummy on the selected variables for the training sample, I estimate the weights to use for prediction. I apply these weights to the characteristics of the teams chosen by participants for the match with stakes. This exercise yields predicted win probabilities for each participant's chosen team.

Out-of-sample performance is similar across the four sets of possible predictors – around 66% of match outcomes in the hold-out sample are predicted correctly based on the model's predictions (Table 7), a moderate improvement relative to a "random guessing" benchmark of 50%. Full collaborative exposure increases the predicted win probability by 3.2 to 4 percentage points, while the effect of adversarial contact is usually close to zero, and statistically insignificant. Given the stakes of the bonus match, an increased win probability of 4 percentage points amounts to a small increase in expected winnings of Rs. 20 (~$0.30) per team.

There are two plausible explanations for why both types of contact increase the number of other-caste teammates chosen, but only collaborative contact increases team quality. First, adversarial contact gives noisier signals of ability for each other-caste player – performance is only observed for one match, whereas teammates are observed repeatedly. Second, since collaborative contact shifts tastes towards other-caste players more generally, collaborative contact can also cause the selection of other-caste players already known (by everyone) to be talented, by reducing social barriers to interaction.

Trade. To test for effects of contact on the efficiency of economic transactions, I explore behavior in the trading exercise. Most participants trade successfully – 88% of goods received by those in homogeneous-caste teams are traded, with no statistically significant effect of collaborative or adversarial contact (column 1, Table 8). It follows that any effects on cross-caste trade are not confounded by extensive margin effects.

The color-switch incentives have large effects, increasing cross-caste trade by 22 to 24 percentage points relative to a mean in homogeneous-caste teams of 52% (column 2). Collaborative contact has marginally insignificant effects – full exposure increases cross-caste trade by six percentage points. But

[37] The λ is chosen through 10-fold cross-validation within the training sample.

this full sample includes 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 Figure 8) and trade payouts by Rs. 15 or 11% (column 4). The treatment effect on cross-caste trade is roughly 50% of the effect of the color-switch incentives, which amount to one quarter to one half of a typical local daily wage of Rs. 200.

The trading data show that collaborative contact is a complement of, not a substitute for, incentives for intergroup interaction. Collaborative contact facilitates intergroup cooperation in contexts 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 own castes 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 trade 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.

Adversarial contact has an insignificant effect on cross-caste trade (whether for the full sample or the subsample with color-switch incentives) and payouts (columns 2-4). The contrast between this effect and the strong negative effect on cross-caste friendships has two plausible explanations. First, spillovers in the trading network reduce the power to detect negative effects on cross-caste trade – the adversarial coefficient is in fact negative and large in magnitude, though insignificant. Second, though adversarial contact reduces the desire for cross-caste social interaction, the trading transactions require only a few minutes of interaction. Adversarial contact may well not affect fleeting economic transactions like these. On the other hand, economic transactions requiring more intensive interaction (e.g. through monitoring of loan use, or through working together on a joint project) might be more affected.

The effect of collaborative contact is much larger for General castes, at up to 30 percentage points, than the other two caste groups (column 5). This effect does not come through General castes trading with their other-caste teammates – the effects are similarly large when considering only those cross-caste trades made with other-caste players from other teams (column 6), and the causal effect of being assigned a teammate on trading with that teammate is small and insignificant (Table C8, column 2). Furthermore, there is no evidence that General castes are trading with the other-caste friends of their other-caste teammates (Table C8, column 2). Taken together, 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.[38] Instead, complementing the evidence on willingness to interact and team selection, collaborative contact shifts the tastes of General castes for interaction and cooperation with other-caste men.

In this case, the effects of contact also interact with pre-existing social structure. Collaborative contact is *asymmetrically* reducing social barriers to trade – making it easier for the high castes to trade with those below them in the hierarchy, but not the reverse. This pattern may be consistent with the asymmetry in social structure. High castes might not like low castes, but they do not *fear* them. In contrast, low castes may well fear high castes. In this context, it may be easier to encourage the dominant group to interact and cooperate with the minority group than vice versa.

5.4 The Resilience of Collaborative Contact

Collaborative contact increases cross-caste friendships, team formation, and trade, and reduces own-caste favoritism in voting. Adversarial contact has negative or null effects on each of these outcomes. These results suggest that this type of integration can affect future intergroup interaction and attitudes. In the real world, however, solely collaborative contact is rare – even members of sports teams compete for positions in the starting line up, co-authors in economics compete on the job market, and colleagues at work compete for promotion. Organizations may face a tension – efficiency requires meritocracy, but does meritocracy undo the collaborative forces that promote cohesion?

I explore this tension directly by exploiting the random assignment of teams to incentives. Half of the teams were randomly assigned to receive Team Pay and the rest to receive Individual Pay. Individual Pay creates competition for payouts *within* teams, conditional on the same proximate on-team contact. As a result, players on Individual Pay teams receive much more dispersed payouts (Figure C4). These incentives create the same tension inherent in organizations – players (from different castes) remain on the same team, but for half the teams there are much stronger incentives for competition.

Even with these incentives, the collaborative effects do not unravel (Table 9). When considering all the main outcomes, the interaction between collaborative contact and Individual Pay is small and never statistically significant. Furthermore, in contrast to the effects of being a teammate vs. opponent on

[38] In addition, the collaborative contact effect appears not to be driven by a reduction in mistrust, given the insignificant effects of contact on cross-caste trust in Section 5.2.

interactions (Table C4), cross-caste interactions on Individual Pay teams are no more likely to be hostile (Table C9). This pattern of results supports the claim of the model with the fundamental attribution error: collaborative and adversarial contact have divergent effects through incentivizing different types of 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.

6 Alternative Explanations

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

6.1 Intensity of Contact

Though players observe the behaviors of both teammates and opponents during each match, contact with teammates is somewhat more intensive than contact with opponents – for one thing, teammates interact more often, whether these interactions are friendly or hostile (Table C4). We might model this in a learning framework (as in Section 2) by assuming that signals from teammates are more precise than signals from opponents. This signal precision argument is unlikely to explain the main results for two reasons. First, in general the signal precision should affect the speed of learning, but not the direction, inconsistent with the opposite effects on tastes for social interaction (Table 1, 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 selection task (Table 4). This result rules out the extreme possibility that players do not learn about their opponents at all.

6.2 Duration of Contact

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

I test this hypothesis by exploiting another feature of the experimental design: control participants served as backup players, and these backup players played not just on one team, but on whichever team they were asked to play as a substitute. Backup players then experienced cross-caste collaborative contact, but it was short-term in nature relative to that of the players assigned to mixed teams. Players in mixed teams played on average 3.7 matches with each of the other-caste men they were exposed to as teammates. In contrast, backup players played on average only 1.5 matches with each of the other-caste men they were exposed to. A natural test is to compare high-priority backups with low-priority backups. High-priority backups have more collaborative cross-caste contact than low priority backups, but this contact is more short-term in nature than that experienced by non-backups. If the negative effects of cross-caste contact with opponents is driven by the duration of interactions, then there should be negative effects here too.[39]

In the top panel of Figure C7 I verify that high-priority backups experience more cross-caste exposure than low-priority backups. High-priority backups play on teams with more other-caste players in total (top-left panel), and with more unique other-caste players (top-right panel). High-priority backups also list significantly more other-caste participants that they would like to spend more time with, and significantly more other-caste participants that are friends (bottom panel). This suggests that the duration of contact is not the reason for the negative effects of adversarial contact.

6.3 Winning

A large body of work finds that ethnic diversity affects productivity and efficiency, often negatively (Alesina and Ferrara (2005), Hjort (2014), Marx et al. (2016), Hoogendoorn and Van Praag (2012)). In this experiment, it is possible that caste composition affects team performance, and that performance in turn affects outcomes. The warm glow from winning, for example, could lead respondents to list more other-caste friends when asked. I rule this out in Table C10 – 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).

[39] 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.

7 Caste Differences

Inferences about outgroups may also depend on social structure. For example, minorities integrated with a dominant group may experience discrimination that they would not be aware of otherwise. This experience may worsen their views of the dominant group, and yet be consistent with the dominant group becoming less prejudiced as a result of contact. I explore this question by testing for discrimination and caste heterogeneity of contact effects.

7.1 Ability and Discrimination

SC/ST players are worse at cricket according to baseline measures, conditional on age (columns 1 to 3, Table C11). Their maximum bowling speed is on average 3.45 km/h slower than that of General castes (0.34 s.d.), and they hit 0.31 fewer fours/sixes (0.24 s.d.), and catch 0.22 fewer balls (0.19 s.d.). OBC players do not differ significantly from General castes for any ability measure. The SC/ST difference affects payouts – the hypothetical performance-related pay is 25% lower for SC/STs than General castes (column 4). Fifty-eight percent of these payout differences remain after controlling for ability (column 5), suggesting that these differences are not just due to ability, but also on-team discrimination.

To earn payouts while playing, individuals must first get the chance to bat (to earn money from scoring runs) and/or the chance to bowl (to earn money from taking wickets).[40] Opportunities to bat and bowl are influenced by the team's allocation choices. Before each match, each team selects a captain from among the non-backup players that have attended. The captain, together with the team, then chooses a "batting order" before the match starts. This choice entails ranking the players from one to five, with the top two players batting first, and the rest waiting their turn to bat. In practice, this order matters because those later in the batting order may not get the chance to bat at all, losing the opportunity to make money from batting. When fielding, each team must also decide which players will bowl. Since there can only be one bowler at a time, there are again constraints – it won't necessarily be the case that all players get the chance to bowl. In choosing captains, the batting order, and who bowls, teams can show favoritism towards one caste over others.

Favoritism of upper castes exists for all three types of allocation (Table C12). SC/STs are significantly less likely to be chosen as captains, and less favored in the batting and bowling orders. 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

[40]On average, 14% of players in a given match didn't get the chance to bat, and 44% didn't get the chance to bowl.

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.

7.2 Contact Effects by Caste

Given the evidence for on-team discrimination against SC/STs, 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 (compared with General or OBC castes) is cross-caste trade, as already explained in Section 5.3 (upper-panel of Tables C13 and C14). The lack of heterogeneity suggests that affirmative action policies that place under-performing and discriminated against minorities in contact with a dominant group may nevertheless lead to improved intergroup relations. Otherwise, no reliable pattern is observed for caste heterogeneity of adversarial contact effects (lower-panel of Tables C13 and C14).

8 The Net Effect of Contact

The results on contrasting effects of different types of contact raise a question: if effects can go either way, does the integrative cricket intervention have positive effects overall? Since participants were first randomized to either play in the leagues or serve as backup players, I address this question by comparing outcomes for the backups with those that played in the leagues. Since high-priority backups played on average as many, or more, matches than the league players (Figure C5), I exclude backup players with priority one to three from this analysis. The remaining backup players played on average 1.6 matches each, compared with 6.1 matches for league players. Since low-priority backup players still played some matches, I consider these effects to be a lower bound on the overall effects of the program.

While those in homogeneous-caste teams have significantly more other-caste friends, higher team quality, and send more in the trust game than those in control, these players are not significantly different to control players when considering other-caste team choice, cross-caste trade, and trade payouts (Figure 9, Table 10). League participation alone does not lead to effects on cross-caste interaction and trade. In contrast, those in mixed teams fare significantly better than control players on all outcomes except trust: they have 1.36 (0.3 s.d.) more other-caste friends, choose 0.45 (0.4 s.d.) more other-caste players for their future team for a match with stakes, engage in nine percentage points (0.18 s.d.) more cross-caste trade and make Rs. 12 (0.15 s.d) more in trade payouts.

Using the estimates in Table 10, I can calculate a rough estimate of counterfactual program effects if participants were allowed to choose their own teams. First, those in the control group (low-priority backups) chose 1.2 other-caste teammates (column 2). Let us assume this would be the level of cross-caste exposure in the case where participants choose their own teams. In contrast, simple random assignment to teams would ensure players have 2.7 other-caste teammates on average. Assuming that treatment effects are linear in the number of other-caste teammates (whether self-selected or not), column 5 implies that the effect of the program on cross-caste trade would be 40% smaller if participants were able to choose their own teams.

Taken together, these results suggest that even a short-term program with collaborative contact might lead to greater cross-caste interaction and economic exchange, and that preventing self-segregation within the program is crucial for effectiveness.

9 Conclusion

This paper provides evidence that the effects of integration depend on the type of contact. While collaborative on-team contact increases cross-caste friendships, adversarial contact with opponents reduces them. Both of these effects extend to the outgroup as a whole. Collaborative contact increases cross-caste friendships even with non-teammates, and adversarial contact harms friendships even with non-opponents. These findings suggest that the two types of contact have opposite effects on inferences about the friendliness of other castes. In contrast, both types of contact convey similar information about the ability of other castes, reducing statistical discrimination as measured by the number of other-caste players chosen as future teammates. These effects have broader implications for efficiency. Collaborative contact, but not adversarial, increases team quality and cross-caste trade when gains from trade exist.

Conceptually, my findings suggest that Allport (1954) was right – contact may reduce prejudice only if certain conditions, including that groups have common goals, are met. In this experiment the results go further than this – in the case of adversarial contact it is not just that contact becomes ineffective, it can even lead group differences to increase.

Though beyond the scope of this paper, many other types of intergroup contact exist: contact in private versus in public, contact through traumatic experiences, and contact that is known in an advance to be temporary versus that which is known to be sustained. The conceptual framework in this paper – a learning model with attribution errors – may be applicable to these other cases. In particular, collaborative and adversarial contact may have divergent effects on tastes toward social interaction through the mechanism of attribution errors. The attributions an individual makes about others depend on the context in which they interact. If the context promotes cooperative behavior, attributions about cooperativeness will be made. If the context promotes competitive behavior, individuals will infer others to be competitive and selfish. An implication is that it may be possible to design social policies to promote desired inferences. For example, one intervention might maximize the common goals and cooperation between groups to address segregated social networks. Another intervention might maximize the observability of the ability of outgroups to address incorrect stereotypes regarding intelligence.

Finally, avenues exist for future work. Researchers might go further by systematically varying the other types of contact that Allport emphasizes: whether groups have equal status and whether there is authority support of integration. Second, we should test whether the effects of short-term interventions like the one in this paper have durable effects, or whether groups simply retreat into enclaves in the months that follow. Third, we should test for the effects of competitive interaction in other economic settings. With anti-immigrant sentiment in mind, competition in labor markets in particular seems worthy of exploration.

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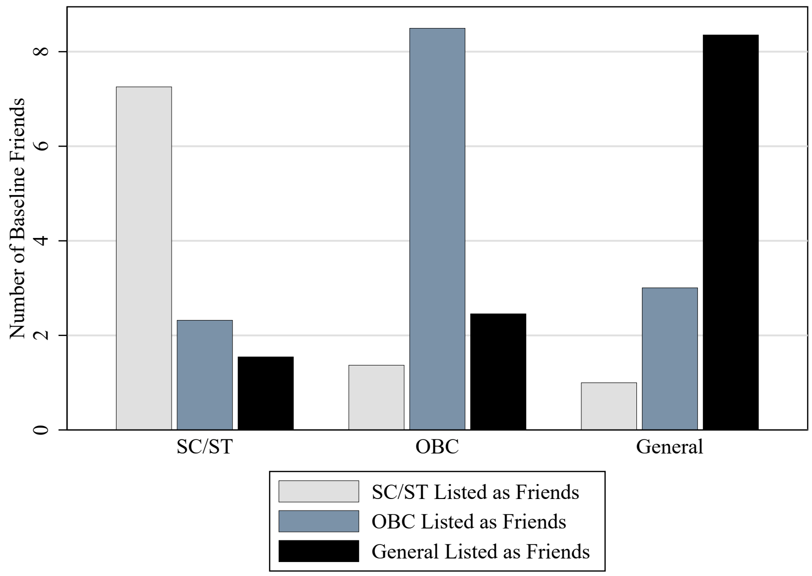
A Figures

Figure 1: Caste-wise Friendships at Baseline



Notes: For those that completed the baseline social network survey (1,251 of 1,261 participants) the bars show the mean number of friends listed of a given caste, averaging across all participants from a given caste. For example, the far-left bar shows that SC/ST caste participants report having 7.3 SC/ST friends on average.

Figure 2: Cricket Explained

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

Figure 3: Timeline

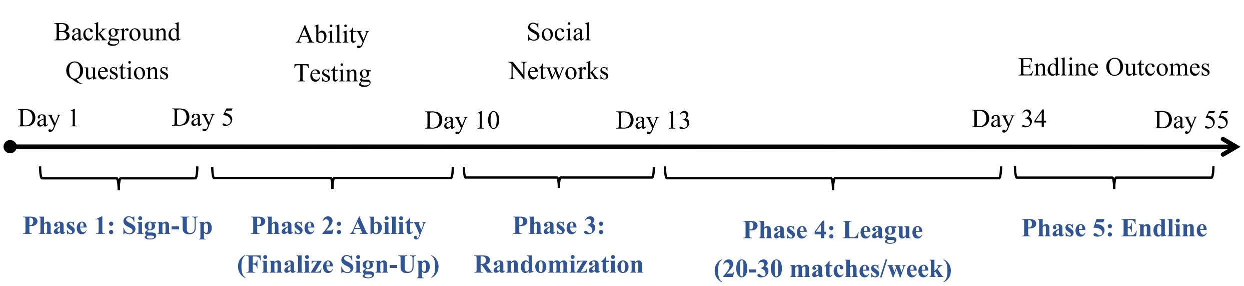
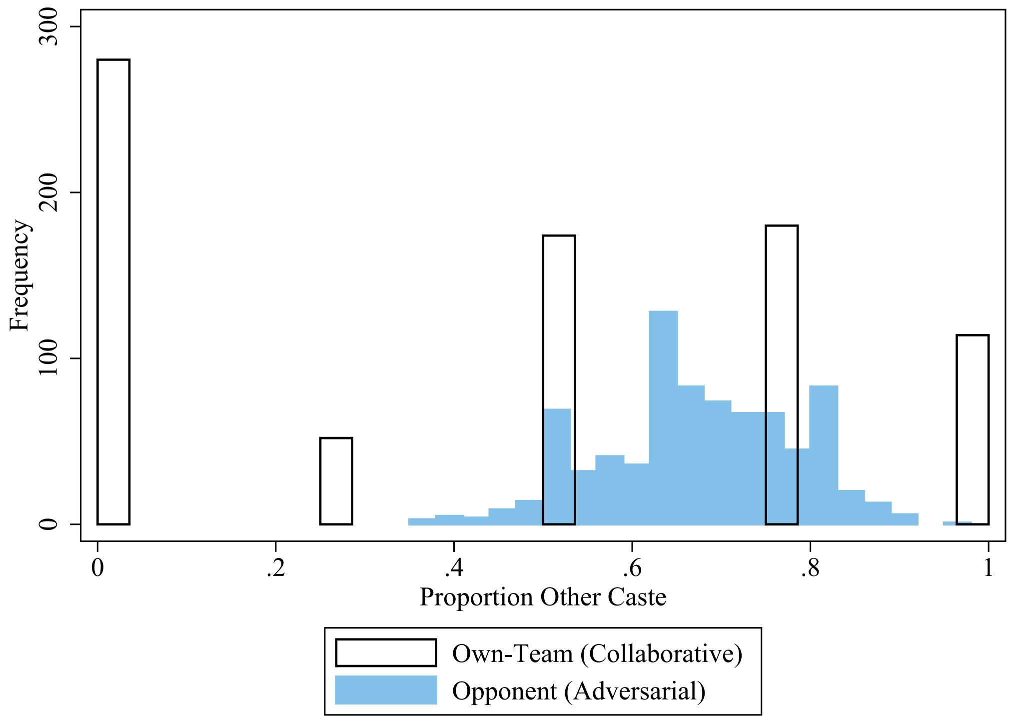
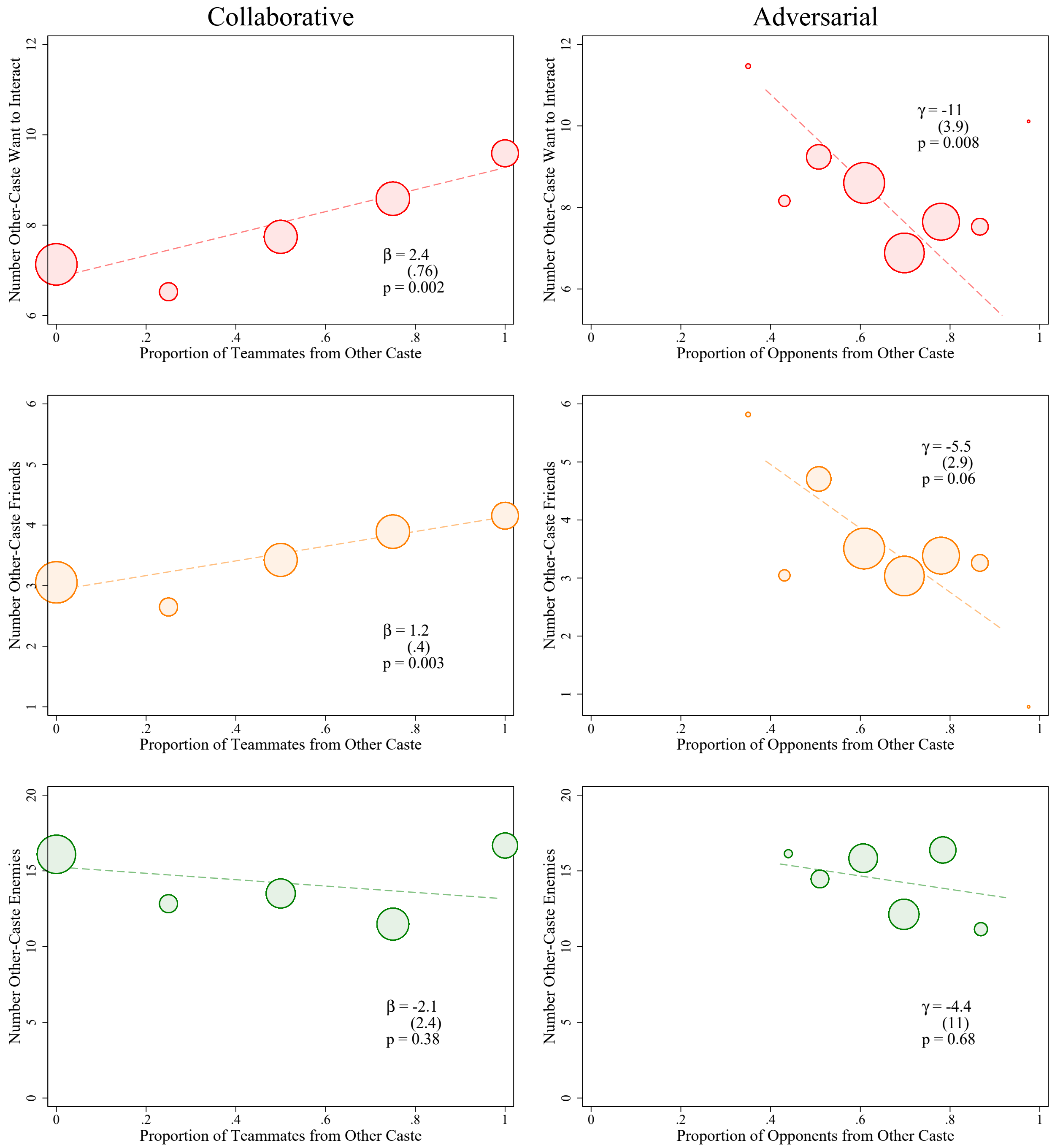


Figure 4: Variation in Collaborative and Adversarial Contact



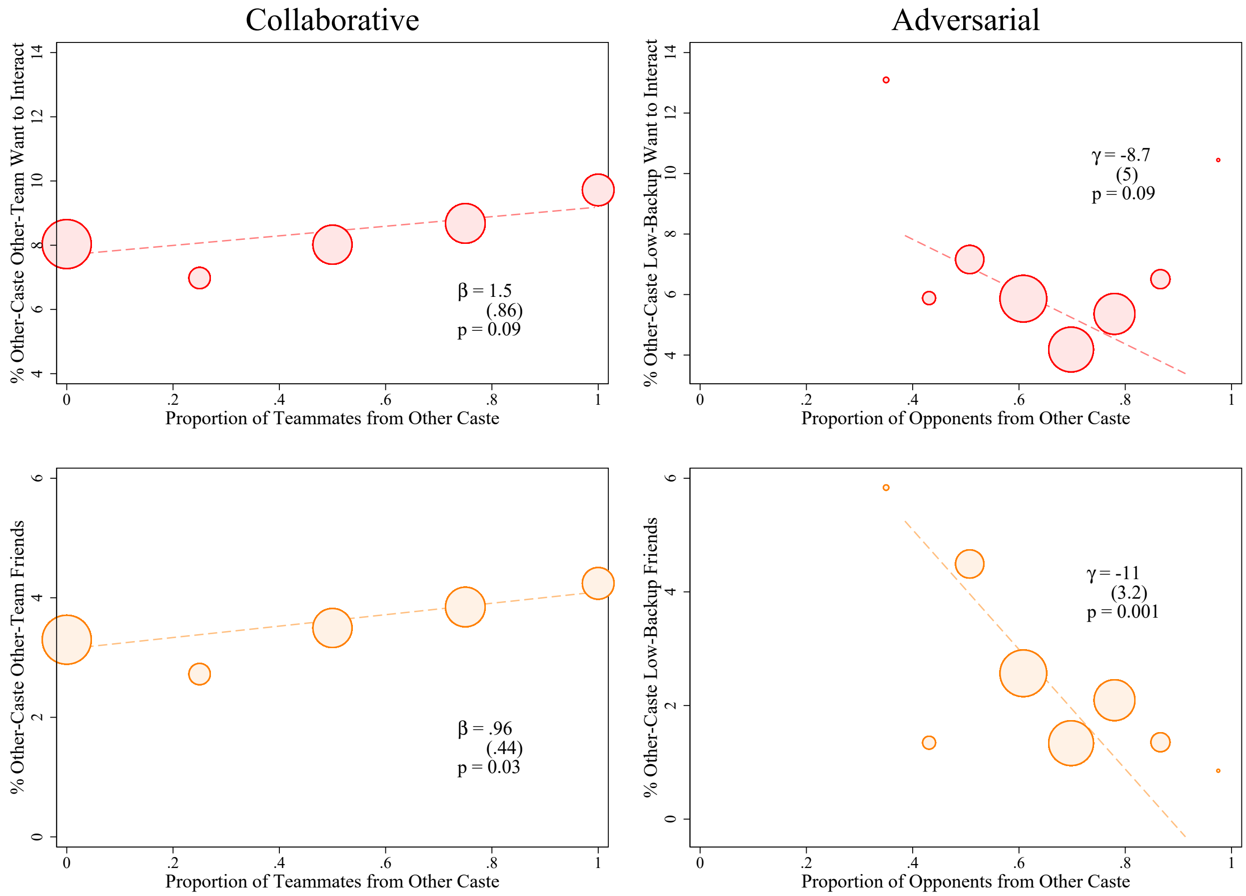
Notes: The white histogram shows the variation in the proportion of a player's teammates that belong to a different caste (collaborative contact). The blue histogram shows the variation in the proportion of a player's opponents that belong to a different caste (adversarial contact).

Figure 5: Contact and Social Interaction



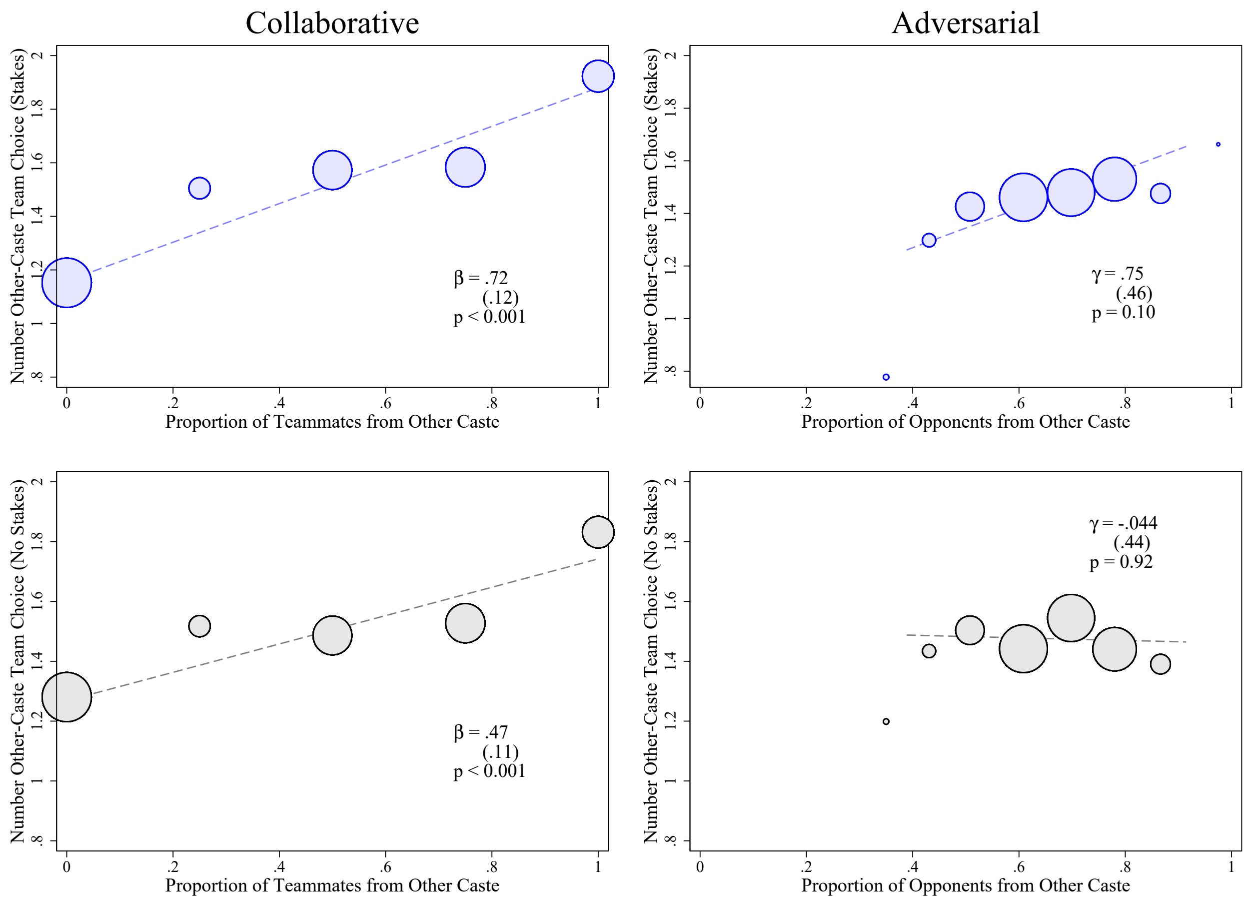
Notes: The three left-hand-side figures show the effects of collaborative contact. The estimated β from the main specification (equation 7) along with its standard error and associated p-value is shown in each figure, with a binned scatterplot showing the effects of contact across the support of the variable. The bins for collaborative contact are the five possible values (0, 0.25, 0.5, 0.75, and 1). The dashed line shows the linear fit from the main specification, with the size of the bubbles reflecting the sample size in each bin. The three right-hand-side figures show the effects of adversarial contact, with the estimates γ from the main specification shown. The bins for adversarial contact are ten percent bins, starting from 30-40%, with the bubbles centered on the mean Prop. Oth. Caste of Opponents within each bin. The top-panel outcome is the number of other-caste men the participant wants to spend more time with. The middle-panel outcome is the number of other-caste men the participant considers friends. The bottom-panel outcome is the number of other-caste men selected as those which the participant specifically does *not* want to spend more time with in future.

Figure 6: Effects of Contact Beyond Immediate Interactions



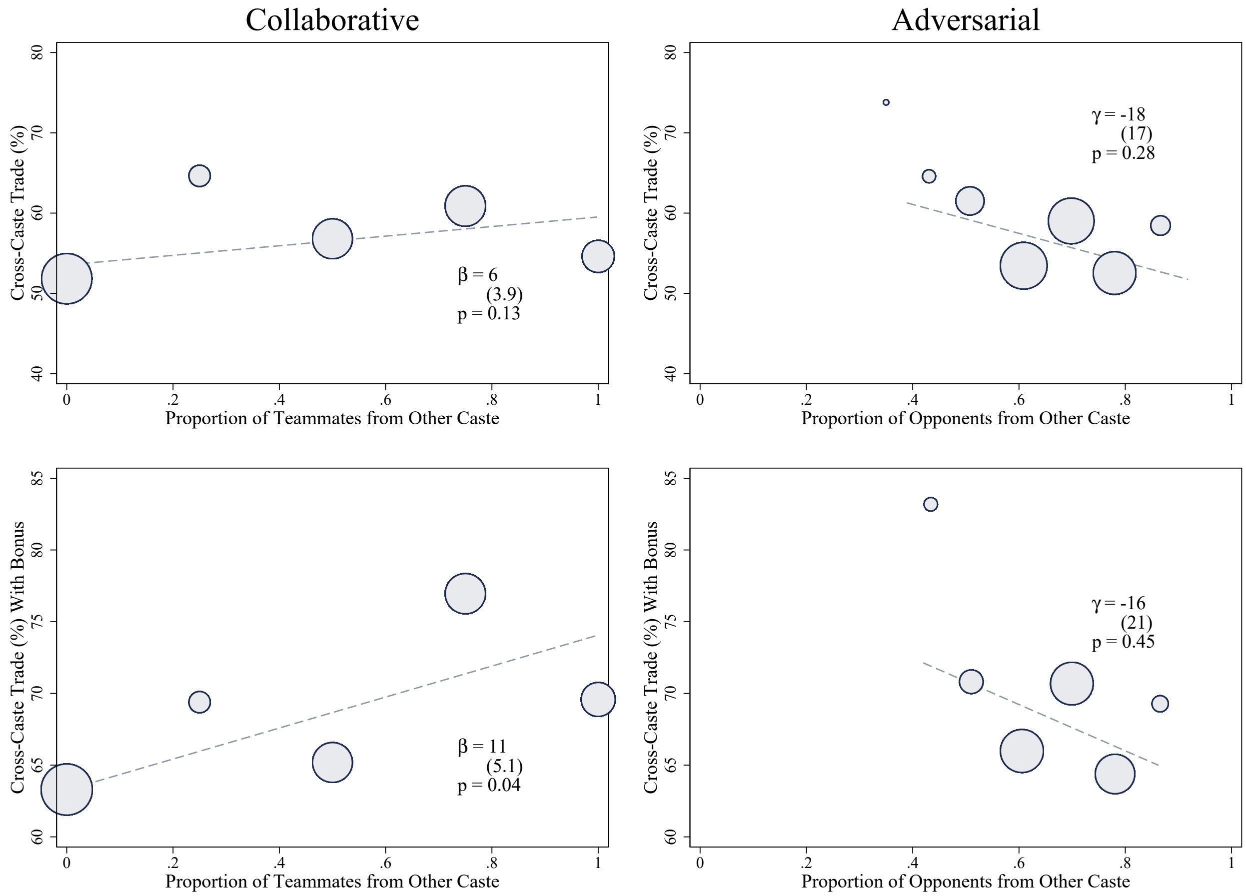
Notes: The [top-left/bottom-left] panel outcome is the percentage of other-caste men from among the other teams (in the same league) that the participant [wants to spend more time with/considers friends]. The [top-right/bottom-right] panel outcome is the percentage of other-caste men from among backups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

Figure 7: Contact and Team Formation



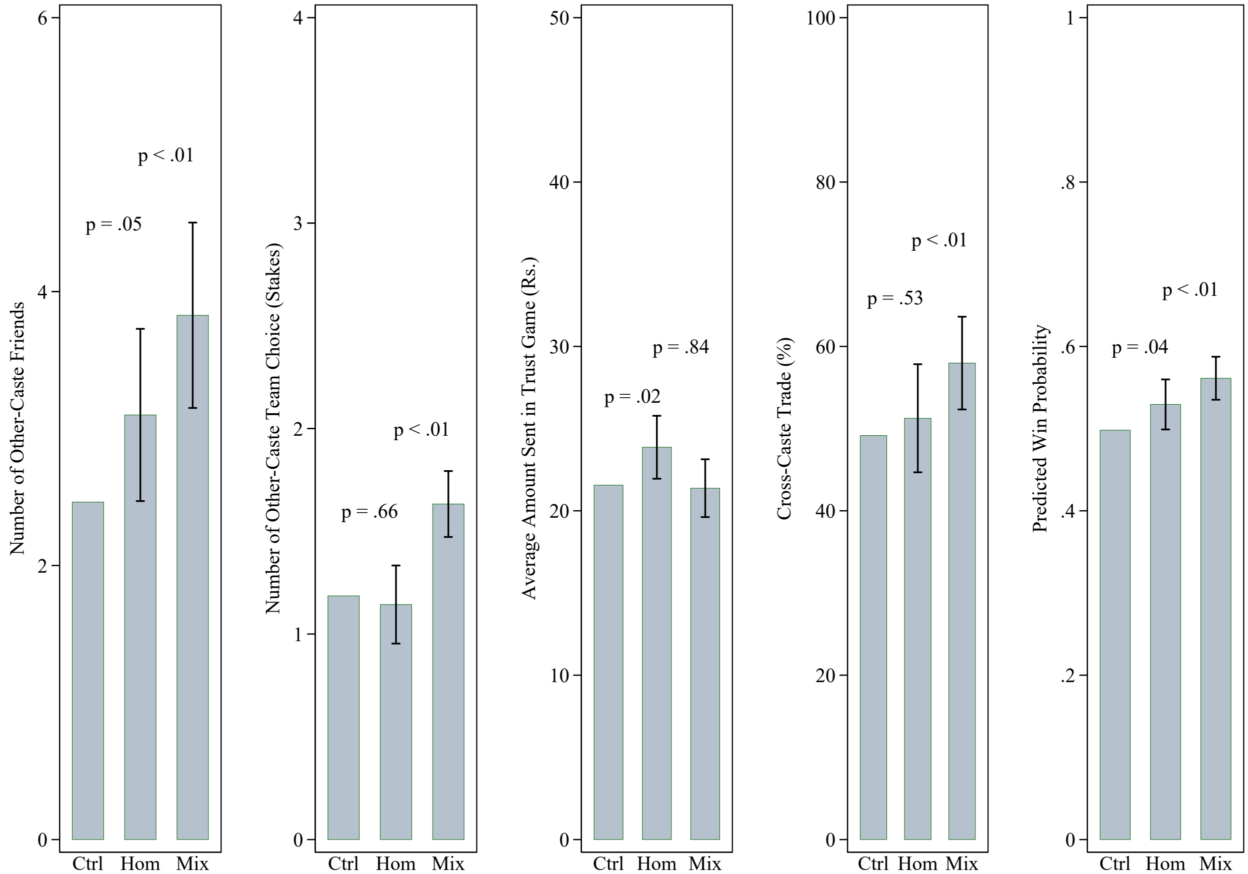
Notes: The top-panel outcome is the number of other-caste men (from zero to four) chosen as teammates for the future match with stakes. The bottom-panel outcome is the same, for the future match without stakes. To aid the comparison with common axes, the bottom-right figure does not display one outlying observation at (0.98,2.74).

Figure 8: Contact and Cross-Caste Trade



Notes: The outcome is the percentage of goods traded across caste. The top-panel shows the effects for the full sample. The bottom-panel shows the effects for those with a positive monetary incentive to switch sticker color (implicitly these are incentives for cross-caste trade). The figures are based on regressions that include Trade and Color-Switch bonus fixed effects – these are dummy variables for each type of monetary bonus received for trading. To aid the comparison with common axes, the top-right figure does not display one outlying observation at (0.98,5.21).

Figure 9: Effects of Program Participation



Notes: The estimates come from the specification:

$$y_{icl} = \alpha_{cl} + \beta_1 \text{Homog. Team}_{icl} + \beta_2 \text{Mixed Team}_{icl} + \beta_3 \text{High Backup}_{icl} + \varepsilon_{icl}$$

where α_{cl} are caste*league fixed effects. Homog. Team is equal to one if the participant is assigned to a homogeneous caste team, and zero otherwise. Mixed Team is equal to one if the participant is assigned to a team with at least one other-caste teammate, and zero otherwise. High Backup is equal to one if the participant is assigned to the control group with a priority number of one to three. Backups with a priority number of four and above are in the omitted category. The figure shows treatment effects of Homog. Team (Hom) and Mixed Team (Mix) relative to the omitted category (Ctrl). The p-values in the figure are for Hom vs. Ctrl ($\hat{\beta}_1$) and Mix vs. Ctrl ($\hat{\beta}_2$). Standard errors are clustered at the team-level for those assigned to play in the leagues, and at participant-level otherwise. For the cross-caste trade and trade payout outcomes, the regression additionally includes the Trade and Color-Switch Bonus dummy variables. From left-to-right the outcomes are: (1) number of other-caste men participant considers friends, (2) number of other-caste men chosen as teammates for future match with stakes, (3) average amount sent in the trust game to the three recipients, (4) percentage of cross-caste trade, and (5) predicted probability of winning future match with stakes (given team choice) from LASSO model with baseline characteristics, actual past match performance, and their squares.

B Tables

Table 1: Contact and Social Interaction

	Want to Interact w/ (1)	Friends (2)	Enemies (3)	Friends (4)
Panel A:	Number of Other-Caste Participants			
Prop. Oth. Caste on Team	2.43***	1.21***	-2.11	1.36***
	(0.76)	(0.40)	(2.40)	(0.43)
Prop. Oth. Caste of Opponents	-10.50***	-5.50*	-4.42	-5.08*
	(3.91)	(2.87)	(10.70)	(2.77)
Prop. Oth. Caste on Team*Oth. Caste Team Ability				0.40
				(0.69)
Prop. Oth. Caste of Opp.*Oth. Caste Opp. Ability				5.12
				(3.12)
Caste*League FE	Yes	Yes	Yes	Yes
Outcome Mean	7.9	3.5	14	3.5
Panel B:	Number of Own-Caste Participants			
Prop. Own Caste on Team	0.90	0.42	0.25	
	(0.57)	(0.43)	(0.97)	
Prop. Own Caste of Opponents	6.29**	3.76*	1.65	
	(2.75)	(2.20)	(4.74)	
Caste*League FE	Yes	Yes	Yes	
Outcome Mean	12	6.6	5.8	
Observations	770	770	573	770

Notes: Standard errors clustered at team-level. Column (1) outcome is number of other/own-caste participants the respondent wants to spend more time with. Column (2) and (4) outcome is number of other/own-caste participants the respondent considers friends. Column (3) outcome is number of other/own-caste participants the respondent specifically does not want to spend more time with. This outcome was collected from the third league onwards. Oth. Caste Team Ability is the average ability index across all other-caste players in a given player's team, where the ability index is the average across three standardized baseline ability measures: maximum bowling speed, number of 4s/6s when batting, and number of catches when fielding. Oth. Caste Opp. Ability is the average ability index across all other-caste opponents. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Effects of Contact Beyond Immediate Interactions

	Want to Interact w/ (1)	Friends (2)	Enemies (3)
Panel A:	% of Oth. Caste Oth. Team		
Prop. Oth. Caste on Team	1.49*	0.96**	-2.19
	(0.86)	(0.44)	(2.58)
Prop. Oth. Caste of Opponents	Yes	Yes	Yes
Caste*League FE	Yes	Yes	Yes
Outcome Mean	8.4	3.6	16
Panel B:	% of Oth. Caste Low Backups		
Prop. Oth. Caste of Opponents	-8.65*	-10.53***	-0.70
	(5.04)	(3.17)	(13.98)
Prop. Oth. Caste on Team	Yes	Yes	Yes
Caste*League FE	Yes	Yes	Yes
Outcome Mean	5.5	2.3	16
Observations	770	770	573

Notes: Standard errors clustered at team-level. Panel A outcomes are for generalization of collaborative contact effects. They are the percentage of other-caste men among other teams (in the same league) listed as those the participant wants to spend more time with (column (1)), considers friends (column (2)) or specifically does not want to spend time with (column (3)). The column (3) outcome was collected from the third league onwards. Panel B outcomes are for generalization of adversarial contact effects. They are the percentage of other-caste men among backups with priority seven or above selected for each question. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Friendships with Friends of Teammates

	Whether i lists j as... (=0/1)		
	Baseline Friend (1)	Want to Interact w/ (2)	Friend (3)
Teammate	0.01	0.24***	0.13***
	(0.007)	(0.020)	(0.016)
Friend of Oth. Caste Teammate	-0.0000	-0.0006	0.0008
	(0.004)	(0.004)	(0.004)
Observations	53540	52171	52171
Outcome Mean	.038	.08	.035
α_{jcl}*Prop. Oth. Caste on Team FE	Yes	Yes	Yes

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

Table 4: Contact and Team Formation

	Num. Other Castes for Team for Match with			
	Stakes (1)	No Stakes (2)	Stakes (3)	No Stakes (4)
Prop. Oth. Caste on Team	0.72***	0.47***	0.72***	0.49***
	(0.12)	(0.11)	(0.12)	(0.11)
Prop. Oth. Caste of Opponents	0.75	-0.04	0.77*	0.02
	(0.46)	(0.44)	(0.45)	(0.44)
Prop. Oth. Caste on Team*Oth. Caste Team Ability			0.32*	0.34*
			(0.19)	(0.19)
Prop. Oth. Caste of Opponents*Oth. Caste Opp. Ability			0.18	0.76
			(0.70)	(0.68)
Observations	768	768	768	768
Outcome Mean	1.5	1.5	1.5	1.5
Caste*League FE	Yes	Yes	Yes	Yes
Collaborative: p(Stakes = No Stakes)	.00053			
Adversarial: p(Stakes = No Stakes)	.0042			
p(Stakes has same effect on Collaborative and Adversarial)	.042			

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, where the ability index is the average across three standardized baseline ability measures: maximum bowling speed, number of 4s/6s when batting, and number of catches when fielding. Oth. Caste Opp. Ability is the average ability index across all other caste opponents. Tests for equality of the coefficients in columns (1) and (2) come from a pooled regression with an interaction term between each contact variable and whether the choice was for the match with stakes or not. The bottom row gives the p-value from a test that the effect of removing Stakes on the collaborative contact effect is equal to its effect on the adversarial contact effect. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Contact and Caste Favoritism in Voting

	Vote Rank = 1 to 5, where 5 is worst					
	Gen	OBC	SC/ST	All	All	Non-Opp
	(1)	(2)	(3)	(4)	(5)	(6)
Other 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)			
Oth. Caste Voted On*Prop. Oth. Caste on Team					-0.13*	-0.20*
					(0.07)	(0.11)
Oth. Caste Voted On*Prop. Oth. Caste of Opp.					0.22	-0.26
					(0.39)	(0.48)
Observations	3035	3200	2945	9180	9180	4570
Votee FE	No	No	No	Yes	Yes	Yes
Prop. Oth. Caste on Team	No	No	No	No	Yes	Yes
Prop. Oth. Caste of Opponents	No	No	No	No	Yes	Yes
Caste*League*Other Caste Voted On FE	No	No	No	No	Yes	Yes

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

Table 6: Contact and Trust

	Amount Sent in Trust Game (Rs. 0 to 50)					Stated Trust
	Gen	OBC	SC/ST	All	All	All
	(1)	(2)	(3)	(4)	(5)	(6)
Other Caste Recipient	-2.50***	-1.75**	0.45			
	(0.74)	(0.76)	(0.89)			
Oth. Caste Recip.*Prop. Oth. Caste on Team				0.22		
				(1.49)		
Oth. Caste Recip.*Prop. Oth. Caste of Opp.				-0.16		
				(5.55)		
Prop. Oth. Caste on Team					-2.97**	0.02
					(1.18)	(0.04)
Prop. Oth. Caste of Opponents					-8.03	-0.42**
					(5.66)	(0.18)
Observations	741	789	723	2253	2253	770
Sender FE	Yes	Yes	Yes	Yes	No	No
Age of Recipient	Yes	Yes	Yes	Yes	No	No
Caste*League*Other Caste Recipient FE	No	No	No	Yes	No	No
Caste*League FE	No	No	No	No	Yes	Yes
Outcome Mean	21.8	21.4	23.6	22.2	22.2	.205

Notes: The unit of observation is a sender-recipient pair. Senders are partnered with one General, one OBC, and one SC/ST recipient, such that there are three observations per sender. Robust standard errors for columns (1) to (3). Team-clustered standard errors for columns (4) to (6). Outcome in columns (1) to (5) is amount sent by sender to recipient in trust game. Column (1) includes sample of General caste senders, column (2) includes OBC senders, and column (3) includes SC/ST senders. Stated trust is a dummy variable coming from the question "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?". Stated trust equals one if the respondent answers "Most people can be trusted" and equals zero if the respondent answers "Need to be very careful". *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Contact and Team Quality

	Predicted win probability from LASSO model with			
	Baseline	Baseline Plus Interactions	Actual Performance	Actual Plus Interactions
	(1)	(2)	(3)	(4)
Prop. Oth. Caste on Team	.032*	.039**	.036**	.04**
	(.019)	(.018)	(.018)	(.017)
Prop. Oth. Caste of Opponents	-.017	.016	.0084	.03
	(.079)	(.073)	(.073)	(.071)
Caste*League FE	Yes	Yes	Yes	Yes
Out-of-sample % Correct Prediction	66.3	65.6	66.9	65.6
Observations	768	768	768	768
Number Available Predictors	140	4648	152	5372
Number Chosen Predictors	19	18	19	18

Notes: Standard errors clustered at team-level. Outcome variable is the predicted probability of winning the bonus match with stakes given the team actually chosen. The prediction comes from a linear model where LASSO is used to select the predictors of whether a team wins a given match. The set of possible predictors differ for each column. Column (1) considers only baseline characteristics and their squares, while column (2) also includes interactions between baseline characteristics. Column (3) includes baseline characteristics and actual past match performance and their squares. Column (4) includes these in addition to all possible interactions. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Contact and Trade

	Traded	Cross-Caste Trade		Trade Payout	Cross-Caste Trade	
	All	All	Bonus>0	Bonus>0	Bonus>0	Oth. Team, Bonus>0
	(1)	(2)	(3)	(4)	(5)	(6)
Prop. Oth. Caste on Team	0.01	0.06	0.11**	15.02**		
	(0.02)	(0.04)	(0.05)	(7.13)		
Prop. Oth. Caste of Opponents	-0.08	-0.18	-0.16	-21.13	-0.18	0.02
	(0.10)	(0.17)	(0.21)	(30.39)	(0.20)	(0.22)
Color Switch Bonus = 50	-0.00	0.22***				
	(0.02)	(0.04)				
Color Switch Bonus = 100	0.02	0.24***	0.03	86.22***	0.02	-0.04
	(0.02)	(0.04)	(0.04)	(6.48)	(0.04)	(0.04)
Prop. Oth. Caste on Team: General					0.30***	0.29***
					(0.10)	(0.10)
.... OBC					-0.04	-0.14
					(0.07)	(0.08)
.... SC/ST					0.10	0.07
					(0.09)	(0.08)
Caste*League FE	Yes	Yes	Yes	Yes	Yes	Yes
Trade Bonus Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1510	1510	760	760	760	760
Homog. Team Mean	.88	.52	.65	138	.65	.44

Notes: Standard errors clustered at team-level. The unit of observation is the participant-good, meaning there are two observations per participant. Columns (1) and (2) include all those assigned to play in the leagues. Columns (3) and (4) include only those assigned to the league that were also assigned a positive monetary incentive (Rs. 50 or Rs. 100) to switch the sticker color of their gifts. The outcome for column (1) is a dummy variable equal to one if the good was successfully traded. The outcome for columns (2) and (3) is a dummy variable equal to one if the good was successfully traded with someone from a different caste. The outcome for column (4) is the total payouts received for trading that good, including any successful trade or color-switching incentive. Trade Bonus Dummy is equal to one if the participant was assigned Rs. 20 for each successful trade, and zero if the participant was assigned Rs. 10 for each successful trade. Column (5) is equivalent to column (3), but with the collaborative contact effect fully interacted with the three caste group dummy variables. The outcome for column (6) is a dummy variable equal to one if the good was successfully traded with an other-caste player in one of the other teams in the league. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Competitive Incentives and Collaborative Contact

	N. Oth. Caste		Voting	Trust	Team Quality	Trade
	Friends	Team Choice Stakes	Rank (1-5)	Amount Sent	LASSO Predict	Cross-Caste
	(1)	(2)	(3)	(4)	(5)	(6)
Ind. Pay*Prop. Oth. Caste on Team	0.31	-0.04			0.01	-0.01
	(0.87)	(0.24)			(0.03)	(0.08)
Ind. Pay*Prop Oth. Caste on Team*Oth. Caste			0.11	1.29		
			(0.17)	(2.74)		
Observations	770	768	9180	2253	768	1510
Outcome Mean	3.5	1.5	3	22	.54	.56
Prop. Oth. Caste on Team	Yes	Yes	Yes	Yes	Yes	Yes
Prop. Oth. Caste of Opponents	Yes	Yes	Yes	Yes	Yes	Yes
Ind. Pay*Prop. Oth. Caste of Opponents	Yes	Yes	Yes	Yes	Yes	Yes
Ind. Pay*Prop. Oth. Caste on Team	Yes	Yes	Yes	Yes	Yes	Yes
Ind. Pay*Prop. Oth. Caste of Opponents*Oth. Caste	No	No	Yes	Yes	No	No
Prop. Oth. Caste of Opponents*Oth. Caste	No	No	Yes	Yes	No	No
Prop. Oth. Caste on Team*Oth. Caste	No	No	Yes	Yes	No	No
Caste*League*Ind. Pay FE	Yes	Yes	Yes	Yes	Yes	Yes
Caste*League*Ind. Pay*Oth. Caste FE	No	No	Yes	Yes	No	No
Ind. Pay*Votee FE	No	No	Yes	No	No	No
Ind. Pay*Age of Recipient	No	No	No	Yes	No	No
Sender FE	No	No	No	Yes	No	No
Trade and Color-Switch Bonus FE	No	No	No	No	No	Yes

Notes: Standard errors clustered at team-level. The unit of observation is the participant for columns (1), (2), and (5). The unit of observation is a voter-votee pair in column (3), a sender-recipient pair in column (4), and a participant-good in column (6). Ind. Pay is a dummy variable equal to one if the participant's team receives Individual Pay incentives. Oth. Caste is a dummy variable equal to one if the votee/recipient is not the same caste as the voter/sender for the voting and trust outcomes. The outcome for each column is: (1) number of other-caste men participant considers friends, (2) number of other-caste men participant chose as teammates for match with stakes, (3) vote rank given for field trip, (4) amount sent in trust game (Rs. 0 to 50), (5) predicted probability of winning future match with stakes (given team choice) from LASSO model with baseline characteristics, actual past match performance, and their squares, and (6) dummy variable equal to one if good was traded with someone from a different caste. The specifications in columns (3) and (4) parallel the previous specifications for voting and trust analysis, only with all terms fully interacted with Ind. Pay. Trade and Color-Switch Bonus FE are dummy variables for the participant's trading and color-switching incentives. *** p<0.01, ** p<0.05, * p<0.1.

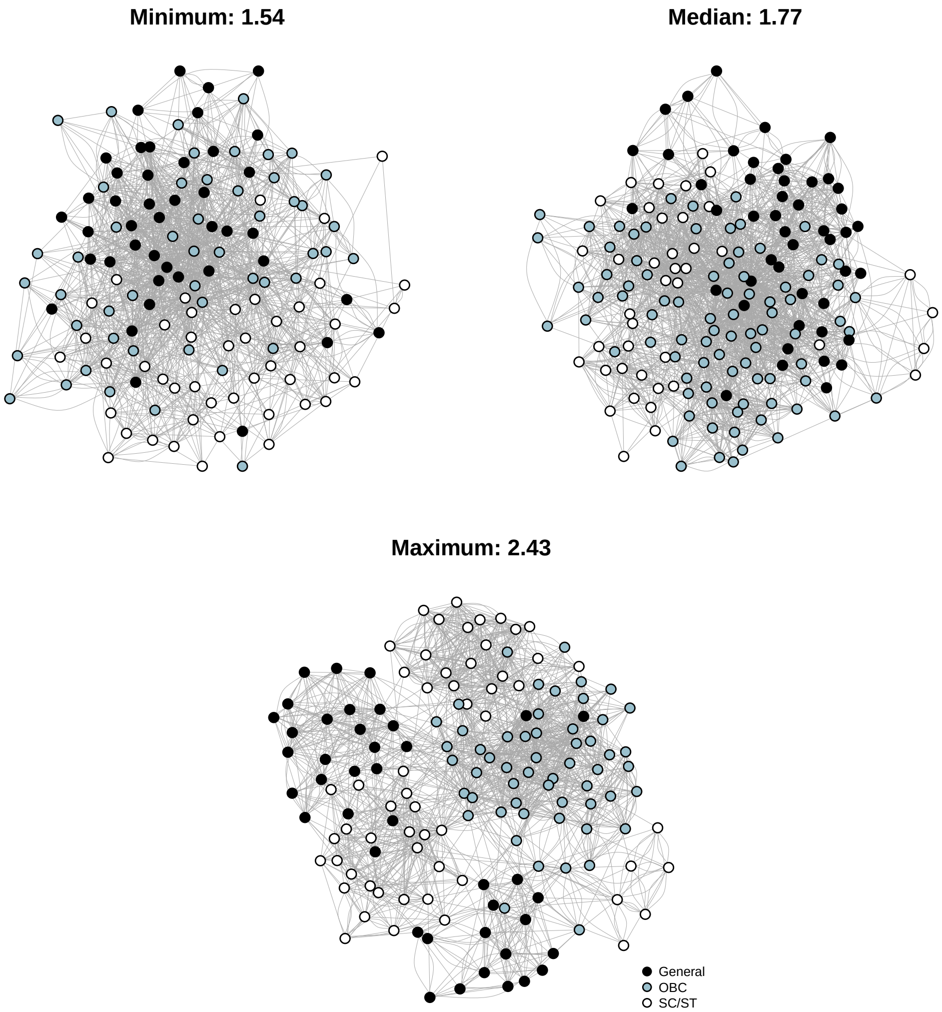
Table 10: Effects of Program Participation

	N. Oth. Caste		Trust	Team Quality	Trade	
	Friends	Team Choice Stakes	Avg. Sent	LASSO Predict	Cross-Caste	Payout
	(1)	(2)	(3)	(4)	(5)	(6)
Mixed Team	1.36***	0.45***	-0.19	0.06***	0.09***	12.00*
	(0.35)	(0.08)	(0.90)	(0.01)	(0.03)	(6.24)
Homog. Team	0.64**	-0.04	2.30**	0.03**	0.02	3.08
	(0.32)	(0.10)	(0.98)	(0.02)	(0.03)	(7.12)
High Backup	0.94*	0.19	0.40	0.04**	0.07*	4.70
	(0.50)	(0.11)	(1.27)	(0.02)	(0.04)	(9.15)
Control Mean	2.5	1.2	22	.5	.49	133
Control Standard Deviation	4.4	1.2	12	.2	.5	75
Observations	1211	1207	1178	1207	2368	1186
Caste*League FE	Yes	Yes	Yes	Yes	Yes	Yes
Trade and Switch Bonus FE	No	No	No	No	Yes	Yes
Sample	All	All	All	All	All	Switch Bonus>0

Standard errors clustered at team-level for those assigned to play in the leagues, otherwise participant-level. Homog. Team is equal to one if the participant is assigned to a homogeneous-caste team, and zero otherwise. Mixed Team is equal to one if the participant is assigned to a team with at least one other-caste teammate, and zero otherwise. High Backup is equal to one if the participant is assigned to the control group and has a priority number of 1 to 3. Backups with a priority number of 4 and above are the omitted category. The outcomes are: (1) number of other-caste men participant considers friends, (2) number of other-caste men chosen as teammates for future match with stakes, (3) average amount sent in the trust game to the three recipients, (4) predicted probability of winning future match with stakes (given team choice) from LASSO model with baseline characteristics, actual past match performance, and their squares, (5) whether good was traded with a different caste, and (6) total payouts received for trading that good, including any successful trade or color-switching incentive. *** p<0.01, ** p<0.05, * p<0.1.

C Supplementary Appendix

Figure C1: Social Networks and Caste-based Homophily at Baseline



Notes: The social network is visualized for the three locations (out of eight) with the minimum, median, and maximum caste-based homophily. The homophily measure reflects how prevalent baseline friendships are between same-caste pairs compared to pairs in the network overall, with a measure above one reflecting that friendships are more likely to be formed within groups than across groups. The lowest caste-based homophily measure is 1.54, the median is 1.77, and the maximum is 2.43. Each network displays only nodes that completed the baseline social network survey (1,251 of 1,261 participants). The question asked was: "Now we have finalized the sign-up for all players in the league, I would like to ask one more question. Here are the photos of all the sign-ups. As I scroll through slowly, please can you tell me which of these people you consider to be friends?" Each network is undirected, with an edge between two nodes if either (or both) participant(s) listed the other as a friend. Each network is drawn using a force-directed algorithm (Kamada Kawai) that puts nodes closer together that have a smaller length of the shortest path between them.

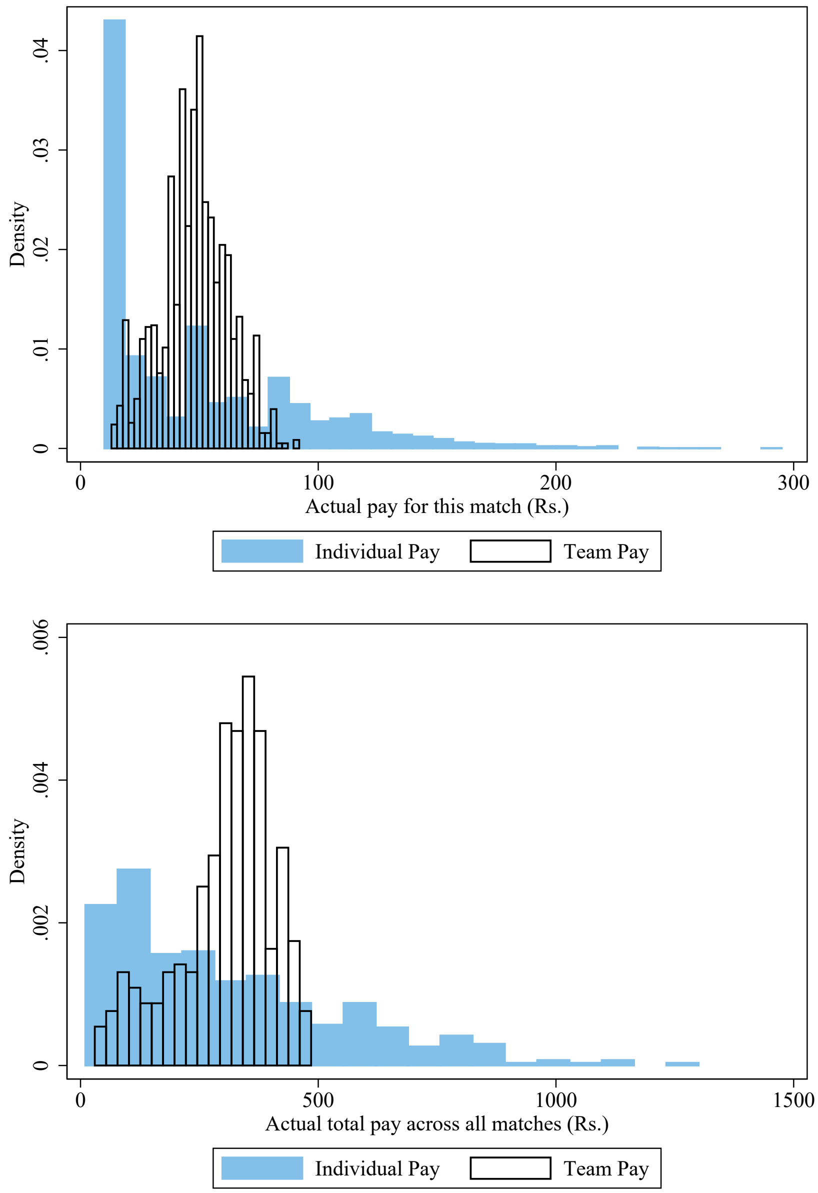
Figure C2: Recruitment Poster

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

Figure C3: League Table

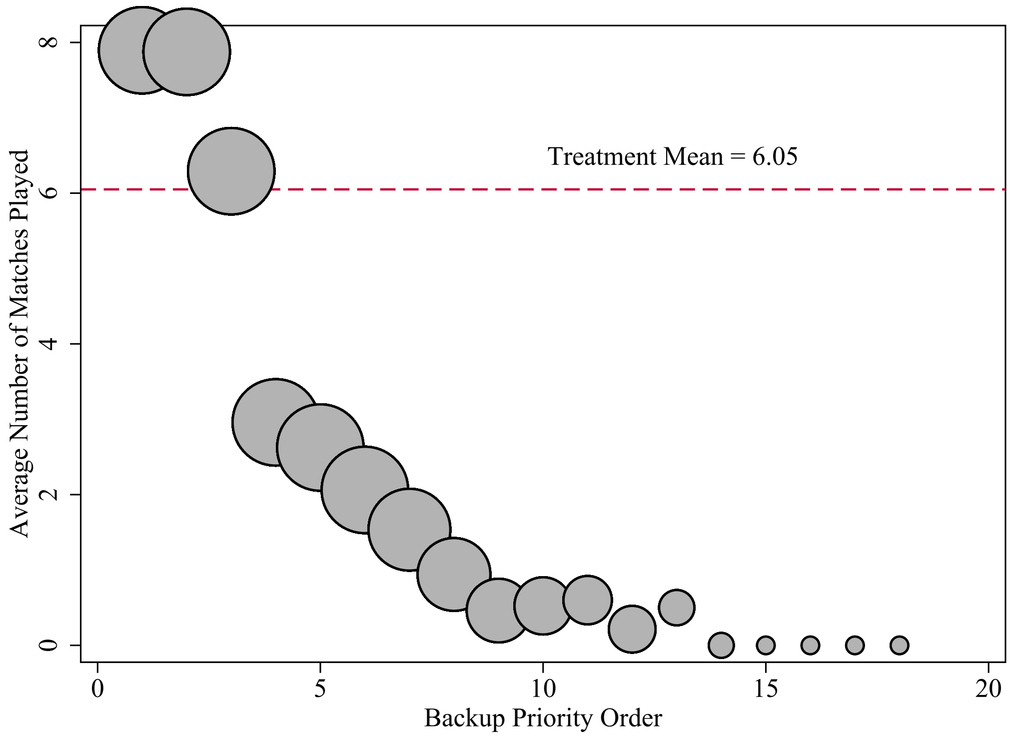
Notes: Example league table after 36 of 80 matches had been played. NRR is net run rate (used to settle ties between two teams with the same number of points). The location is blurred out for confidentiality reasons. Each team chose their own team name – for example, team 2T17, made up of five SC/ST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

Figure C4: Payout Distributions



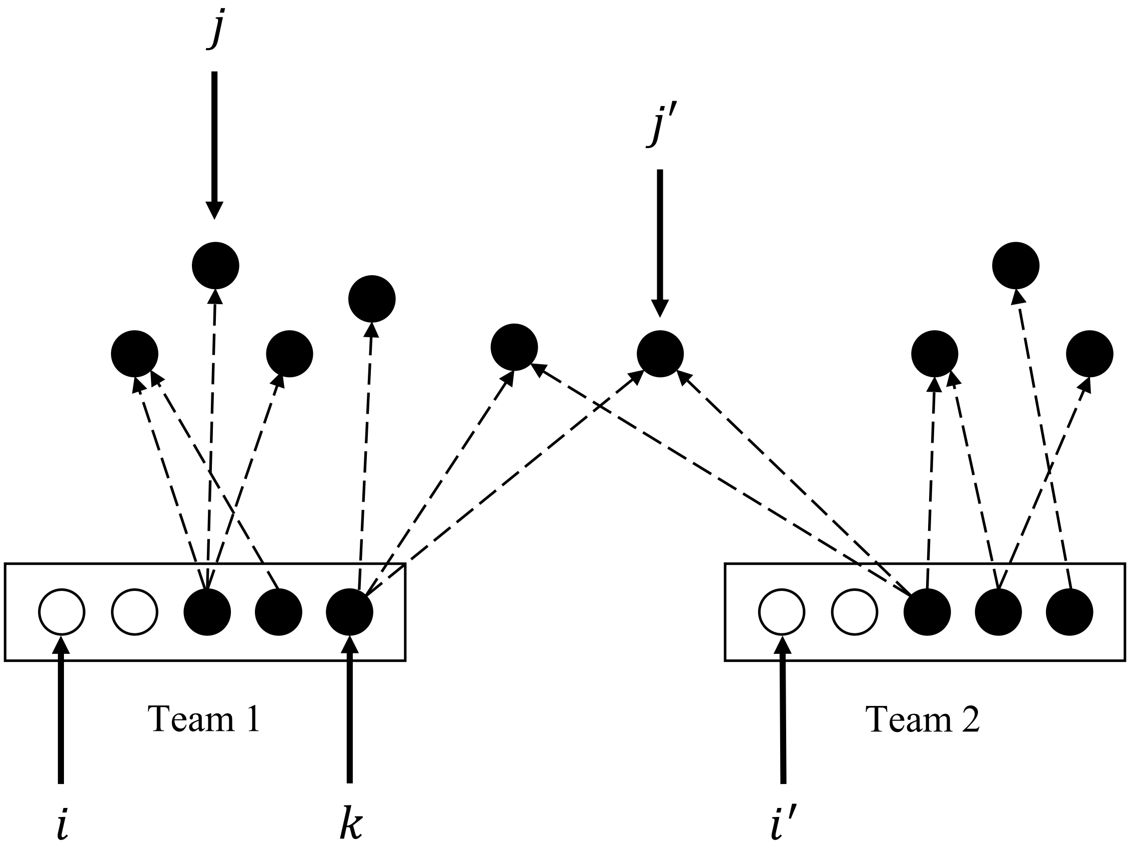
Notes: The top-panel shows histograms of actual incentive pay at the individual-match level, including the Rs. 10 incentive received by everyone for each match. The blue histogram is for those in teams assigned to Individual Pay and the transparent histogram is for those in teams assigned to Team Pay.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

Figure C5: Number of Matches Played by Backups



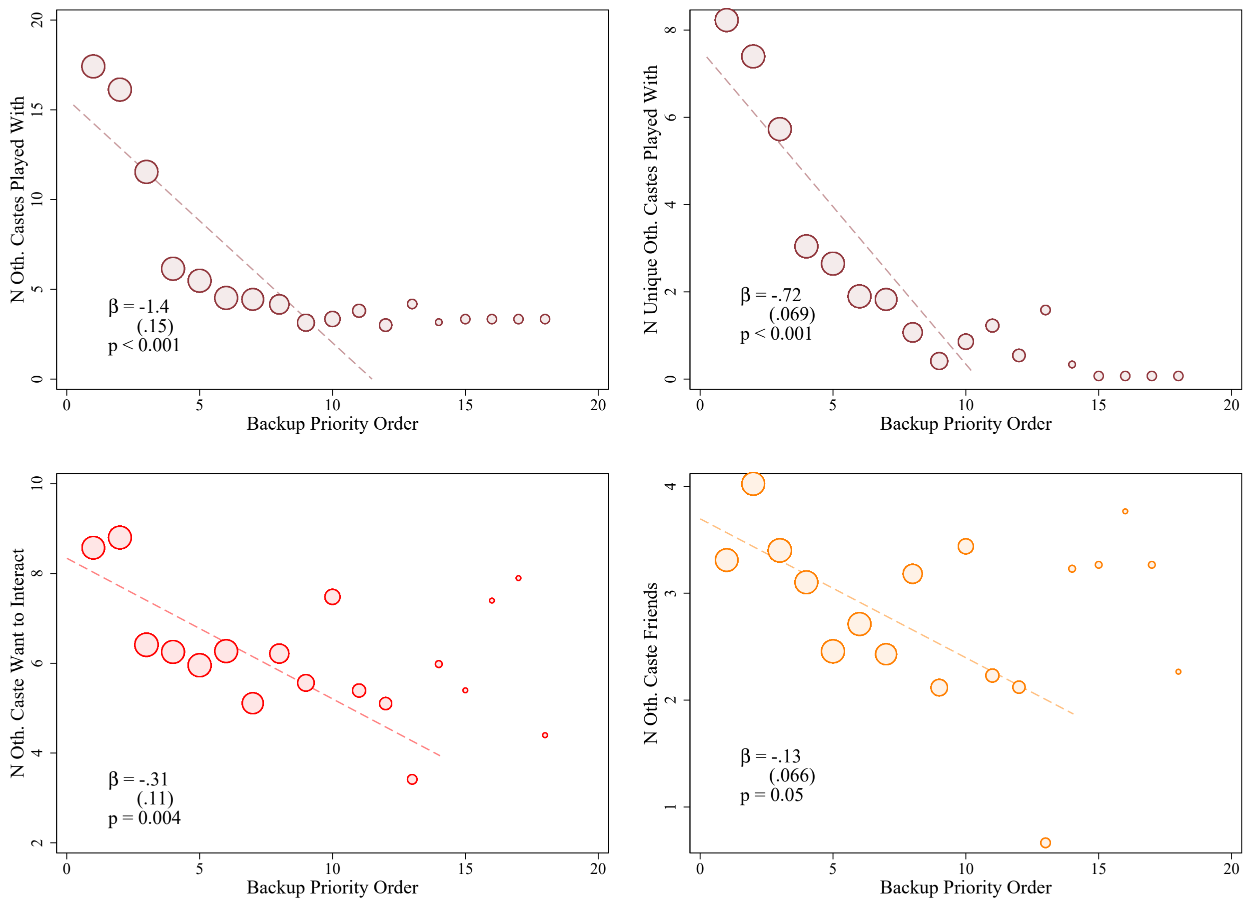
Notes: This is a scatter plot of the average number of matches played against the backup priority number, with the size of the bubbles reflecting the sample size for each priority number. The dashed line is the average number of matches played by the 800 participants assigned to play in the leagues.

Figure C6: Friends of Friends of Teammates – Identification Intuition



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

Figure C7: Short-Term Collaborative Contact and Cross-Caste Friendships



Notes: The estimates come from the specification:

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

where Backup Priority$_{icl} \in \{1, 2, ..., 18\}$ and robust standard errors are used. The outcome for the top-left panel is the total number of other-caste men played on a team with (including double-counting when the same other-caste is played with multiple times). The outcome for the top-right panel is the total number of unique other-caste men played on a team with (with no double-counting). The outcome for the bottom-left panel is the number of other-caste men the participant wants to spend more time with. The outcome for the bottom-right panel is the number of other-caste men the participant considers friends.

Table C1: Randomization Checks

	N. Oth. Caste Friends (1)	Worked Last Year (2)	Played Last Tournament (3)	N. Catches (4)	Age (5)	Max. Bowling Speed (6)	Would Volunteer (7)	In School (8)
				Panel A: Full Sample				
Prop. Oth. Caste on Team	0.40	-0.01	0.02	0.15	0.10	0.84	0.04	0.02
	(0.41)	(0.03)	(0.04)	(0.11)	(0.33)	(0.82)	(0.04)	(0.04)
Prop. Oth. Caste of Opponents	-3.52	0.04	0.18	-0.23	2.60**	3.16	-0.00	-0.28*
	(2.77)	(0.14)	(0.15)	(0.50)	(1.31)	(4.77)	(0.20)	(0.16)
Caste*League FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	795	800	800	800	800	800	800	800
				Panel B: Analysis Sample - Completed All Outcomes				
Prop. Oth. Caste on Team	0.36	-0.02	0.03	0.12	0.14	0.87	0.03	0.01
	(0.46)	(0.03)	(0.04)	(0.12)	(0.34)	(0.84)	(0.05)	(0.04)
Prop. Oth. Caste of Opponents	-3.77	0.07	0.21	-0.30	2.82**	1.64	0.03	-0.34**
	(2.93)	(0.14)	(0.14)	(0.52)	(1.34)	(4.86)	(0.19)	(0.16)
Caste*League FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	745	746	746	746	746	746	746	746
Full Sample Outcome Mean	3.8	.17	.18	5	18	87	.43	.77
Used for re-randomization			No			Yes		

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

Table C2: Attrition and Attendance

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

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

Table C3: Collaborative Contact Effects on Opponents vs. Non-Opponents

	Want to Interact w/ (1)	Friends (2)	Enemies (3)
Panel A:	% of Oth. Caste Opponents		
Prop. Oth. Caste on Team	0.75	0.43	-1.86
	(0.96)	(0.48)	(2.52)
Prop. Oth. Caste of Opponents	Yes	Yes	Yes
Caste*League FE	Yes	Yes	Yes
Outcome Mean	8.3	3.6	15
Panel B:	% of Oth. Caste Non-Opponents		
Prop. Oth. Caste on Team	2.07**	1.35***	-2.47
	(0.94)	(0.51)	(2.71)
Prop. Oth. Caste of Opponents	Yes	Yes	Yes
Caste*League FE	Yes	Yes	Yes
Outcome Mean	8.4	3.6	17
Observations	770	770	573

Notes: Standard errors clustered at team-level. Panel A outcomes are the percentage of other-caste men among opponent teams selected for each question. Panel B outcomes are the percentage of other-caste men among non-opponent teams selected for each question. The Enemies outcome was collected from the third league onwards. *** p<0.01, ** p<0.05, * p<0.1.

Table C4: Team and Opponent Interactions During Matches

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

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

Table C5: Effects of Contact on Team Formation Beyond Immediate Interactions

	Team Choice for Match with	
	Stakes	No Stakes
	(1)	(2)
Panel A:	% of Other Castes from Other Teams	
Prop. Oth. Caste on Team	0.36**	0.11
	(0.16)	(0.16)
Prop. Oth. Caste of Opponents	Yes	Yes
Outcome Mean	1.5	1.5
Panel B:	% of Other Castes among Low Backups	
Prop. Oth. Caste of Opponents	-0.44	-0.34
	(1.38)	(2.17)
	No	No
Prop. Oth. Caste on Team	Yes	Yes
Outcome Mean	.92	1.2
Caste*League FE	Yes	Yes
Observations	768	

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

Table C6: Voting for Friends vs. Same Caste

	Vote Rank = 1 to 5		
	All	All	Non-Opp
	(1)	(2)	(3)
Other Caste Voted On	0.31***		
	(0.03)		
Baseline Friend	-0.83***	-0.89***	-0.63
	(0.05)	(0.33)	(0.47)
Oth. Caste Voted On*Prop. Oth. Caste on Team		-0.11	-0.14
		(0.08)	(0.12)
Oth. Caste Voted On*Prop. Oth. Caste of Opp.		0.16	-0.20
		(0.38)	(0.48)
Baseline Friend*Prop. Oth. Caste on Team		-0.06	0.06
		(0.13)	(0.18)
Baseline Friend*Prop. Oth. Caste of Opp.		0.14	-0.34
		(0.48)	(0.67)
Observations	9100	9100	4539
Votee FE	Yes	Yes	Yes
Prop. Oth. Caste on Team	No	Yes	Yes
Prop. Oth. Caste of Opponents	No	Yes	Yes
Caste*League*Other Caste Voted On FE	No	Yes	Yes

Notes: The unit of observation is a voter-votee pair. Voter-clustered standard errors for column (1). Team of voter-clustered standard errors for columns (2) and (3). All columns exclude votes for teams with players only of the same caste of the voter or players only of other castes. Baseline Friend is a dummy variable equal to one if the voter listed the votee as a friend at baseline. Votee fixed effects can be included because the same person can be voted on by multiple voters. Column (3) only includes votes made on teams that were not faced as opponents during the league. *** p<0.01, ** p<0.05, * p<0.1.

Table C7: Ability Priors and Mistakes

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

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

Table C8: Trading with Teammates and Friends of Teammates

	Whether i trades with j (=0/1)			
	All	Gen	OBC	SC/ST
	(1)	(2)	(3)	(4)
Teammate	-0.001	0.004	0.002	-0.011
	(0.009)	(0.011)	(0.019)	(0.013)
Friend of Oth. Caste Teammate	0.002	0.004	0.004	-0.002
	(0.003)	(0.005)	(0.004)	(0.005)
Observations	25482	9132	8270	8080
Outcome Mean	.013	.013	.014	.012
α_{jcl}*Prop. Oth. Caste on Team FE	Yes	Yes	Yes	Yes

Notes: Unit of observation is i-j dyad (pair of individuals). Sample includes only dyads where (1) i is assigned to a mixed team, (2) i is given a positive monetary incentive to switch the sticker color of his gifts, and (3) j is not the same caste as i. Columns (2)-(4) restrict to the subsamples of i's belonging to General, OBC, and SC/ST castes. Standard errors are dyadic-robust at team-level. Teammate is a dummy variable equal to one if i and j are teammates. Friend of Oth. Caste Teammate is a dummy variable equal to one if j is listed at baseline as a friend of any of i's other caste teammates. α_{jcl} is set of Caste*League (of i) fixed effects fully interacted with person j fixed effects. α_{jcl} is then fully interacted with the categories of Prop. Oth. Caste on Team (0.25, 0.5, 0.75, and 1, since the sample is only those i's assigned to mixed teams). *** p<0.01, ** p<0.05, * p<0.1.

Table C9: Competitive Incentives and Interactions During Matches

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

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

Table C10: Effects of Contact on Winning

	Number of Matches Won	
	Participant (1)	Team (2)
Prop. Oth. Caste on Team	-0.01	
	(0.33)	
Prop. Oth. Caste of Opponents	0.10	
	(0.99)	
Mixed Team		-0.17
		(0.29)
Team Ability Index		2.52***
		(0.47)
Outcome Mean	3	4
Caste*League FE	Yes	Yes
Observations	800	160

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

Table C11: Caste, Ability, and Payouts

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

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

Table C12: Discrimination in Within-Team Allocation

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

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

Table C13: Caste Heterogeneity of Contact Effects (I)

	N. Oth. Caste		Team Quality	Trade	
	Friends	Team Choice Stakes	LASSO Predict	Cross-Caste	Payout
	(1)	(2)	(3)	(4)	(5)
Prop. Oth. Caste on Team: General	1.64**	0.67***	0.05*	0.19***	30.81**
	(0.65)	(0.17)	(0.03)	(0.07)	(14.61)
.... OBC	0.46	0.59***	0.02	-0.03	-5.16
	(0.74)	(0.19)	(0.03)	(0.06)	(9.54)
.... SC/ST	1.50**	0.90***	0.04	0.03	22.57
	(0.62)	(0.22)	(0.03)	(0.07)	(13.67)
p(Gen = OBC)	.24	.74	.46	.017	.044
p(OBC = SC/ST)	.28	.29	.74	.55	.099
p(Gen = SC/ST)	.88	.4	.71	.095	.68
Prop. Oth. Caste of Opponents: General	-13.03*	0.18	0.19*	-0.09	-22.02
	(7.10)	(0.82)	(0.11)	(0.27)	(50.84)
.... OBC	2.66	0.99	-0.12	0.06	-47.20
	(3.09)	(0.79)	(0.12)	(0.28)	(44.92)
.... SC/ST	-5.86**	1.06	-0.05	-0.52*	-7.82
	(2.40)	(0.86)	(0.15)	(0.28)	(60.65)
p(Gen = OBC)	.04	.5	.062	.69	.73
p(OBC = SC/ST)	.029	.95	.69	.15	.6
p(Gen = SC/ST)	.36	.47	.22	.26	.86
Observations	770	768	768	1510	760
Caste*League FE	Yes	Yes	Yes	Yes	Yes
Trade and Color-Switch Bonus FE	No	No	No	Yes	Yes
Sample	All	All	All	All	Switch Bonus>0

Notes: Standard errors clustered at team-level. Each column corresponds to one regression, with the outcome regressed on the two contact regressors fully interacted with the three caste group dummy variables. The unit of observation is the participant for columns (1) to (3), and the participant-good for columns (4) and (5). The outcomes for each column are: (1) number of other-caste men participant considers friends, (2) number of other-caste men participant chose as teammates for future match with stakes, (3) predicted probability of winning future match with stakes (given team choice) from LASSO model with baseline characteristics, actual past match performance, and their squares, (4) dummy variable equal to one if good was traded with someone from a different caste, and (5) total payouts received for trading that good, including any successful trade or color-switching incentive. Trade and Color-Switch Bonus FE are dummy variables for the participant's trading and color-switching incentives. *** p<0.01, ** p<0.05, * p<0.1.

Table C14: Caste Heterogeneity of Contact Effects (II)

	Voting	Trust
	Rank (1-5)	Amount Sent
	(1)	(2)
Oth. Caste*Prop. Oth. Caste on Team: General	0.01	2.13
	(0.14)	(2.03)
.... OBC	-0.28**	0.41
	(0.12)	(2.52)
.... SC/ST	-0.09	-1.75
	(0.14)	(2.80)
p(Gen = OBC)	.14	.59
p(OBC = SC/ST)	.31	.56
p(Gen = SC/ST)	.59	.26
Oth. Caste*Prop. Oth. Caste of Opponents: General	0.61	6.34
	(0.66)	(9.24)
.... OBC	1.01**	2.29
	(0.48)	(9.41)
.... SC/ST	-1.03	-9.18
	(0.66)	(11.17)
p(Gen = OBC)	.55	.76
p(OBC = SC/ST)	.016	.44
p(Gen = SC/ST)	.077	.3
Observations	9180	2253
Caste*League*Other Caste Recipient FE	Yes	Yes
Prop. Oth. Caste on Team*Caste FE	Yes	No
Prop. Oth. Caste of Opponents*Caste FE	Yes	No
Votee FE	Yes	No
Sender FE	No	Yes
Age of Recipient	No	Yes

Notes: Standard errors clustered at team-level. Each column corresponds to one regression, and shows the effect of each type of contact on caste favoritism (in voting and trust) separately for each caste. The unit of observation is a voter-votee pair for column (1), and a sender-recipient pair in column (2). The outcome in column (1) is the vote rank given for the field trip. The outcome in column (2) is the amount sent in the trust game (from Rs. 0 to 50). *** p<0.01, ** p<0.05, * p<0.1.

C.1 Model Proofs

Endogenous Signal Structure

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

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

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

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

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

since $\phi_1 > 0$.

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

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

from the working above.

Posteriors – No Attribution Bias

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

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

Posteriors – Attribution Bias

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

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

Expected posteriors now depend on the type of contact (equation 4):

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

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

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

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

Probability of Selecting Friends – No Attribution Bias

To see equation 5, note that the cutoff c can fall into five relevant regions:

1. $0 \leq c < \tilde{\rho}_{01}$: $P[\tilde{\rho} > c \mid m = 0] = P[\tilde{\rho} > c \mid m = 1] = 1$

2. $\tilde{\rho}_{01} \leq c < \tilde{\rho}_{00}$: $P[\tilde{\rho} > c \mid m = 0] > P[\tilde{\rho} > c \mid m = 1]$

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

4. $\tilde{\rho}_{11} \leq c < \tilde{\rho}_{10}$: $P[\tilde{\rho} > c \mid m = 0] > P[\tilde{\rho} > c \mid m = 1]$

5. $\tilde{\rho}_{10} \leq c \leq 1$: $P[\tilde{\rho} > c \mid m = 0] = P[\tilde{\rho} > c \mid m = 1] = 0$

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

Probability of Selecting Friends – Attribution Bias

To see equation 6, note that there are now only three relevant regions for the cutoff:

1. $0 \leq c < \tilde{\rho}_0^b$: $P[\tilde{\rho} > c \mid m = 0] = P[\tilde{\rho} > c \mid m = 1] = 1$

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

3. $\tilde{\rho}_1^b \leq c \leq 1$: $P[\tilde{\rho} > c \mid m = 0] = P[\tilde{\rho} > c \mid m = 1] = 0$

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

C.2 Randomization Details

Re-randomization

Only caste was used for stratification when randomly assigning participants to the leagues and teams. To avoid other chance imbalances, I re-randomized, following Banerjee et al. (2017). I ran the full randomization 100 times, selecting the run with the minimum maximum t-statistic from a series of balance checks on age, maximum bowling speed, total 4s/6s during the batting test, whether would volunteer, and whether attend school.

Match Schedule Generation

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

1. Start with a set of 20 nodes. Create a set of 20*8 = 160 points, associating each set of 8 points with one of the nodes (teams).

2. Choose two points randomly and pair them.

3. If these two points are associated with the same team (= team playing itself) or are already connected (= teams already assigned to play one another), go back to 2.

4. Add an edge (fixture) between the two teams these points are associated with.

5. Remove the two points that have now been successfully paired.

6. If any points are left, go back to 2. and continue pairing.

7. If no points are left, exit. Create adjacency matrix from the resultant team pairings.

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

C.3 LASSO Variable Selection Details

The linear regression model is:

$$Win_{iotl} = \alpha + \beta' \left(X_{i,t-1,l} - X_{o,t-1,l} \right) + \varepsilon_{iotl}$$

where observations are at the match-level, Win_{iotl} is a dummy variable equal to one if team i beat team o in match $t \in \{1,2,...,60\}$ in league $l \in \{1,2,...,8\}$. For each match I choose randomly which team is i and which is o (without loss of generality). I drop 5 matches that ended in a draw, leaving 475 matches in the training sample and 155 matches in the hold-out sample.

The vectors of regressors are each defined as differences between the pre-determined characteristics of team i and team o. For example, $x_{i,t-1,l}$ could be the best (or second best, or mean, or square of the mean etc.) baseline batting ability score among the players in team i. What matters for whether team i wins the match is not the level of these characteristics, but the difference between these characteristics and the equivalent characteristic for the opposing team. Since teams utilize different backup players at different times, there is variation across different matches even for a given assigned team.

The four sets of predictors considered are:

1. *Baseline*: this set of predictors come only from baseline data. I include all the predictors below, as well as their squares (140 variables in total).

   (a) For each of the following ability measures, I include the score for the 1st/2nd/3rd/4th/5th best in the team, as well as the mean score across all team members: number of hits when batting (out of 6), number of 4s/6s when batting (out of 6), maximum bowling speed (km/h), median bowling speed (km/h), number of wickets when bowling (out of 6), number of valid balls when bowling (out of 6), number of wide balls when bowling (out of 6), number of catches when fielding (out of 6), and number of wickets when fielding (out of 6).

   (b) Age of the oldest/2nd oldest/3rd oldest/4th oldest/5th oldest in the team, as well as the mean.

   (c) How regularly each player plays cricket, from most/2nd most/etc., as well as the mean.

   (d) Number of General/OBC/SC/ST castes on team.

   (e) Whether played in previous tournament in area (mean).

2. *Baseline Plus Interactions:* this includes all the predictors from (1), plus all possible one-way interactions (e.g. the interaction of number of General castes on team with the maximum bowling speed of the fastest bowler on the team) (4648 variables in total).

3. *Actual Performance*: this includes all the predictors from (1), plus the following measures of actual past league performance, missingness dummy variables, and their squares (152 variables in total).

   (a) Cumulative strike rate (total runs/total balls faced) across all previous matches played by any members of this team.

   (b) Cumulative economy rate (total runs conceded/(total balls bowled/6)) across all previous matches played by any members of this team.

   (c) Cumulative wicket rate (total wickets taken/total balls bowled) across all previous matches played by any members of this team.

   (d) Indicator for missing data for (a)-(c) (e.g. first match of league will have no data because no player has played any matches yet).

4. *Actual Plus Interactions*: this includes all the predictors from (3), plus all possible one-way interactions (5372 variables in total).