Creating Cohesive Communities:

A Youth Camp Experiment in India*

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Abstract

Non-family-based institutions for socializing young people may play a vital role in creating close-knit, inclusive communities. We study the potential for youth camps—integrating rituals, sports, and civics training—to strengthen intergroup cohesion. We randomly assigned 412 Hindu and Muslim adolescent boys, from West Bengal, India, either to two-week camps or to a pure control arm. To isolate mechanisms, we cross-randomized collective rituals (such as singing the national anthem, wearing uniforms, chanting support during matches, and dancing synchronously), and the intensity of intergroup contact. We find that camps reduce ingroup bias, increase willingness to interact with outgroup members, and enhance psychological well-being. Different camp elements account for these positive effects. However, against expectations, rituals boosted well-being for the Hindu majority group but had no impact on intergroup relations, while intergroup contact backfired, particularly for the majority. Our findings demonstrate that inclusive youth camps may be a powerful tool for bridging deep social divides. But we also highlight the challenges in crafting optimal integrative camps that benefit all groups.

*This project was pre-registered in the AEA RCT Registry (AEARCTR-0010661), and approved by UBC's Behavioural Research Ethics Board (H22-02671) and UCSD's Institutional Review Board (Protocol #805748). We are grateful for funding from the J-PAL Governance Initiative, the Institute on Behavior and Inequality (briq), a UCSD Hellman Fellowship, and a UCSD Yankelovich Fellowship. We thank Poushali Mohanta and Rahul Paul for outstanding research assistance. For comments and advice, we thank Gaurav Khanna and Jeff Weaver, as well as seminar participants at Oxford, UBC, USC, University of Copenhagen, and briq.

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1 Introduction

A large portion of childhood development occurs outside the household, in spaces that bring together young people of roughly the same age, but from different families. In some cases, the explicit goal of such institutions is to encourage children to build bonds, learn social skills, and imbibe norms. In other cases, socialization is a byproduct of having children mix together and engage in structured activities. Although they take on myriad forms, these agents of socialization are similar in harnessing tools of peer interaction, persuasion and indoctrination, and public ceremonies and rituals. For example, public education systems play a key role not only in human capital formation, but in molding values and transmitting social expectations (Alan et al., 2021; Bandiera et al., 2019; Paglayan, 2022). The scouting movement is estimated to have over 50 million members worldwide, while residential summer programs serve 26 million American children each year.[1] Ethnic groups in Eastern Africa initiate minors into "age sets," leading individuals to feel a greater sense of obligation to their cohort than their kin (Moscona and Seck, 2022). A rite of passage for teenage boys on Nias Island, Indonesia involves joint training for the *hombo batu*: jumping over a two-meter high monolith to signal courage and commitment to the tribe (Fitri and Purba, 2023). By shaping children's behaviors, emotions, and patterns of interacting with others, these non-family-based institutions may be crucial for forging close-knit communities and integrated nations.

To what extent can youth socialization programs give rise to inclusive behaviors and worldviews in adolescents? What components of these multi-faceted interventions matter most in the socialization process? We study the potential for youth camps to strengthen intergroup cohesion in deeply divided societies. Youth camps are strikingly common and have long been used to shape children's moral character, often at vast scale. By the 1980s, the Soviet Union hosted 10 million children each year in Young Pioneers and Komsomol camps whose goal was to "educate the fearless, brave, joyful fighters" to the cause of Marxist-Leninism (Grzybowski, 2017, 72). Sports camps were central to childhood experiences in Mussolini's Italy (Vescovi, 2004); at the camps of the Opera Nazionale Balilla, "enthusiasm runs high ... and [an] atmosphere of religious devotion to the revolution prevails" (Cox, 1935, 269).[2] Youth camps have also been used to promote integration. The European Union has invested over €8 million in the T:BUC Programme in Northern Ireland; its camps "are about challenging historic positions, encouraging debate and discussion and providing a way for young people to ... build longer term relationships" that span sectarian cleavages.[3]

[1] "World organization of the scout movement," shorturl.at/hpzP2; and "Why diversity, equity, and inclusion matters at camp," American Camp Association, shorturl.at/BDHXY.

[2] Infamously, a "Hitler Youth generation" was forged in the weekend camps of Nazi Germany, with 90% of the country's children enrolled in the Hitlerjugend organization by the start of the Second World War.

[3] Executive Office of Northern Irleand, "T:BUC Camps Programme," shorturl.at/ceCD7. Further, according to a 2004 survey, 39% of teenagers in the United States had attended a religious summer camp (Smith and Denton, 2009, 54).

Several features of youth camps make them a promising means of reducing bias and fostering social togetherness. First, camps showcase collective rituals—synchronized and repeated actions or speech, frequently imbued with symbolism—which classic work in sociology suggests are conducive to building shared identity among participants (Durkheim, 1912; Turner, 1977; Henrich, 2020). Despite this, we know of no experimental attempts to use rituals to reduce group divisions in the wild. Second, ethnically mixed camps bring children into close collaborative contact with ethnic outgroups, which can improve intergroup relations (Allport, 1954; Lowe, 2021). Third, programmatic content at camps can be tailored to advance inclusive ideas. Such content can be delivered persuasively in front of captive audiences of impressionable young people. In principle, therefore, youth camps offer a potent mix of remedies for mending social rifts.

We implemented a randomized controlled trial in West Bengal, India that leverages each of the key elements of camps just described, and parses their relative contributions. Reflecting an India-wide trend, religious tensions between Hindus and Muslims have been increasingly strained in the district of our study. We randomly assigned 412 boys aged 13 to 18, from low-income Hindu and Muslim families, to one of two 12-day camps featuring team sports, lectures and discussions on democracy and diversity, and other fun activities, or to a pure control arm. One of the two camps also incorporated ritual elements borrowed from real-world camps—such as singing the national anthem, reciting a pledge, wearing uniforms, chanting support during sports matches, and dancing in unison. The camps were intensive for participants. Each camp provided a total of 48 hours of activities, organized by a team of 28 dedicated staff, recruited via a rigorous selection process. Compliance was high, with camp attendance averaging 87%. We administered a follow-up survey four to seven weeks after the camps had concluded, with a focus on social preferences, willingness to interact with outgroup members, national self-identification, prejudicial and political attitudes, and psychological well-being.

We begin by considering the overall effects of children's participation in the camps vis-à-vis the pure control. For the two families of outcomes looking at behaviors, we find strong evidence that camps improved Hindu-Muslim relations. Campers score 0.19σ higher on our index of prosocial preferences ($p < 0.01$), primarily capturing a 0.28σ reduction in ingroup bias in donations to strangers in a pair of dictator games. Campers also score 0.3σ higher on our index of willingness to interact with the religious outgroup ($p < 0.01$). Breaking that result down, we see that camps more than double the number of enduring outgroup friendships. Control participants have only one in 25 outgroup friends, even though one in three of their classmates are outgroup members. Camps further boost children's enthusiasm to engage with outgroup strangers. We organized a later social event billed as "an hour or two playing board games and other activities" with one other boy. Willingness to pay to attend the event with a stranger bearing an outgroup-sounding name—elicited using the Becker–DeGroot–Marschak method—was 43%

higher among participants assigned to a camp instead of the control arm. Taken alongside the dictator game result, this demonstrates that the camps induced positive behaviors toward members of religious outgroups who had nothing to do with the study itself.

Beyond social preferences and behaviors, the camps were psychologically beneficial. Campers report being happier, less depressed on a standard PHQ-8 scale, and more satisfied with their social lives than non-campers at endline—amounting to a 0.18σ improvement on a well-being index summarizing those three components ($p < 0.01$). These effects are noteworthy since the camps were not set up to target well-being specifically, nor did we organize any events between the last day of the camps and the two-month endline. With that said, the psychological effects are heterogeneous: they are larger for Hindus than Muslims (0.25σ versus 0.06σ, $p = 0.12$ for the estimated difference). The divergent effects of camp rituals explain this heterogeneity, as we discuss below.

We find no measurable impacts of camps on self-reported attitudes. 22% of control participants say that they would be willing to marry an outgroup member when they are older, while 73% say that they would support giving citizenship to an outgroup member. Camp assignment did not affect these views, nor did it shift approval of polarizing politicians, attitudes towards foreigners from countries where outgroups are in the majority, or beliefs about the value of democracy as a system of government. Additionally, the camps did not make children more likely to embrace a composite national identity, as measured by both a self-reported and incentivized choice of national over religious identity. While the camps improve everyday social relations, they do not alter young people's more abstract perceptions of an outgroup or their sense of nationhood.[4] This finding is consonant with recent prejudice-reduction interventions shown to shape behaviors but not attitudes (Paluck et al. 2021).

Camps are bundled interventions. To isolate mechanisms, we cross-randomized collective rituals (across the two camp arms), and the intensity of intergroup contact, through random assignment to teams with five Hindus and five Muslims, or teams with eight Hindus and two Muslims (the former being high-contact for Hindus, the latter being high-contact for Muslims). We exploit quasi-experimental variation in individuals' day-to-day attendance to understand the impact of the civic education modules. Three sets of findings emerge from these additional analyses.

First, boys in the ritual camp expressed greater shared identity with other campers (relative to those in the regular camp), as well as more pride, and more excitement—but only while the camps were ongoing, and only in the second week of the camps. Perhaps more importantly, rituals boosted the well-being of Hindus but not Muslims at endline ($p = 0.06$ for the estimated difference in effects), fully explaining the heterogeneous well-being effects of the camps by religion. As a possible explanation, we find evidence that rituals rouse the majority group (Hindus) but alienate the

[4]To test for the overall effects of the camps, and to allay multiple hypothesis testing concerns, we carry out an omnibus randomization inference-based test (Young, 2018), restricting only to the outcome indexes for the four families of outcomes we pre-specified as primary outcomes (social preferences, willingness to interact, national identity, and attitudes). We confidently reject the null hypothesis that neither of the camps had an impact on these four outcome indexes ($p = 0.004$).

minority (Muslims). In particular, rituals increase Hindus' self-reported quality of social life and their "willingness to do anything for the campers," but reduce camp attendance for Muslims. However, while rituals evoked different emotional reactions from campers, they did not change intergroup behaviors. Endline outcomes in the ritual camp are statistically indistinguishable from those in the regular camp for all four outcome indexes concerning intergroup relations. Thus, the panoply of rituals that are so characteristic of youth socialization efforts do not account for camps' unifying effects in our experiment, a result that goes against findings on rituals' (mostly short-term) impacts in psychology and anthropology (reviewed in Xygalatas 2022).

Second, intergroup contact appears to explain the camps' effects on outgroup friendships at endline, but not effects on other behaviors. In fact, there are signs that greater contact backfired. Campers assigned to teams with more outgroup members are less likely to endorse an inclusive national identity than those assigned to low-contact teams (-0.21σ, $p = 0.03$). High contact also reduces willingness to interact with an outgroup stranger (-0.22σ, $p = 0.07$). The negative contact effects we observe—the first of their kind in the literature on collaborative contact (Paluck et al. 2018; Clochard 2022)—are driven by Hindus. Consistently, we also see Hindu-Muslim heterogeneity in the effects of high outgroup exposure *during* the camp. Whereas Hindus tend to report more negative experiences and have lower camp attendance when in high-contact teams, Muslims, if anything, report more positive experiences. In this setting, the minority are happier with integration than the majority, a finding that we did not predict in advance.[5]

Lastly, while rituals bolstered psychological well-being among the majority-group participants and contact led to more outgroup friendships, we find suggestive evidence that programmatic content matters most for social preferences. Since the daily activities schedule was not announced to campers in advance, an individual's presence or absence on lecture days, conditional on overall attendance, is unlikely to reflect self-selection—a claim we corroborate with balance checks. We find that attending an additional lecture day substantially increases scores on the social preference index, indicating that the camp curriculum helped convince campers of the merits of social inclusion.

Camps, in summary, are potent along some dimensions: they mold social preferences, increase willingness to interact with outgroup members, and increase psychological well-being. We document that at least three features of camps contribute to these treatment effects. Yet, the impact of these features varies by religion: rituals strengthen well-being effects for the majority but not the minority, while contact backfires more for the majority than the minority. The results underscore the challenges in running optimal integrative camps that benefit all groups, and may help to explain the success of exclusionary nationalistic movements – in particular, our results suggest that a

[5]To explore whether the negative effects of contact can be muted by other features of camps, we test for complementarity between intergroup contact and rituals. We find a positive interaction effect for social preferences, though little evidence of an interaction effect for the specific outcomes negatively affected by contact. Thus, collective rituals appear to somewhat mediate the effects of contact, though without overturning the negative effects.

Hindu-only camp would be more effective at building the national identity of Hindus than a mixed Hindu-Muslim camp.

We make several contributions. First, we estimate and unbundle the effects of a key tool for youth socialization outside of the family: youth camps. Related work focuses on the value-shaping effects of new curricula in schools (Cantoni et al., 2017; Alan et al., 2021; Dhar et al., 2022), though not the value-shaping effects of schooling relative to no schooling. Our focus on camps is also inspired by an older tradition in social psychology that uses camps and clubs to understand intergroup relations. Lewin et al. (1939) manipulate the "social climate" of groups of children and measures aggressive behavior, while Sherif (1956) uses teams formed as part of summer camps to show that collaborating to solve common problems reduces intergroup conflict. While these papers compare outcomes among campers, we know of no experimental test comparing intergroup relations of campers with those of *non-campers*.[6][7]

Second, we investigate the effects of rituals on real-world intergroup relations for the first time, inspired by correlational and lab experimental claims that rituals promote prosociality, cooperation, and shared identity (Sosis and Ruffle 2003; Wiltermuth and Heath 2009; Xygalatas et al. 2013; Chwe 2013). Our ritual treatment is highly intensive, including 20 distinct rituals, with some having symbolic meaning, and with many repeated daily. In line with non-experimental evidence on the positive effects of extreme rituals on well-being (Xygalatas et al. 2019), we find evidence of positive effects of rituals on psychological well-being for the majority Hindu group. Nevertheless, rituals do not affect intergroup relations over and above the effects of the regular camp. While we find secular rituals to have limited effects, it may be that religious rituals are more potent. For example, Butinda et al. (2023) show that traditional African spells change the risk of theft perceived by beer sellers in the Democratic Republic of Congo, leading them to make higher profits.

Third, we make one optimistic and one pessimistic contribution to the literature on intergroup contact. On the optimistic side, where recent contact studies have found limited or no "generalized" effects on behaviors towards outgroup strangers (Scacco and Warren (2018); Mousa (2020), though see Rao (2019) for large generalized effects), our camps' effects on social preferences and willingness to interact generalize. On the more pessimistic side, we are the first to find negative effects of additional collaborative intergroup contact (Paluck et al., 2018; Clochard, 2022), revising downward somewhat our confidence in team-based contact interventions.[8] We also contribute methodolog-

[6] Using a difference-in-differences design, Ditlmann and Samii (2016) find mixed effects of a Jewish and Arab-Palestinian sports-only peace camp on attitudes. Green and Wong (2009) experimentally test how participation in racially heterogeneous (compared to racially homogenous) wilderness expedition groups affects intergroup tolerance, but, like Mousa (2020), do not include a pure control group. Lowe (2021) includes a pure control group, but estimates effects of a cricket league, rather than a multi-faceted youth camp.

[7] Beyond camps, we connect to evidence that cross-ethnic, national integration can be achieved through shared experiences (Depetris-Chauvin et al., 2020), interregional contact (Okunogbe, 2023; Bagues and Roth, 2023), radio propaganda (Blouin and Mukand, 2019), and state-led language and education policies (Miguel, 2004; Carlitz et al., 2022).

[8] Enos (2014) finds negative effects of exposure to people speaking a different language on Boston commuter trains, but such contact is not collaborative.

ically by characterizing the contemporaneous emotional effects of contact for the first time.

2 Context

Hindu-Muslim relations in India. 80% of India's population identify as Hindu and 14% identify as Muslim. Deep-rooted social divisions exist between the two groups. Muslims are subject to discrimination (Gaikwad and Nellis, 2021), and lag behind Hindus in literacy, consumption, housing, access to credit, and social mobility (Government of India, 2006; Asher et al., 2023). Muslims live disproportionately in India's towns and cities, where they are residentially segregated and victims of periodic communal conflict (Adukia et al., 2022; Wilkinson, 2006). Hindu-Muslim divisions have been documented to lower firm output (Ghosh, 2022). The rise of the Bharatiya Janata Party (BJP) in national politics from 2014 onward has coincided with an increase in hate speech and vigilante attacks against Muslims (Jaffrelot, 2021).

Study site. Our experiment took place in Barasat, a city in West Bengal, eastern India. Three sets of social and political facts about the region helped motivate the decision to field the intervention there.

First, the district in which the city is located—North 24 Parganas—has experienced worsening intergroup relations in recent years (Nath and Chowdhury, 2019). The district borders Bangladesh, and group tensions have been exacerbated by nativist perceptions that the regional ruling party, the Trinamool Congress (TMC), gives preferential treatment to Muslim migrants who enter India illegally (Chakrabarty and Jha, 2022). Since 2010, there has been a series of Hindu-Muslim riots, leading to internet shutdowns to curb violence. Young men have figured prominently in the clashes.[9] These localized incidents mirror increasing intolerance in West Bengal as a whole. In a large attitudinal survey carried out in 2019–20, 44% of Hindu and Muslim respondents in the state said they would be unwilling to accept a neighbor from the other religion, 96% reported that all or most of their friends shared the same religious background as themselves, and 93% considered communal violence to be a "very big" or "moderately big" problem (Appendix Figure S1).

Second, Hindu nationalist organizations have grown rapidly in North 24 Parganas, anecdotally contributing to—and benefiting from—declining social trust there. The average vote share received by BJP candidates in races for the West Bengal state assembly nearly quadrupled between 2016 and 2021 (Appendix Figure S2, Panel (i)). Affiliates of the Hindu nationalist family of organizations, the *Sangh Parivar*, advocate a muscular version of Hinduism and have

[9]For instance, disturbances broke out across North 24 Parganas in 2017 after a 17-year-old student wrote a viral Facebook post regarded as insulting to Islam (Purakayastha, 2018). "Bands of youngsters" responded with mob violence ("West Bengal: Communal riots break out in North 24 Parganas after controversial Facebook post," *Scroll.in*, July 4, 2017, shorturl.at/mRX34). This created "fertile ground for religious polarisation" (Amit Bhardwaj, "How the ghost of the Baduria-Basirhat communal riots is polarising polls in West Bengal," *Caravan Magazine*, April 16, 2021).

led provocative processions through Muslim neighborhoods during religious festivals (Roy, 2017). The expansion of the Hindu right is especially "visible in the [lower-caste] SC, ST and OBC dominated areas of South and North 24 Parganas" where "the Sangh Parivar campaigns against the Left and the Trinamool, accusing them of pursuing minority appeasement policies" (Kanungo, 2015, 65).

Third, the religious demography of North 24 Parganas is quite representative of West Bengal overall. About 26% of the district's population is Muslim (Appendix Figure S2, Panel (iii)). It is the most populous district of West Bengal and forms part of Kolkata's surrounding industrial belt, with 58% of residents designated as urban.[10] Poverty is relatively low, with 10% of the district population being classified as multidimensionally poor (compared to the West Bengal average of 28%; see Appendix Figure S2, Panel (ii)), although we recruited from low-income neighborhoods within Barasat.

Youth camps, and ideas of India. Camps have been central to elite attempts to instill both inclusive and exclusionary ideologies among India's young people.

Daily camps—*shakhas*, or "branches"—form the backbone of the Rashtriya Swayamsevak Sangh (RSS), a militant Hindu nationalist movement of five million members, founded in 1925.[11] *Shakhas* are early-morning training sessions held in parade grounds across the country. The gatherings, which are male-only,[12] were conceived as "the preeminent site through which to cultivate virtuous Hindus," using both intellectual (*baudhik*) and physical (*sharirik*) instruction (Valiani, 2010, 78). *Shakhas* consist of 5 to 100 participants, disproportionately secondary school and college students.[13] Symbolism and collective rituals abound. Sessions begin with the hoisting of the *bhagwa dhwaj*, a double pennant saffron flag. Attendees wear identical uniforms. "[E]xercises are done in unison, under the command of a drill leader who barks out orders ... The point of the RSS drill is to discipline through coordinated movement" (Alter, 1994, 565 and 576). In the words of one observer, "[m]any apolitical boys are first attracted to the shakhas because of the many games, sports and exercises that form the daily ritual, and are then slowly politicised ... into the ideology of Hindutva."[14] Prominent BJP politicians, including the current Prime Minister, Home Minister, and Defense Minister of India, all attended *shakhas* in their youth.

At the other end of the political spectrum is the Popular Front of India (PFI), a militant Islamist organization.[15] Established in 2006, its declared mission was to empower those facing socio-economic, political, and cultural de-

[10] Census of India 2011, District Census Handbook, North Twenty Four Parganas, bit.ly/3KeGOL4.

[11] Lauren Frayer and Furkan Latif Khan, "The powerful group shaping the rise Of Hindu nationalism In India," *National Public Radio*, shorturl.at/glw18.

[12] The RSS has a "sister" organization, the Rashtra Sevika Samiti, for women.

[13] Participants break out into smaller groups (*gata*) according to age. Each *gata* is assigned a *gatanayak* and a *shishak* (teacher), who arrange games and lead discussions on Hindu nationalist ideas and doctrine, "a regular feature of the shakha" (Andersen and Damle, 1987, 85).

[14] Manini Chatterjee, "Repackaging the RSS," *Indian Express*, March 16, 2003.

[15] "PFI ban: What is the Popular Front of India and why has India outlawed it?" *BBC News*, September 28, 2022.

privation. But it was also seen as an extremist group set up to counter the RSS.[16] The PFI recruited boys aged 15 and above. Like the RSS, participants wore uniforms and performed drills in public spaces.[17] Allegations that the PFI was organizing camps that radicalized Muslim youth culminated the group's being banned by the Government of India.[18]

Youth camps have also been used to foster pluralism. During the Nehruvian era—in the 1950s and early 1960s—the Planning Commission, under the auspices of the Bharat Sevak Samaj (BSS), organized over 10,000 camps focused on village development and eradicating "the bias of caste, creed, religion and untouchability" (quoted in Wilkinson, 2023, 67–8). Unlike RSS *shakhas*, these camps brought young volunteers into sustained, collaborative contact with unfamiliar outgroups, through joint labor on public works and social welfare schemes. India's first president inaugurated one BSS camp by exhorting campers to "develop a broad outlook at a liberal attitude" (Wilkinson, 2023, 68). Contemporary camps have followed this lead. The National Cadet Corps (NCC), which runs *Ek Bharat, Shreshtha Bharat* ("National Integration Camp"), aims to build camaraderie between young Indians from different geographic regions. A Chennai-based NGO, Pudiyador, operates *Bridging the Gaps*, which draws together groups of about 120 children between the ages of 12 and 16 from varied social backgrounds. Activities at their residential camps include an ultimate frisbee league, plus "workshops in art, movement, gender awareness and teamwork"; coaches "work hard to build a strong team identity irrespective of [cultural or language] barriers," creating t-shirts with diversity-affirming logos and team names.[19]

In short, youth camps have been a significant part of India's socio-political, cultural, and national evolution, speaking to the naturalism of our intervention.

3 Experiment design and intervention

3.1 Sampling

The recruitment stage took one month to complete. Enumerators went door-to-door in low-income wards in Barasat municipality soliciting interest from households. Recruiters provided families with information about the study and the camps, which were advertised as extra-curricular youth camps featuring sports and civic education. We highlighted that the camps would be free of charge to participants and would be held during the upcoming school holidays. To be eligible to participate, potential subjects had to be male and between the ages of 13 and 18. We

[16] Bismee Taskin, "Eighteen & disenchanted—why a college student, now a successful Delhi lawyer, joined PFI," *Print*, September 29, 2022.

[17] Ramesh Babu, "The story of Popular Front of India and reason behind its growth," *The Hindu*, Jan 29, 2020.

[18] "India bans Muslim group PFI for alleged 'terror' links," *Al Jazeera*, September 28, 2002

[19] Shweta Padmanaban, "Bridging the gaps ... through ultimate frisbee," *Medium*, June 6, 2015, shorturl.at/cjORT.

limited recruitment to one child per household to avoid spillovers. To ensure high outgroup exposure for Hindu camp participants, we oversampled Muslims relative to their local population share (34% in the sample versus 26% in the district). If a boy wished to take part in the camps and his parents agreed, the child and one parent were asked to give informed consent, and to complete a baseline survey. The consent script stated that the purpose of the study was "to understand how participating in youth activities camps shapes the behaviors and attitudes of male adolescents." To avoid researcher demand effects, we did not mention the study's focus on inter-religious group relations. Finally, to screen boys on commitment to actually attend the camps, the end of the baseline survey instructed the boy to come to a specific location on a specific day, accompanied by a parent, to finalize their enrollment. The 412 boys who attended one of these "randomization days" were then entered into the final sample to be randomized.

Summary statistics. 75% of parents report a monthly household income between Rs. 5,000 and 15,000, or roughly 2 to 6 USD per day. Muslim and Hindu parents report average Narendra Modi (India's current Prime Minister) feeling thermometer scores of 45/100 and 66/100 respectively. Muslims and Hindu boys are similarly polarized, reporting average scores of 47/100 and 65/100. Boys' and their parents' trust levels and political attitudes are strongly positively correlated (Appendix Figure S3), hinting at the intergenerational transmission of values.

While we recruited boys aged 13 to 18, our recruits skew young: 73% are aged 13 to 15, with the remaining 27% aged 16 to 18. 16% of the boys report having attended a camp in the past, with these past camps most often being sports-oriented. Finally, Muslims report more school exposure to Hindus than vice versa, consistent with their population shares: Hindus report the Muslim share of classmates as 26% on average, while for Muslims the share of Hindu classmates is 39%. We report other summary statistics in Appendix Table S1.

3.2 Treatment conditions

Camp-level treatments. We randomized at the child level, assigning 412 boys to one of three main treatment arms (see Appendix Figure S4 for timeline and randomization details). We stratified the randomization on religion (Hindu versus Muslim), randomization day attended (early versus late), and responses to the feeling thermometer score for Narendra Modi (above- versus below-median). The treatment arms are:

1. *Regular Camp ($N = 120$):* Boys in this group were invited to attend a 12-day youth camp that had four main elements: (i) lectures and discussions on Indian history and government, (ii) a ten-a-side soccer tournament, and other sports, (iii) dancing lessons, and (iv) a street theater workshop and performance. A meal was provided everyday. Campers met for four hours per day, for a total of 48 hours of activities (see Appendix

Table S2 for the camp schedule).[20]

2. *Ritual Camp ($N = 120$):* This camp closely mirrored the regular camp—the daily roster of activities was exactly the same—but incorporated additional ritual elements, explained below. To avoid confounding venue effects, we held the ritual camp in the same location as the regular camp. To avoid spillovers from one camp to the other, on any given day, one camp met in the morning, and the other met in the afternoon. To avoid confounding time-of-day effects, the assignment of the morning and afternoon slots alternated between the two camps each day.

3. *Control ($N = 172$):* Participants assigned to the control group did not get to attend either camp. To prevent disappointment, we told boys in the control group that a sports day would be organized for them at a later date. Ultimately, we held the sports day two and a half months after the study camps had concluded, shortly after all endlines were taken. We invited both the campers and the control participants to attend. At endline, we asked participants in the control group how often they did different activities during the two weeks of the camps. The three most common responses were: (i) playing sports (84% answered "Many times"), (ii) taking part in religious activities (34% answered "Many times"), and (iii) doing school work (27% answered "Many times").

Implementation of camp activities. We hired professionals to conduct each core camp activity. Teachers with relevant experience instructing teenagers (and recruited through an interview process) delivered lectures on Indian history and government (see overview in Appendix Section C.3), soccer coaches supervised stretching and exercises and refereed the matches, dance instructors taught dance moves, and finally street theater artists performed a play and ran a theater workshop. The same set of instructors conducted the activities in both camps, keeping the content and messaging of the activities identical.

Rituals treatment. Adapting slightly the definitions of anthropologists and psychologists (Hobson et al., 2018), we conceptualize collective rituals as sets of actions that are (i) rigid, (ii) repetitive, (iii) sometimes symbolic and/or with pre-existing meaning, (iv) causally opaque (i.e., it is not clear why the ritual would deliver a certain outcome), and (v) carried out in groups, usually in a coordinated and synchronous fashion. While some lab experiments aim to isolate specific aspects of rituals—for example, synchrony or repetition (Wiltermuth and Heath, 2009; Hobson et al., 2017)—we intentionally designed our ritual treatment to be bundled and intensive, covering the full gamut of ritualistic features. We introduced 20 distinct rituals to the ritual camp (described in Table 1, photos in Appendix Figure

[20] By contrast, the cricket league intervention in Lowe (2021) had participants play up to eight matches, for only five to ten hours in total.

S5), including the joint recitation of a camp pledge, coordinated dancing and singing, and daily flag ceremonies. For most of the ritual elements, there is a corresponding placebo in the regular camp. The goal of the placebo activities is to ensure that we hold three features constant across the two camps: (i) the extent of contact with other campers and teammates, (ii) the information communicated, and (iii) the approximate length of the activities. However, it is important to note that the rituals introduce distinct aspects that aren't mirrored by the placebo activities: they require coordination among groups, utilize repetition for emphasis, incorporate symbolism to enrich the conveyed message, and create a sense of synchrony among participants. Ex post, one advantage of the bundled treatment is that our null effects on intergroup relations are more stark: they cannot easily be explained as due to our rituals missing ritualistic features present in past studies.

Contact treatments. We randomized those assigned to the camps into teams of ten, stratifying on religion (Hindu versus Muslim) and camp type (Ritual versus Regular). The teams were a central part of the camp experience. Boys played soccer in these teams; they also ate meals together, sat at desks together during the lectures, danced together, and stood together during the daily flag hoisting. Given our interest in the effects of integrative camps, we did not form any religiously homogeneous teams. Instead, we randomly formed six teams with five Hindus and five Muslims (high contact for Hindus, low contact for Muslims), and six teams with eight Hindus and two Muslims (low contact for Hindus, high contact for Muslims), in each camp. This cross-cutting randomization allows us to test for the effect of more versus less inter-religious, collaborative contact.

3.3 Overview of outcomes

Measures taken during the camps. We administered two sets of daily measures during the camps. First, all campers completed a "measurement card" at the end of each day (see Appendix Figure S6). The card includes four questions measuring (i) happiness at the camp today (0 = most sad face, to 4 = most happy face), (ii) feelings of identity fusion with the other boys at the camp (as used by Swann et al. 2009, from 0 = circles for "You" and "Other Campers" are separated, to 4 = circles are fully overlapping), (iii) the number of teammates considered close friends (0 to 9), and (iv) boredom versus excitement at the camp today (1 = very bored, to 10 = very excited). Second, each day we randomly selected 12 boys from each camp to complete a longer survey. We use these outcomes less often (given the loss of precision that comes from surveying only a subsample of campers), and describe them when they appear for analysis.

Endline measures. We administered the endline between four and seven weeks after the camps had concluded, with the median respondent completing the survey 5.9 weeks later. Surveyors revisited the experimental sample

Table 1: List of rituals and their corresponding placebos in the regular camp

Ritual	Placebo	Concepts
1. Attendance register: Roll call taken out loud each day; for each student present whole camp chants in unison, "Good morning [name]"	Instructors take attendance silently by observation	Coordination, Synchrony, Repetition
2. Uniforms: All campers wear identical t-shirts with camp logo on the front, on all days	No uniform: campers dress as they wish	Symbolism, Coordination
3. Flag: Camp forms circle round the flagpole each day; one team nominated to hoist the Indian flag; flag is raised in silence then all shout "Jai Hind! Clap-clap-clap" repeatedly in unison three times	Instructor raises flag and says "Jai Hind" as campers watch (no circle)	Symbolism, Synchrony, Repetition
4. National anthem: All campers collectively sing the national anthem each day to recorded background music while standing round flag pole	Campers silently read words of national anthem to themselves	Symbolism, Synchrony
5. Camp pledge: All campers recite camp pledge in unison each day (see Appendix C.1)	Campers silently read words of the camp pledge to themselves	Symbolism, Synchrony
6. Meal chant: All collect food and sit down; before eating, all chant "Thank you for the food," then bang table with fists, three times, before eating (every day)	Campers collect food and eat as they wish	Synchrony, Repetition
7. Group stretching: Coach demonstrates stretches then all campers do the stretches in unison (all days with sports)	Coach demonstrates stretches and tells campers to disburse and do the stretches themselves	Synchrony, Repetition
8. Mexican wave: Spectators line up in teams along the playing field and do a full Mexican wave three times each time a goal is scored (all days with football)	None	Coordination, Repetition
9. Marching onto field: Teams march onto field in lines from either side of the pitch, walk past one another and shake hands with every member of the other team (all days with football)	None	Coordination
10. Singing at start and end of match: Spectators sing Kolkata Knight Riders chant in unison, five times as players march on and in the final five minutes of the match (all days with football)	None	Coordination, Synchrony
11. Halftime chant: Spectators sing "Jeetega" chant in unison for the team they are supporting (all days with football)	None	Coordination, Synchrony
12. Brazilian dance: Every time a team scores a goal they do the Brazil team dance from the 2022 World Cup (all days with football)	None	Coordination, Synchrony
13. Guard of honor: At the end of the match, losing team forms a "corridor" (guard of honor) that winning team walks through as losing team claps them (all days with football)	None	Coordination
14. Mock election: Candidates give speeches, campers secretly mark their ballots, then assemble in a long line to cast ballots in a ballot box and give "three cheers for democracy" (start and end of camp)	Candidates give speeches, campers secretly mark their ballots, ballots are collected by organizers	Symbolism
15. Dance: Campers taught a set of dances (set to music) and perform the dances as a whole camp in unison (dance days)	Campers taught dance moves by dance instructors then do free-style dancing	Coordination, Synchrony
16. Call and refrain: During lectures, teacher uses call and group refrain ("everyone repeat after me...") to instill key points (lecture days)	Same content, no call and refrain	Coordination, Repetition, Synchrony
17. Rakhis: Campers taught about Tagore's call for Rakshabandhan as a show of strength and unity between Hindus and Muslims, and to protest against Bengal's partition; participants stand in pairs (with a teammate), perform a clap ritual, then tie each other rakhis (once during camp)	After the lecture, participants simply exchange rakhi with a teammate	Symbolism, Synchrony
18. Street play: Campers watched a play on the importance of eating healthily performed by professional street theater artists; campers sat in a circle around the artists, and participated in the play through call and response, led by the actors (once)	Campers watched the play and did not participate in it	Coordination, Repetition, Synchrony
19. Theater games: Campers participated in a series of games that involved performing synchronous movements together, forming a human chain with balloons, and running together; the games revolved around the theme of eating healthy and nutritious food	Activities involved individually presenting the same content on nutrition	Coordination, Repetition, Synchrony
20. Closing ceremony: Camp stood in long line, arms locked together; one minutes silence, remembering Indias Freedom Fighters; all sing Muktiro Mondiro Shopano Tole while slowly walking as a full line toward other side of the field; then close into a full circle and give three cheers for India	All remain seated in the tent and the song Muktiro Mondiro Shopano Tole is played	Symbolism, Coordination, Synchrony, Repetition

at their homes. The endline survey covers five main families of outcomes. We describe these outcomes here, and introduce secondary outcome measures when they appear in the discussion of our findings.

Social preferences. We measure social preferences using dictator games and a public goods game. For the dictator games all participants were randomly matched with a Hindu and Muslim stranger (in random order) from the control group, with first names making religion salient.[21] They were asked to split Rs. 100 with "another boy in Barasat who you do not know," and were informed that one of their choices would be randomly implemented. Our main outcome from the dictator game is the difference in giving to the outgroup stranger versus the ingroup stranger. The boys also played a standard public goods game, with each boy given an endowment of Rs. 50. Total contributions to the pot were tripled and divided equally among participants. Campers played the public goods game in their teams of ten, while control participants were assigned teammates from the control group.[22] Before deciding on a contribution, respondents were reminded of the full names and ages of each of their teammates (or pseudo-teammates in the case of control participants), and then asked four comprehension questions, with surveyors explaining the correct answer when relevant. Our main outcome from the public goods game is the amount the boy contributed to the pot (Rs. 0 to 50).

Willingness to interact. We measure willingness to interact with the outgroup using self-reported friendships and an incentivized willingness to "play" measure (Rao 2019). For friendships, we asked respondents to list the full names of their five closest friends. Our main outcome is then the number of close outgroup friendships, coded using the religion signalled by the listed names. For the incentivized measure, we told respondents that we would be selecting 30 boys to attend a future social event "to give a way for boys to make new friends in the city." We explained that if the respondent was invited, they would be matched with one other boy, and then spend an hour or two playing board games and other activities with him. As with the dictator games, we randomly matched each respondent with one outgroup and one ingroup stranger from the control group, and then we elicited the respondent's willingness to pay (or accept) to go to the social event with each person, in random order. We first asked whether the respondent would attend the social event for Rs. 80, followed by 40, 20, and 0.[23] If at any point the respondent said yes, we advanced to ask about the second partner. If the respondent said no even at 0, we asked whether the respondent would attend

[21] Ten local research assistants coded the 140 unique first names of control participants as either definitely Hindu, probably Hindu, probably Muslim, definitely Muslim, or can't say. We kept the 30 most-distinctive Muslim names, with each name coded as definitely Muslim by seven or eight RAs, and as probably Muslim by the remaining RAs. We randomly selected 30 names from among the 42 joint most-distinctive Hindu names, all scored as definitely Hindus by all RAs. We used the remaining 60 names as dictator game partners.

[22] The assignment followed the same protocol as team assignment for the camps: i.e., 50% of the pseudo-teams included five Hindus and five Muslims, while the other teams included eight Hindus and two Muslims.

[23] For the first 21 surveys, we asked the question "would you attend for free?" first. A surprisingly high fraction answered yes, suggesting possible ceiling effects. As a result, we added the additional 80/40/20 questions for the remaining surveys. We exclude the first 21 surveys from this analysis, giving us a sample size of 380 instead of 401.

the social event if paid Rs. 20, followed by 40, 80, 120, 160, and finally 200. We incentivized truthful reporting by randomly implementing one of the answers. We use the answers to plot demand curves for social interaction.

National identity. We measure national identity using a self-report and an incentivized measure. For the self-report, we follow Depetris-Chauvin et al. (2020) with the question, "Let us suppose that you had to choose between being an Indian and being a [Hindu/Muslim]. Which of these two groups do you feel most strongly attached to?" Respondents chose from 0 = Only [Hindu/Muslim], 1 = More [Hindu/Muslim] than Indian, 2 = Equally Indian and [Hindu/Muslim], 3 = More Indian than [Hindu/Muslim], and 4 = Only Indian. For the incentivized measure, we asked each respondent to choose one of two fridge magnets as an extra gift for completing the survey: either a magnet featuring the Indian flag, or one displaying a religious symbol relevant to their religion (shown in Appendix Figure S7). Our main outcome measure is a dummy variable equal to one if the respondent selected the Indian flag magnet. The self-reported and incentivized measures of national identity are highly correlated: conditioning on religion, control participants that self-report a one-unit higher national identity are 8.5 percentage points ($p = 0.001$) more likely to select the Indian flag magnet. Among control participants, Muslims are 25 percentage points less likely ($p = 0.001$) to select the Indian flag magnet than Hindus, and score themselves 0.51 points lower ($p = 0.006$) on the scale of national identity. Neither measure of national identity is statistically significantly correlated with the baseline Modi feeling thermometer score, among control participants, controlling for religion. This suggests that our measures of national identity are orthogonal to the type of muscular Hindu nationalism emphasized by the BJP.

Attitudes. We have five main measures of self-reported attitudes. We capture inter-religious attitudes with two yes-no questions: (i) would you be willing to marry a [opposite religion] when you're older? and (ii) would you support giving Indian citizenship to a [opposite religion] immigrant? We measure attitudes towards foreigners using feelings thermometer ratings (from 0 to 100) toward Nepalese people for Muslims (as Nepalese people are predominantly Hindu), and the mean of ratings toward Bangladeshi and Pakistani people for Hindus (as Bangladeshis and Pakistanis are predominantly Muslim). For attitudes toward politicians, we take the mean of thermometer ratings for Mahatma Gandhi and reverse-coded ratings for Narendra Modi. Finally, for attitudes towards democracy, we asked respondents which type of political system they think is the best form of government, with options (i) having a strong leader who does not have to bother with parliament and elections, (ii) having experts, not government, make decisions according to what they think is best for the country, (iii) having the army rule, (iv) having a democratic political system, and (v) having a system governed by religious law in which there are no political parties or elections. We code the outcome as a dummy variable equal to one for respondents answering that a democratic system is the best form of government.

Psychological well-being. We measure three dimensions of psychological well-being: respondents' social lives, happiness, and depression. Respondents rated their social lives on a scale from 0 = I feel rather lonely, to 10 = I have a fulfilling social life. For happiness, we asked respondents, "Taking all things together in your life, would you say you are:" with options 0 = Not at all happy, 1 = Not very happy, 2 = Rather happy, 3 = Very happy. For depression, respondents completed the PHQ-8 scale, answering the following question for eight different problems: "Over the last 2 weeks, how often have you been bothered by any of the following problems?" The problems include: (i) little interest or pleasure in doing things, (ii) feeling down, depressed, or hopeless, and (iii) feeling bad about yourself—or that you are a failure or have let yourself or your family down. For each of the eight problems, the answer options are 0 = Not at all, 1 = Several days, 2 = More than half the days, and 3 = Nearly every day. We calculate the overall depression score as the sum of the eight answers, giving a total that could range from zero to 24.

3.4 Internal validity

Compliance. As shown in Appendix Figure S8, attendance at the camps was high, ranging between 81% and 91% for any given camp-day, and averaging 87% overall. While regular-camp attendance is slightly higher than ritual-camp attendance (89% versus 85%), we cannot reject the null hypothesis that regular- and ritual-campers attend the same number of days ($p = 0.24$).[24]

Manipulation check. Enumerators observed sports activities carefully to assess ritual compliance. Since the sports rituals did not have placebos, these are natural rituals for which to assess the ritual-regular camp difference. Appendix Figure S9 shows that rituals were strongly adhered to—spectators in the ritual camp are 47 percentage points ($p < 0.001$) more likely to have clapped during the games, and 77 percentage points ($p < 0.001$) more likely to have chanted, while players are 14 percentage points ($p = 0.03$) more likely to have high-fived. This suggests that the ritual treatment was faithfully implemented.

Balance and attrition. In Appendix Table S3 we show that individual characteristics are well-balanced across treatment arms. The p-values of the F-tests of joint orthogonality are 0.54 for camps versus control, 0.3 for ritual-camp versus regular-camp, and above 0.99 for high- versus low-contact. This suggests that the randomization was successful.

Out of 412 boys in the experimental sample, 401 completed the endline (97%). Appendix Table S4 shows no evidence of differential attrition across the treatment arms, nor of differential patterns of attrition according to

[24] We do find heterogeneous effects (by religion) of intergroup contact and rituals on camp attendance—we describe these findings in Section 5.

baseline covariates.

3.5 Estimation

To examine the overall effects of youth camps, we use the following specification:

$$Y_i = \beta_0 + \beta_1 \text{Camper}_i + \gamma \mathbf{X}_i + \theta \mathbf{Z}_s + \varepsilon_i, \tag{1}$$

where Y_i denotes an outcome for child i, Camper_i denotes treatment status (equal to one for boys randomly assigned to either the regular or the ritual camp, and equal to zero for the control group), and $\mathbf{X}_i$ and $\mathbf{Z}_s$ denote baseline controls and randomization strata fixed effects. As pre-specified, we use the baseline version of the outcome variable as a control when it is available, otherwise, we do not include baseline controls. β_1 denotes the treatment effect of camp assignment relative to the control group. We report robust standard errors for this specification.

To test for differences in the effects of regular and ritual camps, we run the following regression, keeping only the boys assigned to a camp:

$$Y_i = \beta_0 + \beta_1 \text{Ritual}_i + \gamma \mathbf{X}_i + \theta \mathbf{Z}_s + \varepsilon_i, \tag{2}$$

where Ritual_i is equal to one if child i was assigned to the ritual camp and zero if he was assigned to the regular camp. All other variables are defined as above. Since we are analyzing the effects of camps in this regression, as in Equation 1, we use robust standard errors.

Finally, to analyze the effects of contact we use the following specification, again only with those assigned to the camps:

$$Y_i = \beta_0 + \beta_1 \text{High Contact}_i + \gamma \mathbf{X}_i + \theta \mathbf{W}_s + \varepsilon_i, \tag{3}$$

where High Contact_i is an indicator equal to one for individuals randomized into a team with high exposure to outgroup individuals, and equal to zero otherwise. Hindus have high contact when in teams with five Hindus and five Muslims, whereas Muslims have high contact when in teams with eight Hindus and two Muslims. Given that the randomization to teams was stratified on camp and religion, we include camp $\times$ religion fixed effects, $\mathbf{W}_s$. The identifying variation then comes from comparing individuals belonging to the same camp and religion, but assigned to high- versus low-exposure teams. We cluster standard errors at the camp-team-level, with 24 clusters.

4 Treatment effects of camps

This section presents our findings on how youth camps shape intergroup relations and psychological well-being among adolescent boys. We explore the mechanisms behind the camps' effects in Section 5.

4.1 Summary of effects

We examine the overall effects of youth camps by comparing the endline outcomes of campers with the control group. Our core results are summarized in Figure 1, which plots the estimated effects of the camp on our four pre-registered primary outcome families (social preferences, willingness to interact, national identity, and attitudes) and on one pre-registered secondary outcome family (psychological well-being).[25]

Camps positively affect three of the five families of outcomes, improving group-related social preferences by 0.19σ, willingness to interact with the outgroup by 0.3σ, and psychological well-being by 0.17σ. Despite these behavioral changes, we estimate null effects on national identity and attitudes.[26] We can reject positive effects on national identity (0.09σ), and on attitudes (0.14σ), with 95% confidence.

Our indexed outcome measures help reduce concerns about multiple hypothesis testing. But we also use the Westfall-Young procedure (Young, 2018) to assess the significance of the camps in the aggregate. This procedure first conducts a joint test of the sharp hypothesis that neither of the two camp treatments had an effect on each of our four primary outcome indexes, and then performs the test combining the four equations. Consistent with Figure 1, we reject the hypothesis that neither the regular nor ritual camps impacted social preferences ($p = 0.03$), and willingness to interact ($p < 0.001$), whereas we cannot reject the null hypothesis of zero effects on identity ($p = 0.58$) and attitudes ($p = 0.21$). Using the combined test, we reject the hypothesis that neither of the camps had an impact on the four primary outcomes ($p = 0.004$).[27]

4.2 Social preferences

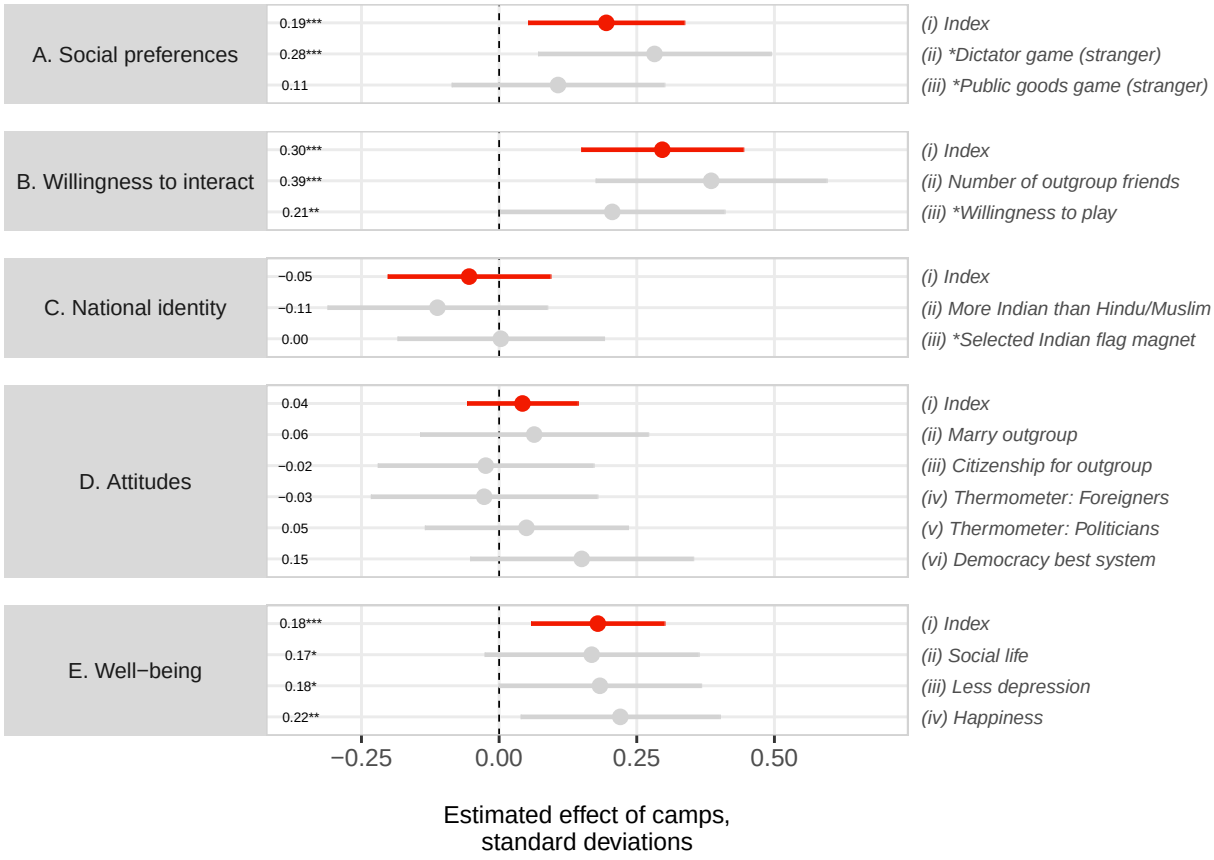
Our social preferences index includes two incentivized measures: ingroup bias in the dictator game, as measured by the difference in money given to an ingroup stranger versus an outgroup stranger, and the amount given to one's group in a public goods game.

[25] We promote psychological well-being to a main outcome to emphasize a result with potentially important implications: youth camps have surprisingly large and enduring effects on psychological well-being. Nevertheless, this finding should be considered more exploratory given that we pre-registered well-being as a secondary outcome.

[26] We might expect camps to be more effective in swaying the attitudes of younger, more malleable campers. Appendix Table S6, however, shows statistically insignificant effects on attitudes for both younger (0.1σ) and older cohorts (-0.01σ). At the same time, this heterogeneity analysis reveals that camps increased social preferences more for younger campers, and well-being more for older campers.

[27] If we also include the (secondary outcome) well-being index, the p-value is 0.0004.

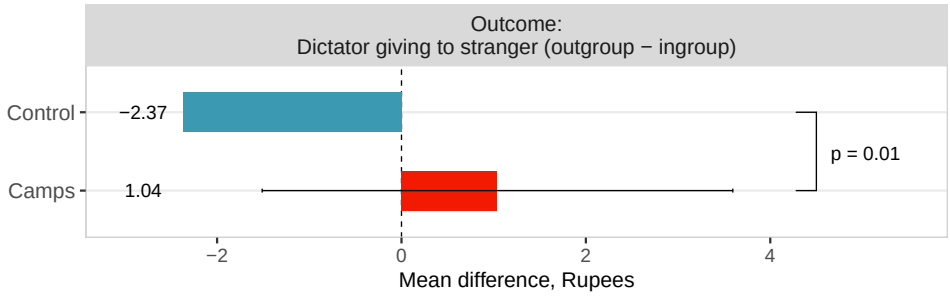
Figure 1: Estimated effects of camps on main outcomes



Notes: This figure plots the effects of the camps on five families of outcomes. Each coefficient plot summarizes a separate regression of the outcome on (i) an indicator for assignment to either of the two camps, (ii) randomization strata, and (iii) where available, a baseline measure of the outcome variable. Each index is the unweighted average of all components within a family of outcomes. Each component is a z-score, centered and standardized using the variable's control-group mean and standard deviation. We include the pre-registered components with clear directional predictions (see Appendix C.2 for full details along with explanations for minor deviations from the pre-registration). All components are from the endline survey, and their definitions are provided in Section 3.3. 95% confidence intervals are based on robust standard errors. Outcomes marked with stars are incentivized. N is 401 for outcome families A, C, D, and E, 400 for outcome B.ii, and 380 for outcome B.iii. Coefficient magnitudes and statistical significance for the treatment indicator are displayed on the left-hand side of the plot: *p<0.1; **p<0.05; ***p<0.01. Full tabulated results are displayed in Appendix Table S5.

Camp attendance reduces ingroup bias in dictator game giving by 0.28σ (Figure 1). On average, control group participants give Rs. 43.1 of their Rs. 100 endowment to ingroup members, and Rs. 40.7 to outgroup members (6% less). Camps eliminate this bias entirely—the difference in payments made to ingroup and outgroup strangers by camp attendees is statistically indistinguishable from zero, with the point estimates showing that campers give roughly Rs. 1 more to the outgroup stranger (Figure 2). This result confirms that the effects of the camps generalize to the broader outgroup, and not just those directly interacted with, as in some work on intergroup contact (e.g., Mousa 2020).

Figure 2: Impact of camps on ingroup bias in dictator giving



Notes: The Control bar shows the average outgroup-ingroup Rs. gap in giving in the dictator game for control participants. The Camps bar adds the camps treatment effect, estimated from a regression with randomization strata fixed effects. The 95% confidence interval and p-value for the difference are based on robust standard errors. N is 401.

The reduction in ingroup bias is driven roughly 50:50 by a *decrease* in payments to the ingroup and an *increase* in payments to the outgroup. It follows that control participants and campers show similar levels of generosity overall ($p = 0.89$ for the difference in average giving to strangers). Here, our findings differ from Rao (2019). In his Indian school setting, exposure to poor children increased generosity to both poor and rich children. In our setting, camps reduce bias in giving, but do not increase generosity overall.

While the dictator game captures social preferences, it cannot quantify the efficiency effects of the camps, given that the dictator game endowment is fixed. For this, we explore effects on the public goods game, where the size of the pie is maximized when participants contribute their full endowment to the group. In this game, campers play with their nine teammates, while the control group play with nine pseudo-teammates, assigned in the same way that campers were assigned teammates.[28] Note, in this case effects do not capture generalization; they instead capture effects on beliefs about, and preferences toward, other boys a camper has directly interacted with.

Control participants contribute Rs. 36.3 of their Rs. 50 endowment to the group on average. Campers contribute a statistically insignificant Rs. 1.6 (0.11σ or 4.4%) more than control participants ($p = 0.28$). This weak positive result is surprising—ex ante we would expect social preferences with respect to strangers (as in the dictator game)

[28] For the control group, the group assignment is similar to that of the minimal group paradigm (e.g., Chen and Li 2009).

to be less affected than social preferences with respect to teammates (as in the public goods game). More concretely, Goette et al. (2012) find that groups that interact with each other are more cooperative in a simultaneous prisoners' dilemma than "minimal" groups that do not interact. We find only weak positive effects of a comparable treatment. Nevertheless, our finding is consistent with the null effect of the camps on overall generosity in the dictator game, since public goods contributions to the team reflect the *level* of altruism toward, and beliefs about, teammates. Camps reduce bias in social preferences without necessarily shifting levels.

4.3 Willingness to interact

We measure the effects on willingness to interact with outgroup members using two different outcomes: the number of outgroup members in the participant's list of five closest friends, and the participant's willingness to pay to attend a social event with an outgroup stranger. The first is an unobtrusive measure of intergroup friendships, but does not directly capture general attitudes towards interacting with outgroup members, since these outgroup friendships can include friendships with other campers. The second measure captures generalized effects.

Camps increase the number of outgroup friendships by 0.24 (120% or 0.39σ, $p < 0.001$, Figure 3), consistent with one in four campers forming one close outgroup friendship from attending the camp. This friendship effect is large relative to the highly segregated counterfactual: in the control group, only 1 in 25 friendships are with outgroup members, despite the fact that roughly 1 in 3 baseline-reported classmates belong to the outgroup. The camps more than doubled outgroup friendships to approximately 1 in 12.

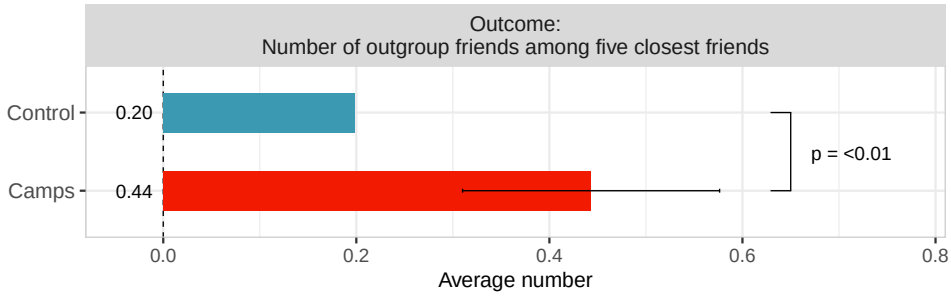
Most of the outgroup friendship effect is driven by campers becoming friends with other campers—campers report 0.15 more outgroup names that match the names of other campers (Appendix Table A), accounting for 0.15/0.24 = 63% of the overall treatment effect.[29] The remaining effect comes from campers listing outgroup names that we are unable to match with any camper or control participant. These names may reflect network effects (e.g., a camper becoming friends with the friend of an outgroup camper), or imperfect matching to campers due to spelling mistakes or nicknames. Given the possibility of imperfect matching, we rely on the willingness to play measure below to test for generalized effects on willingness to interact.

The large effect of the camps on outgroup friendships is striking for two extra reasons. First, given that our measurement was unobtrusive—participants were not told we would code the names as Hindu- and Muslim-sounding—the effect is unlikely to be driven by experimenter demand effects. Second, given that the endline was administered four to seven weeks after the last day of the camp, the friendships formed by the camp far outlast the camp itself. This is despite the fact that campers would not automatically see each other following the intervention: they were

[29] Sensibly, the effects for ritual-campers are driven by names that match other ritual-campers, while the effects for regular-campers are driven by names that match other regular-campers (Appendix Table A, columns 5 and 6).

not recruited from the same school, nor did we arrange any follow-up events for campers in between the end of the camp and the endline.

Figure 3: Camps more than double the number of close outgroup friendships



Notes: The Control bar shows the average number of outgroup friendships listed by control participants as among their five closest friends. The Camps bar adds the camps treatment effect, estimated from a regression with randomization strata fixed effects, a control for the baseline measure of the same outcome, and an indicator for missingness of this baseline control. The 95% confidence interval and p-value for the difference are based on robust standard errors. N is 400.

Campers make durable outgroup friendships, but these effects need not extend to the outgroup as a whole—campers could consider their camp friendships special cases, and remain wary of interactions with outgroup strangers. We use our incentivized willingness to play measure to test for generalized effects.

Control participants are mostly enthusiastic about the social event with outgroup members. 50% of control participants are willing to pay our highest price (Rs. 80) to attend the event, with the share increasing to 82% when the event is free (Figure 4, Panel (i)). Almost all control participants (95%) are willing to attend the event when offered Rs. 200 to attend, our most negative price.[30]

Camps shift the demand curve outwards: at each price, a weakly higher share of campers are willing to attend than control group participants.[31] At the highest price, campers are 12.6 percentage points more likely to want to attend the event (or 0.21σ with strata fixed effects, $p < 0.05$, as shown in Figure 1). At the most negative price, campers and control participants behave similarly, with almost all willing to attend.

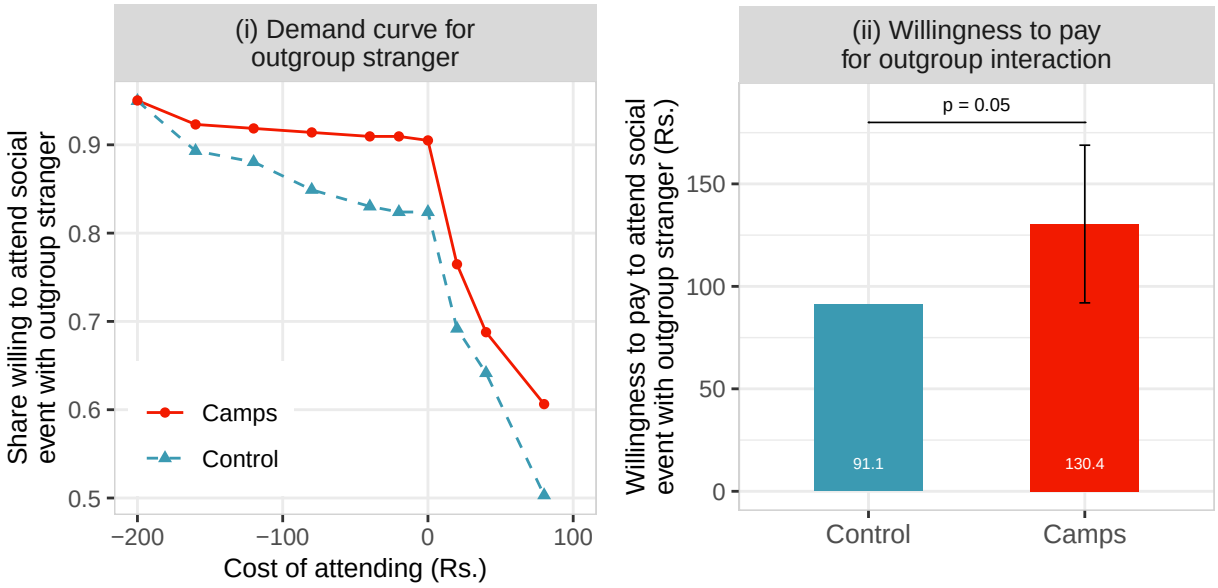
To summarize the effects on willingness to play, we estimate a tobit regression, with willingness to pay coded as: (i) the midpoint between X and Y when the participant said they were not willing to play for Rs. X, but willing to play for Rs. Y, asked in the subsequent question, (ii) censored at Rs. 80 when the participant said they were willing to play for the highest price (here we have a lower bound on willingness to pay), and (iii) censored at Rs. -200 when the participant said they were not willing to play for the most negative price (here we have a lower bound

[30]Note that our demand curves are downward-sloping by construction, as we lowered the price with each question, and stopped asking further questions whenever the participant said they would attend the event.

[31]Though the comparison of demand curves is unconditional, i.e., not strata-adjusted, the conditional differences are similar, given that the probability of being assigned to the camps is similar across the randomization strata.

on willingness to accept).

Figure 4: Camps increase willingness to play with outgroup strangers



Notes: Panel (i) plots demand curves for social interaction with outgroup strangers separately for control participants ($N = 159$) and for those assigned to either of the two camps ($N = 221$). As an example, the bottom-right point of the control group demand curve tells us that roughly 50% of control participants said that they would be willing to attend the social event with an outgroup stranger as their partner with a cost of attending of Rs. 80. Panel (ii) summarizes the results of a tobit regression of willingness to pay to play with the outgroup (with censoring at -200 and 80) on an indicator for camp assignment, along with randomization strata fixed effects. The 95% confidence interval and p-value for the difference are based on robust standard errors.

The tobit model predicts an average willingness to pay of Rs. 91 for the control participants. This number is higher than our highest price, Rs. 80, reflecting the fact that Rs. 80 is a lower bound on willingness to pay for 50% of the control participants. Campers have 43% higher willingness to pay, at Rs. 130 ($p = 0.05$). The camps thus have a substantial impact on the willingness of boys to socially interact with outgroup strangers. Like the effects on social preferences, these findings demonstrate generalizability to the outgroup as a whole.

Unlike outgroup strangers, the camps do not increase willingness to play with ingroup strangers. Consistent with ingroup bias, control participants have higher willingness to pay to play with an ingroup stranger than with an outgroup stranger (Rs. 141 versus 91, Appendix Figure S10). Camps increase this willingness to pay by a statistically insignificant 13% ($p = 0.41$), and the control and camper demand curves for interaction with the ingroup stranger are more often overlapping (Appendix Figure S10, Panel (i)). As with effects on social preferences, camps reduce ingroup bias in willingness to interact more than they shift the general level.

4.4 National identity

Our positive effects on social preferences and willingness to interact demonstrate campers now behaving differently towards outgroup members that they have never met. The generalized nature of these effects is fundamental to nation-building, an activity that requires individuals to feel affinity with their co-nationals, regardless of whether they have met in person. One channel by which this might be achieved is social identity recategorization. Do campers identify more with their nation, and less with their religion, than control participants? On the one hand, the camp experience, which highlights an inclusive vision of India through lectures on the country's freedom struggle and appeals for Hindu-Muslim harmony, could amplify the significance of national identity over religious identity. Conversely, increased interaction with outgroup members could make religious differences salient, diminishing participants' sense of a single national identity.

Control participants report a mix of national and religious identity: 50% report being equally attached to being Indian and being Hindu/Muslim, while 28% report feeling only Indian, and 8% report feeling only Hindu/Muslim. The remaining 10% and 4% report feeling more Indian than Hindu/Muslim and vice versa. As for our incentivized measure of national identity, 79% of control participants chose the Indian fridge magnet over the own-religion magnet. Camps reduce the self-reported measure of national identity by 0.13σ ($p = 0.27$) and have no effect on the choice of the Indian fridge magnet ($p = 0.97$, Figure 1). These effects combine for a statistically insignificant -0.05σ effect on the national identity index (Figure 1). The effects on the two components remain statistically insignificant when considering only Hindus, or only Muslims. This rules out the possibility that nationalistic messaging worked only for the majority Hindu group, who may associate national identity with the Hindu nationalist ideology of India's ruling party, the BJP.

Given the focus of the camp's lectures, the null effects on national identity may seem surprising. We show in Section 5 that negative effects of intergroup contact on national identity help provide an explanation.

4.5 Attitudes

For our fourth family of outcomes, we focus on social and political attitudes. With camps shifting ingroup bias and willingness to interact with outgroup members, we might also expect camps to shift our two binary outgroup attitudinal measures: willingness to marry an outgroup member and support for citizenship for an outgroup immigrant. These two outcomes represent two ends of a continuum of prejudicial attitudes: while only 22% of control participants express willingness to marry an outgroup member when they are older, 73% would support giving Indian citizenship to an outgroup immigrant. The camps do not change these attitudes: we estimate a 2.6 percentage point

effect on willingness to marry ($p = 0.55$), and a -1.1 percentage point effect on citizenship support ($p = 0.81$).[32]

Beyond the religious outgroup in India, the camp effects could extend to outgroup members outside of India. To test for this, we use feeling thermometer questions about people from Nepal (a predominantly Hindu country), Bangladesh, and Pakistan (both predominantly Muslim countries). Specifically, we set the outcome as the thermometer score for the Nepalese for Muslims, and the mean of the thermometer scores for Bangladeshis and Pakistanis for Hindus. As shown in Figure 1 above, we find no evidence of generalization to foreign outgroup members—we estimate an insignificant effect of -0.03σ.[33]

As with social attitudes, we find only limited evidence of effects of the camps on political attitudes. The camps have no effect on thermometer feelings toward the politicians we ask about: Narendra Modi, Mahatma Gandhi, and Mamata Banerjee (Appendix Figure S11). Arguably, the null effects on feelings toward contemporary politicians Modi and Banerjee reveal a strength of our youth camps: they improve intergroup relations and psychological well-being while remaining non-partisan. Finally, the camps have positive, but insignificant, effects on support for democracy: campers are 7.4 percentage points ($p = 0.15$) more likely to report that democracy is the best political system.

Other than social and political attitudes, we included three questions on masculinity. One potential concern about boys' camps is that bringing boys together could induce more traditional attitudes regarding masculinity and violence.[34] Reassuringly, we estimate null impacts on masculinity attitudes (Appendix Figure S12)—those assigned to camps are no more likely than control participants to agree that (i) boys who cry are weak, (ii) boys should appear manly, or (iii) boys should use violence to get respect.

In short, we find little evidence of camps affecting attitudes, summarized by the null effects of camps on the omnibus attitudes index (Figure 1).[35] The pattern of behavioral change without attitudinal change echoes the results of other experiments on prejudice reduction (Paluck et al. 2021). Tentatively, we speculate that more direct messaging—for example, through lectures on the importance of intergroup tolerance—may be necessary to move attitudes.[36]

[32] The effect remains statistically insignificant ($p = 0.46$) for Hindus only, for whom the citizenship question is arguably more relevant.

[33] We also find no effect on feelings toward the English: the point estimate is 1.35 points on a 0 to 100 scale, with $p = 0.61$. Camp lectures on India's freedom struggle do not stir up anti-colonial sentiment.

[34] These concerns relate to evidence on the persistent effects of male-biased sex ratios on conservative attitudes in Australia (Grosjean and Khattar, 2019), and on the correlation between traditional views on masculinity and local violence in Brazil (Matavelli, 2023).

[35] To the extent that experimenter demand effects are as large or larger for self-reported attitudes than for incentivized questions, we note that this null on attitudes helps to rule out the possibility of experimenter demand driving effects on social preferences and willingness to interact.

[36] That would align with the value-shaping educational intervention of Dhar et al. (2022), which shaped attitudes more than behaviors. Importantly, the lectures in our camps did not *explicitly* urge campers to be more tolerant (Appendix C.3). We estimate null effects of our lectures on attitudes in Section 5.3. But they did focus on the value of democracy, and using quasi-random variation in lecture-day attendance, we do observe that they positively affect support for democracy.

4.6 Psychological well-being

Our first four families of outcomes look at different facets of cohesive communities. To understand broader welfare consequences, we turn to effects on psychological well-being. Camps may improve well-being through the creation of lasting social connections (Jose et al. 2012; Banerjee et al. 2023), through engagement in physical activity (Bailey et al. 2018), and by providing a collaborative environment for personal growth (Bialeschki et al. 2007).

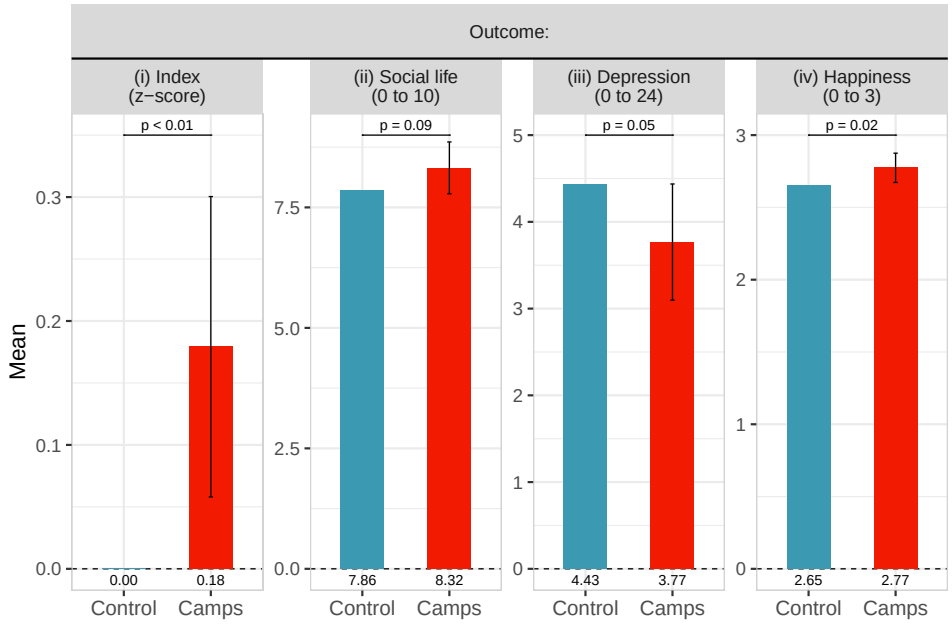
Control participants have high well-being at endline: on average, they rate their social life as 8.1 out of 10, their happiness as 2.7 out of 3 (closer to "Very happy" than "Rather happy"), and they score only 4.3 out of 24 on the PHQ-8 depression scale. Despite the already-high well-being of control participants, campers score even higher on all three dimensions (Figure 5). They rate their social lives 0.47 points higher ($p = 0.09$), their happiness 0.12 points higher ($p = 0.02$), and they score 0.67 points lower on the depression scale ($p = 0.05$). Combining these results, we estimate that the camps increase a well-being index by 0.18σ ($p < 0.01$, Figure 5). This is a substantial improvement considering that (i) the camps were not explicitly designed to improve well-being, and (ii) participants reported their well-being four to seven weeks after the camp's final day.[37]

A natural channel for the effects on happiness and depression would be through camps creating lasting social connections for boys who feel disconnected. Supporting this mechanism, we find suggestive evidence that the effects on happiness and depression are larger for boys who rate their social lives below-median at baseline, compared to boys with above-median ratings (0.33σ versus 0.09σ for happiness, $p = 0.2$ for the difference, 0.28σ versus 0.03σ for depression, $p = 0.19$ for the difference).

The effects on well-being are not equally distributed. For Hindus, we estimate an effect of 0.24σ ($p < 0.01$) on the index. For Muslims, we estimate an effect of only 0.06σ ($p = 0.46$, and $p = 0.13$ for the difference in effects between Hindus and Muslims). The heterogeneity is not explained by differences in lasting friendships: Muslims report continuing to be friends with *more* campers than Hindus (39 versus 33 friends), and report having spent time with a similar number of camp teammates in the past two weeks (2.4 for Muslims, 2.5 for Hindus). The Hindu-Muslim difference in treatment effects also remains after controlling for baseline well-being, suggesting that the difference is not due to lower baseline levels of well-being among Hindus. In the next section, we show that this heterogeneity is not explained by the effects of intergroup contact either, but is instead attributable to the divergent effects of rituals.

[37] One concern would be that the positive effect is driven by control participants' disappointment at not being selected for the camps. We attempted to alleviate this concern by reminding control participants of the post-endline sports day that they were invited to. We think these attempts succeeded—in particular, control participants do not systematically report lower well-being at endline than at baseline (at baseline, the average for control participants is 7.2/10 for social life, 3.7/24 for depression, and 2.5/3 for happiness. Comparing with the endline control means in Figure 5, only the depression measure deteriorates between baseline and endline).

Figure 5: Camps increase psychological well-being



Notes: The figure shows the treatment effect of camps on three dimensions of well-being, and on an unweighted average of the standardized versions of the three dimensions (centered and standardized using the variables' control-group means and standard deviations). The social life component is the answer to the question: On a scale from 0 = I feel rather lonely, to 10 = I have a fulfilling social life, how would you describe your current personal situation? The happiness component is the answer to the question: Taking all things together in your life, would you say you are: 0 = Not at all happy, 1 = Not very happy, 2 = Rather happy, 3 = Very happy. The depression component is the PHQ-8 score, calculated from summing up the answers to eight questions like: Over the last 2 weeks, how often have you had little interest or pleasure in doing things? 0 = Not at all, 1 = Several days, 2 = More than half the days, 3 = Nearly everyday. Each regression includes randomization strata fixed effects and the baseline-measured outcome variable. The 95% confidence intervals and p-values are derived from robust standard errors.

5 Mechanisms

Why does camp attendance improve intergroup behaviors and increase psychological well-being? In this section, we explore the mediating role of rituals, intergroup contact, and civic modules. We find that each of these factors influences different outcomes, though with significant heterogeneity across groups.

5.1 Collective rituals

Collective rituals have long been a fundamental component of youth camps. Psychologists and anthropologists argue that rituals can increase prosociality, group survival, and emotional well-being (Norton and Gino, 2014; Xygalatas et al., 2013). Rituals may thus explain some of the camp's effects. For example, it could be that ritual-campers are more psychologically satisfied and therefore are more willing to interact with outgroup members than regular-campers. It is also plausible that by forging a shared identity collective rituals enhance Hindu-Muslim relations (Durkheim, 1912).

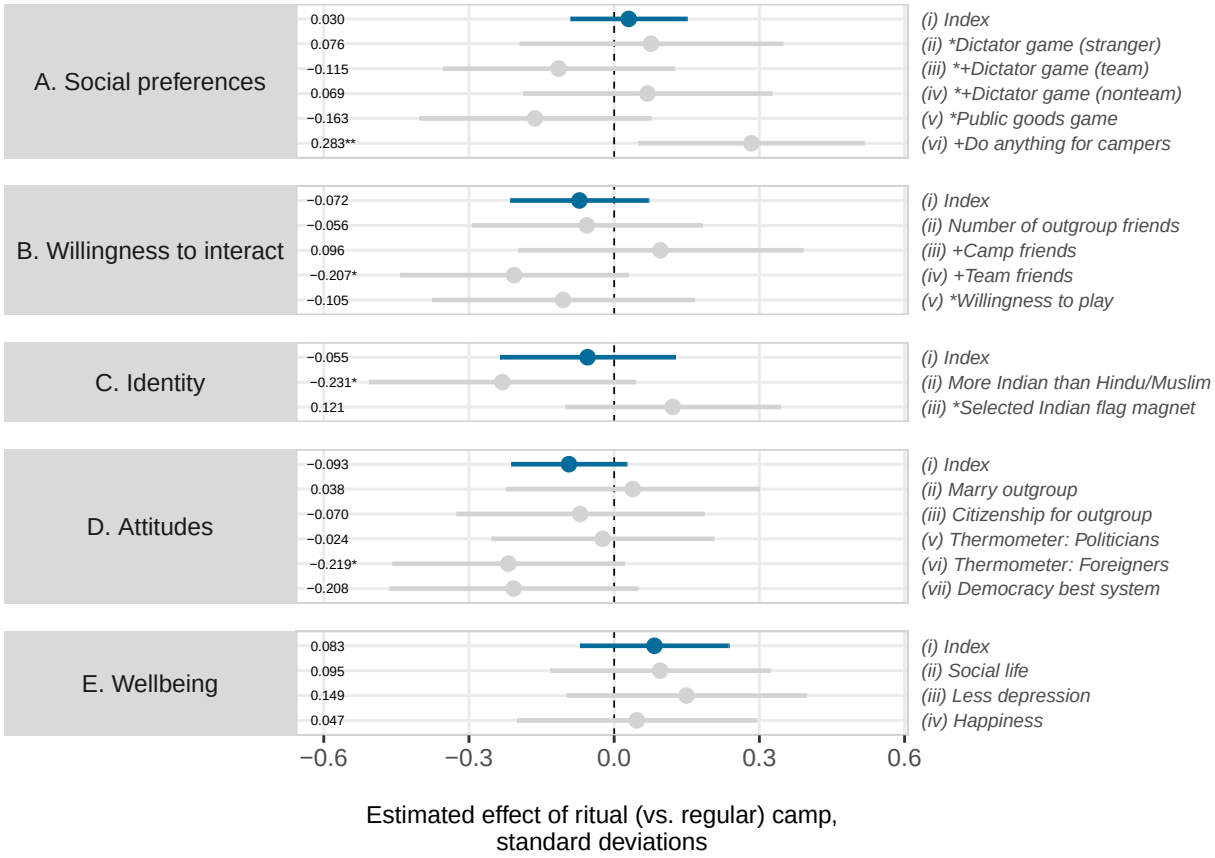
Effects of rituals: during the camp. We first explore the contemporaneous effects of rituals using the daily surveys completed by camp attendees. Appendix Figure S14 presents a day-by-day comparison of the means of these outcomes across the regular and ritual camps. Regular-campers score themselves highly on happiness, excitement, pride, and shared identity with other campers. We fail to reject equality between ritual- and regular-campers for these four measures during the first week of the camp: rituals do not appear to deepen emotions and social connectedness over and above "regular" social interactions in that initial period. However, in the second week ritual-campers report stronger shared identity with other campers (0.18σ, $p = 0.03$), higher excitement (0.14σ, $p = 0.11$), and greater pride (0.42σ, $p < 0.01$). The growth in impact we observe over time suggests that rituals may require repetition before becoming meaningful and taking force. Studies of one-off rituals would miss such dynamic effects.

Effects of rituals: at endline. Rituals increase pride, excitement, and shared identity with other campers in the immediate term, but do they lead to persistent changes even after the rituals end? We present the effects of rituals on our primary endline outcomes in Figure 6, which includes the same set of primary outcomes used to evaluate the effects of camps in Figure 1, in addition to five pre-registered subcomponents that were only measured for boys assigned to the camps.

Overall, Figure 6 indicates that rituals do not account for the positive effects of camps. We find that the effects of rituals on the four families of outcomes concerning intergroup relations are not statistically significant (Panels A to D), with negative point estimates for three of the four. We do see some suggestive changes in individual subcomponents. In particular, rituals increase participants' agreement with the statement, "I am willing to do anything to help the campers" by 0.28σ (or 0.18 points on a 0 to 3 scale, $p = 0.02$). Rituals might, then, engender a deeper sense of camp identity, with participants more willing to make personal sacrifices for the benefit of the group. Having said that, ritual-campers have 0.52 *fewer* teammates ($p = 0.09$) that they have spent time with in the past two weeks. It is clear that despite fostering a stronger camp-identity, rituals do not improve broader intergroup relations.

Rituals and psychological well-being. The effect of rituals on the well-being index is not statistically significant (Figure 6, Panel E). But the magnitude is large in relation to the overall effect of the camps on well-being. Whereas the effect of the regular-camp relative to control is 0.14σ ($p = 0.06$), the effect of the ritual-camp relative to the regular-camp is 0.08σ ($p = 0.30$). Performing rituals, then, increases the well-being effects of the camps by almost 60%.

Figure 6: Rituals do not improve intergroup relations

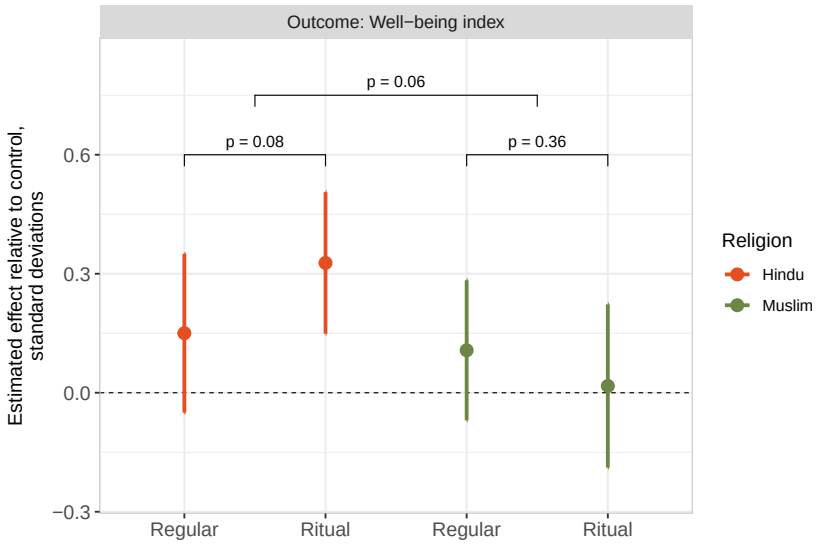


Note: This figure plots the effects of the ritual-camp (relative to the regular-camp) on five families of outcomes. Each coefficient plot summarizes a separate regression of the outcome on (i) an indicator for assignment to the ritual-camp, (ii) randomization strata, and (iii) where available, a baseline measure of the outcome variable. The sample includes only those assigned to the ritual- or regular-camp. Each index is the unweighted average of all components within a family of outcomes. Each component is a z-score, centered and standardized using the variable's regular-camp mean and standard deviation. All components are from the endline survey, and their definitions are provided in Section 3.3. 95% confidence intervals are derived from robust standard errors. Outcomes marked with stars are incentivized. Outcomes marked with plus signs are recorded for campers only, and thus do not appear in the Camps versus Control comparisons. These include: ingroup bias in dictator giving to teammates (A.iii) and non-teammates (A.iv) from the camp (when asked "to split Rs. 100 with Hindu/Muslim teammates and non-teammates"), agreement with the statement, "I am willing to do anything to help the campers" (A.vi), and finally the number of camp friends (B.iii, "Here is a list of all the boys from your camp. Can you select the ones that are still your friends?") and team friendships (B.iv; "Can you select which ones, if any, you have spent time with in the past two weeks?"). Coefficient magnitudes and statistical significance are indicated on the left-hand side of the plot: *p<0.1; **p<0.05; ***p<0.01. Full tabulated results are displayed in Appendix Table S8.

Rituals also explain why we observe larger effects of camps on well-being for Hindus than Muslims (discussed in Section 4.6). In particular, while the effect of the regular-camp on well-being is 0.16σ for Hindus and 0.12σ for Muslims, rituals strengthen the effects for Hindus, and weaken them for Muslims (Figure 7, $p = 0.06$ for the difference in treatment effect). The differences are stark. For Hindus, the ritual-camp increases well-being by 0.33σ relative to the control group. For Muslims, the ritual-camp has no effect on well-being relative to the control group.[38]

[38]One possibility is that Muslims tend to engage in religious rituals more intensively than Hindus. As a result, introducing a new set of

Figure 7: Rituals improve the well-being of Hindus but not Muslims



Note: This figure plots the point estimates and confidence intervals from a single regression of the endline psychological well-being index on (i) religion indicators interacted with regular- and ritual-camp indicators, (ii) the baseline psychological well-being index, and (iii) randomization strata, with robust standard errors.

Why would collective rituals improve the well-being of the majority group but not the minority? One possibility is that some of our rituals are explicitly nationalistic (e.g., singing the national anthem), and participants may perceive these rituals as related to the Hindu nationalist appeals of Modi's BJP. With Muslims less supportive of Modi than Hindus at baseline (averaging 47 versus 65 on the feeling thermometer measure), such rituals may differentially isolate Muslims, leading to negative well-being effects. However, the Hindu-Muslim difference in the effects of rituals barely changes (remaining at 0.26σ) when we add an interaction term between the ritual-camp and the baseline-measured Modi feeling thermometer.

An alternative hypothesis is that rituals promote social bonding more for Hindus than for Muslims. Indeed, the divergent effects of rituals on well-being are driven mostly by the social life component, rather than the depression and happiness components (Table 2, columns 1 to 3). Rituals increase the 0-10 social life score reported by Hindus by 0.8 points (or roughly $1/3\sigma$), and *decrease* the social life score reported by Muslims by a similar amount. A plausible hypothesis would then be that rituals promote lasting friendship formation for Hindus, but not for Muslims. However, if anything, rituals are somewhat more likely to create lasting teammate and other-camper friendships for Muslims (columns 4 and 5). We instead see suggestive evidence for divergent effects of rituals on the self-reported willingness to do anything to help the campers—rituals increase this willingness for Hindus, but not Muslims (with $p = 0.3$ for the difference). A natural interpretation is that rituals increase the depth and meaningfulness of camper

rituals might not be as effective in enhancing well-being for Muslims. Our endline data lends credence to this notion: within the control group, Muslims consistently report higher levels of religious activity and also exhibit greater well-being.

relationships for the majority group, but not for the minority group. While rituals strengthen camp attachment for Hindus, they appear to alienate Muslims, as judged by revealed preference: Muslims attend 1.7 fewer days when assigned to the ritual camp (column 8). Collectively, these findings suggest that rituals were alienating for the minority group but meaningful for the majority group.

Why would rituals alienate Muslims but not Hindus? One possible explanation is that rituals are more satisfying when performed with ingroup members. Given that Hindus were the majority in each camp (65% of campers), they may have consequently benefited more from the rituals. Our experiment design does not permit us to test for this explanation, but future work might do so by estimating the effects of rituals separately for Hindu-only and Muslim-only camps.[39]

Table 2: Heterogeneous effects of rituals by religion

	Outcome:						
	Social life (1)	Depression PHQ-8 (2)	Happiness (3)	Friendships team (4)	Friendships camp (5)	Anything campers (6)	Camp attendance (7)
Ritual × Hindu	0.84**	-0.39	0.09	-0.74*	0.91	0.23**	0.15
	(0.42)	(0.61)	(0.08)	(0.40)	(4.16)	(0.10)	(0.53)
Ritual × Muslim	-0.81*	-0.75	-0.10	-0.11	4.12	0.08	-1.67**
	(0.46)	(0.56)	(0.09)	(0.45)	(4.74)	(0.11)	(0.65)
N	235	235	235	235	235	235	240
Outcome mean	8.1	4.1	2.7	2.4	34.9	2.4	10.4
Hindu vs. Muslim *p*-value	<0.01	0.67	0.12	0.29	0.61	0.30	0.03
Randomization strata FEs	Y	Y	Y	Y	Y	Y	Y
Intervention day FEs	N	N	N	N	N	N	N
Baseline dependent variable	Y	Y	Y	N	N	N	N

Notes: The sample includes only the boys randomly assigned to the camps. Ritual is an indicator for being assigned to the ritual camp. All outcomes are measured at endline. Social Life is the 0 to 10 self-report of how fulfilling the participant's social life is. PHQ-8 Depression is the 0 to 24 total score of how depressed the participant has felt during the last two weeks (higher means more depressed). Happiness is measured from 0 = Not at all happy to 3 = Very happy. Friendships Team are the number of camper teammates spent time with in the last two weeks. Friendships Camp are the number of campers that the participant is still friends with. Anything Campers is the self-reported agreement with the statement, "I would do anything to help the group of boys who attended my camp," from 0 = Strongly Disagree to 3 = Strongly Agree. Column 7 uses the number of camp-days attended (0 to 12). Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

5.2 Intergroup contact

All camp participants had outgroup teammates, with teams containing either five Hindus and five Muslims, or eight Hindus and two Muslims. The latter may be considered as the status quo composition given Hindu-Muslim population shares in West Bengal, whereas in equal share teams Hindus are likely to have much more exposure to Muslims than they are used to. Intergroup contact can foster better intergroup relations (Paluck et al., 2018; Mousa,

[39] A related hypothesis concerns minority status within a *team*, rather than within the camp as a whole. The divergent effects of rituals could be due to Muslims being an absolute minority in teams with eight Hindus and two Muslims. Against this hypothesis, we find a *positive*, although statistically insignificant, interaction between rituals and high intergroup contact for Muslims (a point estimate of 0.20, $p = 0.18$), when considering the effect on the endline well-being index. It follows that the effects of rituals on the well-being of Muslims are more negative (though insignificantly) in teams with five Hindus and five Muslims.

2020), particularly when contact is collaborative (Lowe, 2021), as is the case with our ten-person soccer teams. However, contact studies often find either limited or no effects that generalize toward outgroup strangers (Scacco and Warren 2018; Mousa 2020; Paluck and Clark 2020). In addition, contact may backfire if an expanding minority group is perceived as a threat (Enos, 2016). We see some correlational evidence for the latter in our baseline data: a one standard deviation increase (roughly a 30 percentage point increase mirroring our high- versus low-contact treatment for Hindus) in the share of Muslim classmates is associated with children's parents self-identifying with a stronger (0.15σ, p = 0.04) Hindu (over Indian) identity.[40] The rapid ascent of the BJP in West Bengal—driven in part by their successful campaign to convince voters that the TMC state government favors minorities and promotes (unlawful) Muslim immigration—further suggests that this sentiment may resonate with the majority group.[41] We report the effects of high- versus low-intergroup contact in Figure 8.

If the overall camp effects were driven by the fact that campers experience more collaborative intergroup contact than non-campers, we would expect the pattern of findings in Figure 8—that is, the effects of high-contact among campers—to approximate the main effects of the camps plotted in Figure 1. In reality, the effects of contact are substantively different to the camps' main effects, with two exceptions: the positive effects on close friendships with outgroup members, and the null effects on attitudes.

Relative to low contact, high contact increases outgroup friendships by 0.42σ ($p = 0.01$), explaining the overall 0.39σ effect of the camps on this outcome.[42] In contrast, the effects of contact on four of the remaining five outcomes in Figure 8 are qualitatively different to the effects of the camps. Contact reduces willingness to interact with an outgroup stranger (-0.22σ, $p = 0.07$) and national identity (-0.21σ, $p = 0.03$), and has no effect on ingroup bias (-0.06σ, $p = 0.65$) or well-being (0.02σ, $p = 0.77$). These findings suggest that intergroup contact is not the core mechanism behind the effects of youth camps, with one exception: contact facilitates outgroup friendship-making.[43]

The clearest negative effect of contact is on national identity. Both sub-components are similarly affected:

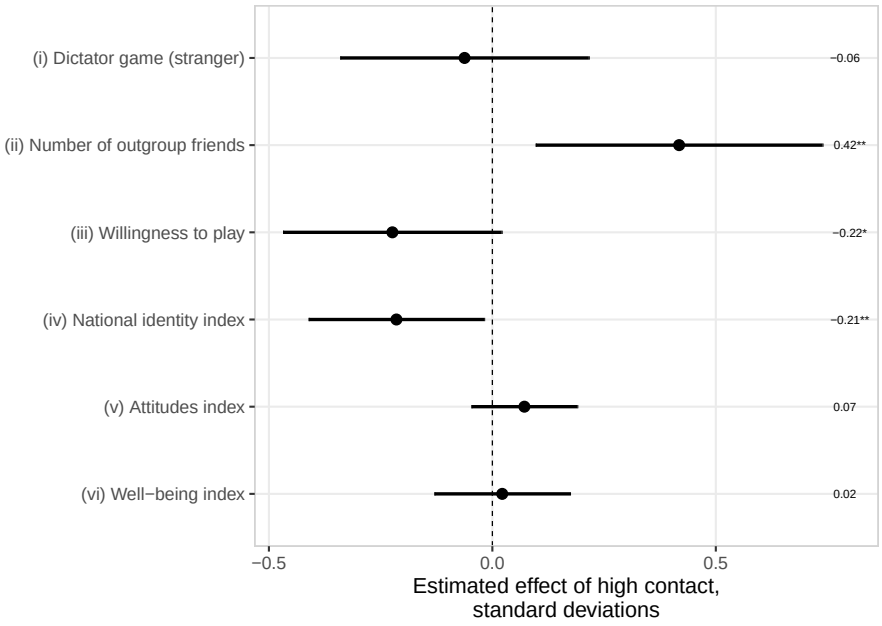
[40] Due to a survey questionnaire error, we have a high share of missing data from children for this outcome. But, given the strong correlation between political attitudes of parents and children (see Appendix Figure S3), the correlation is likely to be similar for children.

[41] "West Bengal: BJP accuses TMC govt of resorting to appeasement politics," *Indian Express*, April 24, 2017.

[42] More formally, assuming linear effects of intergroup contact, the 0.42σ coefficient implies an effect of 0.42/3 for each 10 percentage points increase in collaborative contact. Assuming zero collaborative contact in the control group, and the fraction of outgroup team members as the extent of collaborative contact for the campers, the estimated effect of the camp on collaborative contact is 42 percentage points. The effect of the camps that come through intergroup contact is then (0.42/3)*4.2 = 0.59σ. In this sense, the effect of contact can fully account for the 0.39σ effect of the camps. The mediated effect of contact is smaller if we assume that the control group experienced some collaborative contact, but this effect can still fully account for the effect of the camps provided that the control group has collaborative contact of less than 14%.

[43] One counterpoint to our reasoning would be that contact may have non-linear effects: with positive effects going from zero to some contact (call this effect S), and negative effects going from some to high contact, the effect that we can estimate (call this effect H). In the regular camp, 65% of campers have low intergroup contact, while the remainder have high intergroup contact. For the effects of non-linear contact to rationalize the effect of the regular camp on willingness to interact, we would need 0.65*S + 0.35*(S+H) = 0.26. Substituting in H=-0.22, we get S = 0.34σ. We do not find such non-linearity likely, especially in light of the largely linear effects of cross-caste contact in our previous work in India (Lowe 2021). In that experiment, there is in fact suggestive evidence of the opposite non-linearity (negative effects as the outgroup share goes from zero to one-quarter, positive effects as the outgroup share increases from one-quarter). That said, without single-religion teams, we cannot fully rule out the possibility of non-linear effects of contact.

Figure 8: The effects of intergroup contact



Notes: This figure plots the effects of high intergroup contact on our main outcomes. We regress outcomes on an indicator for assignment to high (within-team) contact, randomization strata, and, where available, a baseline measure of the outcome variable. Regressions include only participants randomly assigned to a camp. The outcomes parallel those in Figure 1, with two exceptions: (i) we exclude the public goods game since high contact mechanically affects the form of the game (high-contact participants play the game with more outgroup members), and (ii) we break up the willingness to interact index into its components, to unmask the opposite effects of contact on each. Full outcome variable definitions are provided in Section 3.3. Variables are centered and standardized using the variable's low-contact-group mean and standard deviation. 95% confidence intervals are based on team-clustered standard errors (with 24 teams). Coefficient magnitudes and statistical significance are indicated on the right-hand side of the plot: *p<0.1; **p<0.05; ***p<0.01. Full tabulated results are displayed in Appendix Table S9.

high contact reduces self-reported national identity by 0.25 points ($p = 0.11$), and the choice of the Indian fridge magnet by 9 percentage points ($p = 0.11$) (leading to the overall effect of -0.21σ on the index). This backfiring helps account for the insignificant -0.05σ pooled effects of the camps on national identity (Figure 1). If outgroup exposure increases the salience of religious differences, intergroup contact can actually enhance religious identity, muting the effectiveness of integrated camps in creating a shared national identity. Specifically, the significance of religious attachment might be heightened in more polarized (equal share teams) contexts, due to an increased perception of threat to the religious aspect of one's identity (Bazzi et al., 2019). This finding raises the question of whether national identity can coincide with social integration. Relatedly, this finding may also help explain the enduring appeal of ethnocentric nationalism (e.g., Hindu nationalism in India) across many regions worldwide.

Our findings on contact are notable for an additional reason: they are the first experimental findings of negative effects of collaborative contact (Paluck et al., 2018; Clochard, 2022). So far, researchers only estimate negative effects of contact when contact is adversarial (Lowe 2021), or when contact involves the presence of the outgroup

without any actual social interaction (Enos 2014). Why does contact backfire for some outcomes? We explore three potential explanations. First, if negative effects are due to the group threat of a growing minority population, we would expect the effects to be driven by Hindus. Second, the effects of contact may be negative because larger outgroups self-segregate—for example, two Muslim participants may have no choice but to befriend some of their eight Hindu teammates, whereas five Muslim participants might revert to interacting only among themselves. For this channel, we test for effects of contact on ingroup bias in within-team friendship formation. Third, and related, larger outgroups (e.g. five Muslims rather than two) may invest less effort in cooperating and getting along with their other teammates. If this channel is at play, we would expect high-contact participants to have more negative perceptions of their outgroup teammates' prosociality than low-contact participants.

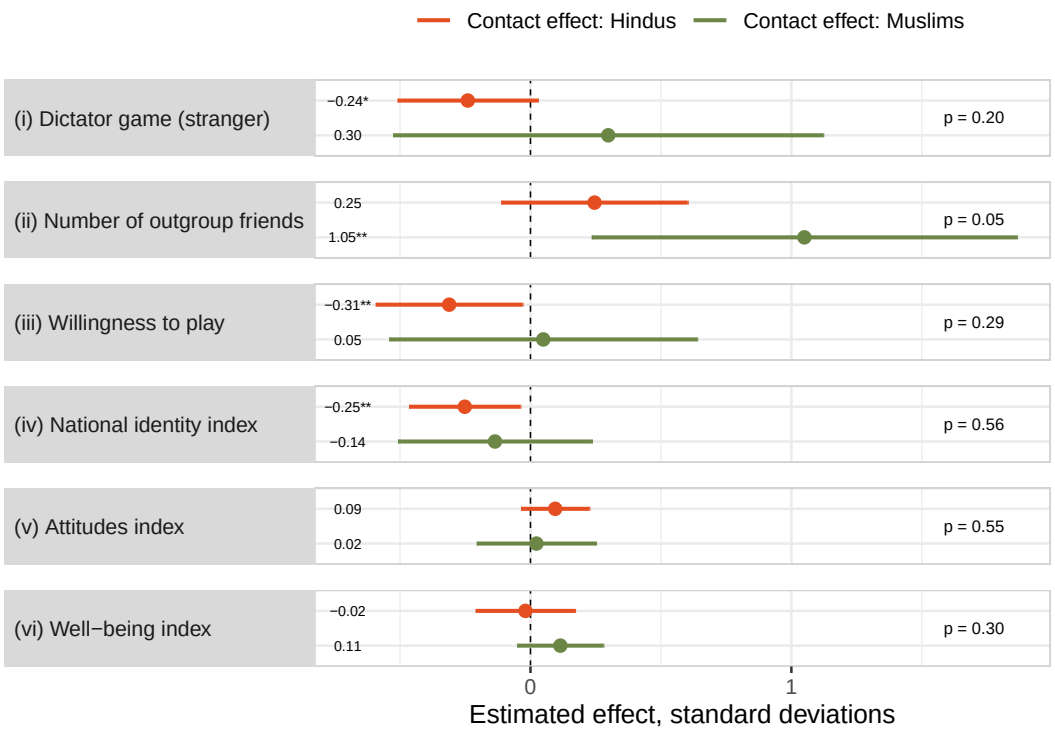
Heterogeneity by religion. Hindus drive the negative effects of contact (Figure 9), while the effects for Muslims are imprecisely estimated. Higher contact increases close outgroup friendships for Muslims more than for Hindus (1.05σ versus 0.25σ). For Hindus, contact reduces willingness to interact with the outgroup (-0.31σ, $p = 0.03$) and national identity (-0.25σ, $p = 0.02$), and increases ingroup bias (0.24σ, $p = 0.08$). We estimate imprecise null effects of contact on these three outcomes for Muslims, and null effects on attitudes for both Hindus and Muslims. Finally, while effects of contact on well-being are not statistically significant for both Hindus and Muslims, the point estimates are somewhat more positive for Muslims.

This heterogeneity at endline is consistent with our first hypothesis: that contact backfires because of the perceived threat posed by a growing minority population. Our daily measurement during the camps echoes these findings. In Table 3, we show that the effects of contact on emotions and teammate friendships—as recorded on the daily measurement cards—are more negative for Hindus than for Muslims. These differences are also reflected by the revealed preferences of participants: Hindus attend one less day of the camp when assigned to high-contact teams, whereas Muslims attend 0.8 *more* days when in high-contact teams ($p = 0.04$ for the difference in the two effects). We might expect such polarizing effects to diminish over time, but the asymmetric impact of contact is if anything greater during the second week of the camp than the first (Table 3, Panel B versus Panel C).

Contact and self-segregation. The formation of mixed peer groups can backfire when subgroups self-segregate (Carrell et al., 2013). In our setting, the natural parallel would be that Hindus and Muslims self-segregate in teams with five Hindus and five Muslims, but integrate when there are only two Muslims. That could lead to negative effects of contact for Hindus and positive effects for Muslims, in line with the findings of heterogeneity in Figure 9. To test for self-segregation, we use dyadic endline data on teammate friendships. At endline we showed each

camper a list of their nine teammates, and asked them, "Can you select which ones, if any, you have spent time with in the past two weeks?" We use the answers to explore whether ingroup bias in friendship formation is greater for Hindu participants—and weaker for Muslim participants—with high intergroup contact.

Figure 9: Negative effects of contact are driven by Hindus



Notes: This figure shows the effects of intergroup contact on our main outcomes, separately for Hindu and Muslim campers. We run regressions of each outcome on (i) an indicator for assignment to a team with a high outgroup share, (ii) randomization strata, and (iii) where available, baseline measures of the outcome variable. The outcomes exactly parallel those in Figure 8. Full outcome variable definitions are provided in Section 3.3. 95% confidence intervals are derived from team-clustered standard errors (with 24 teams). p-values on the right-hand side test for the equality of effects of high contact for Hindus and Muslims. Coefficient magnitudes and statistical significance are indicated on the left-hand side of the plot: *p<0.1; **p<0.05; ***p<0.01.

Both Hindus and Muslims display ingroup bias in their teammate friendships: Hindus with low intergroup contact are 12 percentage points more likely to report having spent time with ingroup teammates than outgroup teammates, while for Muslims the ingroup bias is 31 percentage points (Figure 10, Panel (a)). The effects of contact are the opposite of those suggested by the self-segregation hypothesis. Hindus are statistically significantly less ingroup-biased when assigned to five-Hindu-five-Muslim teams ($p = 0.05$); Muslims, on the other hand, are more ingroup-biased when assigned to eight-Hindu-two-Muslim teams, although the difference is not statistically significant ($p = 0.2$). It therefore appears that the 50:50 teams actually promote integration, with both Hindus and Muslims showing less ingroup bias in their teammate friendships when in 50:50 teams. These findings also indicate that self-segregation cannot explain the negative effects of contact that we identify. This may be partly due to the

fact that many of the camp activities required collaboration across the entire ten-person teams.

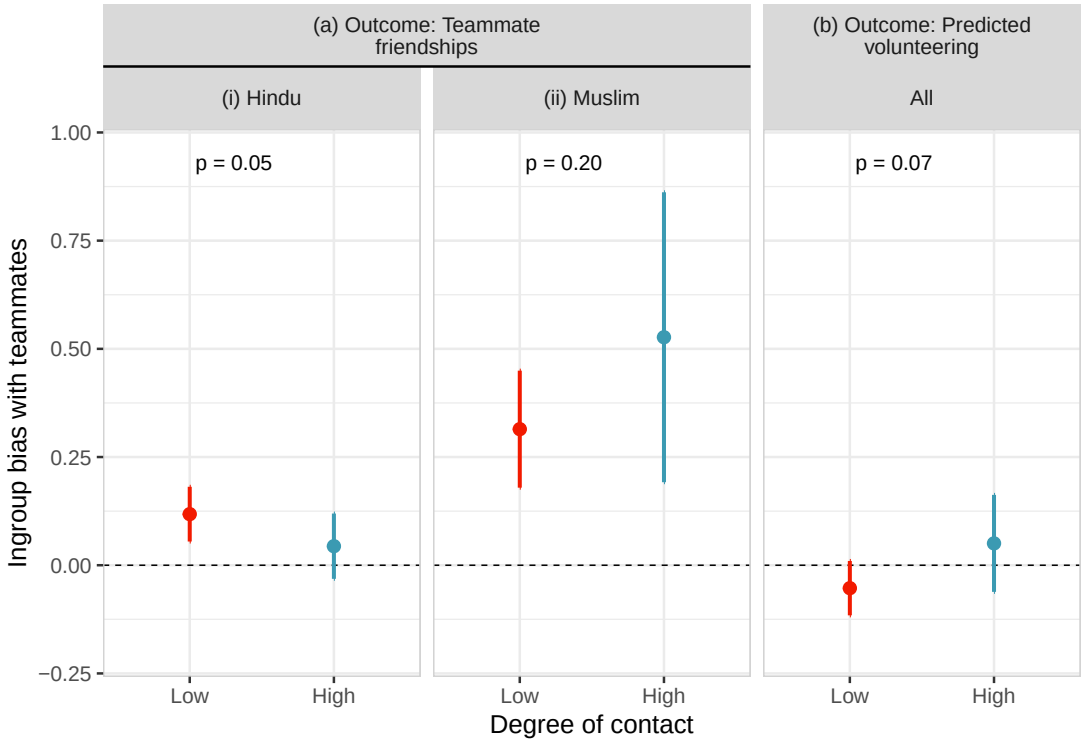
Table 3: Intergroup contact, daily emotions, and team friendships

	Outcome:			
	Excited (1)	Team friends (2)	Happy (3)	Camp attendance (4)
Panel A: Pooled				
High contact × Hindu	-0.40	-0.76**	-0.15	-1.02*
	(0.27)	(0.35)	(0.11)	(0.55)
High contact × Muslim	0.68*	0.18	0.03	0.79
	(0.39)	(0.33)	(0.12)	(0.79)
Hindu vs. Muslim effect, *p*-value	0.08	0.11	0.40	0.04
Low contact outcome mean	8.79	7.67	3.57	10.58
N	2,524	2,526	2,521	240
Panel B: Week 1				
High contact × Hindu	-0.29	-0.68	-0.19	-0.55*
	(0.39)	(0.41)	(0.14)	(0.29)
High contact × Muslim	0.77	-0.06	0.07	0.33
	(0.53)	(0.38)	(0.18)	(0.40)
Hindu vs. Muslim effect, *p*-value	0.23	0.38	0.38	0.06
Low contact outcome mean	8.79	7.67	3.57	5.31
N	1,254	1,254	1,251	240
Panel C: Week 2				
High contact × Hindu	-0.52**	-0.83**	-0.10	-0.47
	(0.24)	(0.34)	(0.15)	(0.32)
High contact × Muslim	0.59*	0.42	0.00	0.47
	(0.32)	(0.35)	(0.11)	(0.41)
Hindu vs. Muslim effect, *p*-value	0.02	0.03	0.66	0.05
Low contact outcome mean	8.79	7.67	3.57	5.28
N	1,270	1,272	1,270	240
Strata FEs	Y	Y	Y	Y
Day FEs	Y	Y	Y	N

Notes: This table presents regression estimates of the heterogeneous effects of intergroup contact on outcomes measured during the camps. The outcomes for columns 1 to 3 are from the daily question cards answered by all campers (see Appendix Figure S6), while the outcome for column 4 is camp attendance as recorded by the camp supervisors (number of days attended from 0 to 12). The survey questions for each column are: (1) "How bored or excited did you feel during the camp today?" (1 = "very bored" to 10 = "very excited"); (2) "Which picture best describes your relationship with the other boys at the camp today?" (0 = self and other-campers circles fully apart to 4 = self and other-campers circles fully overlapping); and (3) "Which picture best describes your emotions at the camp today?" (0 = very sad emoji to 4 = very happy emoji). The unit of observation is the participant in column 4 and participant/day otherwise. Standard errors are clustered at the team level in columns 1 to 3 and are robust (unclustered) in column 4. *p<0.1; **p<0.05; ***p<0.01.

Contact and perceived prosociality. While larger outgroups do not self-segregate, it may still be the case that outgroup members behave differently when they have large numbers. In particular, when outgroup members are many, they may not need to make as much effort to be accepted by the broader team. Relatedly, smaller minority

Figure 10: Effects of contact on ingroup bias in friendships and perceived prosociality



Notes: We estimate ingroup bias for low-contact campers using the specification: $y_{ij} = \alpha_i + \gamma \text{Same Religion}_{ij} + \varepsilon_{ij}$, where y_{ij} is respondent i's answer about teammate j—an indicator for having spent time with teammate j in the past two weeks ("Teammate friendships"), or an indicator for guessing that teammate j volunteered to help with the camp at baseline ("Predicted volunteering"). α_i are respondent fixed effects, and $\text{Same Religion}_{ij}$ is an indicator equal to one when i and j are either both Hindus, or both Muslims. We run this regression with the friendship outcome including only Hindus in low-contact teams (far-left coefficient), and Muslims in low-contact teams (third-from-left coefficient). We run this regression with the predicted volunteering outcome for all campers in low-contact teams (second-from-right coefficient). The three remaining high-contact coefficients are equal to low-contact ingroup bias ($\hat{\gamma}$) plus $\hat{\theta}$ from the following specification: $y_{ij} = \alpha_i + \theta \text{Same Religion}_{ij} \times \text{High Contact}_i + \eta \text{Same Religion}_{ij} \times \mathbf{W}_{s(i)} + \varepsilon_{ij}$, where $\mathbf{W}_{s(i)}$ are the randomization strata for the intergroup contact randomization. 95% confidence intervals are based on team-clustered standard errors, while the p-values test the null hypothesis that there is no effect of contact on ingroup bias (i.e., that $\theta = 0$).

groups have stronger incentives to "assimilate" by cooperating with the majority group (Lazear, 1999). To test for this channel, we again use dyadic endline data. We now measure ingroup bias in the perceived prosociality of teammates, testing whether high-contact participants are more likely to believe their own group to be more prosocial than the outgroup. Specifically, at baseline we asked all recruits, "At some point during or after the camps, we may need volunteers to help us with organization. The volunteers would not be paid for helping. Would you be willing to volunteer? If yes, we may ask for your help later." While 80% said yes, we did not ultimately ask any of the boys for help. At endline, we asked respondents, "Here are your nine teammates from the camp. Which of your teammates do you think offered to volunteer? If you guess the right number of people, you will get INR 10." We use these answers to measure ingroup bias in the perceived prosociality of teammates.

Participants with low intergroup contact show negative ingroup bias, predicting that ingroup teammates are five percentage points ($p = 0.09$) less likely to have volunteered than their outgroup teammates (Figure 10, Panel (b)). Consistent with our hypothesis, greater intergroup contact increases ingroup bias in perceived prosociality ($p = 0.07$ for the difference). Thus, intergroup contact shapes social inference in a direction that makes the effects of contact more negative.

Summarizing, we find suggestive evidence of two mechanisms behind the null or negative effects of contact seen in Figure 8. First, the effects are more negative for Hindus, consistent with the idea that a larger minority presence is viewed as a threat. Second, greater contact increases ingroup bias in perceived prosociality, consistent with the claim that larger outgroups need not act as prosocially to integrate. Our broader takeaway is that intergroup contact can *only* rationalize the positive effect of the camps on intergroup friendships.

Rituals and contact. Do rituals, when combined with increased intergroup contact, produce effects that differ qualitatively from either rituals or contact alone? If rituals and contact complement each other, optimal program design would require the bundling of both features. To test this, we restrict our sample to the camp attendees, and run a specification with the contact and ritual treatments, as well as their interaction as the main explanatory variables. We report standardized effects on the interaction term for each of our five outcomes indices, as well as on the individual components (Appendix Figure S13). We note that, while pre-registered, we have limited statistical power for the interaction term, making this analysis more exploratory.

The results indeed show some evidence of complementarities, especially for social preferences and well-being (0.26σ, $p = 0.01$ and 0.21σ, $p = 0.1$ respectively).[44] But, we estimate statistically insignificant interaction effects

[44]However, we note that we do not observe a statistically significant interaction term for dictator giving to strangers—the component of the social preferences index affected positively by the camps (Figure 1). The positive interaction effect is driven by camper-only outcomes.

for willingness to interact (-0.06σ, $p = 0.62$), attitudes (0.1σ, $p = 0.38$), and national identity (0.22σ, $p = 0.25$); although the effect on the national identity index is substantially more positive than the other two indices. Recall that greater intergroup contact led to negative effects on national identity overall. Therefore, the positive interaction here provides suggestive evidence that rituals might be able to mediate some of the backfiring from increased outgroup exposure. However, given power issues, the results are generally less precise. Future work could focus on more precisely estimating whether bundling rituals with contact could be beneficial.

5.3 Programming content

Rituals and intergroup contact can affect behaviors through emotion-based channels. Camp lectures, meanwhile, might affect behaviors through reasoning. Previous research has shown curriculum content can indeed shape attitudes and behaviors of children in the direction intended by the content (Cantoni et al., 2017). We use quasi-random variation in lecture attendance to probe this possibility. While each camp was 12 days long, only three days included lectures: days 2, 7, and 11. Given that the first and last days of the camps were somewhat special—including introductions and the closing ceremony—we focus on the effect of attending the three lecture days as opposed to the non-lecture days excluding the first and last days. Since each day's activities were not announced in advance, attendance of lecture days is plausibly exogenous conditional on overall attendance. That motivates the following specification:

$$Y_i = \gamma_0 + \gamma_1 \text{Lecture Days Attended}_i + \gamma_2 \text{Middle Days Attended}_i + \gamma_3 \mathbf{X}_i + \varepsilon_i, \tag{4}$$

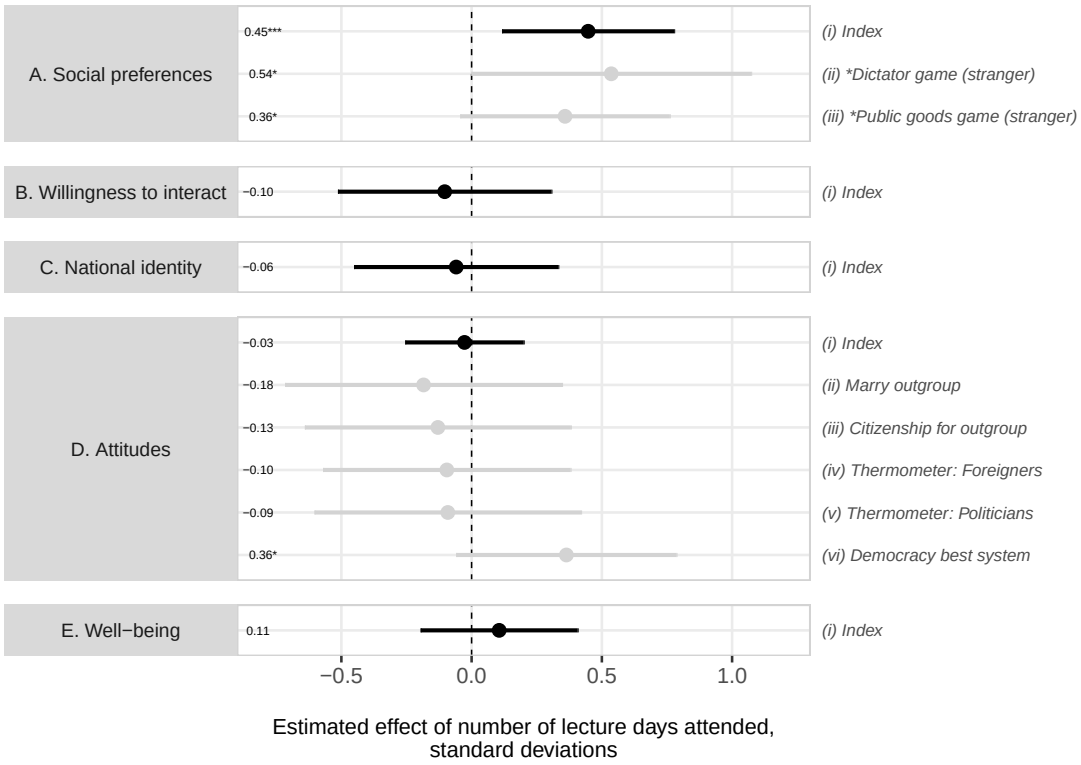
where $\text{Lecture Days Attended}_i \in \{0,1,2,3\}$ is the key regressor and $\text{Middle Days Attended}_i \in \{0,1,...,9,10\}$ is the key control: the number of days attended excluding the first and last days of the camp. γ_1 is the coefficient of interest, while γ_2 is not interpretable as causal. As with our analysis of the effects of camps, we include baseline dependent variables as controls when available, and we estimate robust standard errors. As hypothesized, balance tests show that lecture-day attendance is as-good-as-random after conditioning on non-lecture-day attendance (Appendix Table A). Thus, the effects of lecture days are cleanly identified, although less precisely estimated than the effects of the treatments that we explicitly randomized. Given the lack of precision, we consider these findings more exploratory.[45]

Recall that the camps have statistically significant positive effects on three of five outcome families: social preferences, willingness to interact, and well-being (Figure 1). Lecture-day attendance positively affects only social

[45]The decline in precision comes from the fact that relatively few individuals contribute variation in lecture attendance conditional on attendance from days 2 to 11. In particular, of the 235 camp participants who completed the endline, 155 attended every day from day 2 to day 11, and 10 attended zero days—these 165 participants do not contribute identifying variation. Of the remaining 70 participants, 69 contribute variation, with 51 of these participants attending eight or nine days from day 2 to day 11.

preferences (Figure 11). Each additional lecture-day attended increases the social preference index by 0.45σ ($p < 0.01$), driven by positive effects of 0.54σ and 0.36σ on the dictator game and public goods contributions sub-components, respectively. These findings suggest that the educational component of camps plays a role in molding social preferences in the domain of money, but not, for example, in molding feelings of national identity and attitudes. Tentatively, it might be that lectures around topics of inclusiveness are effective at molding reason-based decisions (like monetary decisions), and less so at moulding emotion-based decisions (like attitudes).

Figure 11: Programming improves social preferences



Notes: The figure plots the point estimates and 95% confidence intervals from 12 separate regressions. Each outcome is regressed on the number of lecture days that the camper attended, controlling for the total number of camp days attended (not including the first and last day). The sample includes only the boys randomly assigned to the camps. Outcome variables are centered and standardized using the variables' control-group means and standard deviations. Confidence intervals are based on robust standard errors. Coefficient magnitudes and statistical significance for number of lecture days attended are displayed on the left-hand side of the plot: *p<0.1; **p<0.05; ***p<0.01.

While the lectures did not explicitly discuss monetary judgments, they did directly cover the history and success of India's democratic political system. At endline, 43% of control group participants consider democracy to be the best political system. Each lecture-day attended increases this fraction by 18 percentage points ($p = 0.09$), suggesting that the lecture content was persuasive among the participants driving the lecture-day effects. One interpretation of these results is that lectures shift attitudes only to the extent that they explicitly target a particular topic (the strength of India's democracy, in our case). Participants change their mind on the targeted topic, but do not make the

inferential leap to change their views on related topics (e.g., support for citizenship for outgroup members). In principle, such inferential leaps (or "generalization") may be more likely among older and more educated participants, a claim that might be tested in future work.

6 Conclusion

Most societies have organizations and traditions that involve bringing together children of similar ages, but from different families, for group activities. Whether intentionally or not, such institutions have the potential to build bonds, develop social skills, and instill progressive community norms. We assess whether youth camps can strengthen ties between adolescents from historically antagonistic religious groups, and so forge social cohesion in ethnically polarized societies. In a randomized controlled trial fielded among Hindu and Muslim boys in West Bengal, India, we show that youth camps reduce ingroup bias, increase willingness to interact with outgroups, and bolster psychological well-being. Different components of camps explain different effects: intergroup contact facilitates intergroup friendship-making, rituals add to psychological well-being (although only for the majority group), while programmatic content influences social preferences. At the same time, these components affect majority- and minority-group children in heterogeneous ways. Additional outgroup exposure tends to backfire for the majority-group Hindu boys, while rituals alienate Muslims and promote psychological well-being only for Hindus. Table 4 summarizes these findings.

Robert Baden-Powell, the founder of modern scouting, wrote that "a week of [camp] life is worth six months of theoretical teaching in the meeting room" (Baden-Powell, 1949, 35). We find that camps integrating team sport, group rituals, and programmatic instruction can have powerful impacts on young people, and remake social relations for the better. Still, our results raise questions for future research. The first is what can be done to bring about attitudinal and not just behavioral change in adolescents. We detect positive shifts in attitudes regarding democracy, which was the focus of our lecture plans. Studies targeting intergroup attitudes might therefore concentrate lesson material on explicit dimensions of tolerance—for instance, arguments favoring intermarriage. Second, how might camps' design be modified to ensure that intergroup contact works equally well for majority- and minority-group campers? It may be that an even more immersive residential (rather than day) camps might provide more opportunities for positive interactions between campers of different groups, and fewer chances to self-segregate. In that vein, Corno et al. (2022) demonstrate that having outgroup roommates in a university dorm setting reduces stereotypes and improves attitudes. Based on our suggestive evidence of complementarities, future research could also measure more precisely the consequences of bundling rituals with high contact. Coming up with an optimal template for tolerance-promoting camps would be valuable for policymakers.

Last, the overall null effect of rituals is an important takeaway from our study. The ritual component of the intervention was highly intensive, featuring 20 distinct rituals, and encompassing at least four different concepts said to matter in the literature (coordination, symbolism, synchrony, and repetition). Thus, the null cannot be attributed to a "light touch" treatment. We conclude that rituals may not have the community-building power often ascribed to them, and claims by Durkheim (1912), Turner (1977) and others about the potency of "collective effervescence" in ritual contexts may be incorrect. But there are types of rituals we do not evaluate. Rituals that are extreme (especially those that are physically demanding or painful), or those that involve costly signals of loyalty, might perform better in creating cooperation across diverse groups (Aronson and Mills, 1959; Xygalatas et al., 2013). Longstanding religious rituals, replete with pre-existing meaning, might also be more effective for identity fusion. While difficult to test in field experimental settings, these types of ritual deserve more investigation.

Table 4: Summary of results

	Primary outcome index:				
	Social preferences	Willingness to interact	National identity	Attitudes	Well-being
Main effects:					
Camps	+	+	Null	Null	+
Mechanisms:					
Contact	Null	+ friendships – willingness to play	–	Null	Null
Rituals	Null	Null	Null	Null	+ Hindus
Content	+	Null	Null	+ democracy as best system	Null

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ONLINE APPENDIX

"Creating Cohesive Communities: A Youth Camp Experiment in India"

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A Supplementary Tables

Table S1: Summary statistics

Measure	N	Mean	St. Dev.	Min.	Median	Max.
Baseline (parent):						
Monthly HH income: Rs. 5,001-15,000	412	0.75	0.43	0.00	1.00	1.00
Thermometer: Narendra Modi	412	59.01	30.94	0.00	56.00	100.00
Thermometer: Mamata Banerjee	412	72.23	26.63	0.00	79.00	100.00
Thermometer: Mahatma Gandhi	412	76.36	25.51	0.00	84.00	100.00
Trust: other religion	412	3.12	0.73	1.00	3.00	4.00
Trust: other nationality	412	2.69	0.86	1.00	3.00	4.00
Trust: other state	412	2.83	0.78	1.00	3.00	4.00
Baseline (child):						
Thermometer: Narendra Modi	412	58.42	30.54	0.00	60.00	100.00
Thermometer: Mamata Banerjee	412	72.84	26.75	0.00	81.00	100.00
Thermometer: Mahatma Gandhi	412	73.00	26.81	0.00	81.00	100.00
Trust: other religion	412	3.04	0.71	1.00	3.00	4.00
Trust: other nationality	412	2.68	0.83	1.00	3.00	4.00
Trust: other state	412	2.83	0.77	1.00	3.00	4.00
Aged 13 to 15	412	0.73	0.44	0.00	1.00	1.00
Previously attended camp	412	0.16	0.36	0.00	0.00	1.00
Number of Hindu students in class	412	6.12	2.83	0.00	7.00	10.00
Number of Muslim students in class	412	3.80	2.85	0.00	3.00	10.00
Number of outgroup friends	412	-6.47	25.02	-99.00	0.00	5.00
Opinion of democracy	412	1.74	0.75	1.00	2.00	4.00
Loneliness	412	7.26	2.46	0.00	8.00	10.00
Depression	412	4.18	3.73	0.00	3.00	19.00
Happiness	412	2.52	0.59	0.00	3.00	3.00
Measures taken during camp (campers only):						
Excited	2,524	8.77	2.04	1.00	10.00	10.00
Team friends	2,526	7.51	2.18	0.00	9.00	9.00
Happy	2,521	3.54	0.91	0.00	4.00	4.00
Camp attendance	240	10.42	3.22	0.00	12.00	12.00
Lecture day attendance	240	2.59	0.86	0.00	3.00	3.00
Endline (all children):						
Dictator game (stranger)	401	-0.53	12.86	-50.00	0.00	75.00
Public goods game (stranger)	401	37.14	14.32	0.00	40.00	50.00
Number of outgroup friends	400	0.33	0.83	0.00	0.00	5.00
Willingness to play	380	0.56	0.50	0.00	1.00	1.00
More Indian than Hindu/Muslim	401	2.40	1.19	0.00	2.00	4.00
Selected Indian flag magnet	401	0.79	0.41	0.00	1.00	1.00
Marry outgroup	401	0.23	0.42	0.00	0.00	1.00
Citizenship for outgroup	401	0.73	0.45	0.00	1.00	1.00
Thermometer: Foreigners	401	49.81	21.97	0.00	50.00	100.00
Thermometer: Politicians	401	56.13	15.82	2.50	54.50	100.00
Democracy best system	401	0.47	0.50	0.00	0.00	1.00
Social life	401	8.12	2.59	0.00	9.00	10.00
Less depression	401	-4.15	3.55	-17.00	-3.00	0.00
Happiness	401	2.72	0.50	0.00	3.00	3.00
Teammate friendships	2,350	0.24	0.43	0.00	0.00	1.00
Predicted volunteering	2,350	0.60	0.49	0.00	1.00	1.00
Boys should appear manly	401	2.42	0.64	0.00	2.00	3.00
Boys should use violence	401	1.45	1.06	0.00	1.00	3.00
Boys who cry are weak	401	1.14	0.93	0.00	1.00	3.00
Endline (campers only):						
Dictator game (nonteam)	235	-0.38	11.45	-40.00	0.00	40.00
Do anything for campers	235	2.39	0.58	0.00	2.00	3.00
Camp friends	235	34.92	24.15	0.00	30.00	117.00
Team friends	235	2.44	2.33	1.00	1.00	9.00

Notes: This table reports summary statistics. We include all outcome variables used in the main analysis, all baseline variables used in the analysis, and baseline variables mentioned in the main paper to characterize the sample.

Table S2: Camp activities by day

	Activities list
Day 1	· Introduction · Football
Day 2	· Lecture and discussion on democracy · Mock election · Football
Day 3	· Dance · Football
Day 4	· Street theater · Cricket
Day 5	· Camp photo · Football
Day 6	· Dance · Football
Day 7	· Lecture and discussion on history and politics of Bengal · Rakhis · Cricket
Day 8	· Football · Sports day: cricket, relays, badminton
Day 9	· Street theater · Football
Day 10	· Football · Cricket
Day 11	· Lecture and discussion on India's freedom struggle · Mock election · Football
Day 12	· Closing ceremony, certificates

Notes: The schedule repeated twice per day. Morning sessions took place from 9am to 1pm; afternoon sessions took place from 1pm to 5pm.

Table S3: No evidence of imbalance in baseline covariates across experimental conditions

		Camps vs. control		Ritual vs. regular			High contact vs. low contact	
	Control mean (SD) (1)	Coef. (SE) (2)	*p*-value (3)	Coef. (SE) (4)	*p*-value (5)	Low-contact mean (SD) (6)	Coef. (SE) (7)	*p*-value (8)
A. Household characteristics								
Household size	4.60	-0.23	0.08	0.18	0.27	4.41	-0.03	0.88
	(1.36)	(0.13)		(0.17)		(1.31)	(0.20)	
Monthly HH income: Rs. 0-10,000	0.41	0.05	0.29	-0.08	0.20	0.45	0.02	0.78
	(0.49)	(0.05)		(0.06)		(0.50)	(0.06)	
Monthly HH income: Rs. 10,001-15,000	0.35	-0.01	0.87	0.09	0.14	0.35	-0.01	0.82
	(0.48)	(0.05)		(0.06)		(0.48)	(0.05)	
Monthly HH income: Rs. 15,001-20,000	0.12	0.01	0.71	0.04	0.34	0.13	-0.02	0.62
	(0.32)	(0.03)		(0.04)		(0.34)	(0.03)	
Monthly HH income: Rs. >20,000	0.12	-0.06	0.05	-0.05	0.12	0.06	0.01	0.76
	(0.33)	(0.03)		(0.03)		(0.25)	(0.04)	
Owns TV	0.81	0.02	0.62	-0.02	0.60	0.83	-0.03	0.57
	(0.39)	(0.04)		(0.05)		(0.38)	(0.06)	
Owns fridge	0.45	0.06	0.24	-0.09	0.16	0.49	0.08	0.22
	(0.50)	(0.05)		(0.06)		(0.50)	(0.06)	
Owns almirah	0.80	-0.01	0.73	0.00	>0.99	0.79	-0.01	0.79
	(0.40)	(0.04)		(0.05)		(0.41)	(0.05)	
Owns microwave	0.06	-0.03	0.18	-0.01	0.74	0.04	-0.02	0.39
	(0.25)	(0.02)		(0.02)		(0.21)	(0.02)	
B. Parent survey								
Mother's education: <10th standard	0.84	-0.01	0.80	0.07	0.13	0.83	-0.05	0.36
	(0.37)	(0.04)		(0.05)		(0.37)	(0.05)	
Father's education: <10th standard	0.77	-0.01	0.85	-0.08	0.13	0.73	0.07	0.25
	(0.42)	(0.04)		(0.06)		(0.44)	(0.06)	
Thermometer: Narendra Modi (0-100)	58.12	2.27	0.41	0.07	0.98	58.52	1.20	0.73
	(30.90)	(2.74)		(3.51)		(30.92)	(3.44)	
Thermometer: Mahatma Gandhi (0-100)	74.56	3.34	0.19	-2.18	0.50	77.83	-0.11	0.97
	(26.31)	(2.56)		(3.22)		(23.84)	(2.82)	
Trust: another religion	3.16	-0.09	0.20	-0.09	0.34	3.14	-0.13	0.09
	(0.70)	(0.07)		(0.09)		(0.75)	(0.07)	
Trust: another nationality	2.74	-0.11	0.19	-0.14	0.20	2.71	-0.13	0.33
	(0.83)	(0.08)		(0.11)		(0.86)	(0.13)	
Trust: other Indian states	2.91	-0.15	0.04	-0.22	0.03	2.82	-0.11	0.31
	(0.73)	(0.08)		(0.10)		(0.82)	(0.11)	
C. Child survey								
Age	14.62	0.08	0.59	0.14	0.43	14.67	0.11	0.59
	(1.47)	(0.15)		(0.18)		(1.34)	(0.19)	
Caste: General	0.50	-0.07	0.17	0.21	<0.01	0.45	-0.01	0.88
	(0.50)	(0.05)		(0.06)		(0.50)	(0.05)	
Caste: OBC	0.15	0.02	0.54	-0.12	0.01	0.17	0.05	0.24
	(0.36)	(0.03)		(0.04)		(0.38)	(0.04)	
Caste: SC	0.32	0.03	0.49	-0.09	0.08	0.32	0.00	0.95
	(0.47)	(0.04)		(0.05)		(0.47)	(0.05)	
Caste: ST	0.02	0.02	0.21	0.02	0.52	0.05	-0.03	0.17
	(0.15)	(0.02)		(0.03)		(0.22)	(0.02)	
Subject: arts	0.52	0.01	0.84	-0.01	0.90	0.51	0.02	0.77
	(0.50)	(0.05)		(0.06)		(0.50)	(0.08)	
Subject: commerce	0.09	0.00	0.90	-0.02	0.64	0.08	0.01	0.72
	(0.28)	(0.03)		(0.04)		(0.27)	(0.04)	
Subject: science	0.24	0.03	0.49	-0.03	0.66	0.30	-0.09	0.19
	(0.43)	(0.04)		(0.06)		(0.46)	(0.06)	
Subject: other	0.02	0.00	0.99	-0.01	0.63	0.01	0.04	0.10
	(0.13)	(0.01)		(0.02)		(0.08)	(0.02)	
News source: newspaper	0.22	-0.04	0.30	0.03	0.61	0.20	-0.06	0.28
	(0.41)	(0.04)		(0.05)		(0.40)	(0.05)	
News source: TV	0.63	0.01	0.78	0.01	0.89	0.67	-0.09	0.14
	(0.48)	(0.05)		(0.06)		(0.47)	(0.06)	
News source: social media	0.70	-0.09	0.05	-0.03	0.59	0.59	0.03	0.72
	(0.46)	(0.05)		(0.06)		(0.49)	(0.07)	
News source: word of mouth	0.61	0.07	0.13	0.09	0.13	0.68	0.00	0.91
	(0.49)	(0.05)		(0.06)		(0.47)	(0.04)	
Social media: Youtube	0.88	-0.04	0.28	-0.01	0.86	0.83	0.04	0.40
	(0.32)	(0.03)		(0.05)		(0.37)	(0.05)	
Social media: Facebook	0.63	-0.04	0.46	0.02	0.79	0.57	0.10	0.05
	(0.48)	(0.05)		(0.06)		(0.50)	(0.05)	

Table S3: (*continued*) Balance across experimental conditions

		Camps vs. control		Ritual vs. regular			High contact vs. low contact	
	Control mean (SD) (1)	Coef. (SE) (2)	*p*-value (3)	Coef. (SE) (4)	*p*-value (5)	Low-contact mean (SD) (6)	Coef. (SE) (7)	*p*-value (8)
Social media: WhatsApp	0.76	-0.04	0.34	-0.03	0.57	0.69	0.11	0.07
	(0.43)	(0.04)		(0.06)		(0.46)	(0.06)	
Social media: Instagram	0.42	-0.04	0.45	0.01	0.89	0.38	0.04	0.61
	(0.49)	(0.05)		(0.06)		(0.49)	(0.07)	
Social media: TikTok	0.05	-0.02	0.43	0.02	0.47	0.03	0.01	0.86
	(0.21)	(0.02)		(0.02)		(0.18)	(0.03)	
Social media: none	0.05	0.06	0.02	0.03	0.42	0.14	-0.07	0.07
	(0.22)	(0.03)		(0.04)		(0.35)	(0.04)	
Owns smartphone	0.33	-0.04	0.35	0.02	0.67	0.28	0.04	0.49
	(0.47)	(0.05)		(0.06)		(0.45)	(0.05)	
Previously attended camp	0.15	0.02	0.63	0.03	0.48	0.17	-0.05	0.28
	(0.36)	(0.04)		(0.05)		(0.38)	(0.04)	
Number of Hindu students in class (out of 10)	6.02	0.18	0.42	-0.03	0.91	6.03	0.16	0.54
	(2.88)	(0.23)		(0.29)		(2.86)	(0.25)	
Number of Muslim students in class (out of -10)	3.89	-0.17	0.46	-0.04	0.89	3.86	-0.06	0.80
	(2.93)	(0.23)		(0.29)		(2.84)	(0.25)	
Political system: strong leader (1-4)	2.46	0.04	0.66	-0.06	0.66	2.48	0.01	0.93
	(0.91)	(0.10)		(0.13)		(0.97)	(0.15)	
Political system: experts (1-4)	2.03	0.01	0.89	-0.03	0.79	2.10	-0.21	0.18
	(0.98)	(0.10)		(0.13)		(1.00)	(0.15)	
Political system: army (1-4)	2.16	-0.01	0.95	0.17	0.17	2.16	0.05	0.74
	(1.04)	(0.10)		(0.13)		(1.02)	(0.15)	
Political system: democracy (1-4)	1.70	0.06	0.46	-0.02	0.87	1.72	0.10	0.38
	(0.74)	(0.08)		(0.10)		(0.75)	(0.11)	
Political system: religious law (1-4)	3.01	0.01	0.94	0.02	0.85	3.04	-0.07	0.64
	(0.99)	(0.10)		(0.13)		(0.98)	(0.15)	
Thermometer: Narendra Modi (0-100)	57.49	1.52	0.35	-1.43	0.49	57.20	3.97	0.21
	(30.73)	(1.63)		(2.06)		(31.69)	(3.08)	
Thermometer: Mahatma Gandhi (0-100)	72.11	1.33	0.61	3.12	0.36	74.76	-2.41	0.53
	(26.48)	(2.62)		(3.43)		(27.67)	(3.76)	
Trust: another religion (1-4)	3.01	0.05	0.48	-0.03	0.78	3.05	0.06	0.51
	(0.71)	(0.07)		(0.09)		(0.74)	(0.09)	
Trust: another nationality (1-4)	2.63	0.07	0.39	-0.10	0.35	2.72	0.03	0.81
	(0.81)	(0.08)		(0.11)		(0.84)	(0.10)	
Trust: other Indian states (1-4)	2.80	0.05	0.54	-0.07	0.50	2.89	-0.10	0.27
	(0.76)	(0.08)		(0.10)		(0.76)	(0.09)	
Well-being: loneliness (0-10)	7.14	0.25	0.32	0.06	0.85	7.53	-0.50	0.15
	(2.57)	(0.25)		(0.31)		(2.30)	(0.33)	
Well-being: depression (0-24)	3.80	0.76	0.04	-0.17	0.71	4.37	0.12	0.82
	(3.76)	(0.37)		(0.47)		(3.74)	(0.53)	
Well-being: happiness (1-4)	2.53	-0.02	0.76	0.13	0.07	2.51	0.00	0.99
	(0.63)	(0.06)		(0.07)		(0.54)	(0.10)	
p-value from joint *F*-test			0.54		0.30			>0.99

Notes: Columns 3, 5, and 8 present p-values (for the treatment indicator) from separate OLS regressions of each baseline outcome on the treatment indicator and randomization strata fixed effects. Regressions in columns 2–5 employ robust standard errors; regressions in columns 7–8 employ standard errors clustered by team. The final row of the table reports the p-value from the F-test of a regression of the treatment indicator on all baseline variables displayed in the table. There were 172 boys in the control group; 240 in the camps group (120 in the regular camp, and 120 in the ritual camp); 156 campers were in the low-contact group, and 84 campers were in the high-contact group.

Table S4: No evidence of differential attrition in the endline survey

	Outcome: Attrition indicator		
	Experimental comparison:		
	Camps vs. control (1)	Ritual vs. regular (2)	High contact vs. low contact (3)
Panel A: Tests of differential attrition by treatment status			
Treatment	-0.017	-0.008	0.024
	(0.017)	(0.019)	(0.023)
Proportion of randomized subjects surveyed at endline	0.973	0.979	0.979
Strata FEs	Y	Y	Y
Panel B: Tests of patterns of differential attrition			
Treatment	-0.196	-0.117	1.317
	(0.565)	(0.556)	(1.051)
Baseline characteristics	Y	Y	Y
Baseline characteristics interacted with treatment	Y	Y	Y
Strata FEs	Y	Y	Y
p-value from joint F-test	0.23	0.78	>0.99

Notes: Panel A presents three separate OLS regressions of a binary attrition indicator (where 1 = attrited) on the treatment indicator and randomization strata fixed effects. Panel B presents three separate OLS regressions of the binary attrition indicator on the treatment indicator, all baseline covariates included in Appendix Table S3, the interactions of all those covariates with the treatment indicator, and randomization strata. The F-tests in Panel B test the hypothesis that the treatment and all baseline variables interacted with the treatment are jointly zero. Standard errors, shown in parentheses, are robust in columns 1 and 2, and are clustered by team in column 3.

Table S5: Estimated impact of camps on main outcomes: tabulated results

	Control mean (1)	ITT (2)	SE (3)	95% CIs (4)	*p*-value (5)	*N* (6)	Baseline dep. var (7)
A. Social preferences							
(i) Index (z-score)	0.00	0.19***	(0.07)	[0.05, 0.34]	0.01	401	N
(ii) Dictator game (stranger)	-2.37	3.41***	(1.30)	[0.86, 5.96]	0.01	401	N
(iii) Public goods game (stranger)	36.33	1.61	(1.47)	[-1.29, 4.50]	0.28	401	N
B. Willingness to interact							
(i) Index (z-score)	-0.01	0.30***	(0.07)	[0.15, 0.44]	0.00	379	Y
(ii) Number of outgroup friends	0.20	0.24***	(0.07)	[0.11, 0.38]	0.00	400	Y
(iii) Willingness to play	0.50	0.10**	(0.05)	[0.00, 0.21]	0.05	380	N
C. National identity							
(i) Index (z-score)	0.00	-0.05	(0.08)	[-0.20, 0.09]	0.47	401	N
(ii) More Indian than Hindu/Muslim	2.46	-0.13	(0.12)	[-0.37, 0.10]	0.27	401	N
(iii) Selected Indian flag magnet	0.79	0.00	(0.04)	[-0.08, 0.08]	0.97	401	N
D. Attitudes							
(i) Index (z-score)	0.00	0.04	(0.05)	[-0.06, 0.14]	0.41	401	N
(ii) Marry outgroup	0.22	0.03	(0.04)	[-0.06, 0.11]	0.55	401	N
(iii) Citizenship for outgroup	0.73	-0.01	(0.04)	[-0.10, 0.08]	0.81	401	N
(iv) Thermometer: Foreigners	50.07	-0.58	(2.24)	[-4.99, 3.83]	0.80	401	N
(v) Thermometer: Politicians	55.65	0.75	(1.41)	[-2.03, 3.53]	0.60	401	Y
(vi) Democracy best system	0.43	0.07	(0.05)	[-0.03, 0.17]	0.15	401	Y
E. Well-being							
(i) Index (z-score)	0.00	0.18***	(0.06)	[0.06, 0.30]	0.00	401	Y
(ii) Social life	7.86	0.47*	(0.27)	[-0.07, 1.00]	0.09	401	Y
(iii) Less depression	-4.43	0.67*	(0.34)	[0.00, 1.34]	0.05	401	Y
(iv) Happiness	2.65	0.12**	(0.05)	[0.02, 0.22]	0.02	401	Y

Notes: This table reports the effects of the camps on five families of outcomes. Each coefficient summarizes a separate regression of the outcome on (i) an indicator for assignment to either of the two camps, (ii) randomization strata, and (iii) where available, a baseline measure of the outcome variable. Each index is the unweighted average of all components (standardized) within a family of outcomes. All outcomes are from the endline survey, and their definitions are provided in Section 3.3. 95% confidence intervals are based on robust standard errors. Outcomes marked with stars are incentivized. *p<0.1; **p<0.05; ***p<0.01.

Table S6: Heterogeneous effects of camps by age group

	Outcome index (z-score):				
	Social preferences (1)	Willingness to interact (2)	National identity (3)	Attitudes (4)	Well-being (5)
Camp × Age 13-14	0.28***	0.30***	-0.07	0.10	0.07
	(0.11)	(0.11)	(0.10)	(0.07)	(0.08)
Camp × Age 15-18	0.10	0.31***	-0.05	-0.01	0.30***
	(0.10)	(0.10)	(0.11)	(0.08)	(0.10)
Age 13-14 vs. Age 15-18 effect, *p*-value	0.23	0.94	0.92	0.29	0.07
Control-mean difference: Age 15-18 − Age 13-14	0.13	-0.26	0.07	0.04	-0.25
N	401	379	401	401	401

Notes: This table reports the heterogeneous effects of camps by age, for the five main outcome indexes. We regress outcomes on camp × age-group indicators, the age-group indicator itself, baseline measures of the outcome variable where available, and randomization strata interacted with the age-group indicator. Robust standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table S7: Camp effects on outgroup friendships are driven mainly by friends made at camp

	Outcome: Outgroup friend...					
	Any (1)	From camp (2)	Not from camp (3)	From control group (4)	From ritual camp (5)	From regular camp (6)
Camper	0.24***	0.15***	0.08*	0.00		
	(0.07)	(0.04)	(0.05)	(0.02)		
Regular camp					0.00	0.23***
					(0.02)	(0.06)
Ritual camp					0.09**	-0.02
					(0.04)	(0.01)
N	400	400	400	400	400	400

Notes: This table presents treatment effects of the camps on outgroup friendships. Outcomes are the number of friends—out of the five closest friends mentioned by participants—with outgroup-sounding names, as coded by research assistants and the authors. The specific outcomes by column are: (1) total number of outgroup friends; (2) number of outgroup friends whose names match the name of another participant assigned to either of the camps; (3) number of outgroup friends whose names do not match the name of any participant assigned to either of the camps and does not match any control participant; (4) number of outgroup friends whose names match the name of a participant in the control group; (5) number of outgroup friends whose names match the name of a participant in the ritual camp; and (6) number of outgroup friends whose names match the name of a participant in the regular camp. Each regression controls for the number of outgroup friends mentioned by participants at baseline, a dummy variable indicating missingness in that baseline control, and randomization strata fixed effects. Robust standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table S8: Rituals do not improve intergroup relations: tabulated results

	Regular mean (1)	ITT (2)	SE (3)	95% CIs (4)	*p*-value (5)	*N* (6)	Baseline dep. var (7)
A. Social preferences							
(i) Index	0.000	0.030	(0.061)	[-0.09, 0.15]	0.623	235	N
(ii) *Dictator game (stranger)	0.299	0.964	(1.744)	[-2.47, 4.40]	0.581	235	N
(iii) *Dictator game (team)	-0.632	-1.561	(1.647)	[-4.81, 1.68]	0.344	235	N
(iv) *Dictator game (nonteam)	-0.786	0.788	(1.495)	[-2.16, 3.73]	0.599	235	N
(v) *Public goods game	38.932	-2.416	(1.794)	[-5.95, 1.12]	0.179	235	N
(vi) Do anything for campers	2.299	0.179**	(0.075)	[0.03, 0.33]	0.017	235	N
B. Willingness to interact							
(i) Index	-0.011	-0.072	(0.072)	[-0.21, 0.07]	0.322	220	Y
(ii) Number of outgroup friends	0.444	-0.052	(0.111)	[-0.27, 0.17]	0.640	234	Y
(iii) Camp friends	33.915	2.029	(3.166)	[-4.21, 8.27]	0.522	235	N
(iv) Team friends	2.709	-0.525*	(0.303)	[-1.12, 0.07]	0.085	235	N
(v) *Willingness to play	0.631	-0.051	(0.067)	[-0.18, 0.08]	0.443	221	N
C. Identity							
(i) Index	0.000	-0.055	(0.092)	[-0.24, 0.13]	0.549	235	N
(ii) More Indian than Hindu/Muslim	2.487	-0.253*	(0.153)	[-0.55, 0.05]	0.099	235	N
(iii) *Selected Indian flag magnet	0.761	0.052	(0.048)	[-0.04, 0.15]	0.283	235	N
D. Attitudes							
(i) Index	0.000	-0.093	(0.060)	[-0.21, 0.03]	0.123	235	N
(ii) Marry outgroup	0.239	0.016	(0.057)	[-0.10, 0.13]	0.772	235	N
(iii) Citizenship for outgroup	0.744	-0.031	(0.057)	[-0.14, 0.08]	0.588	235	N
(v) Thermometer: Politicians	56.598	-0.385	(1.883)	[-4.10, 3.33]	0.838	235	Y
(vi) Thermometer: Foreigners	52.218	-5.159*	(2.867)	[-10.81, 0.49]	0.073	235	N
(vii) Democracy best system	0.556	-0.104	(0.065)	[-0.23, 0.02]	0.111	235	Y
E. Wellbeing							
(i) Index	0.000	0.083	(0.078)	[-0.07, 0.24]	0.287	235	Y
(ii) Social life	8.179	0.264	(0.321)	[-0.37, 0.90]	0.412	235	Y
(iii) Less depression	-4.231	0.514	(0.432)	[-0.34, 1.37]	0.235	235	Y
(iv) Happiness	2.752	0.022	(0.059)	[-0.09, 0.14]	0.711	235	Y

Notes: This table reports the effects of the ritual-camp (versus the regular-camp) on five families of outcomes. Each row summarizes a separate OLS regression of the outcome on (i) an indicator for assignment to the ritual-camp, (ii) randomization strata, and (iii) where available, a baseline measure of the outcome variable. The sample includes only those assigned to the ritual- or regular-camp. Outcomes marked with stars are incentivized. Outcomes marked with plus signs are recorded for campers only. 95% confidence intervals are based on robust standard errors. *p<0.1; **p<0.05; ***p<0.01.

Table S9: The effects of intergroup contact: tabulated results

	Control mean (1)	ITT (2)	SE (3)	95% CIs (4)	*p*-value (5)	*N* (6)	Baseline dep. var (7)
(i) Dictator game (stranger)	1.10	-0.86	(1.85)	[-4.69, 2.97]	0.65	235	N
(ii) Number of outgroup friends	0.31	0.32**	(0.12)	[0.07, 0.56]	0.01	234	Y
(iii) Willingness to play	0.64	-0.11*	(0.06)	[-0.23, 0.01]	0.07	221	N
(iv) National identity index	0.00	-0.21**	(0.09)	[-0.41, -0.02]	0.03	235	N
(v) Attitudes index	0.00	0.07	(0.06)	[-0.05, 0.19]	0.22	235	N
(vi) Well-being index	0.00	0.02	(0.07)	[-0.13, 0.17]	0.77	235	Y

Notes: This table reports the effects of high (versus low) intergroup contact on our main outcomes, tabulating the results visualized in Figure 8. We regress outcomes on an indicator for assignment to high (within-team) contact, randomization strata, and, where available, a baseline measure of the outcome variable. The regressions include only participants randomly assigned to a camp. The outcomes parallel those in Figure 1, with two exceptions: (i) we exclude the public goods game since high contact mechanically affects the form of the game (high-contact participants play the game with more outgroup members), and (ii) we break up the willingness to interact index into its components, to unmask the opposite effects of contact on each. Full outcome variable definitions are provided in Section 3.3. 95% confidence intervals are based on team-clustered standard errors (with 24 teams). *p<0.1; **p<0.05; ***p<0.01.

Table S10: No evidence of imbalance in baseline covariates for the analysis of the impacts of attending lecture days at the camp

		Number of lecture days attended	
	Variable mean (SD) (1)	Coef. (SE) (2)	*p*-value (3)
A. Household characteristics			
Household size	4.38	-0.15	0.59
	(1.31)	(0.28)	
Monthly HH income: Rs. 0-10,000	0.46	-0.05	0.65
	(0.50)	(0.11)	
Monthly HH income: Rs. 10,001-15,000	0.35	0.04	0.68
	(0.48)	(0.10)	
Monthly HH income: Rs. 15,001-20,000	0.13	0.00	0.99
	(0.34)	(0.10)	
Monthly HH income: Rs. >20,000	0.07	0.01	0.82
	(0.25)	(0.04)	
Owns TV	0.82	-0.02	0.70
	(0.38)	(0.06)	
Owns fridge	0.51	0.06	0.60
	(0.50)	(0.11)	
Owns almirah	0.78	0.01	0.94
	(0.41)	(0.08)	
Owns microwave	0.04	0.03	0.65
	(0.19)	(0.06)	
B. Parent survey			
Mother's education: <10th standard	0.81	0.03	0.76
	(0.39)	(0.09)	
Father's education: <10th standard	0.75	0.02	0.85
	(0.43)	(0.10)	
Thermometer: Narendra Modi (0-100)	59.65	-7.66	0.26
	(31.03)	(6.78)	
Thermometer: Mahatma Gandhi (0-100)	77.65	-5.74	0.37
	(24.89)	(6.40)	
Trust: another religion	3.09	-0.03	0.87
	(0.75)	(0.16)	
Trust: another nationality	2.65	-0.09	0.64
	(0.87)	(0.19)	
Trust: other Indian states	2.78	0.10	0.58
	(0.81)	(0.19)	
C. Child survey			
Age	14.71	0.24	0.47
	(1.40)	(0.33)	
Caste: General	0.44	0.01	0.93
	(0.50)	(0.11)	
Caste: OBC	0.18	0.05	0.59
	(0.38)	(0.09)	
Caste: SC	0.34	-0.05	0.66
	(0.47)	(0.11)	
Caste: ST	0.04	-0.01	0.87
	(0.20)	(0.05)	
Subject: arts	0.52	-0.13	0.26
	(0.50)	(0.11)	
Subject: commerce	0.08	0.05	0.52
	(0.28)	(0.08)	
Subject: science	0.27	0.06	0.50
	(0.45)	(0.09)	
Subject: other	0.02	-0.02	0.32
	(0.14)	(0.02)	
News source: newspaper	0.18	-0.04	0.62
	(0.38)	(0.09)	
News source: TV	0.64	-0.10	0.40
	(0.48)	(0.11)	
News source: social media	0.60	-0.05	0.68
	(0.49)	(0.11)	
News source: word of mouth	0.68	0.01	0.93
	(0.47)	(0.11)	
Social media: Youtube	0.85	0.06	0.45
	(0.36)	(0.08)	
Social media: Facebook	0.60	0.03	0.77

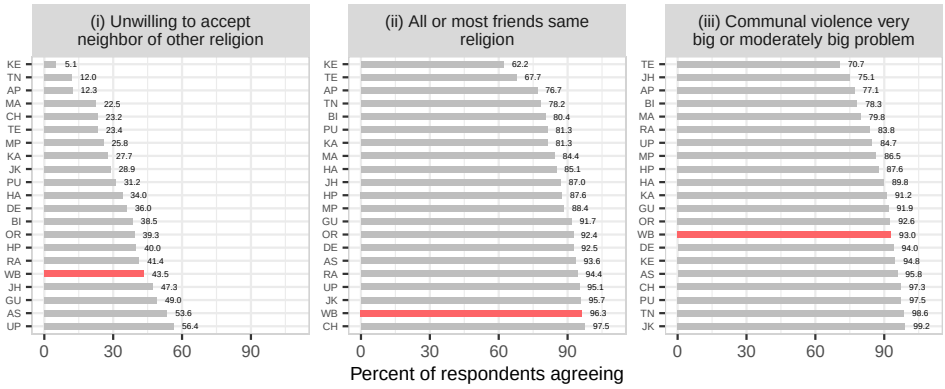
Table S10: (*continued*) Balance in effects of programming analysis

		Number of lecture days attended	
	Variable mean (SD) (1)	Coef. (SE) (2)	*p*-value (3)
	(0.49)	(0.11)	
Social media: WhatsApp	0.72	0.03	0.76
	(0.45)	(0.10)	
Social media: Instagram	0.39	-0.02	0.83
	(0.49)	(0.11)	
Social media: TikTok	0.03	0.00	0.94
	(0.18)	(0.05)	
Social media: none	0.12	-0.05	0.41
	(0.32)	(0.06)	
Owns smartphone	0.29	0.08	0.44
	(0.45)	(0.10)	
Previously attended camp	0.16	-0.02	0.83
	(0.37)	(0.10)	
Number of Hindu students in class (out of 10)	6.19	-0.49	0.40
	(2.81)	(0.58)	
Number of Muslim students in class (out of -10)	3.73	0.48	0.41
	(2.79)	(0.59)	
Political system: strong leader (1-4)	2.50	0.24	0.26
	(1.01)	(0.22)	
Political system: experts (1-4)	2.03	-0.26	0.25
	(0.99)	(0.22)	
Political system: army (1-4)	2.17	0.36	0.07
	(1.00)	(0.20)	
Political system: democracy (1-4)	1.76	0.05	0.71
	(0.77)	(0.14)	
Political system: religious law (1-4)	3.01	0.02	0.94
	(0.98)	(0.24)	
Thermometer: Narendra Modi (0-100)	59.08	-9.25	0.19
	(30.44)	(7.05)	
Thermometer: Mahatma Gandhi (0-100)	73.65	-10.54	0.08
	(27.08)	(6.06)	
Trust: another religion (1-4)	3.07	0.01	0.97
	(0.70)	(0.15)	
Trust: another nationality (1-4)	2.72	-0.09	0.64
	(0.84)	(0.19)	
Trust: other Indian states (1-4)	2.85	-0.26	0.06
	(0.78)	(0.14)	
Well-being: loneliness (0-10)	7.35	0.19	0.76
	(2.38)	(0.61)	
Well-being: depression (0-24)	4.45	-0.82	0.27
	(3.70)	(0.74)	
Well-being: happiness (1-4)	2.51	-0.02	0.86
	(0.56)	(0.13)	
p-value from joint *F*-test			0.99

Notes: Column 1 shows the mean and standard deviation for the baseline variable among respondents assigned to the camps condition. For columns 2–3, each row represents a separate regression in which we regress the baseline covariate on the number of lecture days the camper attended and the total number of camp days the camper attended (excluding the first and last days of the camp). Columns 2 and 3 show the estimate, robust standard error, and p-value associated with the coefficient for number of lecture days attended. In the final row we run an OLS regression of the number of lecture days the camper attended on the total number of camp days the camper attended (excluding the first and last day of the camp) and all baseline variables displayed in the table. We report the F-test for the joint hypotheses that all the coefficients associated with the baseline variables are zero in that regression. There are 240 observations.

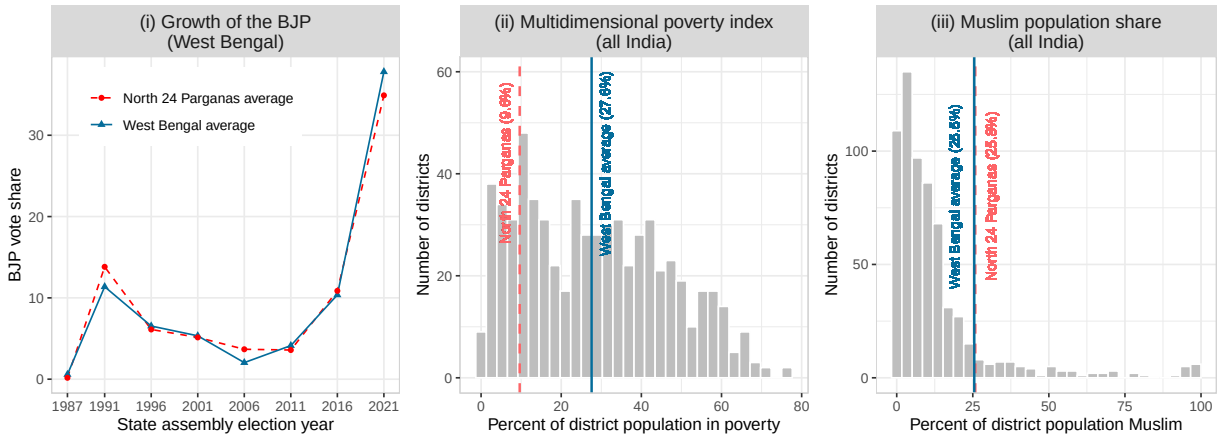
B Supplementary Figures

Figure S1: Inter-religious group tensions are high in West Bengal



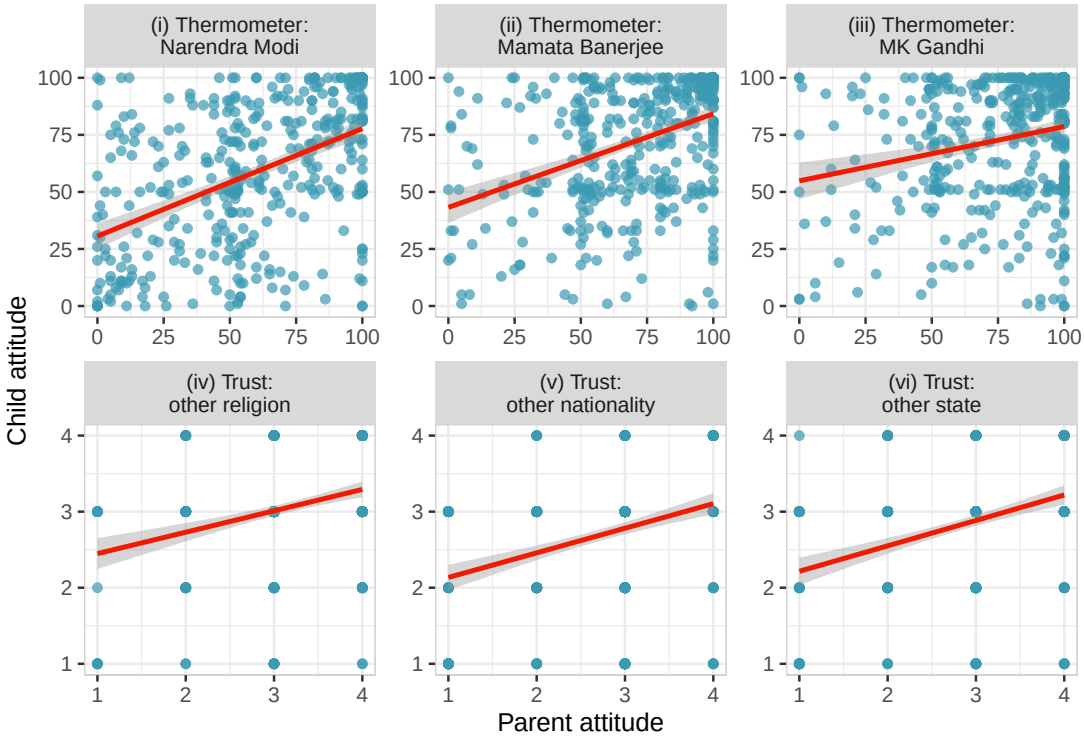
Notes: This figure plots descriptive data from the Pew "Religion in India" survey, fielded nationwide between 2019 and 2020. Individual responses are averaged by state/union territory. Only Hindu and Muslim respondents are included in the averages (N = 26,058). Only states or union territories in which at least 100 people were interviewed are included. Red bars denote the state of West Bengal ("WB"). The question wordings, response options, and recodes by panel are: (i) "Would you be willing to accept a [Hindu/Muslim] as a neighbor?" (Yes = 1; No = 0; Other/Both/Neither/Depends = 0); (ii) "How many of your close friends are [Hindu/Muslim]" (All of them = 1; Most of them = 1; Some of them = 0; Hardly any of them = 0; None of them = 0); (iii) "Now I am going to read you a list of things that may be problems in India. As I read each one, please tell me if you think it is a (Very big problem = 1; Moderately big problem = 1; Small problem = 0; Not a problem at all = 0): Communal violence."

Figure S2: Characteristics of North 24 Parganas district, West Bengal



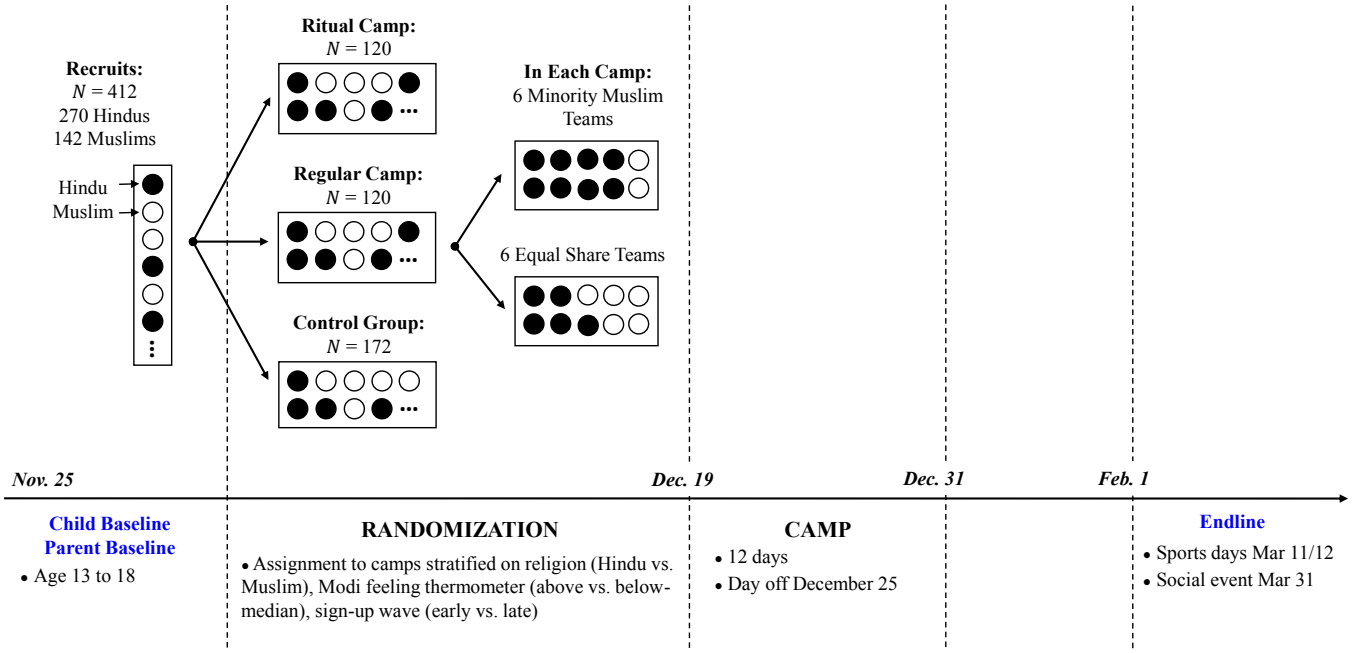
Notes: Panel (i) presents the average performance of the BJP in state assembly elections in West Bengal between 1987 and 2021; data are from the Trivedi Center for Political Data at Ashoka University. Panel (ii) plots a histogram of multidimensional poverty for all Indian districts, using the district-level index created by NITI Ayog and based on the fourth round of the National Family Heath Survey (2015–16). Panel (iii) plots a histogram of the Muslim population share of Indian districts based on the 2011 Census of India.

Figure S3: Comparison of parent and child attitudes at baseline



Notes: This figure plots attitudinal data from the baseline survey, comparing answers to six questions asked to both the parent and the child.

Figure S4: Project timeline and randomization



Note: This figure describes the timeline of the experiment and the randomization design. The dotted vertical lines refer to different time periods, while text under the dark bold horizontal line describes the particular activity that was undertaken during that period. After the endline survey, we organized a sports day with all the participants (campers and control), and also held a social event to implement choices that the participants made with regards to their willingness to pay to interact with outgroup members.



(a) National Anthem: Ritual



(b) National Anthem: Regular



(c) Dancing: Ritual



(d) Dancing: Regular



(e) Mock Election: Ritual

Figure S5: Rituals and Placebos

Notes: This figure shows images from various camp activities. For each activity, we include an image from the ritual camp as well as the regular camp. For mock elections, we only include an image from the ritual camp where the boys stood in a line (like in polling booths), submitted their ballots and received an electoral ink on their finger. In the regular camp, the boys simply submitted their ballots to the camp organizers while sitting in their lecture seats.

Figure S6: Daily camp measurement card (English translation; the original is in Bengali)

Name:_____

Roll number:_____

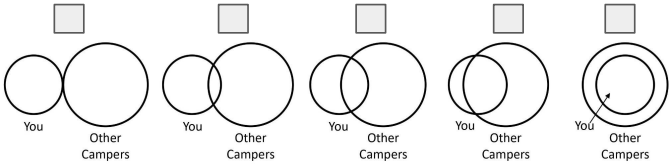
1. Which picture best describes your emotions at the camp today?

TICK ONE BOX



2. Which picture best describes your relationship with the other boys at the camp today?

TICK ONE BOX



3. How many of your teammates do you consider to be close friends? **CIRCLE ONE NUMBER**

0 1 2 3 4 5 6 7 8 9

4. How bored or excited did you feel during the camp today? **CIRCLE ONE NUMBER**

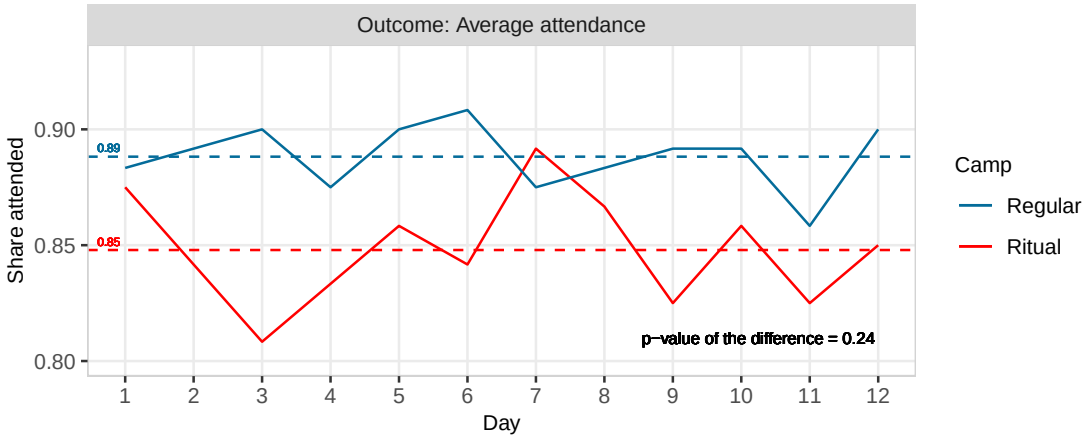
1 2 3 4 5 6 7 8 9 10

= VERY BORED **= VERY EXCITED**

Figure S7: Badge designs

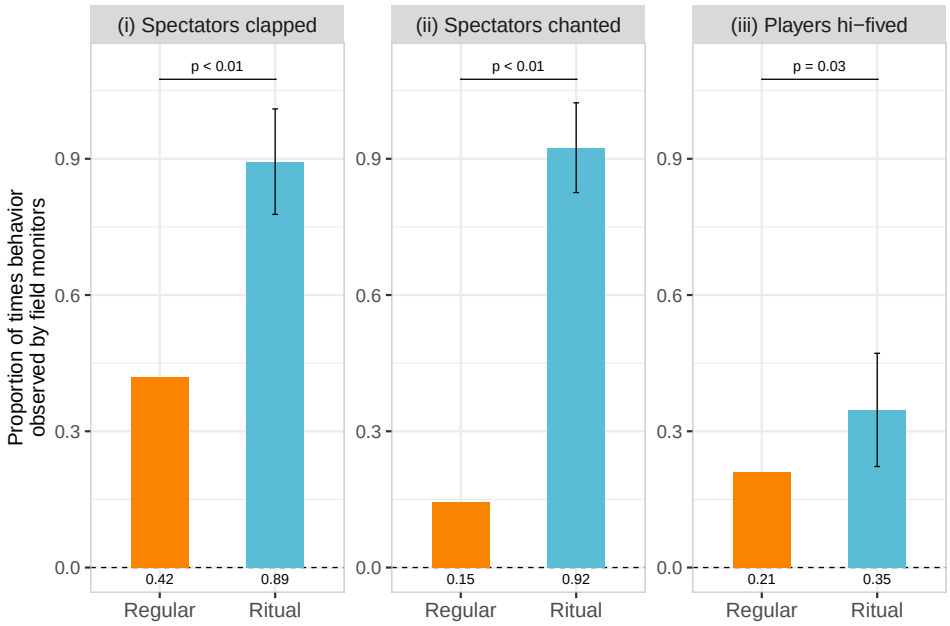


Figure S8: Camp attendance was high and did not significantly differ across the two camps



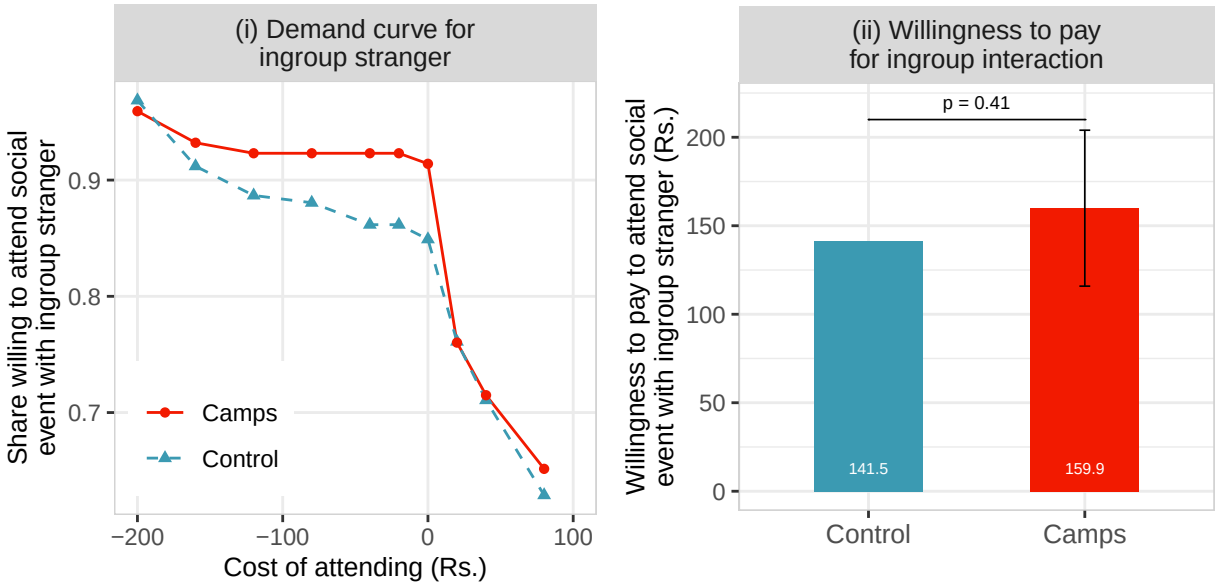
Notes: The reported *p*-value is from a regression of the number of days attended by each camper on an indicator for assignment to the ritual camp, using strata fixed effects and robust standard errors.

Figure S9: Sports rituals were faithfully adhered to in the Ritual camp



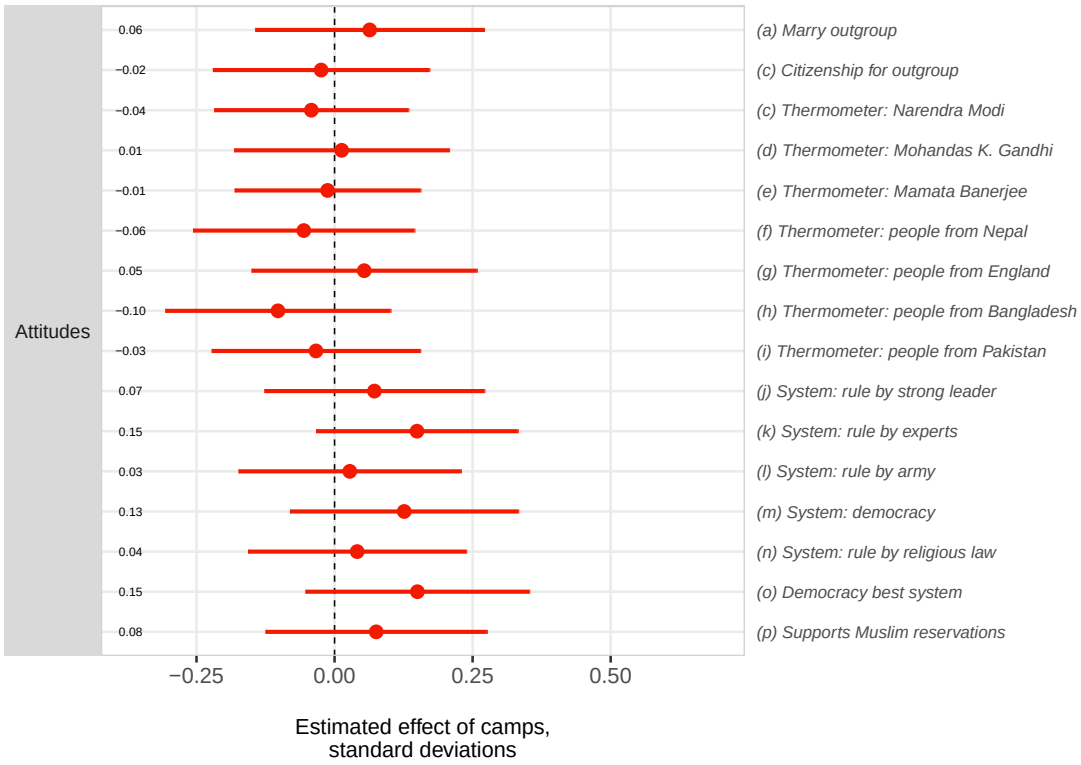
Note: Bars marked "Regular" present raw means in the regular camp. Levels for the "Ritual" bars are obtained by adding the treatment effect obtained from OLS regressions that include day and field-monitor fixed effects. 95% confidence intervals and p-values are derived from robust standard errors.

Figure S10: Camps do not increase willingness to play with ingroup strangers



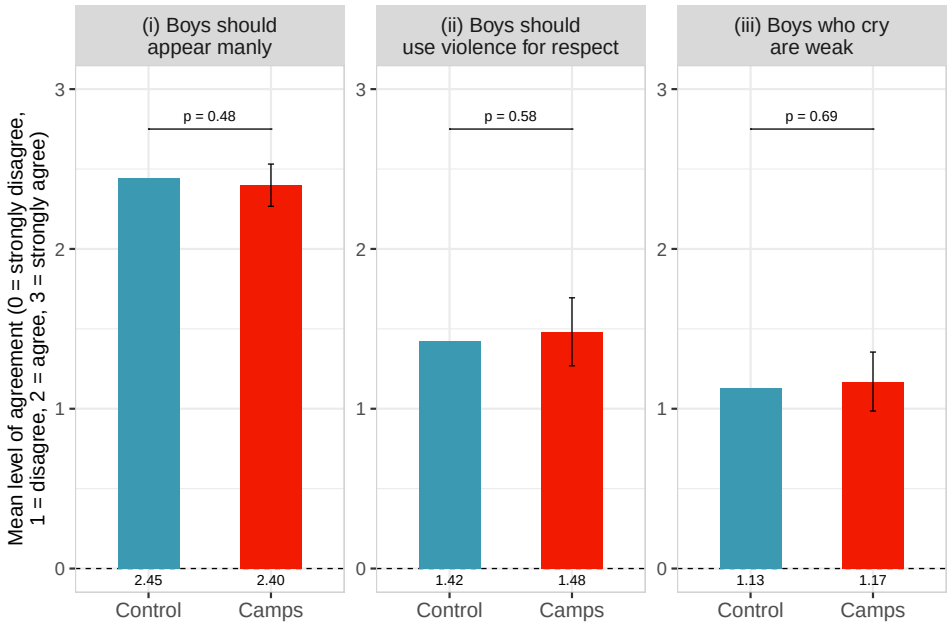
Note: Panel (i) plots demand curves for social interaction with ingroup strangers separately for control participants ($N = 159$) and for those assigned to either of the two camps ($N = 221$). As an example, the bottom-right point of the control group demand curve tells us that roughly 63% of control participants said that they would be willing to attend the social event with an ingroup stranger as their partner with a cost of attending of Rs. 80. Panel (ii) summarizes the results of a tobit regression of willingness to pay to play with the ingroup (with censoring at -200 and 80) on an indicator for camp assignment, along with randomization strata fixed effects. The 95% confidence interval and p-value for the difference are derived from robust standard errors.

Figure S11: Camps did not impact attitudes



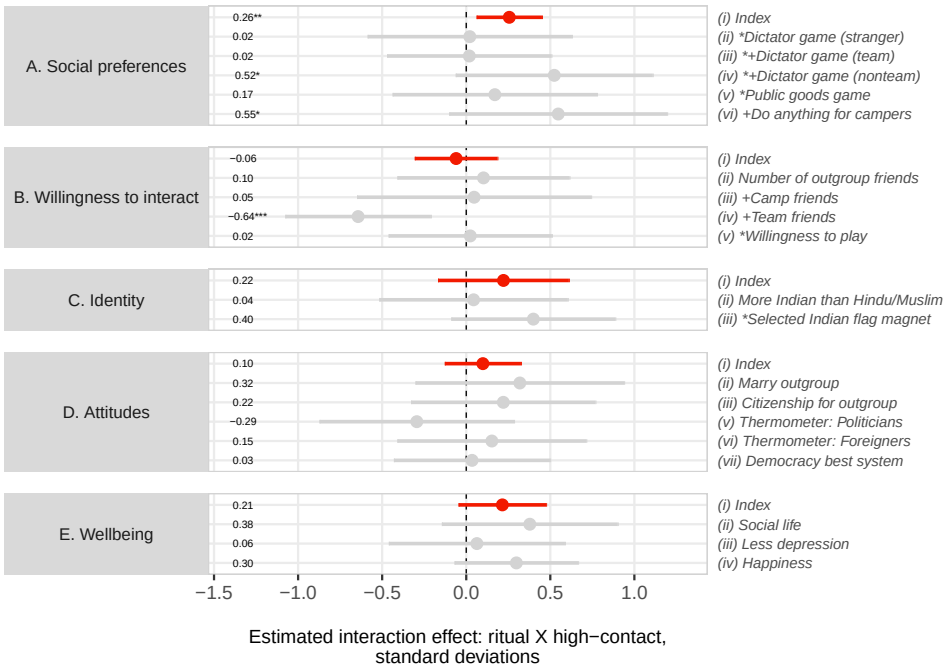
Notes: This figure plots the effects of the camps on a range of attitudinal outcomes. Each coefficient plot summarizes a separate OLS regression of the outcome on (i) an indicator for assignment to either of the two camps, (ii) randomization strata, and (iii) where available, a baseline measure of the outcome variable. Each component is a z-score. All components are from the endline survey. 95% confidence intervals are derived from robust standard errors. Coefficient magnitudes and statistical significance for the treatment indicator are displayed on the left-hand side of the plot: *p<0.1; **p<0.05; ***p<0.01.

Figure S12: Camps do not affect attitudes about masculinity



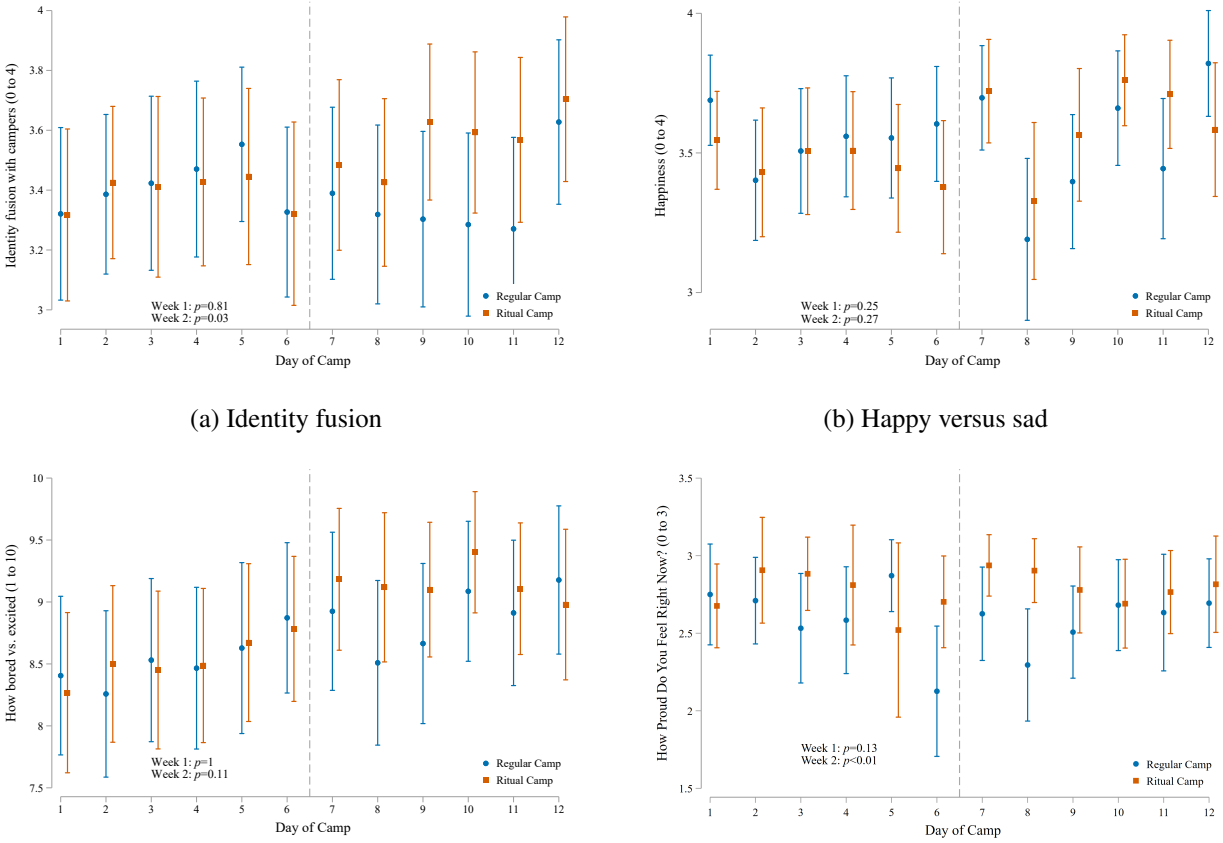
Note: This figure shows the camps treatment effect on boys' self-reported attitudes about masculinity. The question wordings were: "We are now going to ask you three questions about how you think boys should behave. To what extent do you agree or disagree with the following statements: (i) Boys should try to appear manly in almost all situations; (ii) Boys should use violence to get respect if necessary; (iii) Boys who cry are weak." Bars marked "Control" present raw control-group means. Levels for the "Camps" bars are obtained by adding the treatment effect obtained from OLS regressions that include randomization strata fixed effects. 95% confidence intervals and p-values are derived from robust standard errors.

Figure S13: Evidence of complementarity between contact and rituals



Note: This figure plots the interaction effect between the ritual treatment and the high-contact treatment on five families of outcomes. Each coefficient plot summarizes a separate OLS regression of the outcome on (i) indicators for assignment to the ritual-camp, assignment to a high-contact team, and the interaction of those indicators, (ii) randomization strata, and (iii) where available, a baseline measure of the outcome variable. The sample includes only those assigned to the ritual- or regular-camp. 95% confidence intervals are derived from team-clustered standard errors. Outcomes marked with stars are incentivized. Outcomes marked with plus signs are recorded for campers only. Coefficient magnitudes and statistical significance are indicated on the left-hand side of the plot: *p<0.1; **p<0.05; ***p<0.01.

Figure S14: Effects of rituals during the camp



(a) Identity fusion

(b) Happy versus sad

(c) Bored versus excited

(d) Proud

Note: The four panels visualize the means and 95% confidence intervals of four outcomes, by day and by camp. Each panel's estimates come from a single regression of the outcome on (i) camp type fully interacted with day dummies and (ii) randomization strata, with standard errors clustered at the individual-level. The p-values test for the difference between ritual- and regular-campers during the first and second week of the camps. The outcomes for panels (a) to (c) are from the daily question cards answered by all campers, while the outcome for panel (d) is from the daily survey answered by six randomly selected boys per camp. The survey questions for each panel are: (a) Which picture best describes your relationship with the other boys at the camp today? 0 = self and other-campers circles apart to 4 = self and other-campers circles fully overlapping, (b) Which picture best describes your emotions at the camp today? 0 = very sad emoji to 4 = very happy emoji, (c) How bored or excited did you feel during the camp today? 1 = very bored to 10 = very excited, and (d) How proud do you feel right now? 0 = not at all, 1 = somewhat, 2 = moderately so, 3 = very much so.

C Supplementary Text

C.1 Camp pledge

We are a group of brothers who have come together to learn, play games, and develop new skills. By doing these things, we plan to understand ourselves, each other, and our country better. We pledge to support and honor each other inside and outside the camp. We will respect people who disagree. We won't fight. We will listen to our teachers and follow their instructions. We will ask questions when we're confused. We will work, we will have fun, and we will grow. We are in this together!

C.2 Relation to AEA pre-registration

We pre-registered the experiment in the AEA RCT Registry (AEARCTR-0010661) on December 18, 2022, one day before the two camps began. Our sampling, experiment design, and analysis specifications are exactly as we pre-specified. We explain minor deviations with respect to outcome variables here, including (i) justification for dropping two primary outcome measures, and (ii) justification for which components we include in our main outcome families, both relevant to Figure 1. For our pre-registered secondary outcomes, we do not report exhaustive analysis of these outcomes in the main text; instead, we analyze the outcomes that help to narrow down mechanisms for the overall effects of the camps.

Willingness to plank. We pre-registered "willingness to plank" as one measure of social preferences. A plank is a core strength exercise which involves holding a position similar to a push-up for as long as possible. During the endline survey, we asked each boy to plank twice, with a gap in between. One plank was a plank for others: for every five seconds the boy held the plank, he earned Rs. 10 to be divided among eight other boys (four Muslims and four Hindus, randomly chosen from non-teammates at the same camp for campers, and from control participants for non-campers). The other plank was a plank for self: for every five seconds the boy held the plank, they earned Rs. 0, 2, or 10 (randomly assigned) for themselves. The boys did the plank for others and plank for self in random order.

We included the plank measure to test whether campers are willing to endure more physical comfort (and hold the plank for longer) to help other campers, than control participants, who are planking for boys that they didn't camp with. The key test would then be whether campers plank for others longer than control participants, with the plank for self serving as a placebo check (e.g., to check that campers are not more physically fit as a result of the sports played at the camp).

The cross-randomization of monetary incentives for the plank for self was to serve two purposes: (i) as a sanity check that plank time is responsive to incentives, and (ii) as an input into behavioral structural estimation, allowing us to interpret the treatment effect in terms of a change to a boy's altruism parameter (following DellaVigna and Pope (2018) and DellaVigna et al. (2022)). Ex-post we find that the plank for self is barely affected by even large monetary incentives (Table S11). We find evidence for fatigue: boys plank for themselves roughly 13 seconds less (15%) when randomly assigned to do the plank for self after having done the plank for others. We cannot reject null effects of monetary incentives, whether comparing each of the three incentive groups to each other (column 1) or assuming linear effects in the incentive size (column 2). Most surprisingly, we estimate a statistically insignificant increase of eight seconds (9%) of going from zero cash incentive to Rs. 10—a meaningful increase in incentives, giving a mean payout of roughly Rs. 180, or over half a typical daily wage. Anecdotally, this lack of responsiveness comes from the fact that the boys were motivated to plank for as long as possible even if no money was at stake.

Ex-ante, we would expect the effect of the camps on the plank for others time to be much smaller than that of the effect of a Rs. 10 incentive on the plank for self time. In some sense, the effect of the Rs. 10 incentive is an upper bound of the effects of camps—for the camps to increase the plank for others time by the same amount, it would be as if control participants have no altruism towards their plank partners (playing the game as if their plank partners were earning no money), with campers having perfect altruism towards their plank partners (treating the Rs. 10 earned by their plank partners as utility-enhancing as Rs. 10 earned by themselves). Given that (i) any camp effects would be plausibly meaningfully smaller than the effects of the Rs. 10 incentive, and (ii) we do not detect effects of the Rs. 10 incentive, we drop the plank measures from the main analysis.

For completeness, we show the effects of the camps in column 3 of Table S11. We estimate an imprecise null effect, with the confidence interval of -10 to 8 including the point estimate for the effect of the Rs. 10 incentive.

Proud to be Indian. We pre-registered the question "How proud are you to be Indian?" as one component of national identity. Boys could answer (i) not at all proud, (ii) not very proud, (iii) quite proud, or (iv) very proud. 91% answered very proud, and 9% answered quite proud. Give the lack of variation, we drop this component from the main analysis. Inclusion of this component would not change our core conclusion of a null effect of the camps on identity—for the dummy variable equal to one if the boy responded "very proud," we estimate an effect of -2.1 percentage points of the camp ($p = 0.48$).

Table S11: Effects on plank performance

	Plank Self (1)	Plank Self (2)	Plank Others (3)
Plank for others first (0/1)	-12.57***	-12.56***	16.15***
	(4.23)	(4.23)	(4.42)
Plank for self incentive = Rs. 2 (0/1)	3.27		
	(4.91)		
Plank for self incentive = Rs. 10 (0/1)	7.83		
	(5.34)		
Plank for self incentive (Rs.)		0.72	
		(0.51)	
Camper			-1.30
			(4.52)
Observations	401	401	401
Outcome mean	84.2	84.2	78.8
p(Rs. 2 = Rs. 10)	.39		
Randomization strata FE	No	No	Yes

Notes: Standard errors are robust. The outcome is the number of seconds planked for self in columns 1 and 2, and the number of seconds planked for others in column 3. *** p<0.01, ** p<0.05, * p<0.1.

Justification for exclusion of other components for main families of outcomes.

- For "Thermometer: Foreigners," we keep only the thermometer variables that can be coded as related to opinions toward the religious outgroup in other countries (Nepal for Muslims, Bangladesh and Pakistan for Hindus). While we don't include the thermometer score for the English as a component in our main index, we discuss the effects of the camp on this outcome in a footnote in Section 4.5.
- For "Thermometer: Politicians," we keep only the two politicians with clear directional predictions: Gandhi (we would predict positive effects given the lectures on Indian independence) and Modi (we would predict negative effects). While we exclude Mamata Banerjee, we report effects on this outcome in the main text in Section 4.5.
- We exclude our measure of support for reservation for Muslims (0 = Strongly disagree to 3 = Strongly agree) from the index since we only have a clear directional prediction (positive) for Hindus. Consistent with our null effects of the camps on attitudes, we do not see an effect of camp assignment on support for reservation for Muslims among Hindus (Camp coefficient = 0.006, $p = 0.95$).
- We exclude political issue importance because with hindsight, there are no clear directional predictions of the effects of camps. For issue importance, respondents ranked four issues from most to least important—welfare programmes for the poor, harmony between communities, corruption, and employment opportunities. While "harmony between communities" relates directly to the purpose of our integrative camps, it is not clear whether we should expect campers to consider harmony more important (because the camps remind them of the importance of good Hindu-Muslim relations) or less important (because the camps make them think Hindu-Muslim relations are good enough already that they do not need to be prioritized). Ex post we estimate an effect of -3 percentage points ($p = 0.44$) of camp assignment on ranking harmony between communities as the most important issue (20% of control participants rank it first).

C.3 Lecture overviews

- **Lecture 1 (Day 2)** – Democracy and Elections: History of Democracy: Growth and Development; Forms of Democracy; Modern use of Democracy; Elections as Means of Exercising Democracy: History and Forms; Modern Practices and Breaches in Practice.

- **Lecture 2 (Day 7)** – Role of Bengal in India's Freedom Struggle: Battle of Plassey and Buxar; Role of Bengal in Indian Nationalism; Bengal Leadership in Congress; Rise of Radicalism in Bengal; Bengali Literature as a form of Protest; Partition and effect on Bengal. In this lecture, we also delved into Rabindranath Tagore's rakhi ritual. Tagore initiated this symbolic act, urging both Hindus and Muslims in Bengal to tie rakhis on one another. This gesture was a poignant protest against the British government's proposed partition of the country. We then organized this in the ritual camp.

- **Lecture 3 (Day 11)** – India's Freedom Struggle: First War of Independence of 1857; Nationalism in India; Establishment of Indian National Congress – Moderates and Radicals; Establishment of Muslim League; Gandhian Era, Subhas Chandra Bose and the Indian National Army (INA); Independence and Partition.