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Creating Cohesive Communities: A Youth Camp Experiment in India

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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 Hindu and Muslim adolescent boys, from West Bengal, India, to 2-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 synchronous dancing) 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. Campers continue to have twice as many outgroup friends than control participants 1 year after the camps ended. Meanwhile, additional camp elements have heterogeneous effects: rituals have more positive impacts for the Hindu majority than the Muslim minority, while higher intergroup contact backfires among Hindus but not Muslims. Our findings demonstrate that inclusive youth camps may be a powerful tool for bridging deep social divides. Yet, we also highlight the conceptual challenges in crafting optimal integrative camps that help all groups.

Key words: Social cohesion, Intergroup contact, Collective rituals, Youth camps, Hindu-Muslim relations, Prejudice reduction, Mental health, Field experiment

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

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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 (Bandiera *et al.*, 2019; Alan *et al.*, 2021; 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 training for the *hombo batu*: jumping over a 2-m-high monolith to signal courage and commitment to the tribe (Fitri and Purba, 2023). By shaping children's behaviours, 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 behaviours 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 common and have long been used to shape children's moral character—for good and ill, and 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, 2003); 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. Seeds of Peace has run annual summer camps for Arab and Israeli teenagers since 1993, using dialogue sessions, group tasks, and community action to "challenge preconceptions … and envision a peaceful Middle East."[3] Three-week orientation camps are the starting point for cadets in Nigeria's National Youth Service Corps, a post-war reconciliation and nation-building program begun in 1973 that accepts 200,000 recruits annually (Okunogbe, 2024).

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; Grady *et al.*, 2023). 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 (Dhar *et al.*, 2022). 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

1. "World organization of the scout movement," www.scout.org; and "Why diversity, equity, and inclusion matters at camp," American Camp Association, www.acacamps.org.

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. See www.usip.org. Further, according to a 2004 survey, 39% of teenagers in the U.S. had attended a religious summer camp (Smith and Denton, 2009, 54).

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 colourful 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 an in-person follow-up survey, 6 weeks after the camps had concluded, with a focus on social preferences, willingness to interact with outgroup members, national self-identification, attitudes, and psychological well-being. We administered a shorter phone survey 12–13 months after the camps' completion.

We begin by considering the outcomes of camper and control participants 6 weeks after the camps ended. For the two families of outcomes looking at behaviours, we find strong evidence that camps improve 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 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 ($p = 0.05$). Taken alongside the dictator game result, this demonstrates that the camps induced positive behaviours toward members of religious outgroups who had nothing to do with the study itself.

Beyond social preferences and behaviours, the camps were psychologically beneficial. Well-being outcomes are important to consider since, in principle, social engineering of the kind attempted by our camps might come at a cost—causing participants to feel disoriented or uneasy. Instead, we identify still further positive impacts. 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 6-week endline.

We find no measurable impacts of camps on self-reported attitudes. Camp assignment did not affect attitudes toward intermarriage or the granting of citizenship to outgroup members, 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. This finding is consonant with recent prejudice-reduction interventions shown to shape behaviours but not attitudes (Paluck *et al.*, 2021; Clochard, 2024).

Importantly, experimenter demand effects are unlikely to drive the positive effects of camps. When asked to guess why we ran the study, only 5% of respondents mentioned intergroup relations as a reason. Most instead cited talent scouting or offering opportunities to underprivileged

children. The finding of treatment effects for behaviours but not self-reported attitudes also indicates that demand effects are unlikely.

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. We also shed light on mechanisms by exploring treatment effect heterogeneity by religion. Four 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 excitement—but only during the camps themselves, and with the effects concentrated in the camps' second week ($p = 0.02$ for the difference in ritual effects between Weeks 1 and 2, for an index outcome capturing positive experiences). However, 6-week endline outcomes in the ritual camp are statistically indistinguishable from those in the regular camp for all four outcome indexes concerning intergroup relations. This overall null effect is precisely estimated; we can reject positive effects of rituals on an omnibus outcome index of 0.06σ. 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 contradicts findings on rituals' (mostly immediate-term) impacts in psychology and anthropology (reviewed in Xygalatas, 2022). Rituals did, however, have some differential effects according to majority versus minority group status, which we discuss below.

Second, intergroup contact appears to explain the camps' effects on outgroup friendships at endline (a 0.42σ effect of high versus low contact, $p = 0.03$), but not effects on other behaviours. In fact, greater contact backfired. Campers assigned to teams with more outgroup members are significantly less likely to endorse an inclusive national identity than those assigned to low-contact teams (-0.21σ, $p = 0.03$). There is also evidence that high contact reduces willingness to interact with an outgroup stranger (-0.22σ, $p = 0.07$). The negative contact effects we observe are the first of their kind in the experimental literature on collaborative contact (Paluck *et al.*, 2019; Clochard, 2024).

Third, we find 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 preferences index, indicating that the camp curriculum helped convince campers of the merits of social inclusion.

Fourth, we examine heterogeneity in effects according to children's religion. The overall impact of camps (versus no camps) are quite consistent, having salutary impacts on Hindu and Muslim children. However, this consistency masks significant heterogeneity in the effects of the camps' two randomized subcomponents—rituals, and intergroup contact—which offset one another by pushing in opposite directions.

Rituals have key positive impacts on Hindu boys: increasing prosociality toward outgroup (versus ingroup) strangers in the dictator game (0.33σ, $p = 0.03$), increasing well-being (0.18σ, $p = 0.09$), and boosting willingness to "do anything for other campers" (0.23σ, $p = 0.02$). Simultaneously, rituals have negative impacts on Muslim boys: reducing social preferences toward outgroups (-0.36σ, $p = 0.04$), diminishing willingness to play with outgroup strangers (-0.46σ, $p = 0.05$), and causing lower attendance at the camps (1.67 fewer days, $p = 0.01$). We find no signs that the negative reactions among Muslims are due to the (inclusive)

nationalistic content of 2 of the 20 rituals.[4] The tendency of rituals to rouse the majority group (Hindus) but alienate the minority (Muslims) is consistent with a framework in which rituals are more fulfilling—giving rise to greater "collective effervescence" and thus more positive camp experiences—when performed with ingroups.

The pattern of heterogeneity for high-intensity contact is almost wholly reversed. Being in high outgroup contact teams has a slate of negative effects for Hindus (on social preferences, willingness to interact, national identity, and attendance), and more positive effects among Muslims (notably on outgroup friendships). These effects are consistent with the idea that unusually high exposure to Muslims in 50:50 teams triggers perceptions of outgroup threat for Hindus (Enos, 2016).

The results described so far are based on our 6-week endline. By the standards of the existing literature on intergroup contact, this already qualifies as a relatively extended follow-up period. However, we go much further by conducting a second endline 12–13 months after the camps' completion. This, we document, is longer than the longest measurement taken by any existing field experiment in the contact literature. More than 1 year after the camps concluded, 73% of campers report maintaining contact with children they met at camp. Campers have twice as many outgroup friendships than control participants ($p = 0.004$), meaning that the strong positive impact of the camps on outgroup friendships almost entirely persists. Moreover, 59% of that effect is due to outgroup friends made *after* the first endline. Thus, campers not only maintained existing friendships but also expanded their social networks to include new outgroup individuals.

To sum up, camps mold social preferences, increase willingness to interact with outgroup members (durably so), and increase psychological well-being. Various features of real-world camps contribute to the aggregate treatment effects. Yet, the impact of these additional features varies by religion: rituals are more harmful for the minority, while contact backfires more for the majority. 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 hint that a 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 investigate the effects of rituals on real-world intergroup relations for the first time—picking up on Durkheim's (1912) famous hypothesis, and inspired by correlational and lab experimental claims that rituals promote cooperation and shared identity (Sosis and Ruffle, 2003; Wiltermuth and Heath, 2009; Chwe, 2013; Xygalatas *et al.*, 2013).[5] We study a "secular" ritual treatment that is highly intensive and placebo-controlled. It includes 20 distinct rituals, with some having symbolic meaning, and with many repeated daily. The growth in rituals' efficacy on emotions that we observe during camps illuminates a dynamic effect of rituals that studies of one-shot rituals are unable to detect. In our setting, the precisely estimated null effect of rituals on intergroup relations overall appears to stem from a split in majority and minority group responses to collective rituals, which is also a novel finding. In line with non-experimental studies on the positive effects of extreme rituals (Xygalatas *et al.*, 2019), we find evidence of positive effects of rituals on psychological well-being for the majority Hindu group; rituals also make the majority more prosocial. But the tendency of rituals to alienate the minority group speaks to the limits of collective rituals in forging social solidarity.

4. At a time of rising Hindu nationalism in India, one worry may be that symbols of the Indian nation may now be regarded as majoritarian symbols by the minority group.

5. Recently, Butinda *et al.* (2023) have shown 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.

Next, 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 influenced 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 measure aggressive behaviour, 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] By designing the camps from the ground up, we go substantially beyond a typical program evaluation, opening up the black box of a socialization institution that is pervasive worldwide.

Our camps, which embed rituals and civic programming, are fundamentally set apart from existing contact interventions. Still, we make two optimistic contributions 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 behaviours towards outgroup strangers (Scacco and Warren, 2018; Mousa, 2020), our camps' effects on social preferences and willingness to interact generalize. Furthermore, effects of the camps on intergroup friendships fully persist over 1 year later—no other pre-registered study on intergroup contact measures outcomes beyond 6 months. On the more pessimistic side, we are the first to find negative effects of additional collaborative intergroup contact (Paluck *et al.*, 2019; Clochard, 2024), revising downward somewhat our confidence in team-based contact interventions.

2. CONTEXT

2.1. *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.*, 2024). Muslims live disproportionately in India's towns and cities, where they are residentially segregated and victims of periodic communal conflict (Wilkinson, 2006; Adukia *et al.*, 2022). Hindu–Muslim divisions lower firm output (Ghosh, 2025). 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).

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 (Bagues and Roth, 2023; Okunogbe, 2024), radio propaganda (Blouin and Mukand, 2019), and state-led language and education policies (Miguel, 2004; Carlitz *et al.*, 2022).

2.2. *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.[8] These localized incidents mirror high 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 neighbour 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 led provocative processions through Muslim neighbourhoods 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.[9] 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 neighbourhoods within Barasat.

2.3. *Youth camps and ideas of India*

Camps have been central to elite attempts to instil 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.[10] *Shakhas* are early-morning training sessions held in parade grounds across the country.

8. 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*, 4 July 2017). This created "fertile ground for religious polarization" (Amit Bhardwaj, "How the ghost of the Baduria-Basirhat communal riots is polarising polls in West Bengal," *Caravan Magazine*, 16 April 2021).

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

10. Lauren Frayer and Furkan Latif Khan, "The powerful group shaping the rise of Hindu nationalism In India," *National Public Radio*.

The gatherings, which are male-only,[11] 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 5–100 participants, who are disproportionately secondary school and college students.[12] 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 politicized … into the ideology of Hindutva."[13] 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.[14] Established in 2006, its declared mission was to empower those facing socio-economic, political, and cultural deprivation. But it was also seen as an extremist group set up to counter the RSS.[15] The PFI recruited boys aged 15 and above. Like the RSS, participants wore uniforms and performed drills in public spaces.[16] Allegations that the PFI was organizing camps that radicalized Muslim youth culminated in the group being banned by the Government of India.[17]

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 labour 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 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.[18]

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.

11. The RSS has a "sister" organization, the Rashtra Sevika Samiti, for women.
12. 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).
13. Manini Chatterjee, "Repackaging the RSS," *Indian Express*, 16 March 2003.
14. "PFI ban: What is the Popular Front of India and why has India outlawed it?" *BBC News*, 28 September 2022.
15. Bismee Taskin, "Eighteen & disenchanted—why a college student, now a successful Delhi lawyer, joined PFI," *Print*, 29 September 2022.
16. Ramesh Babu, "The story of Popular Front of India and reason behind its growth," *Hindu*, 29 January 2020.
17. "India bans Muslim group PFI for alleged 'terror' links," *Al Jazeera*, 28 September 2002
18. Shweta Padmanaban, "Bridging the gaps … through ultimate frisbee," *Medium*, 6 June 2015.

3. EXPERIMENT DESIGN AND INTERVENTION

3.1. *Sampling*

The recruitment stage took 1 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 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 (see replication package for survey instruments). The consent script stated that the purpose of the study was "to understand how participating in youth activities camps shapes the behaviours and attitudes of male adolescents."[19] 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 enrolment. The 412 boys who attended one of these "randomization days" were then entered into the final sample to be randomized.

Two pieces of evidence speak against sample selection concerns. For one thing, at the first endline, only 5% of respondents guessed that we ran the camps to study intergroup relations. Most instead thought the study was a form of talent scouting or a means of bringing opportunity to disadvantaged children. We would worry more about selection if the group relations aspect was salient in the minds of recruits since this could deter more prejudiced households from participating. Second, we characterize one stage of selection directly in Appendix Table S5: we compare those who completed the baseline but did versus did not attend their randomization day. There are few differences between the two groups, indicating that the experimental sample is not selected on observables. Boys in the experimental sample actually report higher support for Prime Minister Modi ($p = 0.05$), suggesting that the sampling process did not disproportionately include boys predisposed against Hindutva ideology.

3.1.1. Summary statistics. Among the experimental sample, 75% of parents report a monthly household income between Rs. 5,000 and 15,000, or roughly 2–6 USD per day. Muslim and Hindu parents report average Narendra Modi feeling thermometer scores of 45/100 and 66/100, respectively. Muslim 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).

While we recruited boys aged 13–18, our recruits skew young: 73% are aged 13–15, with the remaining 27% aged 16–18. 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.

19. Ethical considerations are discussed in Appendix Section C.2.

3.2. *Treatment conditions*

3.2.1. 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 theatre workshop and performance. A meal was provided every day. Campers met for four hours per day, for a total of 48 h of activities (see Appendix Table S2 for the camp schedule).
(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 the first wave of endline surveys was fully completed. We invited both the campers and the control participants to attend. At the first endline, we asked participants in the control group how often they did different activities during the 2 weeks of the camps. The three most common responses were playing sports (84% answered "Many times"), taking part in religious activities (34% answered "Many times"), and doing school work (27% answered "Many times").

3.2.2. 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.4), soccer coaches supervised stretching and exercises and refereed the matches, dance instructors taught dance moves, and finally street theatre artists performed a play and ran a theatre workshop. The same set of instructors conducted the activities in both camps, keeping the content and messaging of the activities identical.

3.2.3. 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 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: the extent of contact with other campers and teammates, the information communicated, and the approximate length of the activities. However, it is important to note that the rituals introduce distinct aspects that are not mirrored by the placebo activities: they require coordination among groups, use repetition for emphasis, incorporate symbolism to enrich the conveyed message, and create a sense of synchrony among participants.

3.2.4. 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*

3.3.1. Measures taken during the camps. We administered daily measures during the camps. 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).

3.3.2. Six-week endline. We administered the first endline between 4 and 7 weeks after the camps had concluded, with the median respondent completing the survey 5.9 weeks later. Surveyors revisited the experimental sample at their homes. The 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.[20] 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

20. Ten local research assistants coded the 140 unique first names of control participants as either definitely Hindu, probably Hindu, probably Muslim, definitely Muslim, or cannot 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.

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

(continued)

TABLE 1
Continued

Ritual	Placebo	Concepts
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 honour: At the end of the match, losing team forms a "corridor" (guard of honour) 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 instil 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 theatre 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. Theatre 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

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.[21] 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. 0–50).

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.[22] 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 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.

21. 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.

22. 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.

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 [Hindu/Muslim] when you're older? and (ii) would you support giving Indian citizenship to a [Hindu/Muslim] immigrant? Muslims are asked about marrying and granting citizenship to Hindus, and vice versa for Hindus. 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.

3.3.3. One-year phone endline. We re-contacted participants by phone 12–13 months after the camps' conclusion. This endline is a longer-term endline than in any existing field experiment on intergroup contact, according to a recent meta-analysis (Clochard, 2024, Appendix Figure S8).[23] The only two papers in that meta-analysis with longer-term endlines are natural experiments (Camargo *et al.*, 2010; Okunogbe, 2024). Furthermore, among pre-registered studies like ours, our long-term endline is twice as long as those of the two nearest papers (Mousa, 2020 and Maiti *et al.*, 2022 both have 6-month endlines).

The phone surveying medium meant that we could only field a short instrument, to prevent young participants from quickly becoming bored. Thus, we focused on three measures from the first endline that were both straightforward to administer and represented the spectrum of study concepts of interest: the number of close outgroup friendships, self-reported national identity, and the three dimensions of psychological well-being. We updated our pre-registration accordingly, with two of the measures pre-registered as primary outcomes: outgroup friendships and

23. Appendix Figure S8 uses the 44 papers in the not-yet-public 2024 version of Clochard's meta-analysis (the paper is not public, but the data are public on Harvard Dataverse). Our conclusions here are the same when considering the smaller sample of 37 papers included in Clochard's public 2022 working paper, but we prefer to use the more comprehensive set of studies.

psychological well-being. 81% of the 412 participating children completed the second endline. The median second endline was completed 379 days following the end of the camps.

3.3.4. Pre-registration. We pre-registered the experiment in the AEA RCT Registry (AEARCTR-0010661) 1 day before the two camps began. We updated the pre-registration with the details of the second endline after administering the second endline to roughly 50 participants. We explain minor deviations from the pre-registration in Appendix C.3.

3.4. *Internal validity*

3.4.1. Compliance. As shown in Appendix Figure S9, 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]

3.4.2. 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 S10 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.

3.4.3. 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 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, respectively. 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.

24. We do find heterogeneous effects by religion of intergroup contact and rituals on camp attendance—we describe these findings in Section 6.

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 analysing the effects of camps in this regression, as in equation (1), we use robust standard errors.

Finally, to analyse 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 × 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 describes how camps shape intergroup relations and psychological well-being 6 weeks after the end of the camps. 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 behavioural changes, we estimate null effects on national identity and attitudes.[26] We can reject positive effects on national identity and attitudes of 0.09σ and 0.14σ respectively, 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

25. We promote psychological well-being to a main outcome to emphasize a result with potentially important implications: youth camps have surprisingly large 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 S8 shows limited evidence in support: the effect on attitudes for those of below-median age is 0.1σ, while for the above-median it is -0.01σ ($p = 0.29$ for the difference). Otherwise, this heterogeneity analysis suggests that camps increased social preferences more for younger campers, and well-being more for older campers.

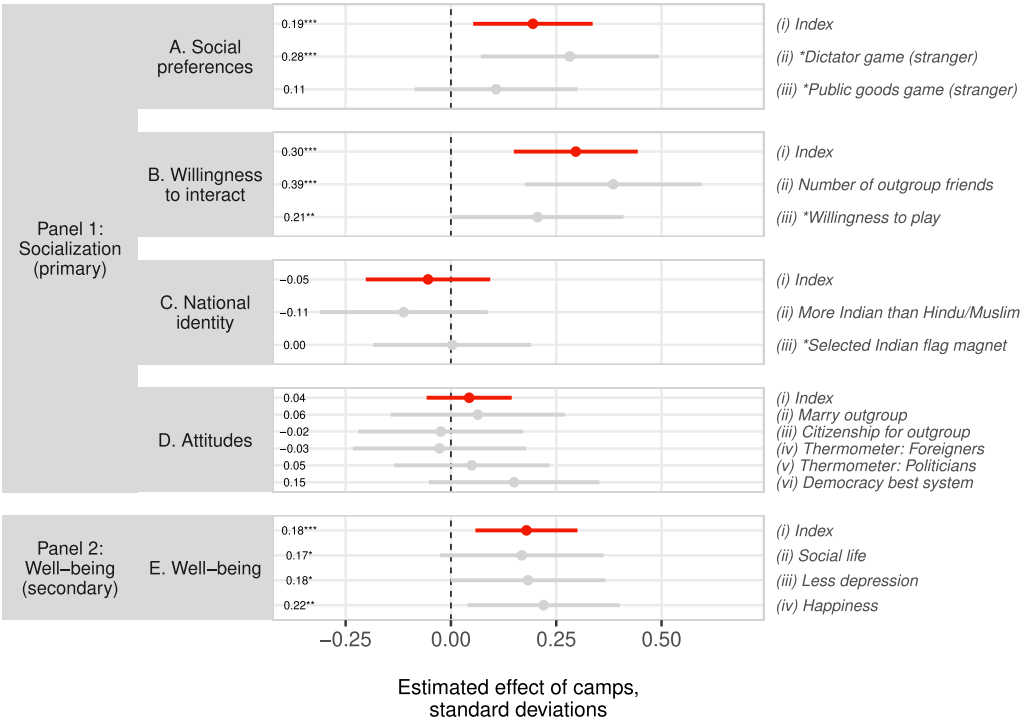

FIGURE 1
Estimated effects of camps on main outcomes

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, centred 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.3 for full details along with explanations for minor deviations from the pre-registration). All components are from the 6-week 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 S6.

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.

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

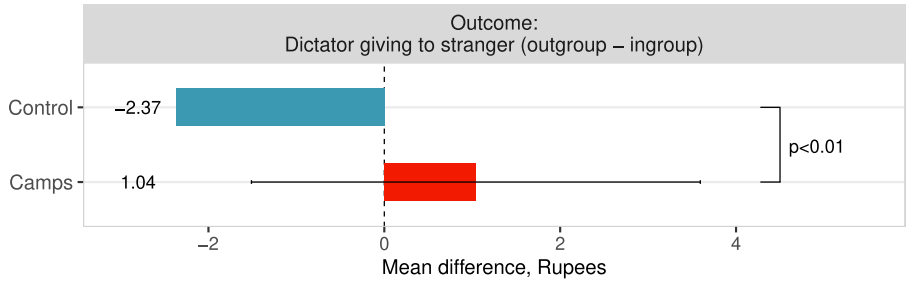

FIGURE 2
Impact of camps on ingroup bias in dictator giving

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.

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).

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) 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

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

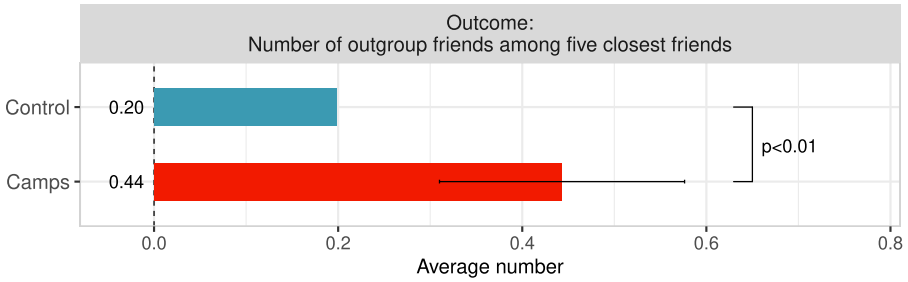

FIGURE 3
Camps more than double the number of close outgroup friendships

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.

team reflect the *level* of altruism toward, and beliefs about, teammates. Camps reduce bias in social preferences without 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 S9), 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 first endline was administered 4–7 weeks after the last day of the camp, the friendships formed by the camp far outlast the camp itself. This is despite

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 S9, Columns 5 and 6).

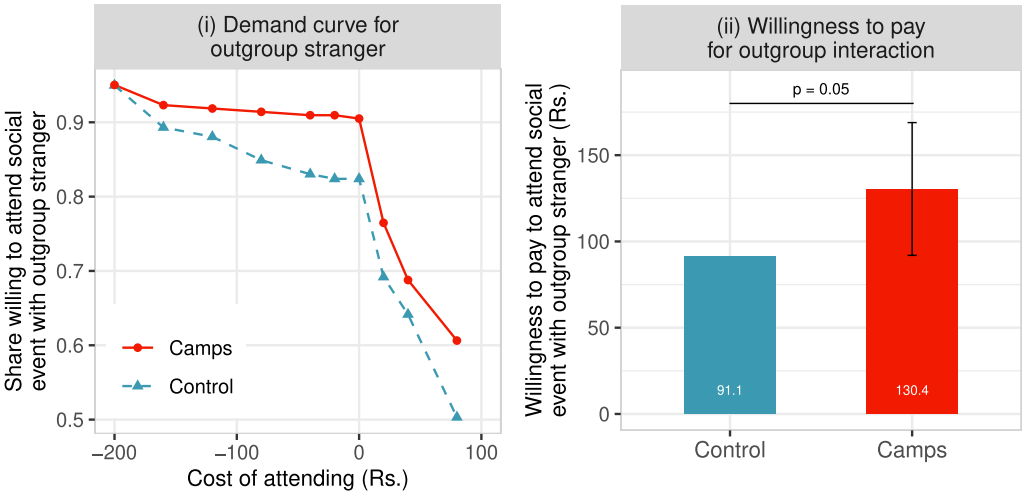

FIGURE 4
Camps increase willingness to play with outgroup strangers

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 fact that campers would not automatically see each other following the intervention: they were 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. Even more striking, we show in Section 7 that these effects on outgroup friendships persist over 1 year later.

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.

50% of control participants are willing to pay our highest price (Rs. 80) to attend the event with an outgroup stranger, 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)

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.

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 on willingness to accept).

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 S11). 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 S11, 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 and attitudes*

Our positive effects on social preferences and willingness to interact demonstrate generalized behaviour change: campers now behave differently towards outgroup members that they have never met. A separate question is whether the camps lead to changes in more abstract attitudes—like identifying with one's nation rather than one's religion, and views toward intermarriage and granting citizenship to outgroup immigrants.

We do not see evidence of broader attitude change. 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).[32] The camps do not change intergroup attitudes either: 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 ($p = 0.81$).[33] With statistically insignificant effects on the other attitude components,[34] we find a statistically insignificant effect of camps on the overall index too (Figure 1).

The pattern of behavioural change without attitudinal change echoes the results of other experiments on prejudice reduction (Paluck *et al.*, 2021) and intergroup contact (Clochard, 2024). Tentatively, we speculate that more direct messaging—for example, through lectures on the importance of intergroup tolerance—may be necessary to move attitudes.[35]

32. 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.

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

34. For example, the camps have no effect on thermometer feelings toward the politicians we ask about: Narendra Modi, Mahatma Gandhi, and Mamata Banerjee (Appendix Figure S12). 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. Also reassuringly, we estimate null effects on masculinity attitudes (Appendix Figure S13).

35. That would align with the value-shaping educational intervention of Dhar *et al.* (2022), which shaped attitudes more than behaviours. Importantly, the lectures in our camps did not *explicitly* urge campers to be more tolerant

4.5. *Psychological well-being*

Our first four families of outcomes look at different facets of cohesive communities. Social engineering might achieve cohesion, but only at the cost of psychological well-being—if intergroup interactions are, for example, anxiety-inducing (Stephan and Stephan, 1985). On the other hand, 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 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 the camps were not explicitly designed to improve well-being, and given that participants reported their well-being 4–7 weeks after the camp's final day.[36]

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).

5. MECHANISMS

Why does camp attendance improve intergroup behaviours and increase psychological well-being? In this section, we explore the mediating role of rituals, intergroup contact, and civic programming modules. We find that each of these factors influences different outcomes. Rituals primarily exert short-term emotional impacts during the camps, and enhance the well-being of the majority Hindu participants during the 6-week endline. Intergroup interactions promote the formation of friendships across different groups, whereas the civic modules contribute to the improvement in social preferences.

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 (Xygalatas *et al.*, 2013; Norton and Gino, 2014). Rituals may thus explain some of the

(Appendix C.4). 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.

36. 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).

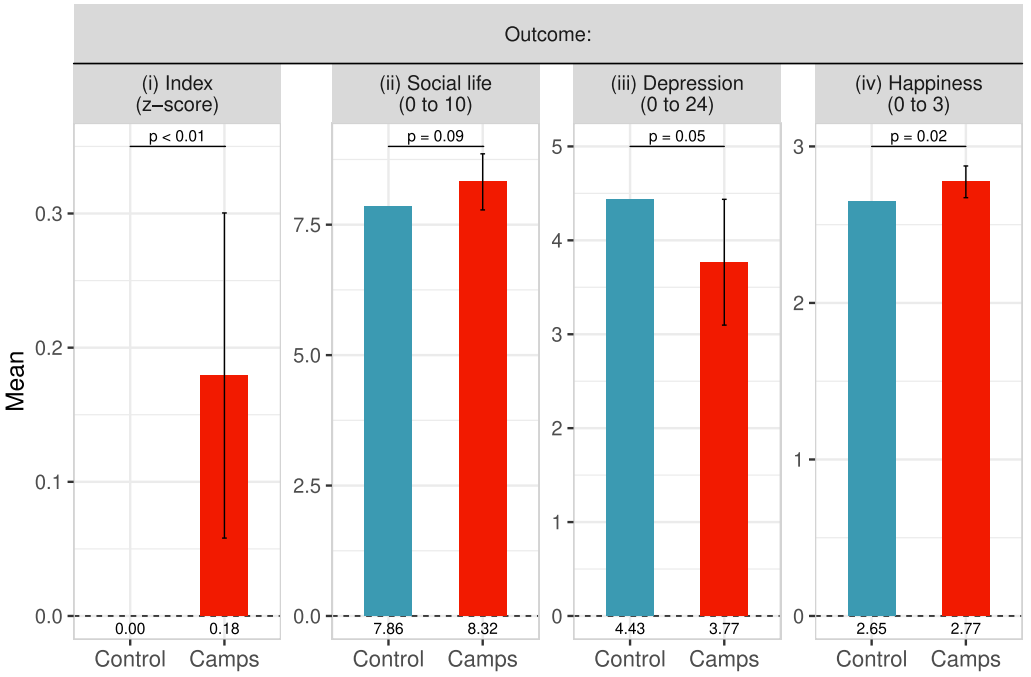

FIGURE 5
Camps increase psychological well-being

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 (centred and standardized using the variables' control-group means and standard deviations; the depression score is reversed when entered into the index). 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. $N = 401$ in all models. The 95% confidence intervals and p-values are derived from robust standard errors.

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).

5.1.1. 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, as displayed in Table 2, rituals significantly intensify camp experiences in the camps' second week. Ritual-campers report stronger shared identity with other campers in Week 2 (0.19σ, $p = 0.04$), higher excitement (0.13σ, $p = 0.08$), and higher values on an index summarizing positive experiences (0.12σ, $p < 0.08$). The effects of rituals between weeks 1 and 2 are statistically different at the 0.05 level for three of the five outcomes in Table 2. The growth in impact we

TABLE 2
Effect of ritual camps on daily measurement outcomes, by week

	Component (z-score):				
	Relationship other boys (1)	Emotions (2)	Excited (3)	Close friends (4)	Index (5)
Ritual × Week 1	−0.03	−0.15	0.01	−0.11	−0.07
	(0.09)	(0.13)	(0.10)	(0.09)	(0.08)
Ritual × Week 2	0.19**	0.13	0.13*	0.05	0.12*
	(0.09)	(0.12)	(0.08)	(0.09)	(0.07)
Ritual × Week 1 versus Ritual × Week 2, *p*-value	<0.01	0.06	0.16	0.04	<0.01
N	2,512	2,521	2,524	2,526	2,488

Notes: The sample includes only the boys randomly assigned to the camps. The outcomes for Columns 1 to 4 cover all four questions from the daily question cards answered by all campers. The survey questions for each column are: (1) 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, (2) Which picture best describes your emotions at the camp today? 0 = very sad emoji to 4 = very happy emoji, (3) How bored or excited did you feel during the camp today? 1 = very bored to 10 = very excited, and (4) How many of your teammates do you consider to be close friends? (0 to 9). All outcomes are z-scores (centred and standardized using the variables' regular-camp-group means and standard deviations), and the index in Column 5 is the unweighted average of the z-scores in Columns 1 to 4. We regress each outcome on ritual camp × week indicators, the week indicator itself, and randomization strata interacted with the week indicator. Standard errors clustered by participant are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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.

5.1.2. Effects of rituals: at endline. Rituals increase excitement and shared identity with other campers in the immediate term, but do they lead to lasting 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. The point estimate of the effect of rituals on an omnibus index (combining the five indexed outcomes from Panels A to E) is −0.018 with a standard error of 0.038. The ex post minimum detectable effect size (MDE) on this combined index is 0.1σ (2.8×0.037), which suggests that we are well-powered to detect reasonably small effects.[37] Nevertheless, we do see some changes in individual sub-components. 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 2 weeks. It is clear that despite fostering a stronger camp-identity, rituals do not improve broader intergroup relations in the full sample. Rituals do, however, have positive effects on Hindu boys

37. Note that our overall sample size is similar to well-published experimental studies on intergroup contact, such as Broockman and Kalla (2016) ($N = 501$, *Science*), Mousa, 2020 ($N = 183$ to 459, *Science*), Dahl *et al.* (2021) ($N = 522$ to 657, *Quarterly Journal of Economics*), and Corno *et al.* (2022) ($N = 499$, *American Economic Review*).

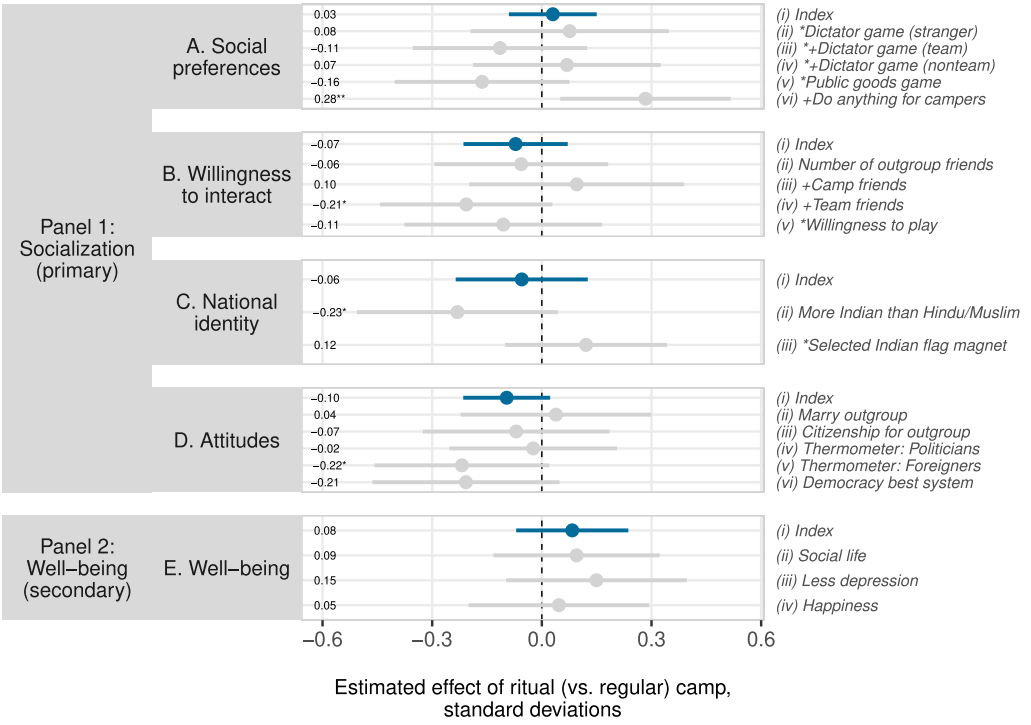

FIGURE 6
Rituals do not improve intergroup relations

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, centred and standardized using the variable's regular-camp mean and standard deviation. All components are from the first 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 2 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 S10.

including on their well-being at endline—something discussed in more detail when we explore treatment effect heterogeneity by religion in Section 6.

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 reflects the status quo 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, and to experience a group composition that is not representative of their context. A more subtle difference is that in equal-share teams, Muslims are themselves overrepresented within the mixed-religion setting, which could

directly influence the dynamics of intergroup contact. Given these shifts across several margins, then, our treatment may be interpreted as capturing the equilibrium effects—allowing for endogenous changes in social behaviours—of contact for both groups.

Intergroup contact can foster better intergroup relations (Paluck *et al.*, 2019; Mousa, 2020), particularly when contact is collaborative (Lowe, 2021), as is the case with our ten-person teams within the camps. 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 Hindu over Indian identity (0.15σ, $p = 0.04$).[38] The rapid ascent of the BJP in West Bengal—driven in part by their successful campaign to convince voters that the TMC state government favours minorities and promotes (unlawful) Muslim immigration—further suggests that this sentiment may resonate with the majority group.[39] We report the treatment effects of high- versus low-intergroup contact in Figure 7.[40]

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 7—that is, the effects of high-contact[41] among campers—to approximate the main effects of the camps plotted in Figure 1. In reality, the effects of contact are substantively different from 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 7 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]

38. 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.

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

40. In Appendix Table S11, we show that p-values and standard errors are nearly identical when using cluster-bootstrapped standard errors.

41. High-contact for Hindus is defined by their assignment to equal-share teams, while for Muslims, it is defined by their assignment to teams with eight Hindus and two Muslims. Some theory and evidence suggests that 50:50 group proportions may be particularly bad for intergroup relations (*e.g.* Anderberg *et al.*, 2024). In Appendix Figure S15, we reconceptualize the treatment along these lines, and simply examine the effect of assignment to equal-share teams, though we find null results in the pooled sample.

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 $(0.42/3) * 4.2 = 0.59\sigma$. 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. We note one limitation of this analysis: without any single-religion teams, we cannot directly test for the mediating role of the *extensive* margin of contact.

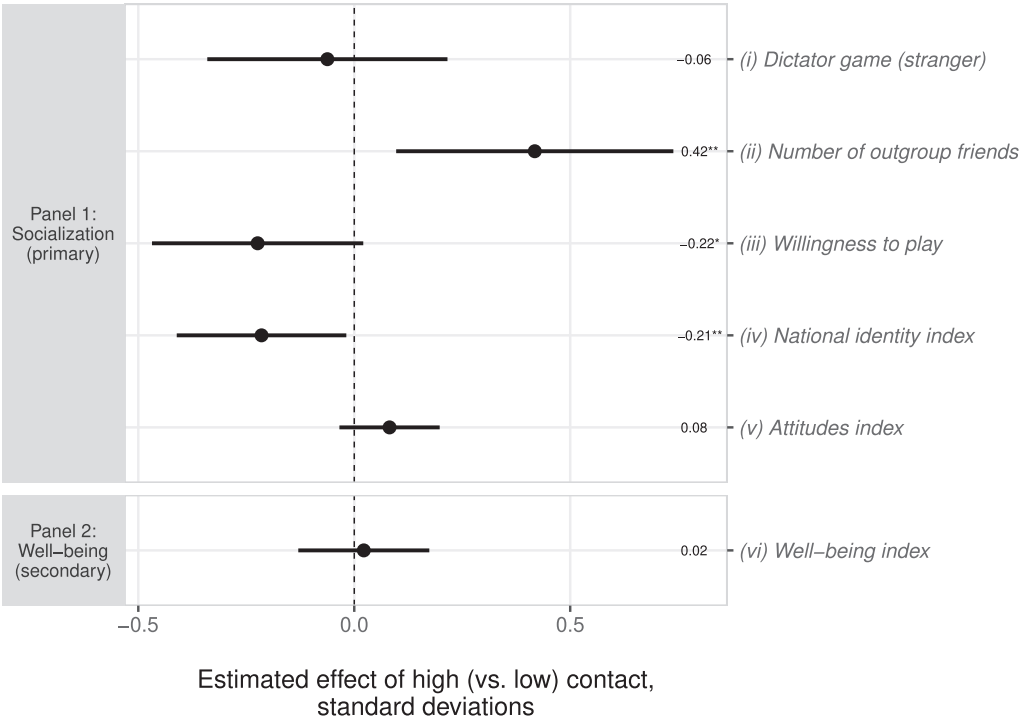

FIGURE 7
Effects of intergroup contact

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. High-contact for Hindus is defined by their assignment to equal-share teams, while for Muslims, it is defined by their assignment to teams with eight Hindus and two Muslims. Regressions include only participants randomly assigned to a camp. The outcomes parallel those in Figure 1, with two exceptions: (a) 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 (b) 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 centred and standardized using the variable's low-contact-group mean and standard deviation. N is 235 in rows (i) and (iv)–(vi), 234 in row (ii), and 221 in row (iii). 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 S11.

The clearest negative effect of contact is on national identity. Both sub-components are similarly affected: high contact reduces self-reported national identity by 0.25 points, and the choice of the Indian fridge magnet by 9 percentage points (leading to the overall effect of -0.21σ on the index). This backfiring helps account for the -0.05σ pooled effects of the camps on national identity (Figure 1). If outgroup exposure increases the salience of religious differences, intergroup contact may enhance religious identity, muting the effectiveness of integrated camps in creating a shared national identity. 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.*, 2019; Clochard, 2024). So far, experiments only find 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). We elaborate on this finding in Section 6, where we show that the negative effects of contact are driven by Hindu boys.

5.3. *Programming content*

Rituals and intergroup contact can affect behaviours through emotion-based channels. Camp lectures, meanwhile, might affect behaviours through reasoning. Previous research has shown curriculum content can indeed shape attitudes and behaviours 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 3 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 $\text{Middle days attended}_i \in \{0, 1, \ldots, 9, 10\}$ 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 S12). Thus, the effects of lecture days are cleanly identified.

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 preferences (Figure 8). 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 affecting social preferences in the domain of money, but not in shaping 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 molding emotion-based decisions (like attitudes).

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. However, we note that unlike the effect on social preferences, this effect is sensitive to how we control for Middle Days Attended—it becomes insignificant ($p = 0.22$) if we include dummy variables for each possible number of Middle days attended (Appendix Figure S16).

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 may 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.

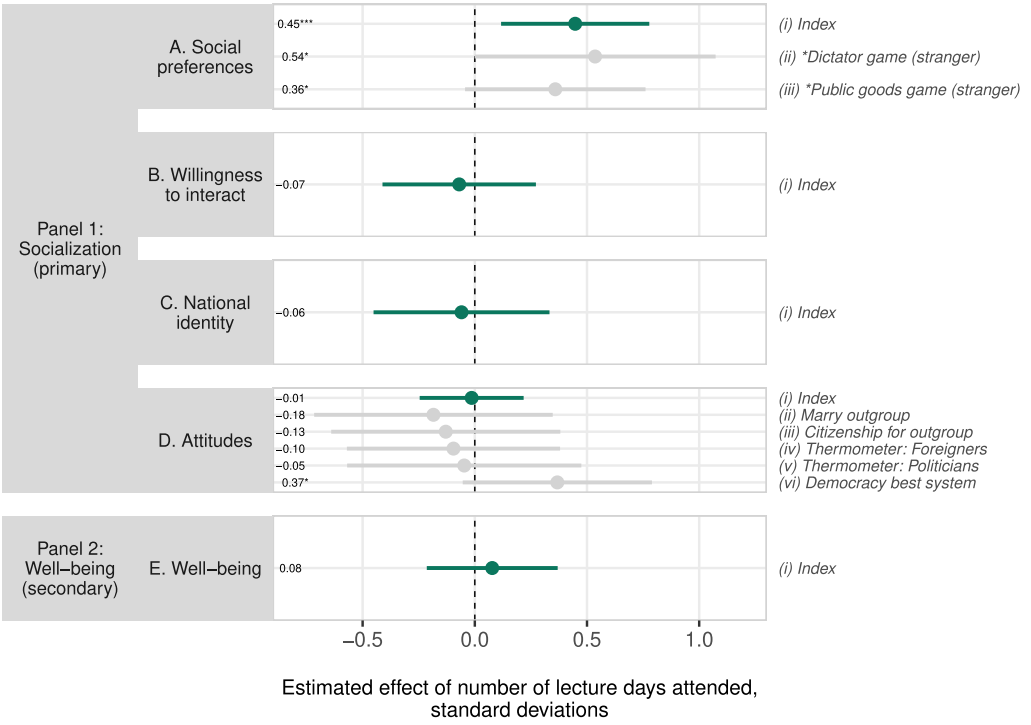

FIGURE 8
Programming improves social preferences

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 centred and standardized using the variables' control-group means and standard deviations. N is 235 for all models except B.i, where N is 220. 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: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

6. HETEROGENEOUS TREATMENT EFFECTS BY RELIGION

Do the camps and their various components have uniform effects across children, or do effects differ by children's religion? We investigate this question in Table 3. Panel A shows no heterogeneous treatment effects of the camps overall: across all primary outcomes and the well-being index, the differences between campers and control participants are statistically indistinguishable for Hindu and Muslim boys. Yet, Panels B and C show that these overall similarities mask important divergent effects produced by the two randomized camp components. Rituals work better for Hindus than Muslims, while higher outgroup contact works better for Muslims than Hindus.

6.1. *Heterogeneous effects of rituals*

The results presented in Table 3, Panel B show that rituals tend to have more positive effects for Hindus than for Muslims, with the difference statistically significant for social preferences (Columns 1 and 2), willingness to play (Column 5), the well-being index (Column 8), and camp attendance (Column 10).

TABLE 3
Heterogeneous effects by religion

	Primary outcomes:							Secondary outcomes:		
	Social preferences		Willingness to interact			National identity	Attitudes			
	Index (1)	Dictator: stranger (2)	Index (3)	Outgroup friends (4)	Willingness to play (5)	Index (6)	Index (7)	Well-being index (8)	Anything campers (9)	Camp attendance (10)
A. Camps versus control										
Camp × Hindu	0.24***	0.33***	0.24***	0.24**	0.24*	−0.04	0.05	0.24***		
	(0.09)	(0.13)	(0.08)	(0.10)	(0.13)	(0.08)	(0.06)	(0.08)		
Camp × Muslim	0.11	0.20	0.41***	0.66***	0.14	−0.08	0.03	0.06		
	(0.13)	(0.20)	(0.15)	(0.25)	(0.18)	(0.15)	(0.09)	(0.08)		
N	401	401	379	400	380	401	401	401		
Hindu versus Muslim *p*-value	0.42	0.58	0.34	0.12	0.65	0.82	0.89	0.13		
Baseline dependent variable	N	N	Y	Y	N	N	Y	Y		
B. Ritual versus regular (campers only)										
Ritual × Hindu	0.13	0.33**	0.02	−0.01	0.07	−0.14	−0.15*	0.18*	0.23**	0.15
	(0.12)	(0.15)	(0.11)	(0.11)	(0.17)	(0.10)	(0.08)	(0.11)	(0.10)	(0.53)
Ritual × Muslim	−0.36**	−0.41	−0.29	−0.15	−0.46**	0.10	0.00	−0.09	0.08	−1.67**
	(0.18)	(0.26)	(0.20)	(0.28)	(0.23)	(0.18)	(0.09)	(0.11)	(0.11)	(0.65)
N	235	235	220	234	221	235	235	235	235	240
Hindu versus Muslim *p*-value	0.02	0.02	0.17	0.65	0.07	0.24	0.22	0.08	0.30	0.03
Baseline dependent variable	N	N	Y	Y	N	N	Y	Y	N	N
C. High contact versus low contact (campers only)										
High contact × Hindu		−0.21*	−0.04	0.21	−0.33**	−0.25**	0.11*	−0.03	−0.11	−1.02*
		(0.11)	(0.09)	(0.14)	(0.14)	(0.10)	(0.06)	(0.10)	(0.14)	(0.55)
High contact × Muslim		0.26	0.46**	0.88**	0.05	−0.14	0.02	0.13	0.10	0.79
		(0.35)	(0.21)	(0.33)	(0.30)	(0.18)	(0.10)	(0.09)	(0.10)	(0.79)
N		235	220	234	221	235	235	235	235	240
Hindu versus Muslim *p*-value		0.20	0.03	0.05	0.29	0.60	0.45	0.28	0.15	0.04
Baseline dependent variable		N	Y	Y	N	N	Y	Y	N	N

Notes: All outcomes are measured at endline, except that in Column 10, which is the number of camp days attended (0 to 12). 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. All other outcomes are z-scores. Indexes contain the same components as those shown in Figure 1. The Social Preferences Index is omitted in Panel C since high contact mechanically affects the partners in the public goods game—one component of that index. All regressions include randomization strata fixed effects. Robust standard errors are in parentheses in Panels A and B. Standard errors are clustered at the team level in Panel C. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A natural interpretation of the heterogeneity is that rituals deepen the engagement of Hindus in the camp, but alienate Muslims, with these engagement effects spilling over to intergroup behaviours and well-being. Correspondingly, Muslims in the ritual camp attend 1.7 fewer days than Muslims in the regular camp (Column 10), while rituals increase the willingness of Hindus to do anything for other campers (Column 9). Rituals lead to divergent well-being effects 6 weeks after the camps ended; those effects almost fully account for the somewhat differential impact of the camps overall on well-being (Panel A, Column 8). These varying well-being effects are driven by the social life component of the well-being index (see Appendix Table S7), aligning with the notion that different levels of engagement in the camps entail differentially rich social lives later on. The disengagement of Muslims may also explain why the positive effects of rituals for Hindus are driven more by these well-being effects (Table 3, Panel B, Column 8) than by effects on intergroup attitudes and behaviours. Camps may have less power to positively shape the attitudes of Hindus toward Muslims when Muslims at the camps are disinterested or detached.

Why would collective rituals alienate Muslims but not Hindus? An immediate possibility is that some of the rituals are explicitly nationalistic (singing the national anthem and hoisting the national flag), 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 baseline feeling thermometer measure), such rituals may distinctively isolate Muslims, triggering a variety of negative feelings. But several pieces of evidence strike against such a theory. The Hindu–Muslim difference in the effects of rituals barely changes (0.16σ rather than 0.15σ) when we add an interaction term between the ritual-camp and the baseline-measured Modi feeling thermometer. Neither do we observe a positive effect on the endline-measured Modi feeling thermometer among Hindus in the ritual (versus regular) camp. Moreover, ritual effects on the national identity index are the opposite from what this story would predict—if anything, the rituals increase national identity more for Muslims than for Hindus (Table 3, Panel B, Column 6).

At least two other explanations are consistent with these findings. First, the observed effects could be due to the fact that rituals are more fulfilling when carried out with members of one's own group, as the presence of more in-group members makes the more rituals more comfortable to perform as well as more satisfying. This concept is similar to the principle of "participatory crowding" found in club goods models of religion, which suggest that the rewards of engaging in religious activities—like collective worship, rituals, or ceremonies—grow with the number of participants. These activities provide social, spiritual, and psychological benefits that are amplified by the presence of a community (Berman, 2000). Given that Hindus were the majority in each camp, making up 65% of participants, they might have gained more from the rituals. Additionally, it is possible that Muslims, who reported higher engagement in religious activities outside of the camp (85% of Muslims versus 60% of Hindus participated in religious activities in the 2 weeks before our endline survey), perceived the camp rituals as less genuine or more forced, diminishing rituals' efficacy.

6.2. *Heterogeneous effects of contact*

We identified some negative effects of contact in Figure 7. In Panel C of Table 3, we see that these negative effects are driven by Hindus. The pattern is also reflected in the revealed preferences of participants: Hindus attend one fewer camp days when assigned to high-contact teams, whereas Muslims attend 0.8 *more* camp days when in high-contact teams ($p = 0.04$ for the difference in the two effects in Panel C, Column 10). Recall that Hindus have high contact in the 50:50 teams, whereas for Muslims high contact comes in the 80:20 (Hindu/Muslim) teams. For Hindus, then,

TABLE 4
Endline 2 (one year) results, with endline 1 comparisons

	Outgroup friends (1)	Number of new outgroup friends (2)	Proportion friends same (3)	More Indian than Hindu/Muslim (4)	Well-being index (5)
A. Endline 2					
Camper	0.17***	0.10*	−0.05**	0.02	−0.04
	(0.06)	(0.06)	(0.03)	(0.12)	(0.08)
Control group mean	0.17	0.13	0.52	2.38	0.00
N	334	329	329	334	334
Baseline dep. variable	Y	Y	Y	N	Y
B. Endline 1 (estimated for endline 2 sample)					
Camper	0.23***			−0.13	0.20***
	(0.07)			(0.13)	(0.07)
Control group mean	0.20			2.48	−0.02
N	329			329	329
Baseline dep. variable	Y			N	Y

Notes: Outgroup friends is the number of close friends (out of 5) belonging to the religious outgroup. Number of new outgroup friends is the number of outgroup friends mentioned in endline 2 who were not mentioned in endline 1. Proportion of friends same is the share of friends mentioned in endline 1 also mentioned in endline 2. More Indian than Hindu/Muslim is the self-report of attachment to religious versus national identity: 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. The Well-being index is a z-score, and is described above. All regressions include randomization strata fixed effects. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the high contact setting results in far more interactions with Muslims than they are accustomed to in their daily lives. But the same is not true for Muslims, for whom 80:20 teams reflect a typical level of exposure, given the demography of North 24 Parganas and West Bengal. Taken together, this points to heightened outgroup threat perceptions felt by Hindus as a plausible explanation for the divergent responses seen in Table 3, Panel C. Local political discourse on (illegal) Muslim immigration from Bangladesh—and the demographic change it is said to induce—resonates with these heterogeneous effects. Prevailing socio-political narratives may have a hand in shaping individual behavioural responses to intergroup contact.

7. LONG-RUN EFFECTS OF CAMPS

Over 1 year after the camps concluded, 73% of campers report during the phone endline that they remain in touch with people they met at the camp. Consistent with this, we observe that the strong positive impact of camps on the formation of outgroup friendships almost entirely persists a year after the camps have ended (see Column 1, Panel A of Table 4; Panel B re-estimates the results for the same outcomes measured during the first endline, using the second endline sample for comparability). The mean number of outgroup friends in the control group is 0.17; among campers, it is twice as high ($p = 0.004$). The closest existing finding we know of is the long-run positive effect of being assigned a Black roommate on friendships with Black individuals (Camargo *et al.*, 2010). What is remarkable in our case is that we find enduring intergroup friendships from a *2-week* camp, as opposed to a full year of living together as roommates.

One concern may be that camp participants reflexively offered up the same list of names that they gave to surveyors in the first endline. Further analyses reveal this is not the case. On average, 52% of the names a respondent lists at endline 2 were not mentioned at endline 1. Moreover, 59% (0.1 out of 0.17) of the camp effect on long-run outgroup friendships is explained by the formation of new outgroup friendships (Panel A, Column 2). *i.e.* outgroup individuals

participants did not list as close friends at endline 1. Network effects can account for this pattern. Our hypothesis is that a typical camper makes friends with outgroup campers by the time of the first endline, but then goes on to make friends with some of those campers' friends by the time of endline 2. The evidence in Panel A, Column 3 fits with this explanation, demonstrating that the percentage of close friends that stay the same between the first and second endlines is five percentage points lower for campers than non-campers. Campers experience greater friendship churn as they are able to tap into the social networks of the people they met at camp—including outgroup networks that remain relatively inaccessible to non-campers. We view this as a highly encouraging result for intergroup relations, pointing to the ability of camps to break down social barriers in a long-lasting way.

We estimate null effects for the two remaining outcomes in Table 4, Panel A. Just as in the first endline, children's attachment to national versus religious identity is unmoved by camps (Column 4). While there were strongly positive camp effects on well-being in the first endline, these had fully dissipated by the time of the second endline (Column 5). The "high" children that get from attending camps lasts for 1-to-2 months, but not for 13 months. Similarly, we do not find any persistent effects of greater intergroup contact (Appendix Table S13).

8. 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 instil progressive community norms. We assess whether youth camps can strengthen ties between adolescents from historically antagonistic religious groups, and so help manufacture 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. Yet, 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. Uniquely, we demonstrate that the camps increase outgroup friending more than a year after the camps' conclusion.

Our core finding that camps change intergroup behaviours at the *personal*, but not the *abstract* (*i.e.* ideological), level poses a question for future research: when do personal behavioural changes lead to changes in more abstract attitudes? One possibility is that intergroup contact combined with perspective-taking (*e.g.* Alan *et al.*, 2021), as partially implemented in our lectures, could foster broader attitudinal shifts by encouraging participants to actively reflect on their interactions with outgroups. Alternatively, altering deeply-held attitudes, such as those regarding intergroup marriage, may require not only changing individual beliefs but also modifying perceptions of how family and society view these actions (higher-order beliefs, as in Bursztyn *et al.*, 2020). Interventions that engage both parents and young children could be necessary. Beyond molding attitudes, other questions surround what features can prolong and enhance treatment effects. In our case, the lasting effects on friendships were partly sustained by participants expanding their social networks to include new outgroup members, suggesting that the outside-the-camp opportunities to make intergroup friendships matter—opportunities that may be restricted by geographically segregated cities (Adukia *et al.*, 2022).

Robert Baden-Powell, the founder of modern scouting, wrote that "a week of [camp] life is worth 6 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. Beyond the academic contributions highlighted above, our results also offer practical recommendations for designing effective intergroup contact interventions. First, on the issue of bringing about attitudinal change, we detect suggestive 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 favouring inclusive citizenship policies. Second, how might the design of camps be modified to ensure that intergroup contact works equally well for majority- and minority-group campers? It may be that 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. Formulating an optimal blueprint for tolerance-promoting camps would be valuable for policymakers. Last, rituals that are extreme (especially those that are physically demanding), 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 further investigation.

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Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

Data Availability

The data and code underlying this research is available on Zenodo at https://doi.org/10.5281/zenodo.14976627.

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