Leveraging Edutainment and Social Networks to Foster Interethnic Harmony*

Abu Siddique† Michael Vlassopoulos‡ Yves Zenou§

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Abstract

Interethnic tensions pose a significant barrier to the socioeconomic advancement of minority groups. This paper investigates the effectiveness of educational entertainment (or edutainment) in promoting interethnic harmony. We carried out a cluster-randomized field experiment involving over 3,300 households across 121 polyethnic villages in Bangladesh. We find that disseminating information through a documentary film designed to educate the ethnically dominant Bengalis about the ethnic minority Santals in polyethnic villages increased the ethnic majority's prosociality toward minorities. Using emotion-detecting software to analyze facial expressions during the film viewing reveals that empathy played a significant role in this process. On the other hand, we do not find any impact on the prevalence of negative stereotypes and discriminatory opinions toward minorities. In addition, we find that targeting network-central individuals with the intervention generated positive spillovers on others within villages, including Santals. We further corroborate these findings through village-level administrative data showing a reduction in police complaints in treatment villages. Five months after the intervention, we conducted a casual work field experiment involving 720 randomly selected participants from the main intervention. In this casual work task, pairs of ethnic majority and minority participants jointly produced paper bags for a local supplier under a piece-rate compensation scheme. We find treatment effects on productivity for both ethnic groups. For the ethnic majority, exposure to edutainment led to higher productivity, possibly through increased prosociality towards minorities. Among the ethnic minority, reciprocity or peer pressure appears to explain their observed productivity gains. Overall, our findings demonstrate the power of edutainment and social networks

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†Department of Economics, Royal Holloway University of London; Institute for Fiscal Studies; and IZA. Email: abu.siddique@rhul.ac.uk

‡Department of Economics, University of Southampton, and IZA. Email: m.vlassopoulos@soton.ac.uk

§Department of Economics, Monash University, CEPR, and IZA. Email: yves.zenou@monash.edu

in promoting harmony within multiethnic communities.
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1 Introduction

Interethnic tensions and prejudice are pervasive in many parts of the world with important implications for social cohesion and economic development (Alesina & La Ferrara, 2005). In such contexts, ethnic minority groups are often at the receiving end of exclusionary and discriminatory attitudes from the more powerful and dominant ethnic majority group, which severely impedes their socioeconomic progress. Overcoming these ethnic cleavages to ensure opportunities for the economic and social advancement of disadvantaged ethnic minority groups is a crucial policy challenge for many developing countries (Barron et al., 2023).

At the heart of interethnic frictions lie misperceptions about the characteristics and behaviors of outgroup members based on their ethnic background (Bursztyn & Yang, 2022), fostering discriminatory attitudes and exclusionary practices. These distorted perceptions restrict social interactions, limit economic opportunities, and hinder mutual understanding between different ethnic groups, thereby creating a hostile environment in which the full potential of ethnic minorities remains unrealized. A relevant example can be found in polyethnic villages in Bangladesh, where there exists a dominant-subordinate relationship between the ethnic majority (Bengalis) and ethnic minority groups (e.g., Santals), reflecting a long history of stigmatization and marginalization of the minority group at the hands of the majority (Roy, 2012).

Against this background, our study examines a novel intervention designed to foster interethnic harmony within polyethnic villages by reshaping the perceptions of the ethnic majority about ethnic minorities through targeted media exposure. Specifically, we conducted a cluster-randomized field experiment in 121 villages in northern Bangladesh to assess the impact of a documentary film that we produced for the purpose of this intervention. This film was created with the aim of educating the ethnic majority Bengalis about the lives and livelihoods of the Santals, one of the largest ethnic minorities in Bangladesh. The film features three main layers of storytelling narrated by Santals that aim to (i) educate viewers about Santal culture, (ii) expose them to the economic and other social challenges faced by the Santals, and (iii) showcase examples of Santals who have excelled in education and their professional lives. Exposure to entertainment media (edutainment) has shown promise in changing norms and behavior in various domains through the channels of providing new information and changing preferences (La Ferrara, 2016; Grady et al., 2021).

In our context, viewing the film promises to positively influence the attitudes and behavior of the ethnic majority towards the minority outgroup. This effect is expected to occur through the mechanisms of increased empathy and identification, serving as an alternative to direct contact. We thus posit that the documentary could have an affective impact on viewers, influencing their attitudes and stance toward the outgroup (Petty et al., 2003; Lerner et al., 2015). Direct contact has been argued as an effective means to eliminate prejudice and stereotypes by facilitating

learning about the outgroup and inducing positive emotions (empathy) toward them (Allport, 1954; Paluck et al., 2019). However, in our context, encouraging direct contact might not be as effective, given the social acceptability of harboring prejudiced attitudes toward the minority, which could potentially lead to backlash.[1] Therefore, the indirect contact with the outgroup facilitated by the documentary and presented with an entertaining veneer, allows the acquisition of new information about the outgroup and can stimulate empathy toward them (Miles & Crisp, 2014). This type of intervention then emerges as a promising and socially acceptable alternative for enhancing interethnic relations.

A second objective of this study is to investigate whether information dissemination through central agents in a network can be more effective in spreading information than random targeting. A growing body of literature demonstrates that social networks can be leveraged to enhance the diffusion of information and promote the adoption of behaviors in diverse areas such as technology adoption, microfinance, public health and education (Valente, 2012; Banerjee et al., 2013; Kim et al., 2015; Banerjee et al., 2019; Breza & Chandrasekhar, 2019; Beaman et al., 2021; Islam et al., 2021; Alan & Kubilay, 2024). In our study, we leveraged the social relationships of the ethnic majority communities within villages, targeting information towards individuals with high diffusion centrality, to test if the film's information would spread widely and shift the perceptions of those not directly targeted by the intervention. Thus, we provide the first empirical test of both levers—edutainment and network targeting—in the context of ameliorating interethnic tensions.

We collaborated with two local NGOs in northern Bangladesh, Ashrai and SARCH, to carry out the screening of a documentary film through three different treatment arms. In the first arm, we randomly selected ethnic majority Bengalis to watch the documentary (Treatment 'Random'). In the second arm, we included a mix of randomly selected Bengalis and those with high diffusion centrality (selected using the approach in Banerjee et al., 2019) to also watch the film (Treatment 'Central'). The final group served as a 'Control' and watched a placebo documentary film. Individual screening sessions were arranged at each participant's home using tablets. Within each treatment arm, we also collected data from both the ethnic majority and minority populations that did not partake in the intervention to understand the spillover impacts.

We evaluate the impact of the intervention by collecting data through five different methods: (1) Lab-in-the-field experiments to elicit prosociality (altruism and solidarity) toward the outgroup—as receiving more information regarding outgroup members has been linked to increased prosocial behavior towards them (Rao, 2019); (2) Administrative data on interethnic dispute complaints obtained from police stations and village counselors to corroborate if the prosocial behavior observed in the 'lab' translated into real-world behavior; (3) A casual work field experiment to measure the productivity of multiethnic pairs of workers—as recent field experiments have documented the negative impact of diversity on the work productivity of teams in development contexts (Hjort, 2014; Afridi et al., 2020; Marx et al., 2021; Ghosh, 2022); (4) Photographs to capture facial expressions and infer emotions during exposure to edutainment; (5) Quantitative and qualitative surveys to understand a variety of other behaviors, beliefs, and

[1] This is not unique to our setting, but rather a universal aspect. Turner et al. (2007) argue that different religious groups or ethnic groups remain largely isolated from one another in most countries and cities, so direct contact can be difficult to establish.

intentions among Bengalis, including interethnic trust (which we measured using an experimentally validated survey instrument by Falk et al., 2018), as well as several other potential channels.

Our first main result is that three months after viewing the documentary film, treated ethnic majority Bengalis demonstrated significantly more prosocial behavior towards ethnic minority Santals compared to their counterparts in the control group. Specifically, we find that the impact of the intervention on a composite measure of prosociality that combines our three underlying measures (altruism, solidarity, and trust) ranges between 0.24 standard deviations (SD) in Treatment 'Random' to 0.38 SD among those randomly treated in Treatment 'Central'. These positive effects of our intervention on prosocial behavior toward minorities are consistent with other interventions aiming to improve intergroup relations (Rao, 2019; Finseraas et al., 2019).

To unpack the potential channels at play, we collected a rich set of qualitative and quantitative data through in-person interviews and surveys. Post-screening interviews included open-ended questions aimed at determining whether Bengalis gained new information about Santals from the documentary film, and if so, what kind of information they acquired. We also employ additional qualitative interview questions to explore whether the film's information motivated viewers to offer support to Santals, and to understand the underlying motives and specific forms of support they intended to provide. Then, using quantitative survey data, we investigate if the newly gained knowledge affected pre-existing stereotypes and discriminatory opinions among Bengalis towards Santals, and whether it led to improved cross-ethnic social interactions within villages. Our second key finding is that around 82 percent of Bengalis in the treatment groups acquired new information and learned new things about Santals. The newly acquired information was primarily centered on the occupational struggles of the Santal people, their educational pursuits and aspirations, and their potential for economic success. We also find that Bengalis who watched the documentary expressed an increased willingness to help Santals. The support they intended to offer ranged from general assistance to economic aid (jobs, financial assistance, etc.). These intentions were primarily driven by humanitarian concerns and a desire to alleviate the suffering of the Santals. Additionally, Bengalis expressed a willingness to encourage their coethnic neighbors to help the Santals as well.

On the other hand, despite acquiring new information from the film, Bengalis did not significantly change their pre-existing biases and opinions regarding Santals, including the persistence of negative stereotypes and discriminatory opinions. The finding of changes in intergroup behavior without changes in attitudes is consistent with evidence from various interventions targeting prejudice reduction (Paluck et al., 2021), including a recent study focused on improving intergroup cohesion in India (Ghosh et al., 2023). We offer three potential explanations for this finding: (i) cognitive vs. affective components (Tropp & Pettigrew, 2005; Turner et al., 2007)—changing cognitive components of prejudice (such as negative stereotypes) through indirect contact can be more difficult than changing affective components (such as feelings and emotions); (ii) resistance to change (Watson, 1971)—information delivery through a single documentary film may not be sufficient to counter deeply ingrained generational biases, as Bengalis might resist changing their views; and/or, (iii) social conformity (Asch, 2003)—expressing fewer negative stereotypes could be perceived as deviating from the social norm, given that such attitudes towards ethnic minorities are commonly held by Bengalis.

The documentary, however, induced an emotional reaction among the viewers. We analyzed the viewers' facial expressions by taking candid photos during the screening and used emotion-detection software to detect the emotions that were triggered during the viewing.[2] Research on emotions suggests that facial expressions are accurate indicators of emotions (Ekman, 1993). Moreover, it is very difficult to deliberately simulate emotions such as sadness, anger, and fear. Using this facial expression data, which we believe objectively captured emotions activated during the film, we found sadness to be significantly more prevalent ($p < 0.05$), particularly among network-central participants. This emotional response might have motivated them to share and discuss the film's content with their coethnics in the villages, which may explain the positive shifts in prosocial behaviors towards Santals like altruism, solidarity, and trust observed among both treated and untreated Bengalis. This finding is in line with the literature on emotion and decision-making (Lerner et al., 2015), which demonstrates how emotions can heighten attention to a situation (Schwarz & Bless, 1991), activate various goals (Zeelenberg et al., 2008), and facilitate information sharing (Berger & Milkman, 2012). Prosociality is sometimes used as a way to manage emotions, particularly to alleviate sadness or distress (Schaller & Cialdini, 1988).

Our third main finding concerns spillovers on other ethnic majority Bengalis who were not part of the intervention. We find that untreated Bengalis in the 'Central' arm displayed significantly more solidarity (29% higher than the 'Control' group, $p < 0.01$) and trust (7% higher than the 'Control' group, $p < 0.10$) towards ethnic minorities. Regarding spillovers on Santals who were not part of the intervention, we find that Santals in both the 'Random' and 'Central' arms began to trust Bengalis more, by showing an increase of 13% and 25% compared to the 'Control' group (both $p < 0.01$), respectively. In addition, in both arms, ethnic minority Santals were happier and more satisfied with their lives than Santals in the 'Control' group ($p < 0.01$). We also find that Santals in the 'Central' arm become more food secure following the intervention ($p < 0.10$), possibly through help received from the ethnic majorities. The more pronounced spillover effects observed in the 'Central' arm are likely related to the presence of network-central individuals in this arm. These individuals are not only selected for their ability to diffuse information across the village but also experienced stronger emotional reactions from watching the documentary, suggesting they may have been more persuasive and effective when encouraging their neighbors to treat the minorities well.

To corroborate these findings and investigate whether the improved post-intervention interactions translated into better village-level relationships, we analyze administrative data on dispute complaints sourced from local police stations and village counselors' offices. This data provides a more objective measure of interethnic relations and social harmony. We find no significant change in complaints made to village counselors (which are made for arbitration or *shalish* purposes) across any treatment arm. However, when considering complaints made to police stations that involve more serious issues with substantial negative consequences for the parties involved, we find a significant reduction in Bengalis' complaints against Santals post-intervention ($p < 0.05$), only in the 'Central' arm. For Santals, there are no significant reductions in complaints against Bengalis ($p > 0.10$). Consequently, it appears that targeting network-central Bengalis in

[2] We obtained consent from participants about taking pictures at both baseline and before the screening. This was consistent across all arms, including the 'Control' group that watched a placebo film.

the 'Central' arm led to this change due to spillovers on the randomly targeted and the untargeted Bengalis.

Five months after the documentary viewing, we returned to the study area and conducted a casual work field experiment, in the spirit of Hjort (2014), involving 720 randomly selected participants from the two treatment arms and the control group of the main intervention. This experiment, which serves as our second endline, allows us to investigate whether our intervention improved workplace productivity in ethnically mixed teams. We recruited 360 Bengalis and 360 Santals from different villages and paired them to work for a local supplier of paper bags (locally known as *thongga*). It is important to note that all participating Bengalis from the treatment groups had previously watched the documentary film and none were *network-central*. Their task was to jointly produce paper bags, with one worker randomly assigned the role of a *preparer* (preparing the materials), while the other the role of a *finisher* (completing the product). This setup created a vertical interaction in the work environment. Roles were switched halfway through, allowing us to measure each worker's productivity both as a *preparer* and a *finisher*. Their joint productivity determined their earnings, which were split equally between the two and paid at a piece rate. Based on prior evidence of the negative impact of diversity on work productivity of teams (Hjort, 2014; Afridi et al., 2020; Marx et al., 2021; Ghosh, 2022), we expect that productivity may be influenced by one's attitudes toward their co-worker, and thus, through this channel, the intervention could potentially impact productivity.

We find a significant overall increase in productivity by about 5%, but only in the 'Central' arm. Furthermore, in this arm, both Bengalis and Santals significantly increased their productivity only as a *finisher*—the role most crucial in determining their final earnings. What explains the rise in productivity we observe? Our interpretation is that working harder is an expression of prosociality toward one's co-worker in an attempt to raise their income (Rotemberg, 1994). That is, Bengali participants exposed to the documentary exhibited more prosociality toward Santals, prompting them to increase their efforts in order to increase their co-workers' income. In turn, Santals also raise their effort either to conform to the effort level of Bengali workers or because of perceived social pressure to do so (Kandel & Lazear, 1992; Mas & Moretti, 2009; Georganas et al., 2015). To support this interpretation, we examine *finisher* productivity separately, comparing those who started in this role versus those who became *finishers* upon swapping. For Bengalis, productivity is similar regardless of the order in which they assumed the two positions. On the other hand, for Santals, we find that the difference in productivity between the 'Central' and 'Control' groups is more pronounced when they worked as a *finisher* after swapping roles, implying some responsiveness to the higher productivity of the Bengalis when they served as the *finisher* first. These patterns align with our interpretation: elevated prosociality drives the productivity of Bengalis, while conformism and peer pressure drive the productivity of Santals. We present a simple model to illustrate these interpretations of our findings.

To summarize, our study sets out to address two main questions: (i) Does edutainment contribute to promoting interethnic harmony? (ii) Are network-central individuals more effective in generating spillovers? Our evidence, gathered through incentivized measures, self-reported beliefs and attitudes, complaints, and observations of behavior in a natural casual work setting, offers two key insights. First, while deep-seated beliefs may remain unchanged, behavior can be

positively influenced—we detect positive impacts on prosociality, disputes, and team productivity. These findings are consistent with insights from the psychology literature regarding the stability of one's values and resistance to attitude change (Schwartz & Bilsky, 1990; Albarracin & Shavitt, 2018) and the difficulty in changing them through indirect contact (Tropp & Pettigrew, 2005; Turner et al., 2007). Second, the consistent and robust impacts of edutainment observed in the treatment arm where central individuals were exposed to the intervention suggest that network targeting plays a key role in ensuring the effectiveness of this approach.

Literature. This paper contributes to a recent literature in economics that studies the impact of media exposure, showing that it can be effective in altering attitudes and behavior in a wide range of domains, such as women's status and acceptability of domestic violence (Jensen & Oster, 2009), attitudes towards the West (Gentzkow & Shapiro, 2004), fertility (La Ferrara et al., 2012; Kearney & Levine, 2015), and interethnic conflict (Yanagizawa-Drott, 2014; Blouin & Mukand, 2019). Much of this literature exploits the expansion of access to television or natural variation in radio coverage to identify the effect of media.

Only a handful of previous studies have studied the effectiveness of edutainment interventions experimentally in the context of a developing country. Berg & Zia (2017) evaluate the impact of an edutainment soap opera in South Africa in 2012 on financial literacy and financial habits. Bjorvatn et al. (2020) assess the effectiveness of an edutainment show on entrepreneurship in making the viewers more interested in entrepreneurship and business. Banerjee et al. (2019) evaluate the effectiveness of an edutainment television series in changing attitudes and behaviors related to HIV/AIDS in Nigeria. Green et al. (2019) evaluate a media campaign in Uganda to counter violence against women. Riley (2024) screens a film for students in Uganda to study the impact of a role model on student educational outcomes. Tanguy et al. (2014) use video documentaries to change the future aspirations of poor people in Ethiopia.

Our study extends this strand of literature by experimentally examining the impact of edutainment on improving interethnic relationships in a context characterized by a deep-rooted legacy of discrimination and marginalization against the minority group, a topic that has received limited attention.[3] We demonstrate that indirect contact with outgroup members, facilitated by an entertaining documentary, allows for the acquisition of new information about the outgroup. In this manner, the paper contributes to the literature showing that contact is an effective means to eliminate prejudice and stereotypes (Allport, 1954; Paluck et al., 2019).[4] Methodologically, employing a randomized experiment enables us to offer clean identification of a link between exposure to edutainment and changes in attitudes and behavior among the ethnically dominant group, as well as the underlying mechanisms. Furthermore, we provide first direct evidence that emotions evoked through exposure to media can act as an important channel for treating members of an outgroup with more empathy.

[3] A few recent studies in political science and psychology have investigated edutainment interventions (Paluck, 2009; Murrar & Brauer, 2018; Weiss et al., 2023) and narratives (Audette et al., 2020) as a means to reduce prejudice toward outgroups. However, these studies have not addressed our specific context or the wide range of outcomes we examine. In addition, our study advances the understanding of the underlying mechanisms involved.

[4] The effects of contact on intergroup outcomes has been demonstrated in various settings exploiting random assignment into groups (Scacco & Warren, 2018; Rao, 2019; Lowe, 2021; Boucher et al., 2021; Corno et al., 2022; Anderberg et al., 2024).

This paper also connects with the literature on network targeting. A growing literature demonstrates that social networks can be leveraged to enhance the diffusion of information and promote the adoption of behaviors across various domains (Banerjee et al., 2013; Kim et al., 2015; Banerjee et al., 2019; Beaman et al., 2021; Islam et al., 2021; Zárate, 2023; Alan & Kubilay, 2024). Our contribution lies in that we provide evidence that targeting connected individuals can prove more effective in improving relationships between distinct ethnic groups. In doing so, we extend the scope of the existing literature on behavior adoption by demonstrating the potential of network targeting to not only influence individual behaviors but also foster intergroup cohesion.

Finally, a large literature in economics and the social sciences more broadly has been concerned with uncovering the nature, roots, and consequences of ethnic and racial discrimination using laboratory experiments, field experiments, natural experiments, and non-experimental approaches. Altonji & Blank (1999), Charles & Guryan (2011), Lang & Lehmann (2012), and Neumark (2018), provide general overviews. We contribute to this literature by providing field-experimental evidence of a new approach to improving inter-ethnic relations.[5]

2 Context, intervention, and conceptual framework

2.1 Context

Bangladesh is a suitable place to study interethnic intolerance, given that it has around 45 different ethnic minority groups. These groups are different culturally, racially, ethnically, and linguistically from the majority Bengali population—the primary ethnolinguistic group in Bangladesh. Interactions between the Bengalis and these ethnic minorities, referred to as *Adivasis*, often result in conflicts and acts of violence. Historically, *Adivasis* have been subjected to stigma, marginalization, and discrimination by the ethnic majority across a variety of domains, including access to fundamental social services like health care, food and nutrition, education, employment, justice, and politics (Roy, 2012).

Our study takes place in the northwestern region of Bangladesh, specifically in the Rajshahi and Naogaon districts. This region is home to the second-largest ethnic minority community, the Santal. Like other ethnic minorities in Bangladesh, the Santals struggle with challenging economic conditions, limited educational opportunities, and inadequate healthcare. Moreover, they have faced the loss of their agricultural lands to land grabbers (Roy, 2012). Predominantly landless farmers, the Santals have experienced discrimination in agricultural markets (Siddique et al., 2023), and show an aversion to interethnic competition (Siddique & Vlassopoulos, 2020).

2.2 Documentary film

Against the backdrop outlined in Section 2, we collaborated with the Bangladesh-based film production team, *Chitrakkhi*, operated by Bangladeshi film students, to produce a documentary film shedding light on the lives and livelihoods of the Santals in northwestern Bangladesh.[6] The

[5] Bertrand & Duflo (2017) provide an overview of field experiments on discrimination.

[6] The director and editor, Labib Haque, has won numerous filmmaking awards and many of his films have been screened at international film festivals. A segment of this film was a finalist at the Dhaka OIC Youth Capital Film Competition.

film, called '*Ami Santal*' or 'I am Santal', features three main layers of storytelling: culture, economic conditions, and occupational success. We focus on these three aspects based on studies in anthropology and sociology suggesting the prevalence of negative stereotypes among the ethnic majority regarding these aspects of the minority population in Bangladesh (Bal, 2007; Siraj & Bal, 2021). Firstly, the film familiarizes viewers with the Santali culture, showcasing aspects of their rituals, cuisine, etc., that are often unfamiliar to the ethnic majority. Most of this filming took place during the *Baha* festival. The focus then shifts to the trials and tribulations of the Santals, such as housing issues, lack of access to clean water, low incomes, labor market struggles, and the educational challenges faced by young Santals. Finally, viewers are introduced to the aspirations of the Santals, highlighting their potential for success, and showcasing how some Santals have overcome obstacles to achieve success in various domains, including education and profession.

All stories are narrated from a Santali perspective by local non-actor Santals. Filming took place outside the study region, in different villages from our study villages. Importantly, these stories depict the hardships Santals experience without attributing blame to Bengalis. The documentary aims to naturally capture the social issues and contexts without resorting to scripted narratives, serving as an ethnographic record of the lives of the Santals in Bangladesh. To maintain this authenticity, the film was created under the close supervision of visual anthropologist Kazi Robiul Alam and economic anthropologist Golam Faruk Sarker, both Professors of Anthropology at the University of Rajshahi, Bangladesh. We also received comments and suggestions on the film from Hopna Kisku, the Deputy Director of *Ashrai* (an NGO working for the welfare of ethnic minorities), who is also a Santal. The film is about 45 minutes long and can be viewed through this link: YouTube Link.

2.3 Conceptual framework

We present here a simple conceptual framework to illustrate how the documentary might influence the prosocial behavior of the Bengali majority group (i.e., type B) toward the Santal minority group (i.e., type S). For simplicity, we only consider the case of altruism here, which we measure using a dictator game in this study. We postulate that the impact of the documentary on viewers' willingness to help the minority operates through two channels: information ($\text{Info}^{B\to S}$) about the minority group and empathy ($\text{E}^{M\to S}$) toward them. In turn, empathy is influenced by the information obtained in the documentary and some baseline level of empathy ($\text{E}_0^{B\to S}$) that one might possess: $\text{E}^{B\to S} = \text{E}_0^{B\to S} \times \text{Info}^{B\to S}$. The utility function of a majority individual is given by

$$U^B = y^B + \alpha\, \text{E}^{B\to S}\, U^S = y^B + \alpha\, \text{E}_0^{B\to S}\, \text{Info}^{B\to S}\, U^S, \tag{1}$$

where $\alpha > 0$ is the degree of empathy and y^B is the payoff of the majority group. This utility function suggests that majority individuals care about their own payoff y^B but may also weigh the utility of the minority U^S if $\text{E}^{B\to S} > 0$; the latter may be influenced by both their initial empathy for the minority group and the additional information they receive from the documentary.

Consequently, sharing in the dictator game will be higher among treated individuals because the weight on others is larger, as their empathy parameter has increased due to the information

received, leading to a higher weight on the utility of others. However, for information to increase altruistic giving, a person needs some non-negative baseline level of empathy $E_0^{B\to S} > 0$. In other words, information and empathy act as complements in influencing the majority utility since

$$\frac{\partial^2 U^B}{\partial \text{Info}^{B\to S} \partial E_0^{B\to S}} = \alpha U^S > 0.$$

This simple framework highlights the channels through which the intervention might impact prosocial behavior.

3 Research design and data

3.1 Research design

Sampling. We evaluate the effectiveness of the documentary film through a cluster randomized field experiment in Rajshahi and Naogaon districts of Bangladesh. These districts were chosen because they are home to the Santals, the second-largest ethnic minority group in Bangladesh.[7] Moreover, discrimination, conflicts, and violence between the ethnic majority (Bengalis) and the Santals are commonplace in this region.

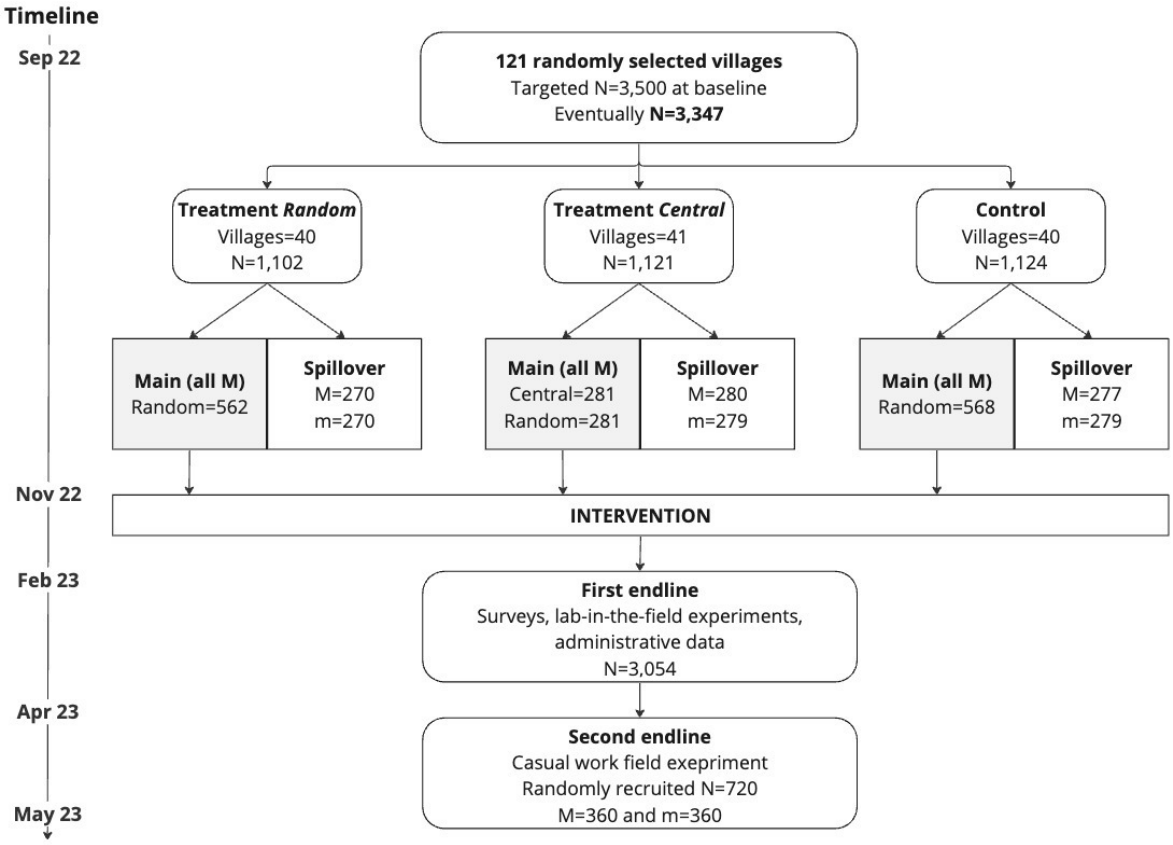
To facilitate the fieldwork, we partnered with two local NGOs, *Ashrai* and *SARCH*. From *Ashrai*, we obtained a list of approximately 150 multiethnic villages where the minority population constitutes between 20-70 percent. That is, any multiethnic village with roughly one-fourth of its population being either Bengali or Santal is considered for this study. We did this to ensure an adequate number of both ethnic groups per village and to facilitate more interaction between these groups following the intervention.

We then randomly selected 121 villages from this list, assigning one-third to each of the three treatment arms. The study flowchart is given in Figure 1. Within each village, we randomly selected about 28 households—14 for the main treatment and 14 for spillovers. Enumerators from SARCH were tasked with visiting households in each village randomly, ensuring to skip at least two households after interviewing either the male or female head of each household. Since our study villages were new to the enumerators, we expected that the villages and households being surveyed would also be new to them, and that their selection would be entirely random.

The male or female heads of households in our main study sample were informed that the NGO *Ashrai* would jointly screen documentary films three months from now, but they were not told about the specific topic of the film. They were also informed that participation in the study involved taking part in a survey now, watching a documentary film in three months, and then participating in follow-ups later. For participating in the baseline survey, households were offered a bar of soap and 20 Taka ($1 = 100 Taka) top-ups on their mobile phones immediately after completing the survey. For participating in the screening, participants were informed that they could win up to 40 Taka in cash prizes by taking part in a short quiz based on the film being screened, and a chance to win a lottery of 5,000 Taka. At the endlines, participants had the

[7] The largest ethnic minority group, the Chakma, lives in the Chittagong Hill Tracks region in the southeast, where traveling is restricted due to crime and safety issues.

Figure 1: Study flowchart



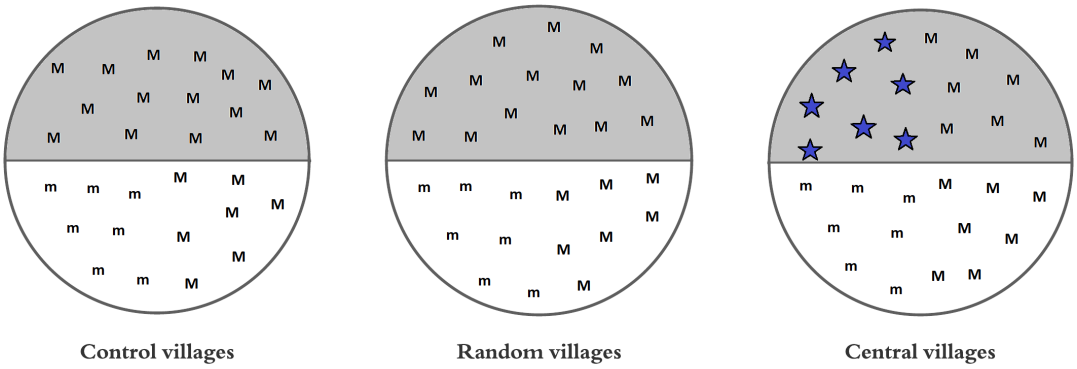
Note: M denotes the ethnic majority Bengalis and m the ethnic minority Santals. *Random* denotes the participants that were randomly selected, whereas *Central* denotes those selected following the approach in Banerjee et al. (2019). N is the sample size.

opportunity to win cash prizes from decision-making experiments or the casual work experiment. Our participants were not informed at the baseline about the specific type of documentary film they would be watching, though they knew it would focus on people living in villages in the Rajshahi Division. They were also made aware that the screenings would take place in their homes using tablets and not in public spaces.

The main sample and treatment arms. The treatment arms are as follows:

- Treatment *Random* (40 villages, $N = 562$ Bengalis): approximately 14 randomly selected ethnic majority households per village watched the documentary film. We refer to this group as 'Random in Random' or *RR*.
- Treatment *Central* (41 villages, $N = 562$ Bengalis): 7 ethnic majority households with members with high-diffusion centrality (referred to as 'Central in Central' or CC) and approximately 7 randomly selected ethnic majority households per village (referred to as 'Random in Central' or RC) watched the documentary film.
- Control (40 villages, $N = 568$ Bengalis): approximately 14 randomly selected households per village watched a documentary film on floriculture or flower farming in Rajshahi Division (of the same length as the documentary film on Santals).

Figure 2: Sampling



Note: M = ethnic majority, m = ethnic minority, $\star$ = ethnic majority with high diffusion centrality. The shaded region includes participants who watched the documentary film, while the white region includes untreated participants who are considered for spillovers.

From each household, we targeted either the male or female head of the household for data collection and treatment delivery. However, during the documentary screening, many non-targeted household members became curious and ended up watching together with the targeted member—an outcome we did not anticipate. In fact, discussions with enumerators suggest that about half of them had at least one additional adult member watching the film, and in nine out of ten households, at least one child watched the film alongside the targeted member. This is why we consider the treatment to have been delivered at the household level rather than at the individual level. However, we do not have measures of outcomes of non-targeted household members to check for within-household spillovers.

The spillover/untreated sample. To examine the spillover effects within villages, we also randomly selected participants in each treatment arm who did not watch either the documentary film on Santals or the film on floriculture:

- Treatment *Random* (40 villages): $N = 270$ Bengalis and $N = 270$ Santals.
- Treatment *Central* (41 villages): $N = 280$ Bengalis and $N = 279$ Santals.
- Control (40 villages): $N = 277$ Bengalis and $N = 279$ Santals.

To provide more clarity on the sampling within each village, Figure 2 provides a visual representation of the village-level sample selection process. Similar to the main participants who watched the documentary films, the spillover sample also took part in both the baseline and endline data collections, and was offered the same incentives for taking part in surveys as the main sample. Out of the 3,500 participants initially approached, a total of 3,347 (95.6%) participated in the baseline survey.[8]

Selection of farmers with high diffusion centrality. To identify farmers with high diffusion centrality (referred to as network-central from here on) in our Treatment *Central* villages, we

[8]While we do not have individual or household characteristics data for those who opted not to participate at baseline, one of the main reasons for non-participation was the absence of the household head.

follow the approach of Banerjee et al. (2019). Prior to the baseline, we conducted a brief survey involving approximately 18-20 random individuals per village (in Treatment *Central* only). We asked them to nominate the 15 people in their villages (and give a rough idea of where their households are located) whom they believed would be the most effective in disseminating information. Our enumerators visited central locations in each village in the *Central* arm, such as the village market, where they randomly surveyed passers-by to create this list. Each survey took roughly 5 minutes to complete. The specific question we asked was:

> *"If we want to spread information to everyone in the village about events, immunization programs, new loan projects, or a fair that we plan to organize, whom should we approach? Please nominate 15 such individuals/households from your village."*

From this list, we selected the seven most consistently and highly ranked names for the *Central* treatment.[9]

Screening. The screening was conducted individually at each participant's house. To ensure that all screenings within a village were completed on the same day, simultaneous screenings took place across different households. The screening began in November 2022 and ended in January 2023. From the baseline data, we had mobile phone numbers for all participants—to send them top-ups but also to call them in advance to arrange visits. That is, enumerators called to schedule a day and time for each screening visit. The documentary film was displayed on 8-inch tablets, allowing participants and other household members to watch and listen to the documentary together. At the end of the screening, there was a four-question quiz based on the film. Correctly answering each question allowed participants to win 10 Taka (chance to win a maximum of 40 Taka, approximately one-seventh of their daily income). We organized this incentivized quiz to encourage participants to pay attention to the video content and reduce attrition. There was a break at the 25-minute mark of the video.

3.2 Data collection

We collected data at baseline and two endlines. While the baseline data collection only involved surveys, the endline data collection involved self-reported surveys, a lab-in-the-field experiment, and a field experiment. We collected data from approximately 28 participants per village (one per household), involving both the main and spillover samples. Specifically, around 14 of these participants watched the film and are considered part of the 'main' sample, while the remaining 14 are considered for spillover analysis, resulting in a total of 3,347 participants.

The baseline data collection was conducted in September-October 2022. The first endline, which involved survey and lab-in-the-field experiments, took place in February-March 2023, while the second endline involved a field experiment in April-May 2023. Since the screenings were conducted individually, we scheduled the first endline approximately 3 months after the screening for each participant, and the second endline approximately 5 months after the screen-

[9] Note that we collected this information only in the 'Central' arm. We also do not know if the passers-by surveyed were residents of the same village or nearby ones. However, since we could successfully match most names with actual village residents, we believe those surveyed at village markets had a good understanding of the villages and their residents.

ing. This ensured that the time gap between the screening and the first endline was about 3 months for each participant, and the individual time gap between the screening and the second endline was about 5 months. The full project timeline is summarized on the left-hand side of Figure 1.

Lastly, we also collected administrative data on interethnic complaints from both police stations and village counselors' offices at both baseline and endline. However, this data is at the village level, as neither the police stations nor the village counselors were willing to reveal the names and addresses of those lodging or facing complaints, which would have allowed us to match this information with our intervention participants.

3.2.1 Outcomes

Experimental and survey measures of prosociality. At the first endline, our main outcomes of interest for both the ethnic majority and minority groups are altruism, solidarity, and trust towards non-coethnics. Altruism and trust are measured using incentivized lab-in-the-field experiments conducted on the entire sample, using established behavioral games: the Dictator game (Eckel & Grossman, 1996) and the Solidarity game (Selten & Ockenfels, 1998). Participants received compensation for one randomly selected game. Due to logistical constraints associated with implementing the incentivized Trust game (Berg et al., 1995) sequentially on a large sample size of over 3,000 participants, we opted for the measure of interethnic trust proposed by Falk et al. (2018). This measure relies on the following experimentally-validated survey question: "Santals (Bengalis) have only the best intentions".[10] Detailed definitions of Altruism, Solidarity, and Trust outcomes are provided in Appendix B.1. Experimental instructions are available in Appendix B.2.

Dispute complaints using administrative data. We pre-registered to use interethnic dispute data based on police complaints as village-level outcomes—Bengalis' complaints against Santals, and Santals' complaints against Bengalis. These complaints, obtained from the *Tanore* and *Godagari* police stations, cover our 121 villages, and were collected at both baseline and endline. In addition, we obtained complaint data made to local village councilors from their respective offices. The complaints made to police stations typically involve more extreme and violent matters, whereas those made to village councilors primarily relate to arbitration, locally known as *shalish*. We use both sets of village-level count variables as outcomes. This data allows us to distinguish between complaints made by ethnic majorities against minorities and complaints made by minorities against majorities. However, it does not allow us to identify the names of the individuals who filed them.

Work productivity using a field experiment. At the second endline, about 4.5 months after the intervention ended, we conducted a casual work field experiment in partnership with a lo-

[10] For the Dictator and Solidarity games, we provided participants with an endowment of 100 Taka (equivalent to $1) in 5 Taka coins, amounting to 20 coins for each game. As the games involving altruism, solidarity, and trust were played/answered in this exact order, we used a scale from 0 to 20 to measure trust. This ensures that all three measures share a similar scale, 0-20, making it easier to explain the scale of the trust question to our non-standard 'subjects'.

cal supplier of paper bags. For this experiment, we recruited 720 participants from the pool of 3,054 who participated in the first endline on a first-come-first-served basis—360 Bengalis and 360 Santals—equally selected from the three treatment arms. This field experiment involved two participants—one ethnic majority and one ethnic minority—engaging in a casual work opportunity that lasted for 3 hours. Workers in pairs were drawn from different villages. The work was paid at a piece rate, and individual earnings depended on joint productivity. This field experiment and its outcomes were also pre-registered. Further details regarding the field experiment are provided in Section 6.

Secondary outcomes. In addition to the primary outcomes, we collected data on various secondary outcomes to capture the broader impacts of the intervention. These included measures of interethnic friendships and water usage charges for individuals from different ethnicities within the same village accessing water from private tube-wells, considering their significant economic implications in these village contexts. Furthermore, we collected data on three economic outcomes for ethnic minorities: household-level food insecurity, new employment opportunities, and income. Finally, to evaluate whether the intervention had any psychological implications for either ethnic group, we also measured participants' prevalence of anxiety and depression using PHQ-4 (Kroenke et al., 2009), along with four measures of subjective well-being derived from the World Values Survey. All of these outcomes were pre-registered, and detailed definitions are provided in Appendix B.1.

3.2.2 Potential mediators

Information and intention channels. After watching the documentary film and completing the incentivized quizzes, participants in the treatment arms were asked a series of open-ended question by the enumerator. The first question was *"Name five new things you learned from the video today."* This open-ended question allows us to measure whether participants in the treatment arm received new information about the Santals, and if so, what kind of information. Note that the control group was not asked this question because they watched a documentary film on floriculture.

The next questions that participants were asked were: *"Now imagine a Santal in your village is in similar conditions as the Santals in the film that you just watched... (2) What would you do in that situation? (3) Why? (4) What would you advise your neighbors to do in that situation?"* These three questions were designed to capture a more qualitative overview of participants' intentions and reactions. The responses to these questions can help in understanding the underlying mechanisms driving behavioral changes. Additionally, these questions were designed to encourage participants to deeply reflect on the social issues presented in the documentary and contemplate potential solutions. All questions were open-ended, primarily to minimize response biases.

Stereotypes, discriminatory opinions, and interethnic interaction channels. We collected the following information through surveys at both baseline and endline: prevalence of common stereotypes and misconceptions, discriminatory opinions, various instances of social interaction and interethnic visits, and intercultural competence. The stereotype index was measured using 6

questions on culture, profession, potential for success, and education. The discriminatory opinion index included 7 questions related to culture, education, honesty, profession, and work relationships. Each question was answered on a scale 0-10, where 10 indicates complete agreement. For the exact wording of the questions, please refer to Appendix B.1.

Emotion channels. Given that films can stir emotions that may have long-lasting impacts that influence behavior, we investigate potential *empathy* channels by measuring the emotions experienced while watching the documentary film. Field assistants took candid photographs (with consent) of each participant's face at a random point during the screening.[11] Using the Emotimeter software (developed by reAImagine), which employs machine learning to detect emotions in portrait photos, we assess whether certain emotions, such as sadness, anger, disgust, happiness, and other emotions (seven types in total), were triggered among each participant. This software assigns a score ranging between 0% and 100% to each emotion category, where a higher score indicates a stronger activation of that particular emotion. This methodology allows us to measure participants' emotions more objectively.

3.2.3 Baseline characteristics and balance

At baseline, we collected data on a wide range of individual and household characteristics. These included but were not limited to factors such as age, gender, education, income, household size, and occupation. These data were gathered from all 3,347 households participating in the baseline survey. Table A1 summarizes these characteristics separately for the main sample (Panel A) and the spillover sample (Panel B), while also testing the differences between treatment arms. Participants are around 40 years old, predominantly male household heads, Muslim, and primarily engaged in farming. Note that participants in the Central arm, which includes the selected high central diffusion centrality individuals, are slightly more educated and have more income.

The last three columns in this table report the p-values obtained by individually regressing the baseline characteristics on the treatment variable with union council fixed effects and standard errors clustered at the village level (the unit of randomization). We found that some characteristics, such as education and income, show statistically significant differences between treatment arms. However, when considered jointly, the baseline characteristics are well balanced across all arms (all joint $p > 0.10$).

3.2.4 Attrition

Total attrition at the endline is 9.7%. To investigate the possibility of differential attrition across treatment arms, we regressed the indicator for attrition (1 if missing at the endline or 0 otherwise) on the treatment variable, with union council (a rural administrative unit) fixed effects and standard errors clustered at the village level, as pre-registered. We find that attrition is 2 percentage points higher in *RR*, 1 percentage point higher in *RC*, and 3 percentage points

[11] Because we do not know the exact time when each video began screening, we cannot match the timing of the photo with specific video content. This makes it difficult to check which parts of the video elicited particular emotional responses.

lower in CC relative to the $Control$ arm (all $p > 0.10$). Therefore, we did not find evidence of differential attrition.

3.3 Hypotheses

1. The *Random* and *Central* arms will improve interethnic outcomes for treated ethnic majority (assessed using survey questions, lab-in-the-field experiments, a field experiment, and administrative data) compared to the *Control* arm.
2. The *Central* arm will generate larger positive spillover effects on neighboring (untreated) ethnic majority participants relative to the *Random* arm.

3.4 Empirical method

Treatment effects. We estimate intent-to-treat (ITT) effects on the main sample using the following regression specification:

$$Y_{1ijc} = \alpha + \beta_1 RR_{ijc} + \beta_2 RC_{ijc} + \beta_3 CC_{ijc} + \zeta Y_{0ijc} + \Gamma' X_{ijc} + \nu_c + \epsilon_{ijc}, \tag{2}$$

where Y_{1ijc} is the outcome of individual i living in village j, in union council c, measured at the endline. RR_{ijc} is an indicator for the 'Random' Treatment (where random Bengalis are targeted), while RC_{ijc} and CC_{ijc} are indicators for the randomly selected and influential Bengalis in the 'Central' Treatment, respectively.[12] Y_{0ijc} is the baseline analogue of the outcome. X is a vector of baseline controls that were selected using the post-double-selection LASSO procedure (Belloni et al., 2014); however, we always control for outcomes measured at baseline whenever they are available. ν are union council fixed effects, which we pre-registered to include (note that union councils are rural administrative units, and each union council consists of around nine villages). We cluster standard errors at the village level, which is our unit of randomization.

Following the Hypotheses outlined in Section 3.3, we expect that β_1, β_2, and β_3 will all be positive (but negative for stereotypes and discriminatory opinions); also, to the extend that spillovers in the Central treatment may be stronger due to the presence of Central individuals, we might expect that $\beta_2 > \beta_1$. Note that care should be applied when interpreting β_3, as it reflects a comparison between 'central' individuals in the Central treatment and individuals in the control group who are selected randomly.

As research assistants visited ethnic majority households in person to screen the documentary film, participation was very high (96.25%). Moreover, research assistants called households (using mobile numbers collected at baseline) to fix a day and time for the screening visit, which reduced non-participation due to unavailability and increased the take-up. Due to high compliance, the ITT effects should be roughly equal to the treatment-on-treated (TOT) effects. We only report ITT results.

[12] When estimating effects on the spillover sample, we instead include a single Treatment dummy corresponding to the Central treatment.

Inference and multiple hypotheses testing. Since we test many hypotheses, we correct *p*-values using the Westfall-Young adjustments (Westfall & Young, 1993, FWER *p*-values). For this, we use 1,000 bootstrap resampling. For each sample type (treated Bengali, untreated Bengali, and untreated Santal), we consider four families of hypotheses as they are measured in different ways: (1) all self-reported beliefs, opinions, and interactions, (2) measures of prosociality, (3) administrative data, and (4) the casual work field experiment. Therefore, we have 12 families of outcomes across three types of samples. In the main tables, we report these *p*-values. We also compute *p*-values by using randomized-based inference (RI *p*-values) with randomization permuted at the village level (Young, 2019, with 1,000 replications). However, RI *p*-values and conventional *p*-values appear nearly identical, so we do not report the RI *p*-values in the main tables.

4 Results

4.1 Information acquisition

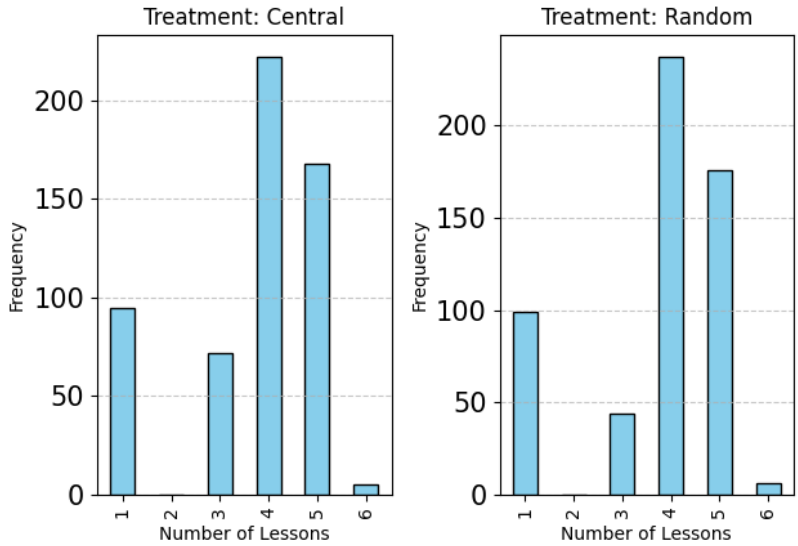
We begin by investigating whether the documentary delivered new information about Santals to the targeted Bengalis. To do this, we use data from post-screening interviews with open-ended questions, focusing on whether Bengalis acquired new information and, if so, the nature of this information. These interviews were conducted only with Bengalis in the 'Random' and 'Central' treatment groups, as those in 'Control' watched a documentary on flower farming unrelated to Santals' lives.

Immediately after finishing the documentary, we asked participants, *"Name five new things you learned from the video."* Field assistants recorded keywords based on responses. For example, if a Bengali said, "I did not know that Santal students are so eager to learn and continue education," the keyword "student" would be recorded. Similarly, if a Bengali mentioned, "I had no idea they struggle so much more in agriculture than we do!", the keyword would be "agriculture". We opted to record keywords rather than full conversations to reduce the workload of field assistants.

We find that 82% of Bengalis who watched the documentary learned at least one new thing about Santals, with an average of 3.7 new pieces of information received. This learning rate was similar across the 'Random' and 'Central' arms. Figure 3 shows the distribution, with a median of 4 new insights among those who learned something new. Importantly, over two-thirds gained four or more new pieces of information about Santals. Note that some Bengalis acquired even more than five new pieces of information, as enumerators were instructed to record all new lessons mentioned by the participants.

Figures A6-A7 (Appendix A) provide histograms with a breakdown of the most frequently mentioned new lessons/information. The top three areas include: (1) the additional challenges Santals face in agriculture, (2) successful Santal teachers in public colleges, and (3) the eagerness of Santal children to continue their education. Therefore, we believe our documentary film was successful at delivering new information to Bengalis about the economic circumstances of Santals: a large number of participants learned new things and a substantial proportion acquired in-

Figure 3: Frequency of new information acquisition among treated ethnic majority



Note: The sample includes treated Bengalis in the two treatment arms. This figure reports frequency of answers to the question "Name five new things you learned from the video".

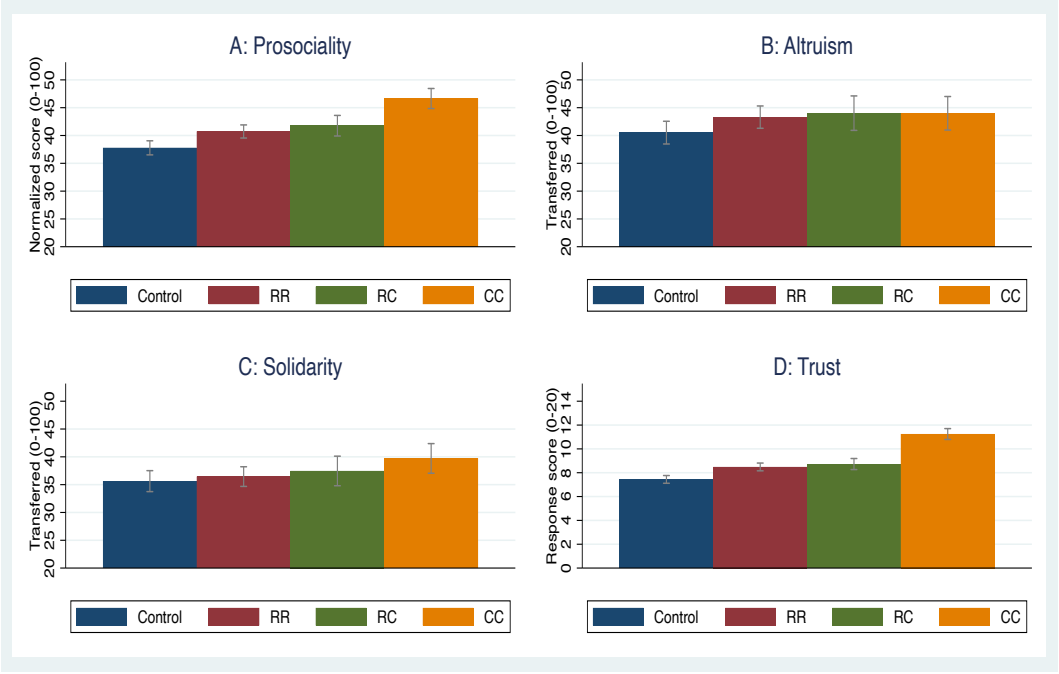
depth knowledge (four or more new pieces of information). This newfound information, through indirect contact, has the potential to shift Bengalis' perceptions and behaviors toward Santals. Research on intergroup contact shows that even indirect or imagined contact has the potential to reduce prejudice and encourage positive social interactions (Turner et al., 2007; Miles & Crisp, 2014).

4.2 Treatment effects on prosociality

We begin by presenting results on the three main behavioral outcomes collected during the first endline through lab-in-the-field experiments and an experimentally validated survey question: altruism, solidarity, and trust toward non-coethnics. Using these three measures, we constructed a composite 'prosociality' measure that combines the three outcomes with equal weight to each and normalized it to have a score between 0-100, where a higher value corresponds to being more prosocial. Figure 4 presents the average for each outcome by treatment.

Following the intervention, treated Bengalis in all three treatment conditions (RR, RC, and CC) displayed increased prosocial behavior towards Santals compared to the control group (t-test: all $p < 0.01$). This pattern holds across all three dimensions of prosociality: altruism, solidarity, and trust. In terms of altruism, Bengalis in all three treatment groups (RR, RC, and CC) transfer significantly more resources to Santals than those in the control group (t-test: all $p < 0.10$). However, no significant differences emerged between the treatment groups themselves. Regarding solidarity, the CC arm showed significantly higher transfers than both the control arm (t-test: $p = 0.015$) and the RR arm (t-test: $p = 0.040$). Finally, trust towards non-coethnics increased progressively across the groups, with Bengalis in the control group showing the lowest levels of trust, while those in the CC arm exhibited the highest (t-test: all $p < 0.01$, except for RR vs RC). It is important to note that treatment differences between CC and control should be interpreted in light of the fact that individuals in CC are not randomly selected whereas those in

Figure 4: Prosocial outcomes at endline



Note: The sample includes treated Bengalis in the two treatment arms and in control arm. The bars indicate 95% confidence intervals. Two-sided t-tests
Graph A: Control vs RR: $p = 0.0009$; Control vs RC: $p = 0.0005$; Control vs CC: $p = 0.0000$; RR vs RC: $p = 0.3378$; RR vs CC: $p = 0.0000$; RC vs CC: $p = 0.0002$.
Graph B: Control vs RR: $p = 0.0565$; Control vs RC: $p = 0.0603$; Control vs CC: $p = 0.0579$; RR vs RC: $p = 0.6954$; RR vs CC: $p = 0.7002$; RC vs CC: $p = 0.9918$.
Graph C: Control vs RR: $p = 0.5367$; Control vs RC: $p = 0.2774$; Control vs CC: $p = 0.0145$; RR vs RC: $p = 0.5276$; RR vs CC: $p = 0.0401$; RC vs CC: $p = 0.2399$.
Graph D: Control vs RR: $p = 0.0000$; Control vs RC: $p = 0.0000$; Control vs CC: $p = 0.0000$; RR vs RC: $p = 0.4011$; RR vs CC: $p = 0.0000$; RC vs CC: $p = 0.0000$.

control are. However, our main interest lies in evaluating the causal impact of the intervention on random individuals in RR and RC, rather than on the central individuals.

We next estimate treatment effects using specification 2. Table 1 presents these results. In Column 1, we report the treatment effects on the prosociality index. We standardized the prosociality variable (described above) by subtracting the control group mean and dividing that difference by the standard deviation of the control group. This way the control group of the prosociality index has a mean of 0 and a standard deviation of 1, and the coefficients in Column 1 are in SD units. We find that prosociality in all treatment arms increases significantly following the intervention: by 0.24 SD in RR, 0.38 SD in RC, and 0.73 SD in CC (all $p < 0.01$). The increase in prosociality in CC is also significantly higher than in RC and RR (both $p < 0.01$); however, the treatment effects in RR and RC are not statistically distinguishable ($p = 0.118$).

In Columns 2-4, we report the treatment effects on each component of the prosociality index. In Column 2, we observe that sharing with a minority in the dictator game is higher in the 'Central' treatment relative to the control group. This is true for both randomly selected and centrally selected individuals. In terms of magnitude, individuals in this treatment group give 5.4 and 6.2 additional Taka relative to the control group. Since the control group shares on average 40.5 Taka, the treatment effect amounts to about 13% and 15.3% additional sharing, respectively. Note that giving is also higher in the Random treatment, however, the effect is smaller and not

Table 1: Treatment effects on prosociality of the treated ethnic majority

	Prosociality Index	**Altruism**	**Solidarity**	**Trust**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
Random in Random (RR)	0.236***	2.675	2.939**	0.949***
	(0.074)	(2.250)	(1.483)	(0.312)
Random in Central (RC)	0.377***	5.434***	2.506	1.704***
	(0.098)	(1.980)	(1.814)	(0.455)
Central in Central (CC)	0.725***	6.232***	4.931***	4.223***
	(0.104)	(2.219)	(1.769)	(0.434)
Control mean	-	40.52	35.63	7.44
		[23.77]	[21.95]	[3.79]
RR=RC p-values	0.118	0.203	0.796	0.067
RR=CC p-values	0.000	0.136	0.210	0.000
RC=CC p-values	0.000	0.696	0.150	0.000
Observations	1,512	1,516	1,515	1,513
FWER p-values on RR	0.004	0.127	0.032	0.004
FWER p-values on RC	0.000	0.006	0.127	0.000
FWER p-values on CC	0.000	0.006	0.006	0.000

Robust standard errors clustered at village-level
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in both treatment arms and the control arm. RR is the treatment arm where participants were selected randomly and are from the *Random* arm; RC is the sample where participants were selected randomly and are from the *Central* arm; CC is the sample where all participants had high diffusion centrality, were selected using the 'gossip' method, and are from the *Central* arm. All specifications include union council fixed effects, outcome measured at baseline, and other controls selected using the post-double selected LASSO (Belloni et al., 2014). FWER p-values correspond to multiple hypotheses testing-adjusted p-values computed using the Westfall & Young (1993) corrections. To compute the FWER-adjusted p-values, we consider the measures of prosociality of the treated Bengalis as a single family of outcomes. Outcomes in the columns are as follows: (1) Prosociality index aggregates altruism, solidarity, and trust measures by putting equal weight on each and then standardizing it, such that the control group has a mean of 0 and a standard deviation of 1—therefore, the coefficients in column 1 are in SD units; (2) Altruism or the amount (0-100) transferred to a minority measured using the dictator game (Eckel & Grossman, 1996); (3) Solidarity or the amount (0-100) transferred to a minority, conditional on a risk shock, measured using the solidarity game (Selten & Ockenfels, 1998); (4) Trust is the level of trust towards minorities measured using the Global Preference Survey question (Falk et al., 2018), which was answered on a scale 0-20. We also report the control group's means and standard deviations in brackets.

statistically significant. Nevertheless, the tests reported at the bottom of the table indicate that we cannot reject the equality of the coefficients in any pairwise comparison.

Turning next attention to the solidarity game reported in Column 3, we observe again larger sharing by centrally selected individuals in the Central arm than the randomly selected individuals, though differences are not statistically significant ($p = 0.150$). Sharing in the Random arm also increases by about 8.1% ($p < 0.05$) relative to the control group. Again, we cannot reject pairwise equality of the coefficients in the Random arm and the two Central arms.

Finally, in Column 4 we present results from the trust game. We find statistically significant improvements in interethnic trust that are more pronounced for individuals in the CC arm than the other two treatment arms. Specifically, trust in CC improves by 57% relative to the control group. It also improves by 22.4% in RR and by 40.3% in RC, the difference in the treatment effects between these two arms being statistically significant ($p = 0.067$).

We also assess the impact of the intervention on other outcomes presented in Table A2 in the Appendix. Across the various outcomes considered, we observe that the intervention did not lead to significant changes in the three treatment arms, with the exception of the self-reported willingness to help (Column 6). Specifically, we find that individuals in the Random treatment (RR) and nominated individuals in the Central treatment (CC) express a higher willingness to help relative to the control group, an increase of about 6%. This finding is consistent with the responses to post-screening qualitative interview questions on viewers' intentions to support ethnic minorities.

To summarize, these findings indicate that behavior in the experimental games was different in the treatment groups relative to those in the control group. Moreover, when comparing the behavior of randomly treated individuals across the two treatment arms (RR versus RC), it emerges that the most robust shifts are evident in the Central arm. This suggests that the presence of centrally treated individuals in this treatment arm may have had a compounding effect on the behavior of the randomly treated individuals, extending beyond the direct impact of the intervention.

4.3 Potential mechanisms

We next examine possible channels for the treatment effects of the intervention documented above. We consider three main classes of channels. The first pertains to whether the documentary influenced participants' beliefs, attitudes, and interactions with the minorities. That is, we examine whether the informational content of the documentary may have shaped individuals' perceptions and behavior, fostering a more positive engagement with minority groups. The second channel relates to the emotional imprint stirred by the contents of the documentary film. Here, we examine the emotional impact of the narrative and visual elements of the documentary, to assess whether the film may have elicited empathetic responses or emotional connections that subsequently influenced participants' actions. The third channel examines Bengali people's intentions and motivations to help Santals, using a rich set of qualitative data collected through in-person interviews conducted immediately after the film screening. In addition, we examine and rule out two alternative mechanisms: experimenter demand effects and social desirability bias.

Stereotypes, discriminatory attitudes, and interactions. Using quantitative survey data, we first examined whether this new information altered pre-existing stereotypes and discriminatory opinions towards Santals and if it promoted cross-ethnic interactions within villages. Table 2 presents results on these intermediate outcomes collected through our endline survey: stereotypes, discriminatory opinions, and behaviors. Results are presented in two panels: the first panel (Columns 1 and 2) presents results on negative stereotypes and discriminatory opinions

Table 2: Potential beliefs and interactions channels for the treated ethnic majority

	Panel A: Beliefs		Panel B: Interactions				
	Stereotypes	Opinions	Interaction with Santal	Interaction with Bengali	Visit Santal	Visit Bengali	Intercultural Competence
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Random in Random (RR)	-0.012	-0.035	0.026	0.037	0.088	0.006	0.046
	(0.074)	(0.079)	(0.076)	(0.088)	(0.069)	(0.078)	(0.073)
Random in Central (RC)	-0.063	-0.056	0.021	-0.122	0.078	-0.043	-0.038
	(0.096)	(0.119)	(0.088)	(0.109)	(0.087)	(0.096)	(0.110)
Central in Central (CC)	-0.003	-0.067	0.057	-0.027	0.095	0.035	-0.008
	(0.094)	(0.105)	(0.091)	(0.116)	(0.081)	(0.092)	(0.093)
RR=RC *p*-values	0.581	0.853	0.955	0.137	0.906	0.602	0.427
RR=CC *p*-values	0.921	0.747	0.736	0.566	0.918	0.748	0.517
RC=CC *p*-values	0.496	0.920	0.717	0.451	0.844	0.357	0.774
Observations	1,519	1,519	1,519	1,519	1,519	1,519	1,519
FWER *p*-values on RR	1.000	1.000	1.000	1.000	0.808	1.000	0.998
FWER *p*-values on RC	0.998	0.999	1.000	0.906	0.979	1.000	1.000
FWER *p*-values on CC	1.000	0.998	0.998	1.000	0.885	1.000	1.000

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in both treatment arms and the control arm. RR is the treatment arm where participants were selected randomly and are from the *Random* arm; RC is the sample where participants were selected randomly and are from the *Central* arm; CC is the sample where all participants had high diffusion centrality and were selected using the 'gossip' method, and are from the *Central* arm. All specifications include union council fixed effects, outcome measured at baseline, and other controls selected using the post-double selected LASSO (Belloni et al., 2014). FWER *p*-values correspond to multiple hypotheses testing-adjusted *p*-values computed using the Westfall & Young (1993) corrections. To compute the FWER-adjusted *p*-values, we consider the measures of beliefs and interactions of the treated Bengalis sample as a single family of outcomes. All outcomes have been control group standardized, where the mean of the control group is 0 and SD is 1. Outcomes in the columns are as follows: (1) stereotypes about non-coethnics (negative coefficient indicates favorable perception); (2) discriminatory opinions about non-coethnics (negative coefficient indicates favorable perception); (3-4) social interactions with non-coethnics and coethnics; (5-6) frequency of visits to non-coethnics' and coethnics' house; (7) competence about the non-coethnics' culture (a proxy for interethnic interactions). For outcomes in columns 3-7, a positive coefficient indicates favorable outcomes.

about ethnic minority Santals, while the second panel (Columns 4-7) concerns reported behavior. Across the board, we do not find that the intervention affected the beliefs that majorities hold about the minority group or the extent to which they interact with them.[13] Appendix Table A8 presents the equivalent analysis for untreated Bengalis, showing again no consistent evidence of changes in beliefs or interactions. The observation that intergroup behavior shifts without corresponding changes in attitudes is in line with findings from Paluck et al. (2021) and Ghosh et al. (2023).

To understand how preexisting attitudes might have influenced the documentary's impact, we conducted heterogeneity analyses by baseline measures of stereotypes (Tables A22 and A23), discriminatory opinions (Tables A24 and A25), and village ethnic composition (Tables A26 and A27). The analysis revealed that Bengalis with stronger negative stereotypes and discriminatory views about Santals were less likely to show solidarity with them. Interestingly, when examining the overall prosociality index, we only observed a statistically significant heterogeneity by discriminatory opinions in the 'Central' treatment arm. We do not observe any consistent pattern regarding the heterogeneous impacts on updating biased beliefs by baseline biased beliefs

[13] For completeness, Appendix Tables A6 and A7 present disaggregated outcomes, for stereotypes and opinions, respectively.

or village ethnic composition (a proxy for village-level contact). However, an interesting pattern emerged regarding network-central Bengalis. In villages with a higher proportion of Santals, these central Bengalis exhibited lower prosocial tendencies towards Santals. This pattern in behavior is similar to the findings in Siddique & Vlassopoulos (2020), which suggests that being a numerical minority can make Bengalis' ethnic identity more salient and potentially lead to harsher behavior towards Santals. Therefore, the main takeaway is that while the documentary film's impact may have varied slightly due to pre-existing biased beliefs and village diversity, it did not directly change Bengalis' underlying stereotypes or discriminatory views towards Santals.

We cannot definitively isolate the reasons why biases and opinions remained unchanged despite receiving new information. However, we propose three potential explanations. Firstly, studies in social psychology suggest changing cognitive components of prejudice (e.g., negative stereotypes) can be more difficult than changing affective components (e.g., feelings and emotions) through indirect contact (Tropp & Pettigrew, 2005; Turner et al., 2007). Secondly, a single documentary film might not be enough to dismantle deeply ingrained, generational biases. Research on resistance to change suggests that individuals often cling to existing beliefs, even when presented with contradictory evidence (Watson, 1971). Therefore, Bengalis may find it challenging to fundamentally shift their views on Santals, given long-standing social conditioning. Finally, expressing less prejudiced views about Santals could be seen as a deviation from the social norm, potentially leading to social disapproval or ostracism. Studies on social conformity demonstrate the powerful influence of group norms on individual behavior (Asch, 2003). That is, Bengalis may be reluctant to openly challenge widespread negative stereotypes for fear of social consequences. It is also important to consider that resistance to change and social conformity likely interact, making it difficult to isolate their individual impact. Additionally, while the documentary may not have produced immediate shifts in attitudes, perhaps it planted seeds of doubt or reflection that could lead to change over a longer time horizon.

Emotion channels. On the other hand, the new information clearly induced empathy among ethnic majority viewers. We analyzed Bengali viewers' facial expressions during the screening using emotion-detection software. Field assistants took candid photos of participants at random times while they were watching the documentary.[14] Later, we used the Emotimeter App (developed by reAImagine), which uses machine learning with a pre-existing training dataset, to identify the primary emotions being activated during the screening. Studies in psychology confirm that facial expressions are indeed reliable indicators of emotion (Ekman, 1993). The literature also suggests that it is difficult to fake emotions like sadness, anger, and fear, and their expression is more universal across cultures than previously thought (Ekman, 1993).

We were unable to analyze the facial expressions of 212 people. About 180 photos could not be analyzed (which is similar across treatment arms) due to insufficient sharpness and clarity, which is a prerequisite for accurate facial expression analysis. The remaining participants did not provide consent for photo use. We report the raw differences in Figure 5 and regression

[14]We obtained informed consent from participants twice: once at signup and again right before screening. Notably, almost all participants who opted to participate consented to having their photos taken, with a few exceptions to photo consent by women due to religious reasons. All screening took place during the day, in participants' front yard (locally known as *uthon*), to ensure the lighting is sufficient to take clear photos.

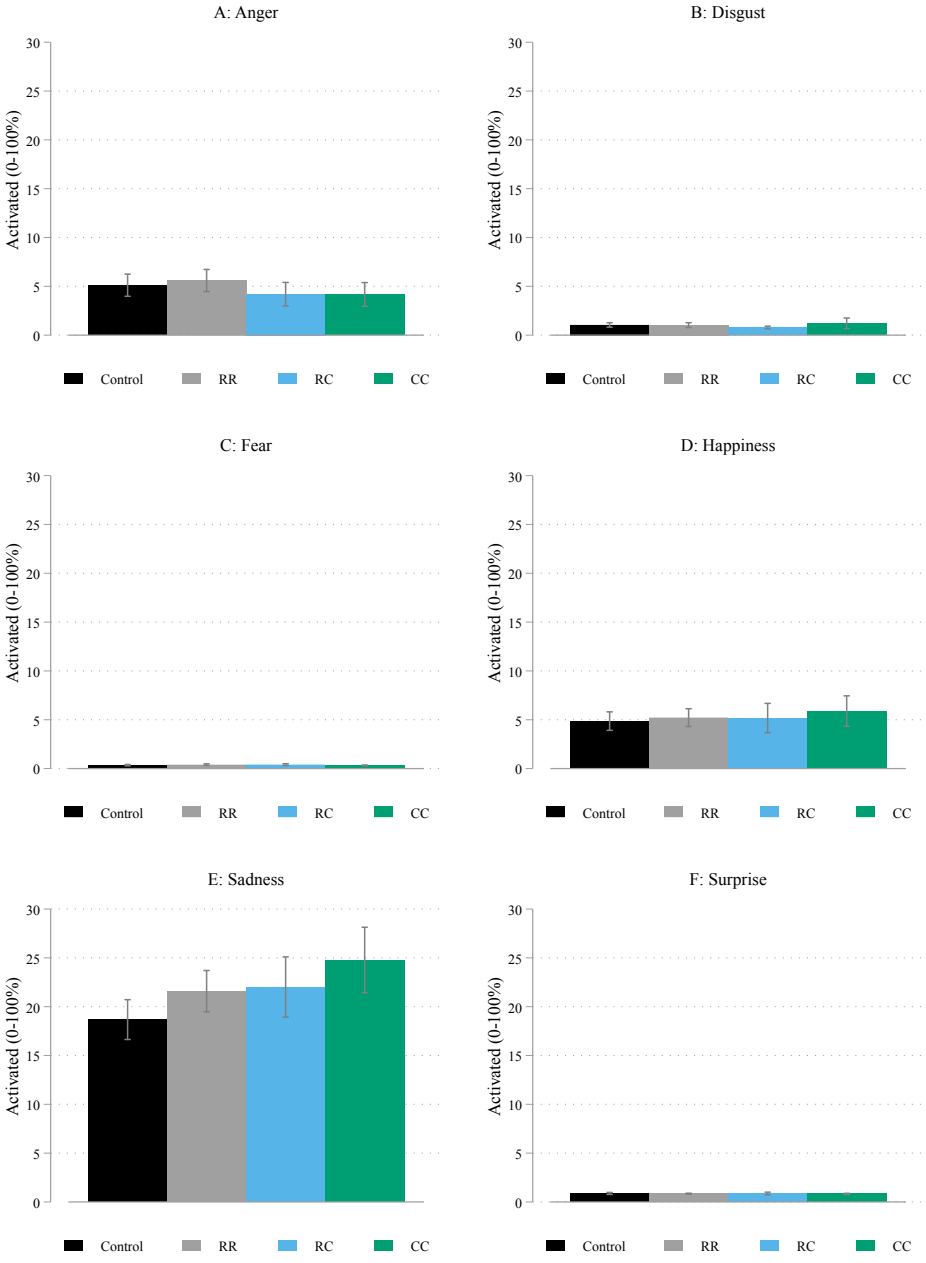
estimates in Table A10. The first observation to make here is that Sadness is the emotion that was triggered the most in the control group, followed by Anger and Happiness. Relative to this baseline, we find that the CC group displays significantly more instances of Sadness. The other two treatment groups, RR and RC, also exhibit increased Sadness relative to the control group, though these differences are not statistically significant. Moreover, Treatment CC displays more instances of Happiness than the control group, with this effect being significant at 10%.[15] This finding aligns with the literature on how emotions influence decision-making (Lerner et al., 2015). Emotions can focus attention to different situations (Schwarz & Bless, 1991), help set various goals (Zeelenberg et al., 2008), promote effective social choices (Keltner et al., 2014), and facilitate information sharing (Berger & Milkman, 2012). Sometimes, people engage in prosocial behavior as a means of managing negative emotions, especially sadness (Schaller & Cialdini, 1988). Therefore, the heightened level of sadness observed among the viewers in the CC arm may have made the minority group's struggles more salient and increased their determination to help. It might also have prompted discussion within their social networks, potentially explaining the positive shifts in altruism, solidarity, and trust observed in both our main sample and the spillover effects discussed in Section 4.4.

Another explanation could be that people who are more empathic in general would be more moved by the documentary film, and hence, would be more prosocial towards the Santals. To explore this interpretation, we also measured three types of empathic tendencies of participants using the Interpersonal Reactivity Index (Davis, 1983) at baseline: (i) perspective-taking (which is the tendency to spontaneously adopt the psychological view of others in everyday life), (ii) empathic concern (which is the tendency to experience feelings of sympathy or compassion for unfortunate others), and (iii) personal distress (which is the tendency to experience distress or discomfort in response to extreme distress in others). We explain these in more detail in Appendix B.1. Using these three empathic tendency scores (continuous, ranging from 0-28 with higher scores indicating greater empathy), we examine heterogeneity in treatment effects on prosociality. This is done by interacting the scores with the treatment dummies. We report these results in Table A11 in the Appendix. We observe no such heterogeneity except for one case: Bengalis who experience distress in response to observing extreme distress in others seem more likely to exhibit prosocial behavior towards Santals.

Qualitative evidence. While the acquisition of new information (presented in Section 4.1) through the documentary film might not have directly altered Bengali viewers' preexisting beliefs and attitudes, it had the potential to influence their intentions to help the Santals. To investigate these motivations and potential actions, we included additional qualitative questions immediately after they watched the film. We asked open-ended questions focused on their willingness to support Santals, the specific forms of support they would consider, their reasoning, and their intentions to encourage other Bengalis in their villages. Since these questions were only asked to the viewers who watched the documentary on Santals, we cannot claim the following qualitative evidence to be causal. We discuss this analysis in detail in Appendix C. Among the 38 unique

[15] We do not have the data on the precise start and end times of each participant's video viewing, which is why we cannot match photo timestamps to specific moments in the video. This prevents us from determining which content elements induced particular emotions.

Figure 5: Raw differences in emotions of treated ethnic majority



Note: The sample includes treated Bengalis in both treatment arms and the control arm. The sum of all six emotions reported above, along with the neutral emotion (i.e., no emotions), equals 100. This means that each individual's emotions were scored between 0-100. A higher value indicates a stronger presence of that particular emotion.

answers obtained from Bengalis, they generally expressed an intention to help Santals (about 50%) or focused specifically on providing financial aid (about 20%). Poverty and daily struggles were the most frequently cited reasons for wanting to offer support (over 50% of respondents). Furthermore, Bengalis who watched the documentary film expressed a desire to encourage their coethnic neighbors to do the same: about 50% indicated a willingness to encourage neighbors to support Santals generally, and roughly 20% specifically mentioned encouraging financial as-

sistance.

Alternative mechanisms. *Experimenter demand effects:* In our intervention, participants in all three treatment arms received a form of intervention: the main two arms were exposed to information through the documentary we produced, while the control arm received a placebo documentary on flowers—making it an active control. While experimenter demand effects are generally less concerning in active control group designs (Haaland et al., 2023), to alleviate any remaining concerns, we adopt an approach similar to Chopra et al. (2024). At the end of the endline survey, participants were asked the following open-ended question: *"If you had to guess, what would you say was the purpose of this study? You will only get one guess, and if you guess it correctly, you will get 50 Taka."* Enumerators categorized participants' responses into one of eight options, with only one being correct. The results, summarized in Figure A1, indicate that less than 3% of the sample accurately predicted the purpose of the study. Note that participants had a strong incentive to answer this question correctly, as the reward for doing so (50 Taka) was equivalent to one-sixth of their daily income.[16] We then estimate treatment effects on prosociality and beliefs after dropping the 3% sample who correctly guessed the study's purpose. The results of this analysis, presented in Tables A18 and A19, show that all our findings remain robust.

Social desirability bias: Another related concern is social desirability bias, which might arise because of the thematic similarity between the Santal film contents and survey questions. This similarity might still induce some biases, especially among participants more inclined to give socially desirable responses to survey questions. To address this concern, we closely follow the approach in Dhar et al. (2022). Using the 13-point social desirability bias (SDB) scale, we carry out heterogeneity analyses. Table A20 presents the analysis of prosociality measures, while Table A21 provides those of beliefs. Our analysis reveals some evidence of heterogeneity in prosociality within the 'CC' group (limited to network-central people) and in discriminatory opinions. Interestingly, the variation in discriminatory opinions based on the SDB score aligns with our earlier interpretation that conformity to social norms might influence Bengalis' willingness to express negative views towards Santals (Asch, 2003), as people with higher SDB scores are more likely to report discriminatory views towards Santals in both 'RR' and 'CC' groups. Detailed definitions of the SDB scale and its components are provided in Appendix B.1.

4.4 Spillover Effects

We next examine the possible spillover effects of the intervention on untreated ethnic majority (Bengalis) and minority (Santals) within treated villages.

On the untreated ethnic majority. Figure A9 in the Appendix, illustrates the average prosociality index and its three constituent outcomes by treatment. The pattern that emerges is one of a greater impact on the prosocial behavior of people in Central villages, where half of the treated Bengalis were nominated. This observation is confirmed by regression analysis of treatment effects presented in Table 3, which also permits quantification of the effects. In the Central

[16] Participants were informed if they had guessed correctly or not after the casual work field experiment. Those who guessed correctly received their money through mobile money transfers.

Table 3: Spillover effects on prosociality of the untreated ethnic majority

	Prosociality Index	**Altruism**	**Solidarity**	**Trust**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
Random (R)	-0.025	0.550	-1.911	0.196
	(0.240)	(3.684)	(4.464)	(0.369)
Central (C)	0.794***	5.965	10.743***	0.594*
	(0.198)	(3.704)	(4.082)	(0.328)
Control mean	-	33.45	36.50	8.35
		[21.80]	[22.49]	[0.98]
R=C p-values	0.000	0.116	0.001	0.234
Observations	797	800	800	797
FWER p-values on R	0.961	0.961	0.766	0.753
FWER p-values on C	0.000	0.084	0.003	0.039

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes untreated Bengalis in both treatment arms and the control arm. 'Random' and 'Central' are the random and central treatment arms. All specifications include union council fixed effects, outcome measured at baseline, and other controls selected using the post-double selected LASSO (Belloni et al., 2014). FWER p-values correspond to multiple hypotheses testing-adjusted p-values computed using the Westfall & Young (1993) corrections. To compute the FWER-adjusted p-values, we consider the measures of prosociality of untreated Bengalis as a single family of outcomes. Outcomes are defined under Table 1. The last row reports the control group's means and standard deviations in brackets.

arm, we find a substantial and statistically significant treatment effect on the prosociality index (0.794 SD; $p < 0.01$), while no significant effect is detected in the Random arm. Examining the individual outcomes, we find significant increases in giving in the solidarity game (29% of the average in the control group; $p < 0.01$) and more moderate improvements in trust (about 7.1%; $p < 0.10$) in the Central arm. The impact on altruism is also sizeable in the Central arm but not precisely estimated. On the other hand, in the Random treatment arm, we find no statistically significant effects on any of these three outcomes.

We also estimate the impact on other outcomes, presented in Appendix Table A3. Most notable is the effect on friendships in the 'Central' treatment, where we find a significant increase in the number of minority friends. We also find some positive impacts on the subjective well-being of untreated Bengalis in the Random arm.

What are the possible underlying mechanisms driving these positive estimated spillover effects on untreated Bengalis? Note that these effects were more pronounced in 'Central' arm villages, where half of the treated Bengalis were nominated. There are two key aspects that distinguish central individuals in this treatment arm relating to their baseline characteristics and the intervention's impact. Firstly, as shown in Figure A11 in the Appendix, central individuals demonstrated higher levels of altruism toward minorities at baseline. In addition, we have seen earlier that they experienced larger impacts on their prosocial behavior toward the minorities

(Table 1), and displayed stronger emotional reactions to the documentary (Table A10). Secondly, central individuals might possess greater persuasive abilities and broader influence within their communities. Consequently, the heightened empathy toward minorities among central individuals, combined with their potentially greater persuasiveness, likely contributed significantly to shaping the behavior of untreated Bengalis in this treatment arm.

Our analysis of whether social proximity of untreated Bengalis to network-central individuals, measured by walking distance and number of monthly visits, played a significant role suggests that this was not the case (see Table A17 for correlations between average walking distance/visits and various outcomes). This finding is perhaps not surprising, given the residential patterns within villages. Bengalis (and Santals) tend to reside in separate ethnic clusters, known locally as *paras*. This spatial arrangement likely contributes to a relatively homogenous distribution of walking distances (and visits) to network-central households among co-ethnics.[17]

On the untreated ethnic minority. We next turn attention to the possible spillover effects of the intervention on Santals. Table 4 presents estimates on the prosociality index and the three main outcomes (see Figure A10 in the Appendix for raw averages). We also examine the impacts on two measures that could capture any possible improvements in the economic conditions of Santals: food insecurity and new employment.

We find a significant increase in prosociality in both the Random and the Central treatment arms. The effect is more sizeable in the Central arm (0.718 SD) than in the Random arm (0.375 SD), though the two effects are not statistically distinguishable. This pattern seems to be driven mainly by differences in Trust. For the other two outcomes, altruism and solidarity, impacts are positive for both treatments, and larger for the Central treatment, though not statistically significant. In terms of economic outcomes, we also find effects on food security in the Central arm, which improves by 26% ($p < 0.10$). Finally, we also examine impacts on other outcomes in Appendix Table A4. Among the outcomes considered, we find a significant improvement in subjective well-being, which is more pronounced in the Central arm in which this measure improves by about 50%.

To understand how these outcomes came about, note that Table A5 in the Appendix suggests that Santals in treated villages were more likely to increase their interactions with both Santals and Bengalis by visiting them. These visits indicate improved cohesion between the two ethnic groups that could explain the rise in food security (received help from Bengalis) and the consequent rise in trust toward the majority.

5 Impacts on village-level interethnic disputes

To assess whether the intervention generated wider impacts on interethnic relationships in treated villages, we gathered data on interethnic disputes. Due to confidentiality concerns, the data provided by police stations and village counselors are anonymous and aggregated at

[17]Note that we collected data on social proximity to network-central people only from untreated Bengalis. We do not have similar data for treated but randomly selected Bengalis, untreated Santals, or for social proximity between the network-central individuals themselves.

Table 4: Spillover effects on prosociality and well-being of the untreated ethnic minority

	Prosociality				Economic Well-being	
	Prosociality Index	**Altruism**	**Solidarity**	**Trust**	**Food Insecurity**	**New Employment**
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Random (R)	0.375**	1.613	4.740	0.869**	-0.335	-0.001
	(0.190)	(4.680)	(4.275)	(0.352)	(0.326)	(0.082)
Central (C)	0.718***	5.399	6.729	1.705***	-0.690*	0.022
	(0.224)	(4.814)	(4.807)	(0.433)	(0.378)	(0.088)
Control mean	-	38.63	35.52	6.93	2.65	0.41
		[21.99]	[23.01]	[2.82]	[1.42]	[0.49]
R=C *p*-values	0.165	0.329	0.000	0.266	0.874	0.287
Observations	717	721	721	717	721	722
FWER *p*-values on R	0.044	0.915	0.389	0.008	0.389	0.979
FWER *p*-values on C	0.001	0.389	0.201	0.000	0.044	0.915

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes untreated Santals in both treatment arms and the control arm. *Random* and *Central* are the random and central treatment arms, where all ethnic majority Bengalis were randomly selected. All specifications include union council fixed effects, outcome measured at baseline, and other controls selected using the post-double selected LASSO (Belloni et al., 2014). FWER *p*-values correspond to multiple hypotheses testing-adjusted *p*-values computed using the Westfall & Young (1993) corrections. To compute the FWER-adjusted *p*-values, we consider the measures of prosociality and economic well-being of untreated Santals as a single family of outcomes. Outcomes in columns 1-3 are defined under Table 1. The outcome in column 4 is the food insecurity index (where a lower value indicates a more favorable outcome), in column 5 the outcome is a dummy variable that is equal to 1 if the person started a new job in the past 3 months and 0 otherwise. The last row reports the control group's means and standard deviations in brackets.

the village level. We consider two types of complaints, those directed to village counselors and those reported to the police. In this context, complaints filed with village counselors often serve arbitration purposes (locally known as *shalish*). This traditional approach helps avoid the time-consuming, costly, and complicated formal litigation process and is typically governed by the country's Arbitration Act of 2001. Because the primary goal here is to settle disputes (e.g., for land acquisitions, threats of harm, verbal abuse, etc.), outcomes tend to be less severe for both parties. Police complaints, on the other hand, can have much harsher consequences, including jail time, job loss, or social exclusion. These complaints may also lead to more expensive litigation procedures if they proceed to court. Therefore, the economic consequences of police complaints are significantly more severe and long-lasting than those filed with village counselors. Importantly, many disputes never reach the formal complaint stage, indicating that the complaints data we have here captures particularly serious cases.

Table 5 presents treatment effects on interethnic dispute complaints at the village level, which we collected two months after the intervention ended. Columns 1 and 3 report monthly complaints made by ethnic minority Santals against ethnic majority Bengalis, while Columns 2 and 4 report monthly complaints made by ethnic majority Bengalis against ethnic minority Santals. In the regression analysis, we control for village size (the total number of households), and village ethnic diversity (the proportion of Santal households). We also control for the number of complaints at baseline (a month before the intervention started). Our results indicate a reduction in both types of complaints made by Bengalis in the 'Central' treatment arm, with the

Table 5: Treatment effects on interethnic complaints using administrative data

	To village counselors		To police stations	
	by Santal	by Bengali	by Santal	by Bengali
Variables	**(1)**	**(2)**	**(3)**	**(4)**
Random (R)	-0.034	0.052	-0.023	-0.127
	(0.232)	(0.339)	(0.193)	(0.258)
Central (C)	-0.204	-0.174	-0.029	-0.567**
	(0.214)	(0.370)	(0.184)	(0.227)
Control mean	1.078	1.737	0.923	1.538
	[0.850]	[1.554]	[0.839]	[0.942]
Controls:				
Village size	Y	Y	Y	Y
Village ethnic diversity	Y	Y	Y	Y
Complaints at baseline	Y	Y	Y	Y
R=C *p*-values	0.424	0.513	0.976	0.113
Observations	117	117	119	119
R-squared	0.020	0.033	0.016	0.060
FWER *p*-values on R	0.998	0.932	0.998	0.932
FWER *p*-values on C	0.775	0.932	0.998	0.047

Robust standard errors are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports treatment effects on interethnic dispute complaints in a month at the village level, i.e., Santals filing complaints against Bengalis and vice versa. Therefore, the outcomes in all columns are the number of complaints filed against non-coethnics. Columns 1 and 3 report complaints made by ethnic minority Santals against ethnic majority Bengali; and Columns 2 and 4 report complaints made by ethnic majority Bengalis against ethnic minority Santals. Complaint records from three villages were not available at village counselors' offices. Village size is the total number of households per village, and village ethnic diversity is the proportion of Santal households per village. FWER *p*-values correspond to multiple hypotheses testing-adjusted *p*-values computed using the Westfall & Young (1993) corrections. To compute the FWER-adjusted *p*-values, we consider this a single family of outcomes.

reduction being statistically significant in the case of police complaints only ($p < 0.05$). This reduction is substantive, amounting to a 37% decrease compared to the control group mean. For complaints filed by Santals, the coefficients are negative but not statistically significant at conventional levels.

The decrease in police complaints by Bengalis in the 'Central' treatment arm could be attributed to the improved attitudes towards Santals fostered by the intervention. This shift in attitude may have reduced disputes, encouraged informal conflict resolution, or reduced complaints filed by Bengalis possibly due to increased awareness of the strong consequences their actions could have for the already socially disadvantaged Santals. Since the 'Central' arm targeted Bengalis with high diffusion centrality, the spread of information and potential behavior

changes from the documentary may have reached more people within the village. This may have encouraged others in the village to avoid making formal complaints.

On the other hand, the lack of a significant change in complaints filed by Santals to police stations might be due to the existing power difference between the two groups. Note that Santals filed significantly fewer complaints against Bengalis even in the absence of the intervention, as seen in the control group average. This lower complaint rate among Santals in the control arm suggests that the power imbalance might have discouraged them from making formal complaints, potentially leaving less room for further reduction in their complaint behavior in the treatment arms.

6 The casual work field experiment

6.1 The setting

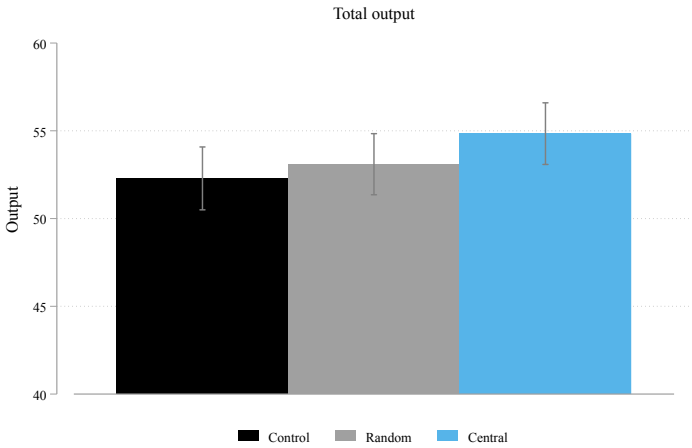
The majority of participants in our intervention were agricultural day laborers or farmers. We conducted our field experiment on casual work immediately after a lean season (April-May), a period of reduced agricultural work and increased financial strain, making casual work opportunities highly attractive (Kaur et al., 2022).

Drawing inspiration from Hjort (2014), we designed a casual work experiment involving pairs of workers, each from a different ethnicity. Our primary focus was on exploring interethnic interactions within a labor-market context and making comparisons across treatment arms. To this end, we carefully matched each majority member with a minority member and avoided altering the ethnic composition of pairs due to concerns regarding statistical power and logistical constraints. Therefore, our field experiment allowed a Bengali and a Santal to collaboratively undertake a small-scale manufacturing job. This job involved producing paper packets, or *thongga*, for a local supplier. See Figure A5 in the Appendix for an illustration of the task and work arrangements. The task lasted three hours, including instructions, training, and a break.

In this task, participants assumed two roles: the *Preparer*, who cut and folded old newspapers into packets, and the *Finisher*, who applied glue and ensured the quality of the packets. After an hour, they switched roles, so each participant experienced both roles. This setup implies that participants could start in the first round as the finisher (referred to as "First") or could be the finisher in the second round (referred to as "Second"). Participants' productivity directly impacted their earnings, with 4 Taka (=4 cents) paid per packet, split equally between the two workers. We measured productivity twice, during the break and at the end, providing data for both workers in both roles. Since, on average, they roughly produce 50 packets during the session, they make on average 200 Taka for the team, thus 100 Taka each. Each participant received a participation fee and a task completion fee, each amounting to 50 Taka. Considering the typical daily wage in this context is approximately 300 Taka, the average earnings (200 Taka) represent roughly two-thirds of a day's income.

We invited 900 Bengali and Santal participants (450 from each group) who had previously participated in our surveys. In total, we recruited 720 casual workers, or 360 pairs, evenly distributed across each treatment arm. We extended invitations beyond our capacity to guarantee

Figure 6: Total casual work output, by treatment



Note: Total causal work output is the combined output (preparer + finisher) of each worker. Each bar shows the average output produced, with 95% confidence intervals. **T-test**, Control vs Random: p-value=0.526; Control vs Central: p-value=0.047; Random vs Central: p-value=0.167.

the formation of 360 majority-minority pairs for the field experiment. Workers not selected for participation received a show-up fee from the local supplier. All Santals in this field experiment came from the spillover sample, and all Bengalis came from the main sample, excluding the network-central Bengalis.

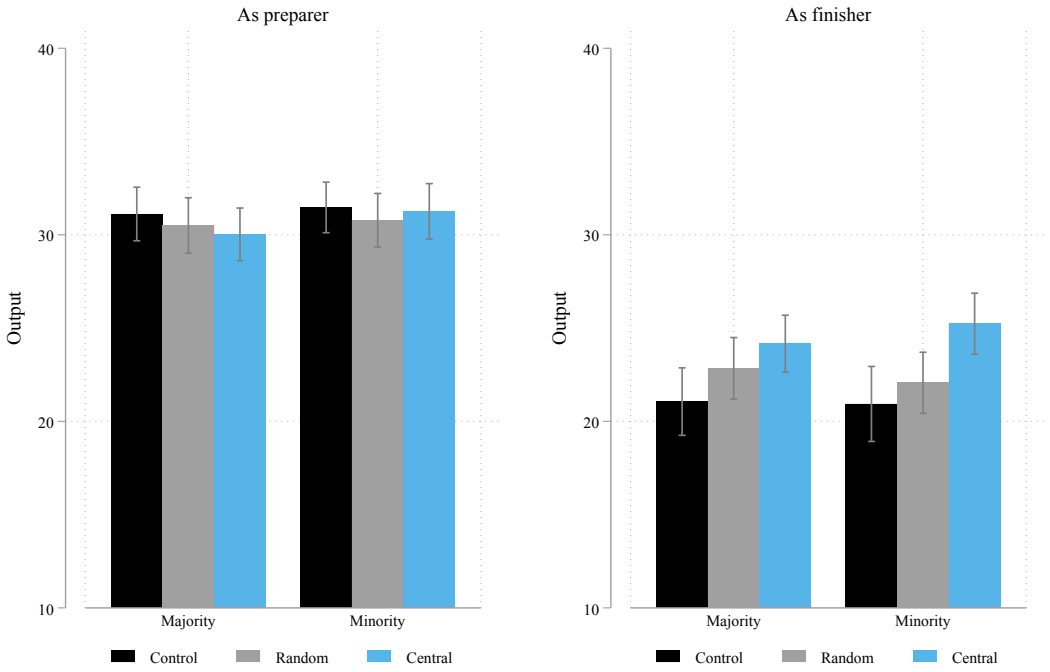
There is evidence suggesting that ethnic prejudices between workers can operate in workplace settings and influence team productivity, even if it results in costs for the discriminatory worker (Hjort, 2014; Hedegaard & Tyran, 2018; Afridi et al., 2020; Marx et al., 2021; Ghosh, 2022). Therefore, we expect that in this task, one's attitudes toward their paired co-worker would influence the effort they put forward, thereby negatively impacting the earnings of both individuals. Our primary interest lies in treatment differences in workers' productivity. We expect that if Bengalis who were exposed to the documentary harbored goodwill towards Santals, they would likely exhibit higher productivity in one or both roles relative to those in the control group.

6.2 Results

The overall output (number of finished packets) produced by pairs of workers showed a marked increase in the 'Central' treatment in comparison to the 'Control' group, as shown in Figure 6 (t-test; p-value=0.047). This gain in output is sizeable, amounting to a 5% increase. Pairwise comparisons between the other groups, 'Random' vs 'Control' and 'Random' vs 'Central', yield no significant differences.

Furthermore, an interesting pattern emerged in the work process. Finishers were consistently provided with sufficient items to process, indicating a bottleneck in this role, whereas Preparers demonstrated spare capacity. This is visually represented in Figure 7, which shows output by position and ethnicity. It is evident that Preparers produced on average about 36% more items than Finishers were able to process.

Figure 7: Total causal work output, by ethnicity and treatment



Note: Each bar shows the average output produced, with 95% confidence intervals. Majority refers to Bengali workers and Minority refers to Santal workers. **Majority Bengali as preparer:** Control vs Random: $p = 0.559$; Control vs Central: $p = 0.288$; Random vs Central: $p = 0.649$. **Minority Santal as preparer:** Control vs Random: $p = 0.497$; Control vs Central: $p = 0.839$; Random vs Central: $p = 0.652$. **Majority Bengali as finisher:** Control vs Random: $p = 0.154$; Control vs Central: $p = 0.011$; Random vs Central: $p = 0.252$. **Minority Santal as finisher:** Control vs Random: $p = 0.392$; Control vs Central: $p = 0.001$; Random vs Central: $p = 0.008$.

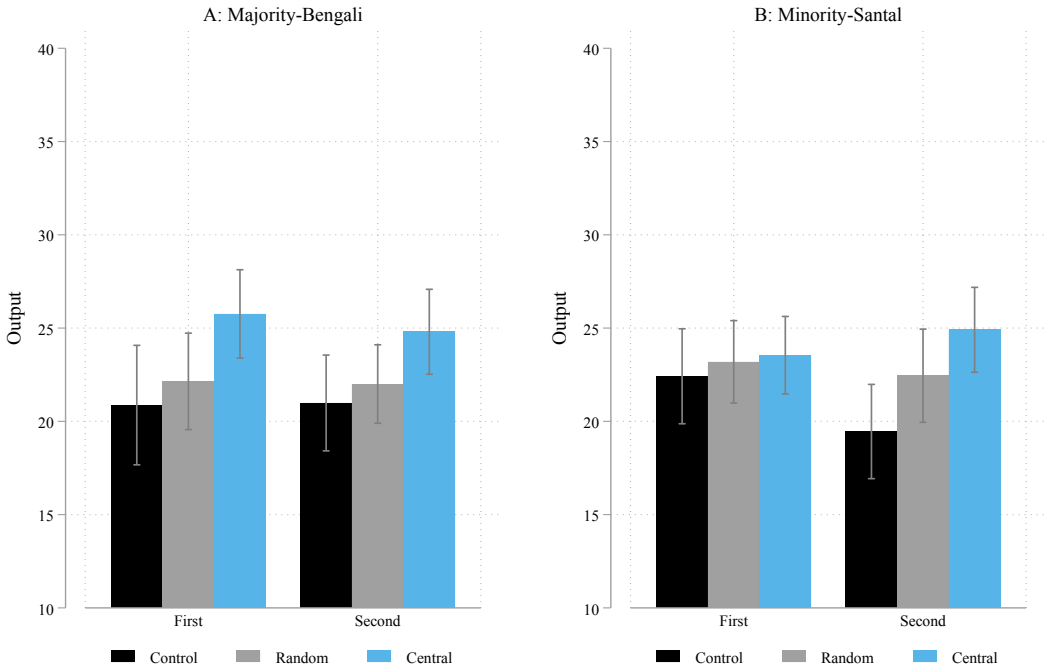
When considering treatment effects for Finishers, whose productivity determined earnings, both ethnic majority and minority participants in the 'Central' treatment exhibited higher productivity than their counterparts in the 'Control' group, with a 15% improvement for the majority and a 21% improvement for the minority. For Preparers, we do not find significant treatment differences, suggesting that the overall treatment differences we see in Figure 6 are driven by Finishers. Regression analysis that includes control variables selected through post-double selection LASSO reported in Tables A13, A14, and A15 in the Appendix corroborates these findings.[18]

Further insights can be obtained by examining the productivity of Finishers (by ethnicity) separately when they were first or second (see Figure 8). In particular, for the Majority, the similarity in productivity, regardless of the order in which they assumed the two positions, suggests that the disparity between the Central and Control groups is predominantly due to the treatment effect and not a response to the productivity of the Minority as Finishers. Conversely, for the Minority, we find that the difference between Central and Control is more pronounced when they acted as second Finishers, implying some responsiveness to the higher productivity of the Majority when they served as the first Finisher.

Figure A13 presents the productivity of Preparers by ethnicity and order. No discernible

[18]Heterogeneity analysis by baseline measures of prosociality, stereotypes, and discriminatory opinions reveal no significant patterns (results available upon request).

Figure 8: Output as Finisher, by ethnicity



Note: Participants assigned the finisher role at the start of the task are classified as 'First'. Participants completing the task in the finisher role are classified as 'Second'. **A: Majority Bengali going 'first' as finisher** Control vs Random: $p = 0.545$; Control vs Central: $p = 0.018$; Random vs Central: $p = 0.045$. **A: Majority Bengali going 'second' as finisher** Control vs Random: $p = 0.549$; Control vs Central: $p = 0.030$; Random vs Central: $p = 0.078$. **B: Minority Santal going 'first' as finisher** Control vs Random: $p = 0.654$; Control vs Central: $p = 0.499$; Random vs Central: $p = 0.817$. **B: Minority Santal going 'second' as finisher** Control vs Random: $p = 0.101$; Control vs Central: $p = 0.002$; Random vs Central: $p = 0.156$.

pattern emerges here, which is consistent with our earlier observation that the task of Preparers could be carried out faster, meaning they had excess capacity.

6.3 Discussion of potential channels

What explains the rise in productivity we observe in pairs involving treated Bengalis? Our interpretation is that working harder is an expression of prosociality toward one's co-worker in an attempt to raise their income (Rotemberg, 1994; Dur & Sol, 2010). That is, Bengali participants who were exposed to the documentary feel more prosocial toward Santals and increase their efforts in order to boost the income of their co-workers.[19] In turn, Santals respond by also raising their effort, either to conform or due to peer pressure (Kandel & Lazear, 1992; Mas & Moretti, 2009; Georganas et al., 2015). To provide empirical support for this interpretation, we examine the productivity of Finishers separately based on whether they were first or second to assume this role (see Figure 8). Interestingly, for Bengalis, productivity is similar regardless of the order in which they assumed the two positions. On the other hand, for Santals, we find that the difference between Central and Control is more pronounced when they acted as second

[19]This interpretation is supported by evidence reported in Table A16 indicating a significant positive correlation between productivity as a finisher and trust levels measured in the first endline.

Finishers, implying some responsiveness to the higher productivity of the Bengalis when they served as the first Finisher.

These patterns are consistent with elevated prosociality acting as a driver for Bengalis and conformity acting as a driver for Santals. Our findings resonate with findings in Bhalotra et al. (2023), who find that intergroup contact improves coordination in mixed groups.

6.4 A simple model

Here, we present a simple model to help explain some of the features of finishers' productivity displayed in Figure 8. The model abstracts away from the role of the preparer and focuses on the finishers. There are two rounds, each with a designated finisher: if a worker is a finisher in the first round, we refer to them as the *first finisher*, while if a worker is the finisher in the second round, we refer to them as the *second finisher*. The second finisher j observes the first finisher i's effort, $j \neq i$, while the reverse is not true. Workers can be one of two ethnicities: $i = B$ (Bengali) and $i = S$ (Santal).

For $i \neq j$, as in our experiment, the team output (number of packets) is given by the sum of efforts of the two finishers:

$$q = e_f^i + e_s^j, \tag{3}$$

where e_f^i is the effort of the *first finisher* i and e_s^j is the effort of the *second finisher* j. Note that the subscripts f and s denote the "first" and "second" finishers, respectively. The team receives $2w$ per packet produced, with each member receiving w (equivalent to 2 Taka in our experiment).

The utility function of the first finisher i is defined as:

$$U_f^i = w\left(e_f^i + e_s^j\right) - \frac{1}{2}\left(e_f^i\right)^2 - \frac{1}{2}\theta^i\left(e_f^i - e_s^j\right)^2 + \alpha^i w\left(e_f^i + e_s^j\right), \tag{4}$$

while that of the second finisher j is given by:

$$U_s^j = w\left(e_f^i + e_s^j\right) - \frac{1}{2}\left(e_s^j\right)^2 - \frac{1}{2}\theta^j\left(e_s^j - e_f^i\right)^2 + \alpha^j w\left(e_f^i + e_s^j\right), \tag{5}$$

where $\alpha^i \geq 0$, and $\theta^i \geq 0$. The first two components of (4) and (5) represent the individual proceeds from production (i.e., wq) minus the individual cost of effort ($C\left(e_f^i\right) = \frac{1}{2}\left(e_f^i\right)^2$ and $C\left(e_s^j\right) = \frac{1}{2}\left(e_s^j\right)^2$). The third part of the utility function captures workers desire to *conform* to or *match* the effort of the other worker, with θ^i denoting worker i's taste for conformity. Finally, the last component of the utility function captures the *altruistic* concern of a worker toward their partner, where α^i is the intensity or weight a worker i places on the income of worker j.

As stated above, each worker $i = B, S$, can be a first or second finisher. Thus, the game is *sequential* in which the first finisher plays first while the second one plays second.

6.4.1 First case: The Santal worker is the first finisher and the Bengali worker is the second finisher

Using (4), the *first finisher Santal*'s utility function is given by:

$$U_f^S = w\left(e_f^S + e_s^B\right) - \frac{1}{2}\left(e_f^S\right)^2 - \frac{1}{2}\theta^S\left(e_f^S - e_s^B\right)^2 + \alpha^S w\left(e_f^S + e_s^B\right), \tag{6}$$

while, using (5), the *second finisher Bengali*'s utility function is equal to:

$$U_s^B = w\left(e_f^S + e_s^B\right) - \frac{1}{2}\left(e_s^B\right)^2 - \frac{1}{2}\theta^B\left(e_f^S - e_s^B\right)^2 + \alpha^B w\left(e_f^S + e_s^B\right). \tag{7}$$

We solve the model using backward induction. Thus, we first solve the *second finisher Bengali*'s problem. The first-order condition is equal to:

$$e_s^B = \frac{\left(1+\alpha^B\right)w + \theta^B e_f^S}{1+\theta^B}. \tag{8}$$

Clearly, the effort of the second-finisher Bengali e_s^B is increasing with the first-finisher Santal's expected effort e_f^S, their income w, and their degree of altruism α^B.

By plugging (8) into (6), we obtain

$$U_f^S = w\left(1+\alpha^S\right)\left(\frac{\left(1+\alpha^B\right)w}{1+\theta^B} + \left(\frac{1+2\theta^B}{1+\theta^B}\right)e_f^S\right) - \frac{1}{2}\left(e_f^S\right)^2 - \frac{1}{2}\theta^S\left(\frac{\left(1+\alpha^B\right)w}{1+\theta^B} - \frac{e_f^S}{1+\theta^B}\right)^2.$$

The first-order condition leads to

$$e_f^S = \frac{\left(1+\theta^B\right)\left(1+2\theta^B\right)\left(1+\alpha^S\right)w + \theta^S\left(1+\alpha^B\right)w}{\left(1+\theta^B\right)^2 + \theta^S}. \tag{9}$$

It is easily verified that

$$\frac{\partial e_f^S}{\partial \alpha^S} > 0, \ \frac{\partial e_f^S}{\partial \alpha^B} > 0.$$

By plugging (9) into (8), we obtain

$$e_s^B = \frac{\left(1+\theta^S+\theta^B\right)\left(1+\alpha^B\right)w + \theta^B\left(1+2\theta^B\right)\left(1+\alpha^S\right)w}{\left(1+\theta^B\right)^2 + \theta^S}.$$

It is easily verified that

$$\frac{\partial e_s^B}{\partial \alpha^S} > 0, \ \frac{\partial e_s^B}{\partial \alpha^B} > 0.$$

Finally, since the team output is given by (3), that is, $q = e_f^S + e_s^B$, we have

$$\frac{\partial q}{\partial \alpha^S} > 0, \ \frac{\partial q}{\partial \alpha^B} > 0.$$

The more the *first finisher Santal* and the *second finisher Bengali* are altruistic and care about each other's income, the higher is their effort and the total quantity produced.

6.4.2 Second case: The Bengali worker is the first finisher and the Santal worker is the second finisher

This case is very similar to the previous case, Relabeling the superscripts we obtain:

$$e_s^S = \frac{\left(1+\theta^S+\theta^B\right)\left(1+\alpha^S\right)w + \theta^S\left(1+2\theta^S\right)\left(1+\alpha^B\right)w}{(1+\theta^S)^2+\theta^B}, \tag{10}$$

and

$$e_f^B = \frac{\left(1+\theta^S\right)\left(1+2\theta^S\right)\left(1+\alpha^B\right)w + \theta^B\left(1+\alpha^S\right)w}{(1+\theta^S)^2+\theta^B}. \tag{11}$$

Thus, the team output is now equal to $q = e_f^B + e_s^S$. We easily obtain

$$\frac{\partial q}{\partial \alpha^S} > 0, \ \frac{\partial q}{\partial \alpha^B} > 0.$$

6.4.3 Interpreting the empirical results in Figure 8

Let us now interpret the results of the field experiment presented in Figure 8 through the lens of our model.

(i) **Figure 8, left panel:** Treated Bengalis produce more packets than untreated Bengalis, regardless of whether they are first or second finishers. One plausible explanation for this result within our model is that treated Bengalis, whether they are first or second finishers, are more altruistic towards the Santals. This is reflected in a higher weight α^B in their utility function, that is, $\alpha_T^B > \alpha_{NT}^B$, where the subscripts T and NT refer to treated and non-treated, respectively. In our model, when $\alpha_T^B > \alpha_{NT}^B$, the effort exerted by treated Bengali workers is higher compared to non-treated Bengali workers. Note also that the similarity in effort for Bengalis between serving as first or second finishers suggests that θ^B does not play a significant role in their utility function, since they do not seem to exactly match the effort of Santals' when the latter act as first finishers.

(ii) **Figure 8, right panel**: When Santals are second finishers, their effort is higher when Bengali workers are treated compared to when they are not treated. According to our model, this occurs because when Santals are second finishers, they observe the effort exerted by the Bengali worker before deciding how much effort to exert. Since treated Bengalis put forth more effort as first finisher, second-finisher Santals *match* their effort, resulting in higher team output. However, this is not true when Santals are first finishers because they do not observe the effort of the (second-finisher) Bengali worker. This suggests, according to equation (9), that Santals do not anticipate significant variation in the altruism of Bengalis (α^B) across treatments, and therefore their own effort also does not differ significantly across treatments.

7 Concluding remarks

This paper provides evidence of the potential of information dissemination through documentaries as a tool for fostering positive interethnic relations. We find a positive impact on both behavior and certain beliefs concerning the minority outgroup. Importantly, we also find a reduction in ethnic dispute complaints by the ethnic majority, particularly at police stations.

Additionally, the intervention has proven successful in enhancing productivity in a labor market setting involving team production by multi-ethnic pairs of workers. It is noteworthy that, while the documentary did not explicitly correct negative stereotypes, it effectively induced empathy among viewers. This suggests that the intervention operates not by challenging existing stereotypes but by fostering a deeper understanding and emotional connection.

We consider these findings as a proof of concept that indirect contact and dissemination of information about a minority outgroup, provided through a documentary, can improve interethnic relationships. Several open questions remain for future research to address. First, it is important to understand the broader scalability of offering this type of documentary at large scale through mass media in which the audience composition might differ from our captive experimental sample, as viewers would self-select into watching the documentary. Second, assessing the long-term persistence of these positive effects is essential to understand the true potential of such carefully designed edutainment interventions. Third, exploring the connections between empathy, social norms, and behavioral change, as well as how these mechanisms might vary in different contexts would be valuable. Finally, replicating the study in settings with varying levels of interethnic tension would help establish the broader applicability of our findings.

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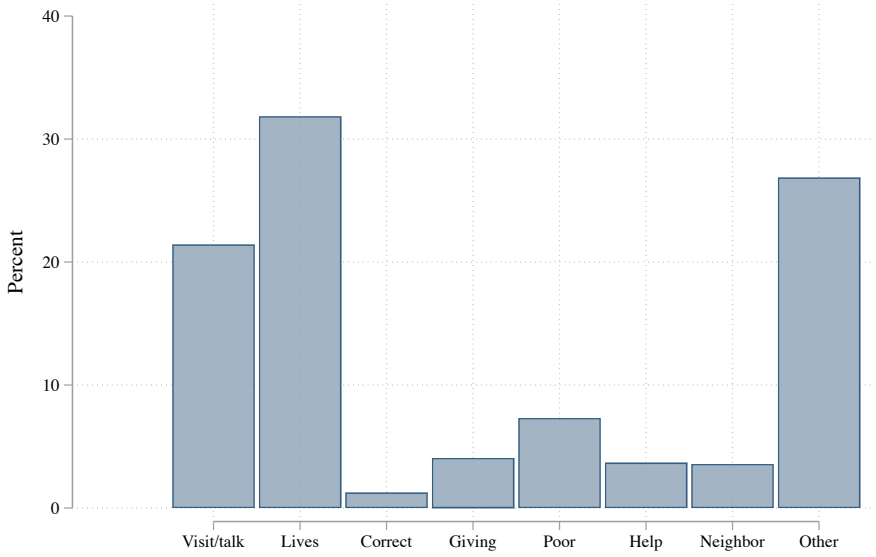
Leveraging Edutainment and Social Networks to Foster Interethnic Harmony

Online Appendix

By Abu Siddique, Michael Vlassopoulos, Yves Zenou

A Appendix: Additional Figures and Tables

Figure A1: Perceived study purpose



Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. This figure illustrates how our respondents perceived the purpose of the study. After gathering all the data, enumerators asked an open-ended question to the respondents following Chopra et al. (2024), which was: "If you had to guess, what would you say was the purpose of this study?" This allowed us to capture their understanding of the study's purpose. Enumerators had eight common options to pick from: *Visit/talk*|how often Bengalis and Santals visit or talk to each other, *Lives*|understanding Santals' (Bengalis') lives in the villages, *Correct*|whether documentary film improves Bengalis' attitudes toward Santals, *Giving*|how to donate more money to Santals (Bengalis), *Poor*|how poor people are in these villages, *Help*|understanding how much financial help Santals (Bengalis) need, *Neighbor*|how good are Santals (Bengalis) as neighbors, *Other*|responses that do not fit into any of the other seven categories. Answering option *Correct* would suggest participants could correctly guess the purpose of this study.

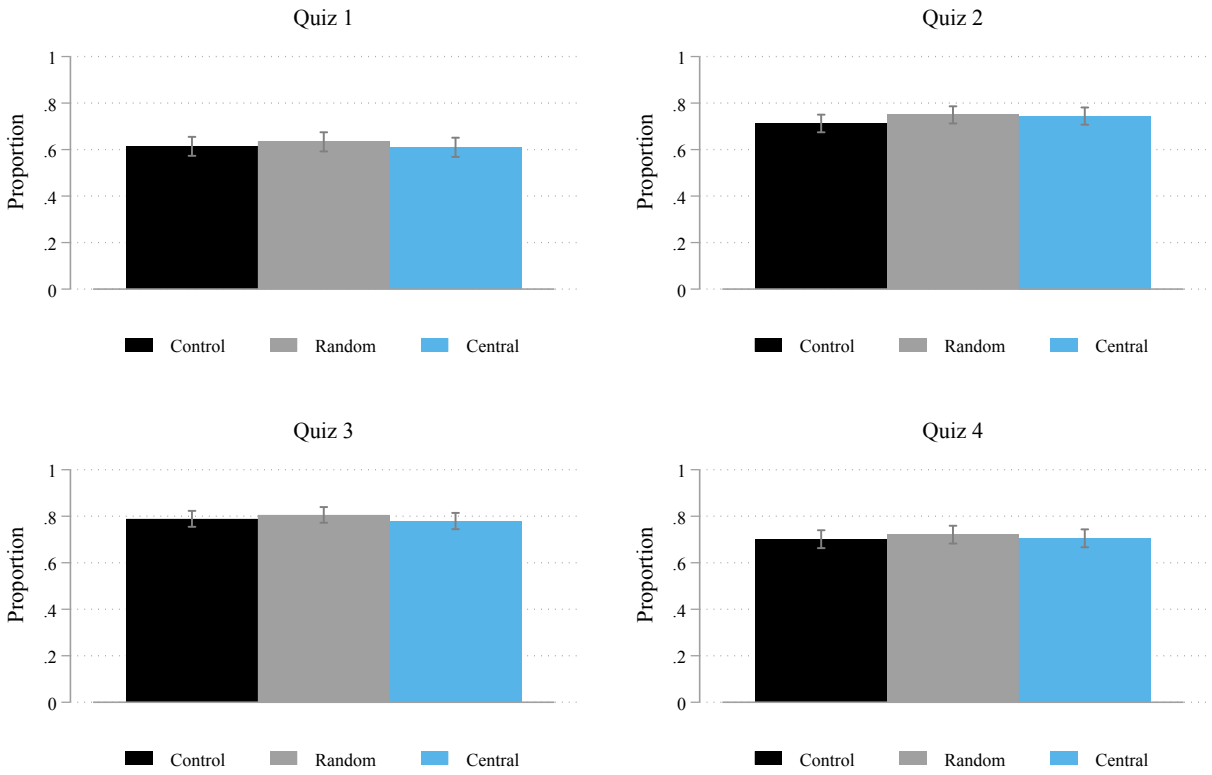
Figure A2: Measuring emotions

Note: Measuring emotions using pictures.

Figure A3: The documentary film

Note: The full film can be accessed here: https://www.youtube.com/watch?v=hWizDrLXLoc.

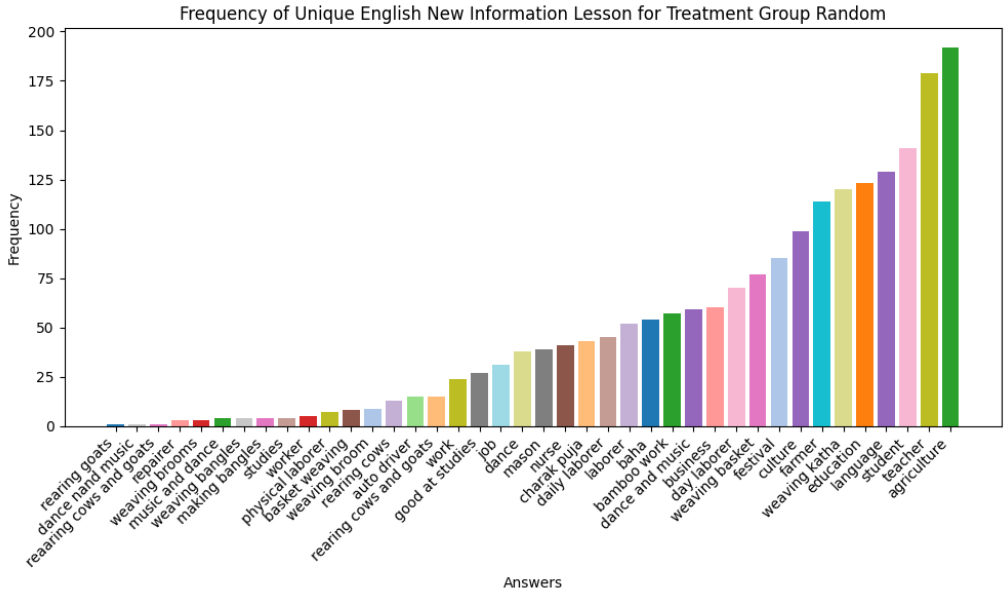
Figure A4: Answers to post-film quiz



Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. Each bar shows the proportion of respondents that got the quiz correct, with 95% confidence intervals. The quiz questions for the treatment group are as follows: (Quiz 1) In the film, you watched a festival celebrated by the Santals. What is the name of that festival? (Quiz 2) What are the main reasons households frequently require repairs? (Quiz 3) You saw a Santal college teacher in the film. What is the name of the college where this teacher works? (Quiz 4) Some secondary school girls shared their future aspirations in the film. Can you tell me what they mostly aspire to become? Quiz questions for the control group were of similar difficulty but concerned flower farming.

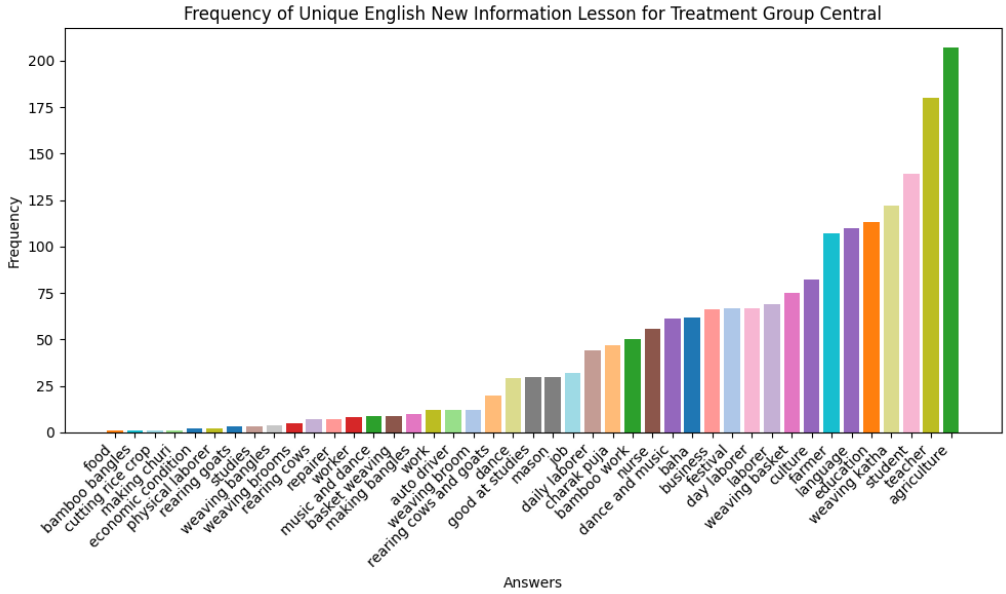
Figure A5: Two casual workers in our field experiment making paper bags

Figure A6: Histogram of most frequent things treated ethnic majority learned in Random arm only



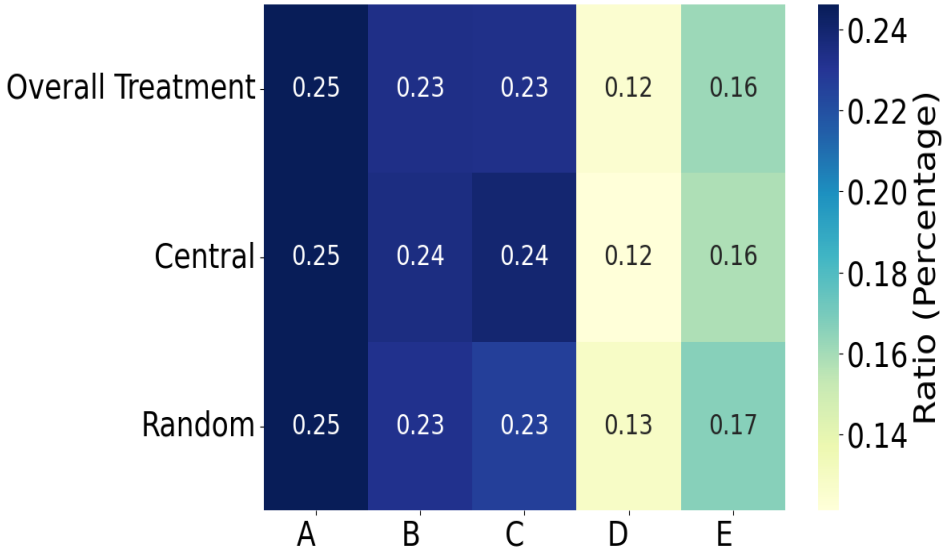
Note: The sample includes treated Bengalis in the two treatment arm villages.

Figure A7: Histogram of most frequent things treated ethnic majority learned in Central arm only



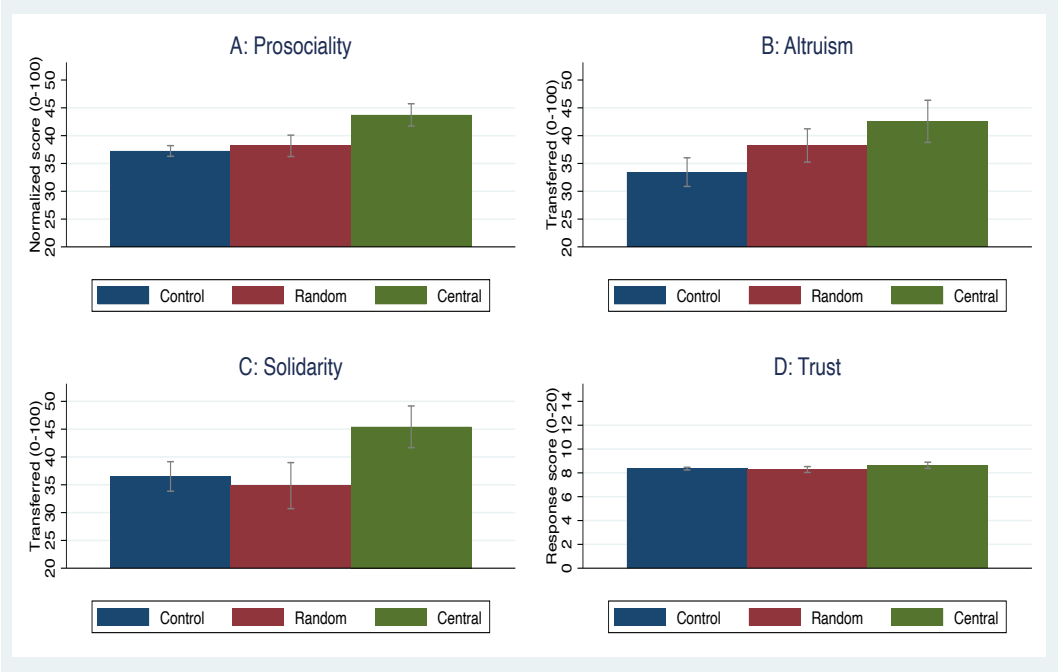
Note: The sample includes treated Bengalis in the two treatment arm villages.

Figure A8: Heatmaps showing distribution of new information categories across treated arms



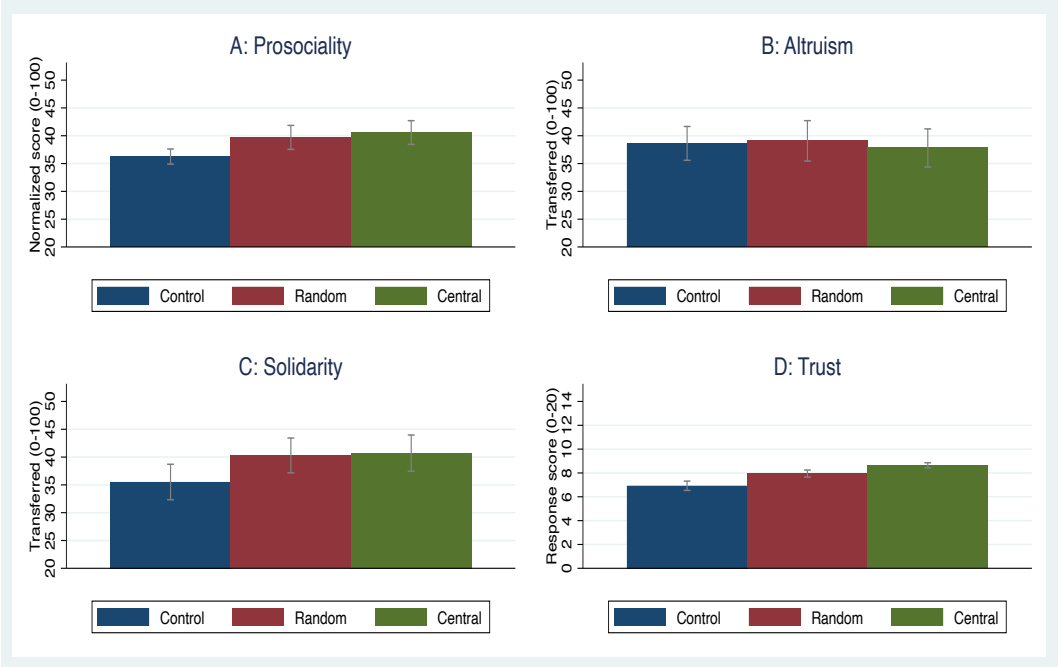
Note: The sample includes treated Bengalis in the two treatment arm villages. Ratio of each category is computed as the number of occurrences of a category by the total number of occurrences of all categories. These values were calculated individually for the overall treated and the subcategories of treated. Category to Label Mapping: Education and Capacity Building: A, Agriculture and Rural Development: B, Livelihood and Artisanal Crafts: C, Missing: D, Cultural: E. Overall Treatment pools Central and Random.

Figure A9: This figure shows raw differences in prosociality among untreated ethnic majority at endline



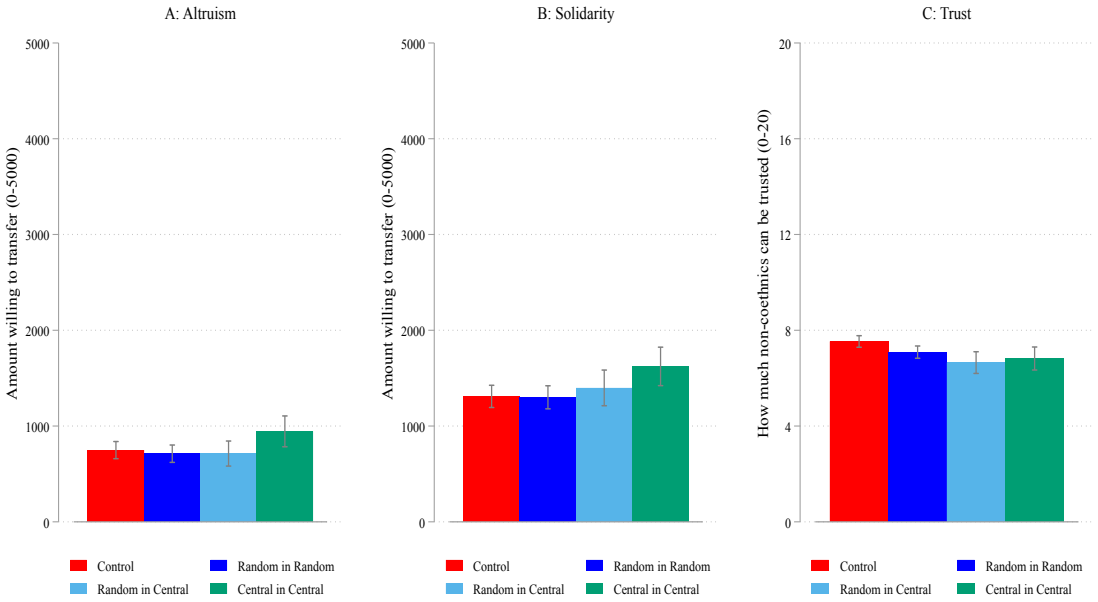
Note: The sample includes untreated Bengalis in the two treatment arm villages and in control villages. The bars indicate 95% confidence intervals. Two-sided t-tests
Graph A: Control vs Random: $p = 0.3928$; Control vs Central: $p = 0.0000$; Random vs Central: $p = 0.0001$.
Graph B: Control vs Random: $p = 0.0173$; Control vs Central: $p = 0.0001$; Random vs Central: $p = 0.0768$.
Graph C: Control vs Random: $p = 0.5083$; Control vs Central: $p = 0.0001$; Random vs Central: $p = 0.0002$.
Graph D: Control vs Random: $p = 0.6050$; Control vs Central: $p = 0.0497$; Random vs Central: $p = 0.0560$.

Figure A10: This figure shows raw differences in prosociality among untreated ethnic minority at endline



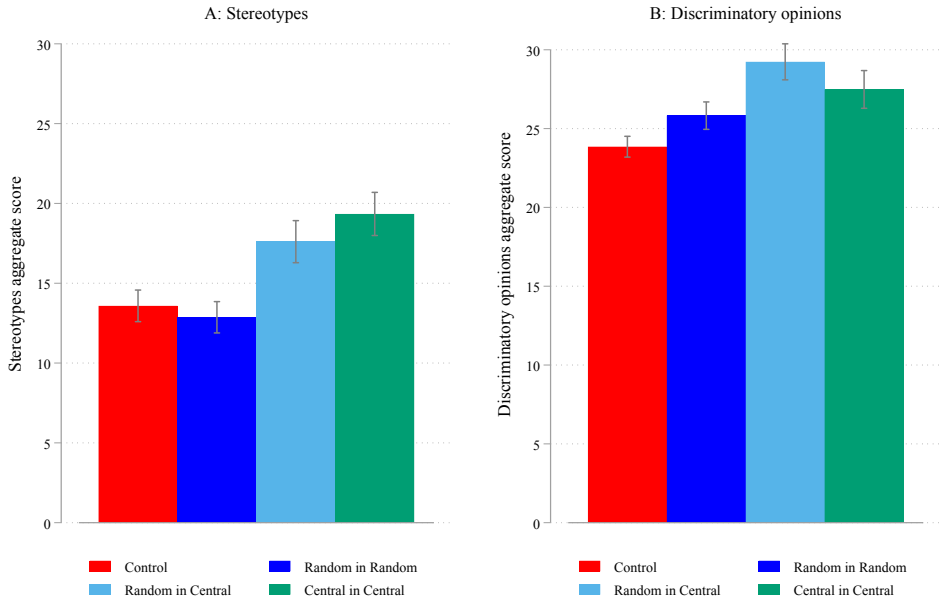
Note: The sample includes untreated Santals in the two treatment arm villages and in control villages. The bars indicate 95% confidence intervals. Two-sided t-tests
Graph A: Control vs Random: $p = 0.0146$; Control vs Central: $p = 0.0017$; Random vs Central: $p = 0.5733$.
Graph B: Control vs Random: $p = 0.8579$; Control vs Central: $p = 0.7325$; Random vs Central: $p = 0.6185$.
Graph C: Control vs Random: $p = 0.0400$; Control vs Central: $p = 0.0276$; Random vs Central: $p = 0.8547$.
Graph D: Control vs Random: $p = 0.0000$; Control vs Central: $p = 0.0000$; Random vs Central: $p = 0.0003$.

Figure A11: Differences in altruism, solidarity, and trust at baseline



Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. We measured altruism and trust using the survey measure in Falk et al. (2018). As Falk et al. (2018) do not have a survey measure of solidarity, and given the similarity between altruism and solidarity, we created a survey question closely following the altruism question from Falk et al. (2018). Altruism and solidarity are measured using hypothetical endowments of 5,000 Taka. Trust is measured on a 20-point scale. The bars indicate 95% confidence intervals. Two-sided t-tests
Graph A: Control vs RR: $p = 0.5666$; Control vs RC: $p = 0.6560$; Control vs CC: $p = 0.0244$; RR vs RC: $p = 0.9845$; RR vs CC: $p = 0.0078$; RC vs CC: $p = 0.0288$.
Graph B: Control vs RR: $p = 0.9129$; Control vs RC: $p = 0.4090$; Control vs CC: $p = 0.0048$; RR vs RC: $p = 0.3718$; RR vs CC: $p = 0.0044$; RC vs CC: $p = 0.1084$.
Graph C: Control vs RR: $p = 0.0133$; Control vs RC: $p = 0.0002$; Control vs CC: $p = 0.0039$; RR vs RC: $p = 0.0761$; RR vs CC: $p = 0.2917$; RC vs CC: $p = 0.6136$.

Figure A12: Differences in stereotypes and discriminatory opinions at baseline

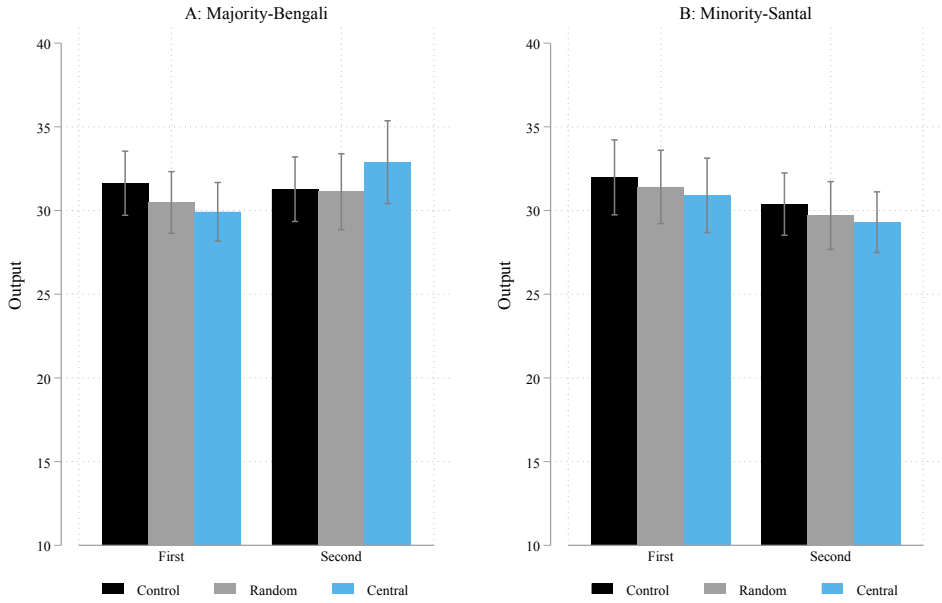


Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. Stereotypes is a score between 0-60 and Discriminatory opinions is a score between 0-70 (both explained in detail in Appendix B.1), where a higher number corresponds to having more negative stereotypes or discriminatory opinions. The bars indicate 95% confidence intervals. Two-sided t-tests

Graph A: Control vs RR: $p = 0.3149$; Control vs RC: $p = 0.0000$; Control vs CC: $p = 0.0000$; RR vs RC: $p = 0.0000$; RR vs CC: $p = 0.0000$; RC vs CC: $p = 0.0716$.

Graph B: Control vs RR: $p = 0.9129$; Control vs RC: $p = 0.4090$; Control vs CC: $p = 0.0048$; RR vs RC: $p = 0.3718$; RR vs CC: $p = 0.0044$; RC vs CC: $p = 0.0383$.

Figure A13: Output as Preparer, by ethnicity



Note: 'First' means a participant started the task as a finisher first, or in this case, finished the task as a preparer. 'Second' means a participant finished the task as a finisher, or in this case, started the task as a preparer.

A: Majority-Bengali going 'first' as finisher

Control vs Random: $p = 0.923$; Control vs Central: $p = 0.312$; Random vs Central: $p = 0.303$

A: Majority-Bengali going 'second' as finisher

Control vs Random: $p = 0.398$; Control vs Central: $p = 0.198$; Random vs Central: $p = 0.665$

B: Minority-Santal going 'first' as finisher

Control vs Random: $p = 0.626$; Control vs Central: $p = 0.414$; Random vs Central: $p = 0.772$

B: Minority-Santal going 'second' as finisher

Control vs Random: $p = 0.721$; Control vs Central: $p = 0.505$; Random vs Central: $p = 0.752$.

Table A1: Baseline characteristics and balance checks

VARIABLES	Control		Random		Central		Difference *p*-values		
	Mean	SD	Mean	SD	Mean	SD			
	(1)	(2)	(3)	(4)	(5)	(6)	(5)-(1)	(3)-(1)	(5)-(3)
	Panel A: Main Sample (Ethnic majority that gets treated)								
Age (in years)	39.59	13.24	38.65	12.50	36.86	12.93	0.976	0.778	0.990
Gender (=1 if male)	0.842	0.365	0.879	0.327	0.920	0.272	0.344	0.731	0.372
Religion (=1 if Muslim)	0.947	0.224	0.952	0.214	0.986	0.119	0.219	0.953	0.156
Household head (=1 if true)	0.785	0.411	0.827	0.379	0.758	0.429	0.970	0.041	0.182
Years of education	5.158	4.813	6.148	4.838	6.977	4.749	0.021	0.014	0.887
Monthly personal income (in Taka)	8,155	6,741	9,302	6,829	10,280	7,383	0.003	0.220	0.076
Works in farming (=1 if true)	0.576	0.495	0.585	0.493	0.555	0.497	0.561	0.961	0.356
Household size	4.808	2.122	4.811	1.905	4.728	1.837	0.418	0.830	0.979
Sample size	**568**		**561**		**562**				
Joint F-test							0.509	0.126	0.180
	Panel B: Spillover Sample (Untreated ethnic majority and minority)								
Age (in years)	37.12	13.10	36.50	12.75	36.36	13.03	0.974	0.930	0.812
Gender (=1 if male)	0.874	0.332	0.870	0.336	0.907	0.291	0.034	0.999	0.026
Ethnicity (=1 if Santal)	0.502	0.500	0.500	0.500	0.499	0.500	0.117	0.365	0.665
Religion (=1 if Muslim)	0.487	0.500	0.487	0.500	0.478	0.500	0.677	0.865	0.660
Household head (=1 if true)	0.766	0.424	0.750	0.433	0.708	0.455	0.046	0.456	0.169
Years of education	5.275	4.635	5.539	4.827	6.013	4.857	0.312	1.000	0.141
Monthly personal income (in Taka)	7,962	5,911	7,926	5,749	7,912	5,669	0.343	0.400	0.777
Works in farming (=1 if true)	0.660	0.474	0.594	0.491	0.649	0.478	0.885	0.204	0.259
Household size	4.782	1.670	4.813	1.779	4.655	1.638	0.869	0.489	0.225
Sample size	**556**		**540**		**559**				
Joint F-test							0.462	0.761	0.434

Note: The columns *Control*, *Random*, and *Central* represent the three arms, each showing the means and standard deviations of the corresponding variables; all variables with "=1 if true" are dummies and are self-explanatory; difference *p*-values in the last three columns were computed by regressing the baseline characteristics on the treatment variable with union council fixed effects and standard errors clustered at the village level.

Table A2: Treatment effects on secondary outcomes of treated ethnic majority

	Ex Solidarity	**Friends**	**Water$**	**M Health**	**SWB**	**Help**
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Random in Random (RR)	0.729	-0.095	-0.155	-0.074	0.098	0.391***
	(1.741)	(0.121)	(0.537)	(0.208)	(0.451)	(0.132)
Random in Central (RC)	3.152	0.168	-0.817	-0.148	-0.109	0.244
	(2.371)	(0.168)	(0.877)	(0.296)	(0.605)	(0.187)
Central in Central (CC)	-0.051	0.081	0.344	-0.215	0.214	0.377*
	(2.217)	(0.157)	(0.990)	(0.327)	(0.613)	(0.194)
RR=RC *p*-values	0.310	0.095	0.392	0.784	0.710	0.369
RR=CC *p*-values	0.734	0.222	0.508	0.645	0.839	0.937
RC=CC *p*-values	0.103	0.532	0.202	0.779	0.522	0.374
Observations	1,511	1,519	444	1,519	1,519	1,394
Control mean	29.84	1.86	0.52	1.61	25.56	6.57
	[22.62]	[1.71]	[5.10]	[2.63]	[5.49]	[2.04]

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. RR is the treatment arm where participants were selected randomly and are from the *Random* arm; RC is the treatment arm where participants were selected randomly and are from the *Central* arm; CC is the treatment arm where all participants had high diffusion centrality and were selected using the 'gossip' method, and are from the *Central* arm. Outcomes in the columns are as follows: (1) Ex Solidarity is the solidarity expected from the minority opponent; (2) Friends is the number of minority friends among the ten closest friends; (3) Water$ is the amount charged to minorities when they come to fetch water from own tubewell; (4) M Health is the aggregated PHQ-4 score, where lower amount corresponds to better mental health; (5) SWB is the aggregate of four subjective well-being indicators from the World Values Survey, where higher value corresponds to better well-being; (6) Help is the participant's willingness to help people in need (answered on a scale between 0-10, where 10 means very willing to help).

Table A3: Spillover effects on secondary outcomes of untreated ethnic majority

	Ex Solidarity	Friends	Water$	M Health	SWB
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Random (R)	-3.634	0.166	-0.669	-0.309	1.030**
	(3.297)	(0.235)	(0.653)	(0.417)	(0.479)
Central (C)	-9.350	1.148***	-1.863*	-0.380	0.276
	(6.070)	(0.302)	(1.014)	(0.463)	(0.738)
Control mean	40.77	2.62	0.91	4.23	19.49
	[21.30]	[1.25]	[3.06]	[2.69]	[3.48]
R=C *p*-values	0.329	0.000	0.266	0.874	0.287
Observations	794	800	214	800	800

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes untreated Bengalis in the two treatment arm villages and in control villages. *Random* and *Central* are the random and central treatment arms, where all ethnic majority Bengalis were randomly selected. All specifications include union council fixed effects, outcome measured at baseline, and other controls selected using the post-double selected LASSO (Belloni et al., 2014). Outcomes are defined under Table 1. The last row reports the control group's means and standard deviations in brackets.

Table A4: Spillover effects on secondary outcomes of untreated ethnic minority

	Ex Solidarity	Friends	Water$	M Health	SWB	Income
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Random (R)	-3.479	0.353	-7.995	-0.102	6.012***	-385.185
	(3.070)	(0.411)	(10.553)	(0.543)	(1.079)	(396.393)
Central (C)	5.745	-0.311	-5.599	-0.822	8.457***	1,152.327
	(3.791)	(0.421)	(11.734)	(0.635)	(1.027)	(813.771)
Control mean	40.72	3.53	18.16	4.03	16.65	8,264
	[22.46]	[1.73]	[10.70]	[1.60]	[4.13]	[1,943]
R=C *p*-values	0.006	0.069	0.859	0.304	0.014	0.098
Observations	720	721	93	721	721	721

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes untreated Santals in the two treatment arm villages and in control villages. *Random* and *Central* are the random and central treatment arms, where all ethnic majority Bengalis were randomly selected. All specifications include union council fixed effects, outcome measured at baseline, and other controls selected using the post-double selected LASSO (Belloni et al., 2014). Outcomes in columns 1-5 are defined under Table 1. The outcome in column 6 is the monthly household income in Bangladeshi Taka. The last row reports the control group's means and standard deviations in brackets.

Table A5: Potential interaction channels for untreated ethnic minority

	Interaction with Santal	**Interaction with Bengali**	**Visit Santal**	**Visit Bengali**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
Random (R)	-0.577**	0.038	1.245***	0.925***
	(0.292)	(0.395)	(0.229)	(0.224)
Central (C)	-0.142	0.556	1.206***	0.497*
	(0.311)	(0.408)	(0.281)	(0.263)
R=C *p*-values	0.104	0.218	0.832	0.004
Observations	721	721	721	721

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes untreated Santals in the two treatment arm villages and in control villages. R is the *Random* arm and C is the *Central* arm. All specifications include union council fixed effects, outcome measured at baseline, and other controls selected using the post-double selected LASSO (Belloni et al., 2014). All outcomes have been control group standardized, where the mean of the control group is 0 and SD is 1. Outcomes in the columns are as follows: (1-2) social interactions with non-coethnics and coethnics; (3-4) frequency of visits to non-coethnics' and coethnics' house. In all columns, positive coefficient means favorable outcomes.

Table A6: Disaggregated impacts on stereotype components of treated Bengalis

	Unclean	Doctors	Establish	Teachers	Education	Agriculture
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Random in Random (RR)	0.225	0.234***	0.164	-0.068	-0.082	-0.611**
	(0.144)	(0.064)	(0.107)	(0.260)	(0.175)	(0.290)
Random in Central (RC)	-0.027	-0.033	-0.067	0.078	-0.248	-0.136
	(0.182)	(0.091)	(0.116)	(0.364)	(0.232)	(0.376)
Central in Central (CC)	0.124	0.004	-0.055	0.128	-0.045	-0.024
	(0.176)	(0.087)	(0.126)	(0.345)	(0.232)	(0.373)
Observations	1,503	1,500	1,496	1,502	1,498	1,495
Control mean	4.74	3.24	3.88	4.82	3.23	5.53
	[2.02]	[1.02]	[1.38]	[3.46]	[2.58]	[3.99]

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. All specifications include union council fixed effects, outcome measured at baseline, and other controls selected using the post-double selected LASSO (Belloni et al., 2014). Outcomes in the columns are as follows: (1) Santals are often unclean; (2) Santals would not make good doctors; (3) I do not know any Santals who have established themselves; (4) Santals do not make very good school teachers; (5) Santals do not continue beyond schools; (6) Santals should continue working in the agriculture sector. In each case, higher values correspond to unfavorable outcomes.

Table A7: Disaggregated impacts on discriminatory opinion components of treated Bengalis

	Honest	Eat	Schools	Teaching	Working	Child's friends	Trust
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Randon in Random (RR)	0.010	-0.051	-0.063	0.003	-0.174	-0.150	0.139*
	(0.061)	(0.155)	(0.238)	(0.104)	(0.231)	(0.109)	(0.078)
Random in Central (RC)	0.007	-0.005	-0.063	0.019	0.152	-0.080	-0.140
	(0.077)	(0.170)	(0.318)	(0.155)	(0.318)	(0.137)	(0.127)
Central in Central (CC)	0.141*	-0.167	-0.001	-0.094	0.091	-0.177	-0.048
	(0.073)	(0.183)	(0.316)	(0.164)	(0.325)	(0.136)	(0.123)
Observations	1,513	1,511	1,511	1,505	1,498	1,500	1,501
Control mean	4.28	7.27	5.02	5.10	5.35	5.36	3.58
	[0.90]	[1.93]	[3.16]	[1.63]	[2.91]	[1.43]	[1.18]

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. All specifications include union council fixed effects, outcome measured at baseline, and other controls selected using the post-double selected LASSO (Belloni et al., 2014). Outcomes in the columns are as follows: (1) All Santals I know are not honest people; (2) I never eat food and drinks offered by Santals; (3) schools should be separate for Santals and Bengalis; (4) there should not be more Santal teachers in my child's school; (5) I do not enjoy working/doing business with Santals; (6) most of my child's best friends are Bengalis; (7) one can easily trust a Santal.

Table A8: Potential belief and interaction channels for untreated ethnic majority

	Panel A: Beliefs		Panel B: Interactions				
	Stereotypes	Opinions	Interaction with Santal	Interaction with Bengali	Visit Santal	Visit Bengali	Intercultural Competence
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Random (R)	0.003	-0.098	0.152	0.045	0.674**	0.520	0.033
	(0.097)	(0.178)	(0.199)	(0.079)	(0.330)	(0.389)	(0.117)
Central (C)	0.038	-0.410	-0.209	0.238**	0.044	0.327	0.259*
	(0.120)	(0.299)	(0.233)	(0.096)	(0.491)	(0.425)	(0.156)
R=C *p*-values	0.860	0.612	0.125	0.018	0.654	0.429	0.338
Observations	800	800	800	800	800	800	800

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes untreated Bengalis in the two treatment arm villages and in control villages. R is the *Random* arm and C is the *Central* arm. All specifications include union council fixed effects, outcome measured at baseline, and other controls selected using the post-double selected LASSO (Belloni et al., 2014). All outcomes have been control group standardized, where the mean of the control group is 0 and SD is 1. Outcomes in the columns are as follows: (1) stereotypes about non-coethnics (negative coefficient means favorable); (2) discriminatory opinions about non-coethnics (negative coefficient means favorable); (3-4) social interactions with non-coethnics and coethnics; (5-6) frequency of visits to non-coethnics' and coethnics' house; (7) competence about the non-coethnics' culture (a proxy for interethnic interactions). For outcomes in columns 3-7, positive coefficient means favorable outcomes.

Table A9: Disaggregated impacts on social interaction components of treated ethnic majority

	Interactions with Santals				Interactions with Bengalis			
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
RR	0.008	0.010	0.009	0.002	0.001	-0.003	0.009	0.003
	(0.030)	(0.029)	(0.022)	(0.022)	(0.002)	(0.002)	(0.007)	(0.007)
RC	0.005	0.014	0.015	-0.001	-0.006	0.000	-0.014	-0.013
	(0.031)	(0.033)	(0.030)	(0.031)	(0.004)	(0.003)	(0.011)	(0.011)
CC	0.029	0.041	0.009	-0.007	-0.002	-0.001	0.003	0.004
	(0.033)	(0.032)	(0.031)	(0.032)	(0.003)	(0.003)	(0.010)	(0.010)
Observations	1,516	1,518	1,517	1,515	1,514	1,515	1,516	1,514
Control mean	0.818	0.816	0.075	0.077	1.000	1.000	0.992	0.992
	[0.386]	[0.387]	[0.263]	[0.266]	[0.000]	[0.000]	[0.087]	[0.087]

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. All specifications include union council fixed effects, outcome measured at baseline, and other controls selected using the post-double selected LASSO (Belloni et al., 2014). Outcomes in the columns are as follows: (1) =1 if offer Santals food when they visit my house; (2) =1 if offer Santals chair/seat when they visit my house; (3) =1 if invite Santals during festivals; (4) =1 if Santals invite me during festivals; (5) =1 if offer Benaglis food when they visit my house; (6) =1 if offer Bengalis chair/seat when they visit my house; (7) =1 if invite Bengalis during festivals; (8) =1 if Benaglis invite me during festivals.

Table A10: Treatment effects on emotions using facial expressions of treated ethnic majority

	Anger	**Disgust**	**Fear**	**Happy**	**Sad**	**Surprise**
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Random in Random (RR)	0.568	0.117	0.016	0.390	2.255	0.003
	(0.980)	(0.199)	(0.038)	(0.787)	(1.499)	(0.043)
Random in Central (RC)	-0.722	0.142	0.061	1.558	3.404	0.045
	(1.122)	(0.254)	(0.064)	(1.165)	(2.116)	(0.108)
Central in Central (CC)	-1.182	0.578	-0.038	2.085*	6.145***	-0.009
	(0.895)	(0.473)	(0.047)	(1.104)	(2.373)	(0.059)
RR=RC *p*-values	0.258	0.898	0.476	0.316	0.587	0.694
RR=CC *p*-values	0.058	0.274	0.228	0.097	0.118	0.827
RC=CC *p*-values	0.618	0.134	0.049	0.654	0.207	0.530
Observations	1,472	1,472	1,472	1,472	1,472	1,472
Control mean	5.11	1.04	0.35	4.86	18.68	0.88
	[13.02]	[2.60]	[0.78]	[10.91]	[23.42]	[1.17]

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. The sum of all six emotions reported above, along with the neutral emotion (i.e., no emotions), equals 100. This means that each individual's emotions were scored between 0-100. A higher value indicates a stronger presence of that particular emotion. All specifications include union council fixed effects, and controls were selected via post-double selection LASSO (Belloni et al., 2014).

Table A11: Heterogeneity in prosociality of treated ethnic majority, by baseline empathic tendencies

height VARIABLES	(1)	(2)	(3)
Treatment RR = 1	0.226	0.324**	0.032
	(0.171)	(0.129)	(0.211)
Treatment RC = 1	0.244	0.286*	-0.136
	(0.212)	(0.160)	(0.243)
Treatment CC = 1	0.727***	0.891***	0.654**
	(0.200)	(0.159)	(0.262)
Perspective taking (PT) score	-0.011		
	(0.014)		
Perspective taking X RR	-0.001		
	(0.017)		
Perspective taking X RC	0.014		
	(0.020)		
Perspective taking X CC	-0.001		
	(0.019)		
Empathic concern (EC) score		0.002	
		(0.009)	
Empathic concern X RR		-0.014	
		(0.014)	
Empathic concern X RC		0.012	
		(0.015)	
Empathic concern X CC		-0.023	
		(0.016)	
Personal distress (PD) score			-0.010
			(0.012)
Personal distress X RR			0.017
			(0.016)
Personal distress X RC			0.047**
			(0.020)
Personal distress X CC			0.007
			(0.021)
Constant	0.132	0.029	0.064
	(0.301)	(0.286)	(0.312)
Observations	1,515	1,515	1,515

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. All specifications include union council fixed effects and other controls selected using the post-double selected LASSO (Belloni et al., 2014). Outcomes in all columns are the prosociality index. We measure the three types of empathic tendencies of participants using the Interpersonal Reactivity Index by Davis (1983): perspective-taking or PT (the tendency to spontaneously adopt the psychological view of others in everyday life), empathic concern or EC (the tendency to experience feelings of sympathy or compassion for unfortunate others), and personal distress or PD (the tendency to experience distress or discomfort in response to extreme distress in others). All three types of empathic tendencies are continuous variables, where a higher value corresponds to higher empathy of that type.

Table A12: Heterogeneity in prosociality of treated ethnic majority, by baseline prosociality

height	(1)	(2)	(3)	(4)
VARIABLES	1	2	3	4
Treatment RR = 1	0.210*	0.231***	0.214**	0.174
	(0.113)	(0.082)	(0.092)	(0.181)
Treatment RC = 1	0.327**	0.372***	0.330***	0.304
	(0.139)	(0.107)	(0.115)	(0.200)
Treatment CC = 1	0.824***	0.804***	0.740***	0.765***
	(0.131)	(0.107)	(0.113)	(0.201)
Prosociality normalized index (0-100) at baseline	0.000			
	(0.002)			
Baseline Prosociality X RR	0.001			
	(0.003)			
Baseline Prosociality X RC	0.002			
	(0.004)			
Baseline Prosociality X CC	-0.004			
	(0.003)			
Altruism 0-5000: towards Santals from own village		0.000		
		(0.000)		
Baseline Altruism X RR		-0.000		
		(0.000)		
Baseline Altruism X RC		-0.000		
		(0.000)		
Baseline Altruism X CC		-0.000**		
		(0.000)		
Solidarity 0-5000: towards Santals from own village			-0.000	
			(0.000)	
Baseline Solidarity X RR			0.000	
			(0.000)	
Baseline Solidarity X RC			0.000	
			(0.000)	
Baseline Solidarity X CC			-0.000	
			(0.000)	
Trust 0-20: I assume that Santals have only the best intentions				-0.001
				(0.017)
Baseline Trust X RR				0.008
				(0.020)
Baseline Trust X RC				0.010
				(0.023)
Baseline Trust X CC				-0.007
				(0.021)
Constant	-0.001	0.010	-0.003	0.014
	(0.283)	(0.283)	(0.282)	(0.313)
Observations	1,515	1,518	1,518	1,515

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. All specifications include union council fixed effects and other controls selected using the post-double selected LASSO (Belloni et al., 2014). Outcomes in all columns are the prosociality index. We explain the construction of the baseline prosociality normalized index (0-100) in section 4.2.

Table A13: Treatment effects on casual work output

	Levels		Logs	
	Preparer	**Finisher**	**Preparer**	**Finisher**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
Random (R)	-0.568	1.108	-0.022	0.103**
	(0.701)	(0.876)	(0.023)	(0.046)
Central (C)	-0.244	3.763***	-0.011	0.247***
	(0.783)	(0.886)	(0.025)	(0.045)
Control mean	31.29	20.99	3.41	2.89
	[7.78]	[10.65]	[0.25]	[0.59]
R=C *p*-values	0.681	0.001	0.662	0.000
Observations	719	719	719	719

Robust SE clustered at pair-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variable in columns 1-2 is the raw output, while in columns 3-4 it is the log of output. Control group means with standard deviations in brackets are reported in the third row. All specifications include control variables selected through post-double selection LASSO (Belloni et al., 2014). Note that one participant (a Bengali) gets dropped because his monthly savings information is missing but LASSO selects savings as a control.

Table A14: Treatment effects on Log [labor output], by ethnicity

	Ethnic majority		**Ethnic minority**	
	Preparer	**Finisher**	**Preparer**	**Finisher**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
Random (R)	-0.021	0.123*	-0.013	0.084
	(0.034)	(0.067)	(0.032)	(0.071)
Central (C)	-0.004	0.254***	-0.007	0.282***
	(0.035)	(0.067)	(0.034)	(0.068)
Control mean	3.41	2.91	3.42	2.88
	[0.26]	[0.58]	[0.24]	[0.61]
R=C *p*-values	0.632	0.018	0.855	0.000
Observations	359	359	360	360

Robust SE are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Columns 1-2 report the log output of ethnic majority workers, and columns 3-4 report the log output of ethnic minority workers. Control group means with standard deviations in brackets are reported in the third row. All specifications include control variables selected through post-double selection LASSO (Belloni et al., 2014).

Table A15: Treatment effects on raw output, by ethnicity

	Ethnic majority		Ethnic minority	
	Preparer	Finisher	Preparer	Finisher
Variables	**(1)**	**(2)**	**(3)**	**(4)**
Random (R)	-0.571	1.528	-0.303	0.565
	(1.047)	(1.299)	(1.012)	(1.345)
Central (C)	-0.129	3.774***	-0.030	4.453***
	(1.109)	(1.287)	(1.075)	(1.359)
Control mean	31.12	21.06	31.47	20.93
	[8.01]	[10.10]	[7.56]	[11.22]
R=C *p*-values	0.678	0.064	0.810	0.001
Observations	359	359	360	360

Robust SE are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Columns 1-2 report the raw output of ethnic majority workers, and columns 3-4 report the raw output of ethnic minority workers. Control group means with standard deviations in brackets are reported in the third row. All specifications include control variables selected through post-double selection LASSO (Belloni et al., 2014).

Table A16: Correlations between raw output as finisher and prosociality of ethnic majority

	Productivity of Bengali as a finisher		
Variables	**(1)**	**(2)**	**(3)**
Altruism (0-100)	-0.014		
	(0.022)		
Solidarity (0-100)		-0.035	
		(0.027)	
Trust (0-20)			0.326**
			(0.127)
Constant	28.943	29.696	26.551
	(5.010)	(5.029)	(4.767)
Observations	324	323	324

Robust SE are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Correlations between levels of productivity as a finisher and prosociality All specifications include control variables selected through post-double selection LASSO (Belloni et al., 2014).

Table A17: Correlation between outcomes of untreated ethnic majority and their social proximity to network central ethnic majority people

VARIABLES	(1) Prosociality	(2) Stereotype	(3) Discr Opinons	(4) Prosociality	(5) Stereotype	(6) Discr Opinons
Average Distance to Central	0.053	0.007	0.064			
	(0.036)	(0.022)	(0.057)			
Average Visits to Central				0.005	-0.002	0.031
				(0.013)	(0.009)	(0.020)
Constant	-0.074	-0.282	-0.883	-0.065	-0.260	-0.929
	(0.657)	(0.388)	(0.617)	(0.662)	(0.392)	(0.646)
Observations	799	728	728	799	728	728

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Average Distance to Central: The average amount of time it takes (in minutes) to walk to the homes of the seven network-central people in a village. Average Visits to Central: The average number of times someone visits the homes of the seven network-central people in their village. All specifications include union council fixed effects and other controls selected using the post-double selected LASSO (Belloni et al., 2014).

Table A18: Experimenter demand effects check: Effects on prosociality of treated ethnic majority

VARIABLES	(1) Prosociality	(2) Altruism	(3) Solidarity	(4) Trust
Random in Random (RR)	0.225***	2.652	2.359	0.975***
	(0.076)	(2.167)	(1.543)	(0.310)
Random in Central (RC)	0.369***	5.603***	1.896	1.754***
	(0.101)	(1.994)	(1.896)	(0.457)
Central in Central (CC)	0.709***	5.823***	4.498**	4.254***
	(0.106)	(2.205)	(1.827)	(0.434)
Control mean	-	40.64	35.67	7.43
		[23.80]	[21.97]	[3.80]
Observations	1,480	1,483	1,483	1,480

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Same as Table 2 in the paper, but dropped the 2% sample (33 people) that correctly guessed the hypothesis of this study.

Table A19: Experimenter demand effects check: Effects on beliefs and interactions of treated ethnic majority

VARIABLES	(1) Stereotype	(2) Discr Opinons	(3) Interact-Santals	(4) Interact-Benaglis	(5) Visit-Santals	(6) Visit-Bengalis	(7) ICC score
Random in Random (RR)	0.018	-0.006	0.055	0.034	0.104	0.027	0.024
	(0.074)	(0.082)	(0.079)	(0.091)	(0.069)	(0.077)	(0.072)
Random in Central (RC)	-0.030	-0.026	0.073	-0.126	0.064	-0.024	-0.063
	(0.095)	(0.123)	(0.085)	(0.113)	(0.080)	(0.094)	(0.115)
Central in Central (CC)	0.025	-0.044	0.085	-0.024	0.095	0.060	-0.016
	(0.092)	(0.107)	(0.090)	(0.118)	(0.077)	(0.091)	(0.096)
Observations	1,486	1,486	1,486	1,486	1,486	1,486	1,486

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Same as Table 5 in the paper, but dropped the 2% sample (33 people) that correctly guessed the hypothesis of this study.

Table A20: Heterogeneity in prosociality of treated ethnic majority, by baseline SDB score

VARIABLES	(1) Prosociality	(2) Altruism	(3) Solidarity	(4) Trust
Treatment RR = 1	-0.213	0.554	-6.064	-0.871
	(0.321)	(7.909)	(8.655)	(1.389)
Treatment RC = 1	-0.128	-3.046	-0.813	-0.263
	(0.423)	(8.551)	(8.865)	(1.587)
Treatment CC = 1	-0.042	-9.109	-6.215	3.216*
	(0.406)	(8.819)	(8.317)	(1.742)
Social desirability bias score (0-13)	-0.049*	-0.807	-0.432	-0.190*
	(0.028)	(0.600)	(0.627)	(0.098)
SDB score X RR	0.047	0.204	0.948	0.195
	(0.033)	(0.763)	(0.933)	(0.141)
SDB score X RC	0.054	0.915	0.364	0.205
	(0.043)	(0.854)	(0.957)	(0.161)
SDB score X CC	0.082**	1.629*	1.209	0.104
	(0.042)	(0.903)	(0.889)	(0.174)
Constant	0.469	46.183***	46.566***	8.337***
	(0.406)	(9.061)	(8.745)	(1.500)
Observations	1,515	1,519	1,518	1,516

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. SDB Score is a score between 0-13, where a higher score means someone has a higher tendency to give socially desirable responses in surveys (Dhar et al., 2022). All specifications include union council fixed effects and other controls selected using the post-double selected LASSO (Belloni et al., 2014).

Table A21: Heterogeneity in beliefs and interactions of treated ethnic majority, by baseline SDB score

VARIABLES	(1) Stereotype	(2) Discr Opinons	(3) Interact-Santals	(4) Interact-Benaglis	(5) Visit-Santals	(6) Visit-Bengalis	(7) ICC score
Treatment RR = 1	-0.622	-0.783**	-0.021	-0.454	-0.817**	-0.323	-0.169
	(0.391)	(0.364)	(0.442)	(0.413)	(0.414)	(0.455)	(0.418)
Treatment RC = 1	-0.169	-0.338	0.408	-0.766	0.004	0.309	-0.183
	(0.467)	(0.443)	(0.418)	(0.606)	(0.411)	(0.406)	(0.438)
Treatment CC = 1	0.111	-0.687*	-0.298	-0.403	-0.013	0.483	-0.819**
	(0.394)	(0.374)	(0.436)	(0.513)	(0.440)	(0.409)	(0.377)
Social desirability bias score (0-13)	-0.049*	-0.070**	-0.032	-0.071	-0.050	-0.008	-0.044
	(0.026)	(0.028)	(0.031)	(0.047)	(0.034)	(0.034)	(0.031)
SDB score X RR	0.065	0.080**	0.004	0.051	0.098**	0.035	0.023
	(0.043)	(0.040)	(0.049)	(0.048)	(0.045)	(0.049)	(0.045)
SDB score X RC	0.014	0.033	-0.041	0.074	0.008	-0.035	0.013
	(0.049)	(0.046)	(0.043)	(0.067)	(0.045)	(0.044)	(0.047)
SDB score X CC	-0.010	0.068*	0.039	0.046	0.011	-0.045	0.084**
	(0.041)	(0.038)	(0.045)	(0.060)	(0.046)	(0.043)	(0.041)
Constant	0.307	0.219	0.439	0.393	1.010**	-0.037	0.098
	(0.361)	(0.414)	(0.403)	(0.487)	(0.448)	(0.377)	(0.428)
Observations	1,522	1,522	1,522	1,522	1,522	1,522	1,522

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. SDB Score is a score between 0-13, where a higher score means someone has a higher tendency to give socially desirable responses in surveys (Dhar et al., 2022). All specifications include union council fixed effects and other controls selected using the post-double selected LASSO (Belloni et al., 2014).

Table A22: Heterogeneity in prosociality of treated ethnic majority, by baseline stereotypes

VARIABLES	(1) Prosociality Index	(2) Altruism	(3) Solidarity	(4) Trust
Treatment RR = 1	0.143	-1.298	4.485**	0.614*
	(0.102)	(2.947)	(2.184)	(0.366)
Treatment RC = 1	0.413***	5.069*	7.004***	1.247**
	(0.139)	(3.062)	(2.647)	(0.588)
Treatment CC = 1	0.719***	3.053	10.116***	3.706***
	(0.167)	(3.875)	(2.937)	(0.596)
Stereotypes score	0.001	-0.021	0.153***	-0.023
	(0.004)	(0.090)	(0.058)	(0.016)
Stereotype score X RR	0.007	0.283**	-0.108	0.022
	(0.005)	(0.120)	(0.094)	(0.022)
Stereotype score X RC	-0.002	0.042	-0.244**	0.022
	(0.006)	(0.147)	(0.118)	(0.028)
Stereotype score X CC	0.000	0.172	-0.273**	0.026
	(0.007)	(0.170)	(0.130)	(0.027)
Constant	-0.026	38.043***	38.671***	7.191***
	(0.284)	(6.377)	(6.961)	(1.049)
Observations	1,518	1,519	1,518	1,516

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. We define the stereotype measure in section 3.2.2 in the paper. All specifications include union council fixed effects and other controls selected using the post-double selected LASSO (Belloni et al., 2014).

Table A23: Heterogeneity in beliefs and interactions of treated ethnic majority, by baseline stereotypes

VARIABLES	(1) Stereotype	(2) Discr Opinons	(3) Interact-Santals	(4) Interact-Bengalis	(5) Visit-Santals	(6) Visit-Bengalis	(7) ICC score
Treatment RR = 1	-0.032	-0.113	0.048	-0.036	0.118	0.097	-0.089
	(0.106)	(0.114)	(0.121)	(0.116)	(0.102)	(0.101)	(0.116)
Treatment RC = 1	0.201	0.265*	0.051	0.198*	0.015	0.227	-0.192
	(0.127)	(0.139)	(0.144)	(0.120)	(0.133)	(0.140)	(0.163)
Treatment CC = 1	0.020	-0.043	-0.020	-0.007	0.131	0.287**	-0.051
	(0.121)	(0.159)	(0.149)	(0.166)	(0.127)	(0.125)	(0.147)
Steretoypes score	0.000	0.001	-0.001	-0.002	-0.004	0.003	-0.003
	(0.005)	(0.005)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Stereotype score X RR	0.002	0.006	-0.002	0.005	-0.003	-0.006	0.010*
	(0.006)	(0.008)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
Stereotype score X RC	-0.014**	-0.017**	-0.002	-0.016*	0.003	-0.014**	0.008
	(0.007)	(0.007)	(0.008)	(0.009)	(0.007)	(0.007)	(0.007)
Stereotype score X CC	-0.001	-0.000	0.003	0.002	-0.002	-0.012**	0.002
	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Constant	-0.123	-0.439	0.197	-0.218	0.705*	-0.134	-0.256
	(0.287)	(0.344)	(0.296)	(0.290)	(0.363)	(0.253)	(0.340)
Observations	1,522	1,522	1,522	1,522	1,522	1,522	1,522

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. We define the stereotype measure in section 3.2.2 in the paper. All specifications include union council fixed effects and other controls selected using the post-double selected LASSO (Belloni et al., 2014).

Table A24: Heterogeneity in prosociality of treated ethnic majority, by baseline discriminatory opinions

VARIABLES	(1) Prosociality Index	(2) Altruism	(3) Solidarity	(4) Trust
Treatment RR = 1	0.411***	3.223	12.567***	0.471
	(0.159)	(4.295)	(3.975)	(0.700)
Treatment RC = 1	0.796***	14.131***	12.289**	1.731**
	(0.221)	(5.456)	(4.867)	(0.801)
Treatment CC = 1	1.059***	8.870*	19.851***	3.422***
	(0.200)	(4.868)	(4.360)	(0.838)
Discriminatory opinion scores	0.009**	0.167	0.330***	-0.022
	(0.004)	(0.125)	(0.090)	(0.023)
Discr Opinons X RR	-0.008	-0.043	-0.410***	0.020
	(0.006)	(0.148)	(0.126)	(0.029)
Discr Opinons X RC	-0.016**	-0.321*	-0.390**	0.000
	(0.008)	(0.189)	(0.152)	(0.032)
Discr Opinons X CC	-0.014**	-0.119	-0.583***	0.030
	(0.007)	(0.177)	(0.143)	(0.032)
Constant	-0.214	34.283***	34.347***	7.292***
	(0.314)	(7.349)	(7.214)	(1.131)
Observations	1,515	1,519	1,518	1,516

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. We define the discriminatory opinions measure in section 3.2.2 in the paper. All specifications include union council fixed effects and other controls selected using the post-double selected LASSO (Belloni et al., 2014).

Table A25: Heterogeneity in beliefs and interactions of treated ethnic majority, by baseline discriminatory opinions

VARIABLES	(1) Stereotype	(2) Discr Opinons	(3) Interact-Santals	(4) Interact-Bengalis	(5) Visit-Santals	(6) Visit-Benaglis	(7) ICC score
Treatment RR = 1	-0.277*	-0.402**	0.016	-0.293	0.124	0.034	0.001
	(0.166)	(0.177)	(0.194)	(0.187)	(0.202)	(0.180)	(0.199)
Treatment RC = 1	-0.254	0.059	-0.288	-0.072	-0.330	-0.060	-0.145
	(0.227)	(0.241)	(0.216)	(0.252)	(0.251)	(0.225)	(0.264)
Treatment CC = 1	-0.243	-0.202	-0.061	-0.184	0.151	0.128	-0.190
	(0.211)	(0.231)	(0.217)	(0.297)	(0.219)	(0.198)	(0.217)
Discriminatory opinion scores	-0.011**	-0.009	-0.002	-0.011	-0.006	-0.002	0.005
	(0.005)	(0.006)	(0.005)	(0.008)	(0.006)	(0.005)	(0.006)
Discr Opinons X RR	0.011*	0.015**	0.001	0.013	-0.001	-0.001	0.002
	(0.006)	(0.007)	(0.007)	(0.009)	(0.008)	(0.007)	(0.008)
Discr Opinons X RC	0.009	-0.002	0.011	0.001	0.015	0.001	0.002
	(0.008)	(0.009)	(0.008)	(0.011)	(0.009)	(0.008)	(0.009)
Discr Opinons X CC	0.011	0.006	0.004	0.009	-0.002	-0.002	0.005
	(0.008)	(0.008)	(0.008)	(0.011)	(0.008)	(0.008)	(0.008)
Constant	0.178	-0.167	0.200	0.051	0.742**	-0.023	-0.443
	(0.287)	(0.347)	(0.315)	(0.335)	(0.378)	(0.270)	(0.385)
Observations	1,522	1,522	1,522	1,522	1,522	1,522	1,522

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. We define the discriminatory opinions measure in section 3.2.2 in the paper. All specifications include union council fixed effects and other controls selected using the post-double selected LASSO (Belloni et al., 2014).

Table A26: Heterogeneity in prosociality of treated ethnic majority, by ethnic composition of village

VARIABLES	(1) Prosociality Index	(2) Altruism	(3) Solidarity	(4) Trust
Treatment RR = 1	0.466**	11.224**	4.524	0.945
	(0.188)	(5.468)	(4.188)	(0.909)
Treatment RC = 1	0.576***	8.612*	10.072	1.216
	(0.223)	(4.419)	(6.444)	(1.126)
Treatment CC = 1	1.560***	21.528***	19.849***	5.440***
	(0.300)	(7.246)	(5.062)	(1.107)
Proportion of Santals in a Village	0.753**	20.963**	7.211	0.968
	(0.329)	(8.162)	(8.494)	(1.617)
% of Santals in Village X RR	-0.658	-23.650*	-5.229	-0.001
	(0.446)	(13.501)	(10.735)	(2.205)
% of Santals in Village X RC	-0.594	-9.443	-20.644	1.036
	(0.511)	(10.427)	(17.537)	(2.555)
% of Santals in Village X CC	-2.225***	-40.889**	-39.538***	-3.272
	(0.760)	(18.391)	(12.188)	(2.834)
Constant	-0.242	31.268***	40.535***	6.264***
	(0.298)	(7.196)	(6.805)	(1.370)
Observations	1,515	1,519	1,518	1,516

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. Proportion of Santals in a village = number of Santals households divided by the total number of households, so this value is between 0 and 1. All specifications include union council fixed effects and other controls selected using the post-double selected LASSO (Belloni et al., 2014).

Table A27: Heterogeneity in beliefs and interactions of treated ethnic majority, by ethnic composition of village

VARIABLES	(1) Stereotype	(2) Discr Opinons	(3) Interact-Santals	(4) Interact-Bengalis	(5) Visit-Santals	(6) Visit-Benaglis	(7) ICC score
Treatment RR = 1	-0.029	0.157	-0.290	0.083	0.074	-0.120	0.288
	(0.245)	(0.279)	(0.210)	(0.198)	(0.219)	(0.275)	(0.209)
Treatment RC = 1	-0.017	0.077	-0.278	0.071	0.093	-0.473	0.419
	(0.305)	(0.362)	(0.262)	(0.223)	(0.265)	(0.324)	(0.335)
Treatment CC = 1	-0.180	-0.181	0.211	-0.110	0.158	-0.233	0.002
	(0.282)	(0.307)	(0.278)	(0.183)	(0.268)	(0.294)	(0.213)
Proportion of Santals in a village	-0.023	0.041	-0.428	-0.236	-0.131	-0.961*	0.717*
	(0.530)	(0.639)	(0.414)	(0.289)	(0.501)	(0.578)	(0.396)
% of Santals in Village X RR	0.036	-0.536	0.860*	-0.157	0.039	0.340	-0.638
	(0.635)	(0.724)	(0.506)	(0.506)	(0.546)	(0.700)	(0.558)
% of Santals in Village X RC	-0.069	-0.337	0.776	-0.428	-0.064	1.153	-1.246
	(0.778)	(0.923)	(0.638)	(0.593)	(0.654)	(0.812)	(0.774)
% of Santals in Village X CC	0.499	0.274	-0.387	0.305	-0.196	0.756	-0.134
	(0.698)	(0.771)	(0.712)	(0.362)	(0.704)	(0.704)	(0.521)
Constant	-0.093	-0.392	0.307	-0.117	0.656*	0.294	-0.558
	(0.310)	(0.375)	(0.322)	(0.266)	(0.381)	(0.314)	(0.365)
Observations	1,522	1,522	1,522	1,522	1,522	1,522	1,522

Robust SE clustered at village-level are in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The sample includes treated Bengalis in the two treatment arm villages and in control villages. The proportion of Santals in a village = number of Santals households divided by the total number of households, so this value is between 0 and 1. All specifications include union council fixed effects and other controls selected using the post-double selected LASSO (Belloni et al., 2014).

B Appendix: Variable Descriptions and Experimental Instructions

B.1 Variable descriptions

Altruism. We employ a standard dictator game to measure altruism towards non-coethnics (Eckel & Grossman, 1996). Each participant (Player A) was paired with a non-coethnic recipient from their own village (Player B). This arrangement paired a majority member with a minority member, and vice versa. Player A received an endowment of 100 Taka (=$1) and was asked to privately divide it between themself and Player B. The share given to Player B (through an envelope) measures Player A's altruism. This game was played individually with an enumerator at Player A's home, and all players remained anonymous.

Solidarity. We used a simplified solidarity game to measure solidarity and expected solidarity towards non-coethnics during unexpected shocks (Selten & Ockenfels, 1998). This game was played at the decision maker's home (while the recipient stayed at their own home), and both players remained anonymous. Again, this game paired a majority member with a minority member, and vice versa. Each player was given an endowment of 100 Taka prior to the game. The enumerator tossed a coin in front of Player A; if it landed on 'heads', Player B's entire endowment was destroyed, while if it landed on 'tails', Player B's endowment remained intact. Before the coin toss, Player A decided how much to give to Player B in the event of a 'heads' result. The money was placed in an envelope in private and handed to the enumerator, who then tossed the coin. If 'heads', the envelope was kept by the enumerator to later give to Player B. If 'tails', the envelope was returned to Player A. Therefore, the amount offered to Player B measured Player A's solidarity towards non-coethnics who lost their wealth due to a risk shock. Before the coin toss, Player A was asked, "If you play this game with Player B, how much do you think they will be willing to give to you?" This question measured their level of expected solidarity.

Trust. At the first endline, we were also interested in the formation of interethnic trust following the intervention, measured using a validated survey question (Falk et al., 2018). We asked "Santals have only the best intentions", which was answered on a scale 0-20, where 0 means 'not agree at all' and 20 means 'completely agree'.

Stereotypes index. Based on the following 6 questions that capture Bengalis' stereotypes about Santals (each answered on a 0-10 scale, where 10 means "completely agree", thus the aggregated score is between 0 and 60):

1. Santals are often unclean/unhygienic
2. Santals would make very good doctors *(reverse scoring required)*
3. I have not met or known any Santals who have established themselves or made a mark
4. Santals do not make very good school teachers.
5. Santals do not continue beyond schools
6. Santals should continue working in the agricultural sector

Discriminatory opinions index. Based on the following 7 questions that capture Bengalis' opinions about Santals (each answered on a 0-10 scale, where 10 means "completely agree",

thus the aggregated score is between 0 and 70):

1. All Santals that I know are honest people
2. I always eat food and drinks offered by Santals
3. Schools should be separate for Bengali and Santal children
4. I think there should be more Santal teachers in my child's school
5. I do not enjoy working/doing business with Santals
6. Some of my children's best friends are Santals
7. One can easily trust a Santal person.

Number of visits to Santal/Bengali neighbors (two variables). Participants were asked "How many times do you visit your Santal neighbors in a month?". We directly use this frequency as the outcome.

Number of Santal/Bengali visitors (two variables). Participants were asked "How often do your Santal neighbors visit you in a month?". We directly use this frequency as the outcome.

Social connections with network-central Bengalis (two variables). Because we have both network central (7 per village) and randomly selected ethnic majority participants (7 per village) in the *Central* arm, we collected data on how connected each Bengali spillover respondent (7 per village) is to each network central in this treatment arm. Specifically, we asked: "How often did you visit .....'s house or s/he visited your house last month?", where ..... is the name of network central participant. Then we asked: "How long does it take you (in minutes) to walk to .....'s house?".[1] We ask these questions 7 times to each Bengali spillover respondent, one for each network central participant in the *Central* arm. Using these responses, we created two averages for each Bengali spillover respondents: (1) average visits to network-central, and (2) average distance to network-central.

Interaction index (two variables—one interactions with Bengalis and another with Santals). Based on the following 4 questions that capture how often Bengalis (Santals) interact with their Santal (Bengali) neighbors (answered as *yes=1* or *no=0*, thus the aggregated score is between 0 and 4):

1. Do you offer them food when they visit you?
2. Do you offer them chair/seat when they visit you?
3. Do you invite them during festivals?
4. Do they invite you during festivals?

Intercultural competence index. The simplified version of this index (4-items), used in Siddique & Vlassopoulos (2020), captures a Bengali's awareness of Santals and their culture. The four questions are as follows:

1. What is the language spoken by Santals?

[1] We wanted to record GPS coordinates of each respondent during the survey but we could not do it because of poor 3G internet coverage in these villages.

2. Do you speak or understand that language?
3. What is their major religion?
4. Name a major Santal festival?

Answering each question will give 1 point, with a maximum total of 4. Therefore, the score is between 0 and 4, where 4 means 'full competence'. This question was only asked to Bengalis.

Number of ethnic minority close friends. Participants were asked "Name your ten closest friends (full name)". Based on surnames, we count the number of ethnic minority close friends they have.

Water bill charge to non-coethnics. Participants were be asked "We know you need to pay water bills. How much do Santals/Bengali pay per fetch when they come to fetch water at your house?". We directly use this monetary amount as the outcome. Note that this question was only asked to respondents who had a tubewell installed at their home.

Mental health index. We measured depressive and general anxiety symptoms using the PHQ-4 questionnaire (Kroenke et al., 2009). We asked the following questions: "Over the last two weeks, how often have you been bothered by the following problems? *Not at all=0 / several days=1 / more than half the days=2 / nearly everyday=3*":

1. Feeling nervous, anxious, or on edge.[a]
2. Not being able to stop or control worrying.[a]
3. Feeling down, depressed, or hopeless.[d]
4. Little interest or pleasure in doing things.[d]

Questions with [a] measures general anxiety symptoms, and that with [d] measures depressive symptoms. We created an index using the standardizing procedure explained before.

Subjective well-being index. We measure this outcome using the following 4 questions from the World Values Survey: "On a scale from 0 to 10, where 0 means *not at all* and 10 means *extremely or all the time*":

1. Overall, how satisfied are you with your life nowadays?
2. Overall, to what extent do you feel the things you do in your life are worthwhile?
3. Overall, how happy do you feel nowadays?
4. Overall, how anxious do you feel nowadays?

Food security index. We used the USDA Household Food Security Survey Module to measure food security in the household of Santals only, each question answered on a 3-point scale: *often true=2, sometimes true=1, never true=0*; thus, the score is between 0 and 6. We ask the following questions "In the last 6 months, can you tell me if these statements were true for you?:

1. We worried whether our food would run out before we got money to buy more.
2. The food that we bought just didn't last, and we didn't have money to get more.
3. We couldn't afford to eat balanced meals.

New employment. Did you start any new job recently that lets you earn more than before? Coded as *yes=1* and *no=0*. We asked this question to Santals only at endline.

Monthly income. Last months' household income. We directly use this monetary amount and $ln(income)$ as the outcome. We asked this question to Santals only at endline.

Economic preferences at baseline. Following Falk et al. (2018), we measured altruism and trust using the following questions. We also measured solidarity in a similar fashion. We asked:

1. *Altruism:* Imagine the following situation|Today you unexpectedly received 5,000 Taka. How much of this amount would you donate to a Santal/Bengali person in your village? We directly used this monetary amount as the outcome.
2. *Trust:* Please indicate your answer on a scale from 0 to 20. A 0 means *does not describe me at all*, and a 20 means *describes me perfectly*. "I assume that Santal/Bengalis have only the best intentions." We directly used this score as the outcome.
3. *Solidarity:* Imagine your neighbor, Hopna Kisku/Iqbal Rahman, lost his house during a heavy storm. Also, imagine that today you unexpectedly received 5,000 Taka. How much of this amount would you donate to Iqbal Rahman and his family? We directly used this monetary amount as the outcome.

Interpersonal Reactivity Index. To measure individual empathy, we use the Interpersonal Reactivity Index by Davis (1983). We measure three dimensions of empathy to understand which channels can get activated from the documentary film: perspective-taking or PT (the tendency to spontaneously adopt the psychological view of others in everyday life), empathic concern or EC (the tendency to experience feelings of sympathy or compassion for unfortunate others), and personal distress or PD (the tendency to experience distress or discomfort in response to extreme distress in others). We ask the following:

"The following statements inquire about your thoughts and feelings in a variety of situations. For each item, indicate how well it describes you by choosing between 0 and 4, where 0 means *does not describe me at all* and 4 means *describes very well*. Thus, the higher the number, the more it describes your thoughts and feelings. Answer as honestly as you can.":

1. I often have tender, concerned feelings for people less fortunate than me. (EC)
2. I sometimes find it difficult to see things from the "other guy's" point of view. (PT) (-)
3. Sometimes I don't feel very sorry for other people when they are having problems. (EC) (-)
4. In emergency situations, I feel apprehensive and ill-at-ease. (PD)
5. I try to look at everybody's side of a disagreement before I make a decision. (PT)
6. When I see someone being taken advantage of, I feel kind of protective towards them. (EC)
7. I sometimes feel helpless when I am in the middle of a very emotional situation. (PD)
8. I sometimes try to understand my friends better by imagining how things look from their perspective. (PT)

9. When I see someone get hurt, I tend to remain calm. (PD) (-)
10. Other people's misfortunes do not usually disturb me a great deal. (EC) (-)
11. If I'm sure I'm right about something, I don't waste much time listening to other people's arguments. (PT) (-)
12. Being in a tense emotional situation scares me. (PD)
13. When I see someone being treated unfairly, I sometimes don't feel very much pity for them. (EC) (-)
14. I am usually pretty effective in dealing with emergencies. (PD) (-)
15. I am often quite touched by things that I see happen. (EC)
16. I believe that there are two sides to every question and try to look at them both. (PT)
17. I would describe myself as a pretty soft-hearted person. (EC)
18. I tend to lose control during emergencies. (PD)
19. When I'm upset at someone, I usually try to "put myself in his shoes" for a while. (PT)
20. When I see someone who badly needs help in an emergency, I go to pieces. (PD)
21. Before criticizing somebody, I try to imagine how I would feel if I were in their place. (PT)

Statements with (-) requires reverse scoring. There are 7 questions on each of the three dimensions, so the score for each would be between 0 and 28. Thus, higher aggregate score corresponds to someone being more empathetic.

Social desirability bias index. To address social desirability bias (SDB) concerns pertaining to self-reported outcomes, we closely follow Dhar et al. (2022). Using this 13-point SDB scale (each item is answered as agree/disagree, where socially desirable answers are coded as 1 and 0 otherwise), we aggregate all 13 responses such that the value range is between 0 and 13, where 13 means the respondent is most likely to give socially desirable responses. We asked the following questions at baseline to all participants: "Please answer as accurately as possible if the following characteristics describe you or not. Please answer as either *agree* or *disagree*":

1. It is sometimes hard for me to go on with my work if I am not encouraged (Disagree)
2. I sometimes feel resentful when I don't get my way (Disagree)
3. On a few occasions, I have given up doing something because I thought too little of my ability (Disagree)
4. There have been times when I felt like rebelling against people in authority even though I knew they were right (Disagree)
5. No matter who I'm talking to, I'm always a good listener (Agree)
6. There have been occasions when I took advantage of someone (Disagree)
7. I'm always willing to admit it when I make a mistake (Agree)
8. I sometimes try to get even rather than forgive and forget (Disagree)
9. I am always courteous, even to people who are disagreeable (Agree)

10. I have never been irked when people expressed ideas very different from my own (Agree)
11. There have times when I was quite jealous of the good fortune of others (Disagree)
12. I am sometimes irritated by people who ask favors of me (Disagree)
13. I have deliberately said something that hurt someone's feelings (Disagree)

The socially desirable responses are given in parentheses. Therefore, for each question, a respondent gets a point if their response matches with the socially desirable response and 0 otherwise.

B.2 Experimental instructions

We will play two games involving real money. After playing both games, we will do a coin toss: 'head' will mean you will be paid according to the outcome of the first game, and 'tail' will mean you will be paid according to the outcome of the second game. The money you win today will be yours to keep.

First game—Dictator: You are playing this game with a Santal/Bengali that lives in your village. For simplicity, let's call this person your opponent. Note that, I will not reveal her/his identity to you, and nor will I reveal your identity to her/him. So, your identity will remain a secret and will never be revealed.

For this game, I will give you 100 Taka in 5 Taka coins. Which means you will have 20 coins. I have not given any money to your opponent. You now have to decide how much of the 100 Taka would you like to share with your opponent. You may share either some, all, or nothing with your opponent. Whatever amount you share will be sent to your opponent, and you can keep the remaining amount.

Here's an envelope. Whatever amount you wish to share should be left in the envelope. I will now turn my back.

If I play this game with your Santal opponent, how much do you think your opponent would share with you? (Record this value)

Once our games end, I will give your envelope to your opponent.

Second game—Solidarity: This game depends on luck. You are playing this game with a Santal/Bengali that lives in your village. For simplicity, let's call this person your opponent. Note that, I will not reveal her/his identity to you, and nor will I reveal your identity to her/him. So, your identity will remain a secret and will never be revealed.

For this game, I will give you 100 Taka in 5 Taka coins. I have also given your opponent 100 Taka. I will then do a coin toss. If the coin shows a 'head', then your opponent will lose the entire 100 Taka I gave her/him. That is, in case of a 'head', s/he will have to return the 100 Taka I gave her/him. However, if it is a tail then s/he can keep the 100 Taka. Is this clear?

Now before I do the coin toss, you have to decide how much of your 100 Taka would you like to give to your opponent if s/he loses her/his money when there is a 'head'. You may pledge to give either some, all, or nothing to your opponent. Remember, your money will only be handed

over to your opponent if the coin toss is a 'head'. If it is a 'tail', this money that you pledge would be returned to you. In case of a 'head', whatever amount you pledge will be sent to your opponent, and you can keep the remaining amount.

Here is an envelope. The amount you pledge to give in case of a 'head' should be left in the envelope. I will now turn my back.

If I play this game with your Santal opponent, how much do you think your opponent would pledge to give you? (Record this value)

(After s/he hands back the envelope, do the coin toss) Once our games end, I will give your envelope to your opponent.

C Appendix: Details on the qualitative evidence

We included additional qualitative questions immediately after Bengalis watched the film to investigate their intentions to help the Santals and their planned actions. We asked open-ended questions focused on Bengalis' willingness to support Santals, the specific forms of support they would consider, their reasoning, and their intentions to encourage other Bengalis in their villages. We specifically asked, *"Now imagine a Santal in your village is in similar conditions as the Santals in the film that you just watched...(1) What would you do in that situation? (2) Why? (3) What would you advise your neighbors to do in that situation?"* Since these questions were only asked to the viewers who watched the documentary on Santals, we cannot claim the following qualitative evidence to be causal.

We received 38 unique responses regarding intentions to help Santals, 20 about underlying reasons for those intentions, and 20 on advice Bengalis would give their neighbors regarding supporting or helping Santals. To narrow down the responses, we used an Application Programming Interface (API) to access the GPT-4 model (Large Language Models or LLM). This allowed us to programmatically assign the responses into broader categories. This approach aims to use the tool as a replacement for human labelling where the final output is a dataset that can be critically examined and falsified by a human or another LLM. To make this categorization both interpretable and falsifiable we ask for—(i) a suggested category and the keywords and phrases used to make the categorization; (ii) what the LLM thinks the definition of these keywords are in the context of the answers; and, (iii) a more detailed reasoning for the categorization explaining in detail how it reached this answer. To get the response categories, we ran our prompts five times. See Appendix D for more details on the categorization and the prompts used.

Based on this exercise, we identified five broad categories of information using the three questions: (1) General support, encompassing non-specific intentions to help Santals (e.g., "I want to help them"); (2) Economic and financial assistance, focused on intentions to provide financial support or help Santals secure better jobs (e.g., "I want to help them financially"); (3) Advisory support, covering intentions to offer Santals practical or economic advice (e.g., "Give them advice"); (4) Humanitarian reasons (e.g., "Because they are suffering"); and finally, (5) Missing, which includes responses that were empty, irrelevant, or indicated an unwillingness to answer.

We visualize the results on intentions to help Santals using a heatmap in Figure C1. General support intentions were most common (about 57% of participants), followed by economic and financial help (about 21%). Advisory support accounted for only 4%, and approximately 18% of responses were missing. These intention patterns were very similar across the 'Random' and 'Central' treatment arms. When exploring the reasons behind these intentions (see Figure C2), we find that approximately 42% of participants cited humanitarian reasons (e.g., "because they are suffering," "because they are in poverty"). Roughly 20% expressed a desire to provide economic or financial help, and another 20% cited more general support reasons (e.g., "they need help"). The remaining 16% did not provide any response. Finally, when asked what they would advise their neighbors to do if the Santals in their villages were suffering (see Figure C3), about half of the participants said they would advise them to help Santals (i.e., general support intentions).

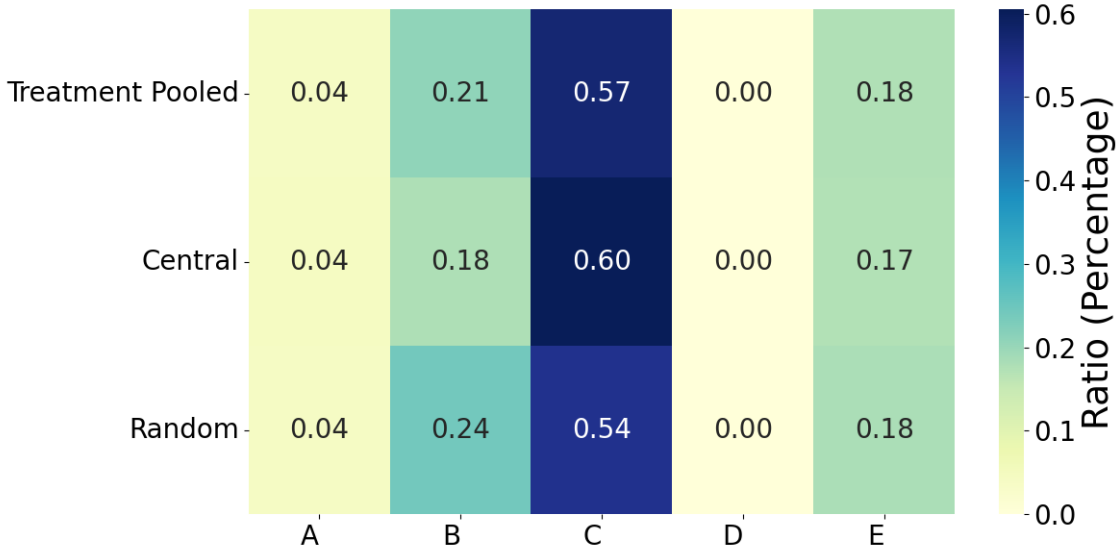
Economic and financial help (20%) and offering better advice to Santals (13%) were the next most common responses. Again, these responses are very similar across 'Random' and 'Central' arms.

Using Sankey diagrams, we also map the new information received from the documentary film (discussed in Section 4.1) into the Bengali peoples' intentions to help Santals. In all diagrams, it is clear that almost all newly received information played some role in developing intentions and motivation among Bengalis to help Santals (Figure C4), the reasons behind developing such intentions (Figure C5), and their positive intentions to also encourage their neighbors to help Santals (Figure C6).

We corroborate the above qualitative evidence (which is not causal) with the following quantitative question, which was asked to all participants (including Bengalis in the 'Control' arm): *"How willing are they to help others"* (without specifying the Santals)? Because we can estimate the treatment effect, we interpret this result as causal. We report this estimate in Column 6 in Table A2 in Appendix A, which shows that Bengalis in both 'RR' and 'CC' arms are significantly more willing to help others. People in 'RC' also seem to have a higher willingness to help others compared to Bengalis in the 'Control' arm, but this difference is not statistically significant at conventional levels.

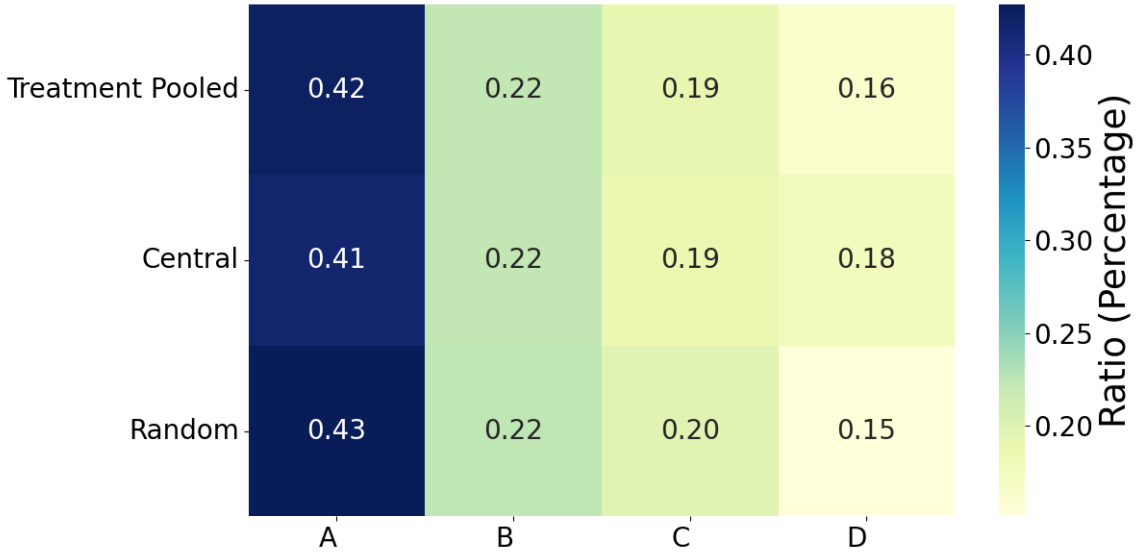
Overall, this analysis highlights the potential for documentary films to promote positive change. Even when deeply held beliefs and opinions remain unchanged, exposure to new information can reshape the intentions and motivations of the ethnically dominant group to support historically marginalized groups.

Figure C1: Heatmap showing the distribution of intentions to help



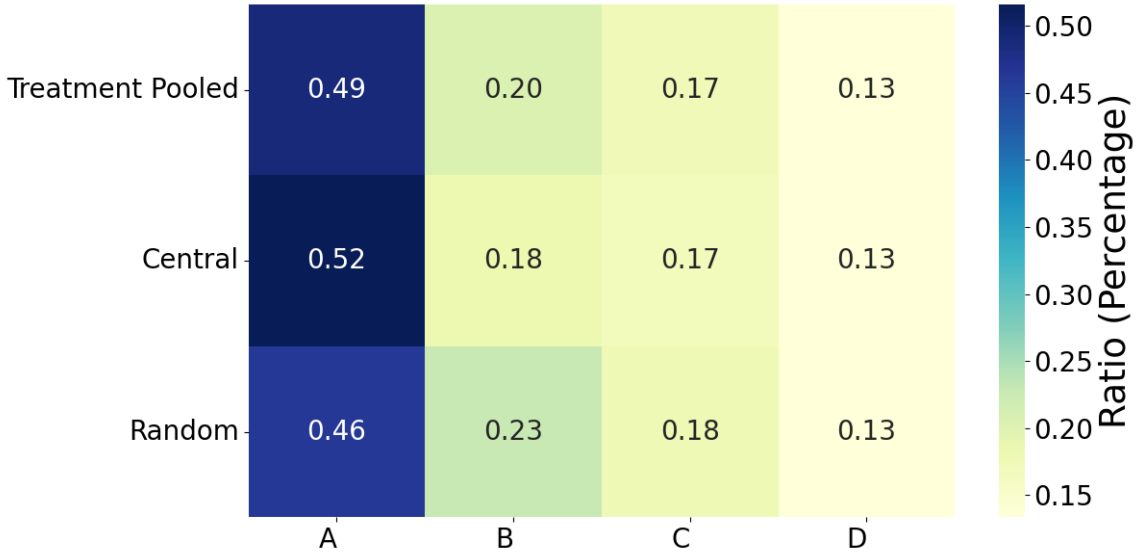
Note: Categories are the top-weighted category for distinct comments aggregating results from five iterations of the GPT-4 model. Weight is the count of the number of times GPT-4 predicts a category for each comment divided by five. In all cases, there is only a single category with a weight above 0.5. There are no cases where a category is below 0.5. Categories not included in the heatmap have no comments where they are the top predicted category. Category to Alphabetic Label Mapping: Advisory Support: A, Economic and Financial Aid: B, General Support Intentions: C, Humanitarian Reasons: D, Missing: E.

Figure C2: Heatmap showing the distribution of reasons behind the intentions to help



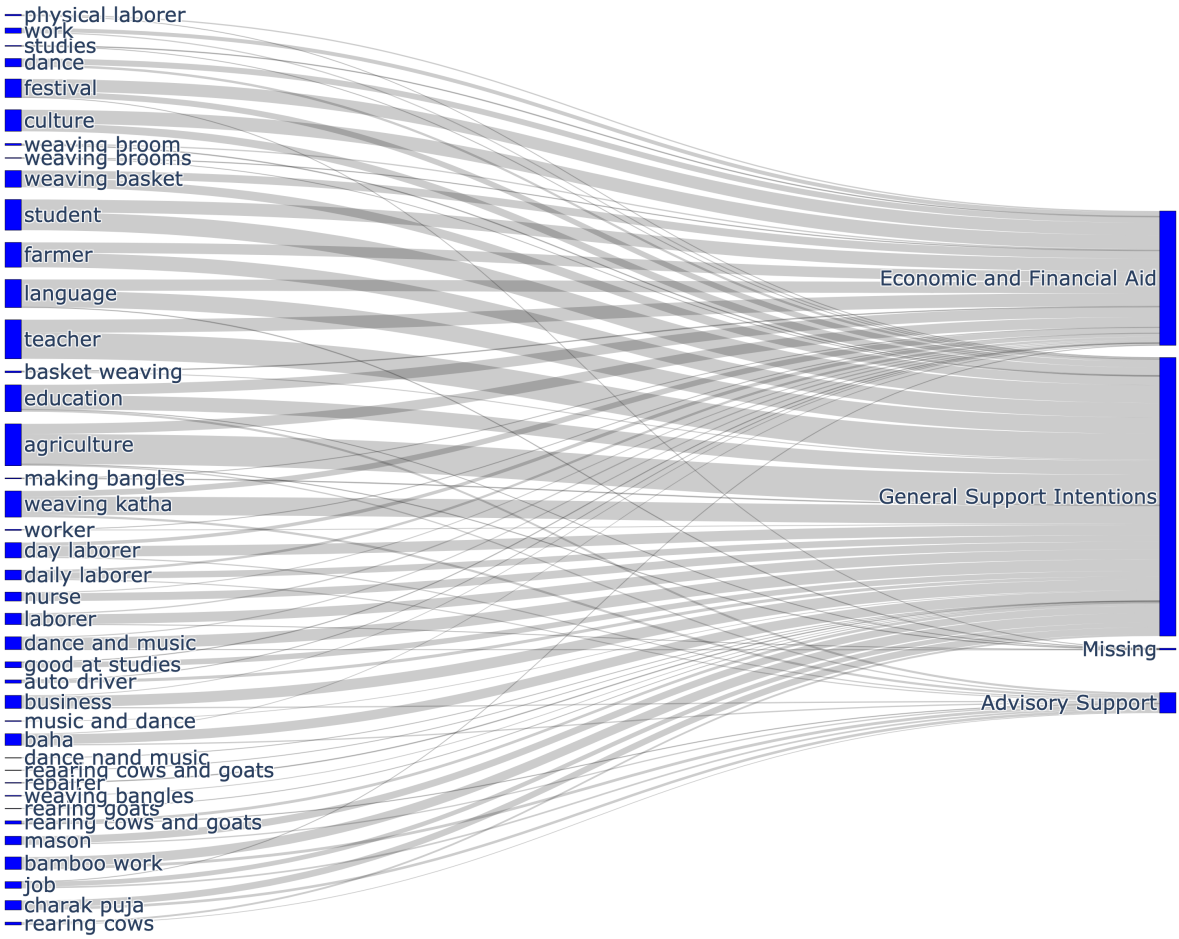
Note: Categories are the top-weighted category for distinct comments aggregating results from five iterations of the GPT-4 model. Weight is the count of the number of times GPT-4 predicts a category for each comment divided by five. In one case, there are two categories with a weight above 0.5. The comment "should help them because they are living in poverty" has the suggested categories 'Humanitarian Reasons' and 'General Support Intentions' in four out of five iterations for both categories. There are no cases where a category is below 0.5. Categories non included in the heatmap have no comments where they are the top predicted. Category to Alphabetic Label Mapping: Humanitarian Reasons: A, Economic and Financial Aid: B, Missing: C, General Support Intentions: D. Therefore, the category 'Advisory Support' is missing here.

Figure C3: Heatmap showing the distribution of the intentions to encourage neighbors when Santals are suffering



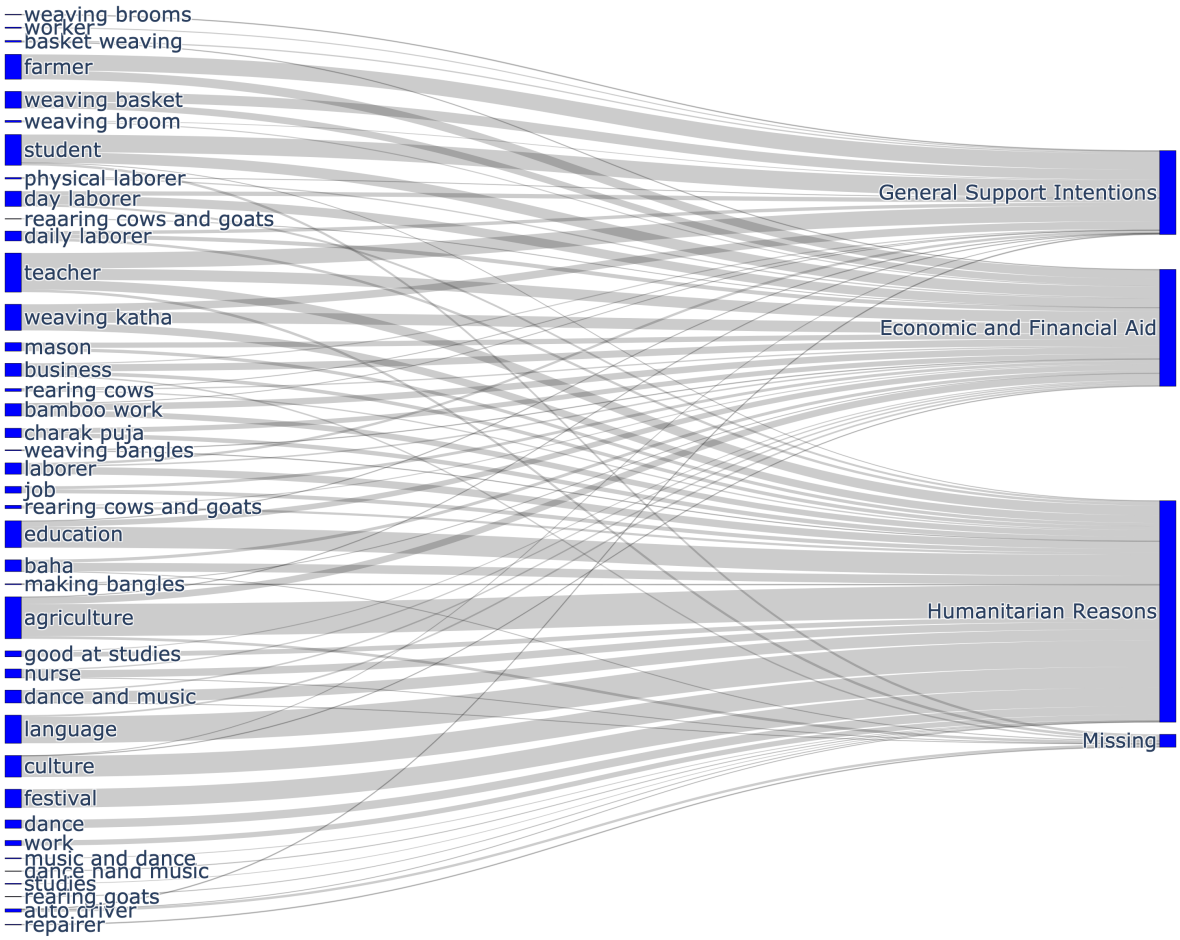
Note: 50 percent of answers fall in the category of general support intentions. Category to Alphabetic Label Mapping: General Support Intentions: A, Economic and Financial Aid: B, Missing: C, Advisory Support: D. Therefore, the category 'Humanitarian Reasons' is missing here.

Figure C4: Mapping new information into intentions to help Santals



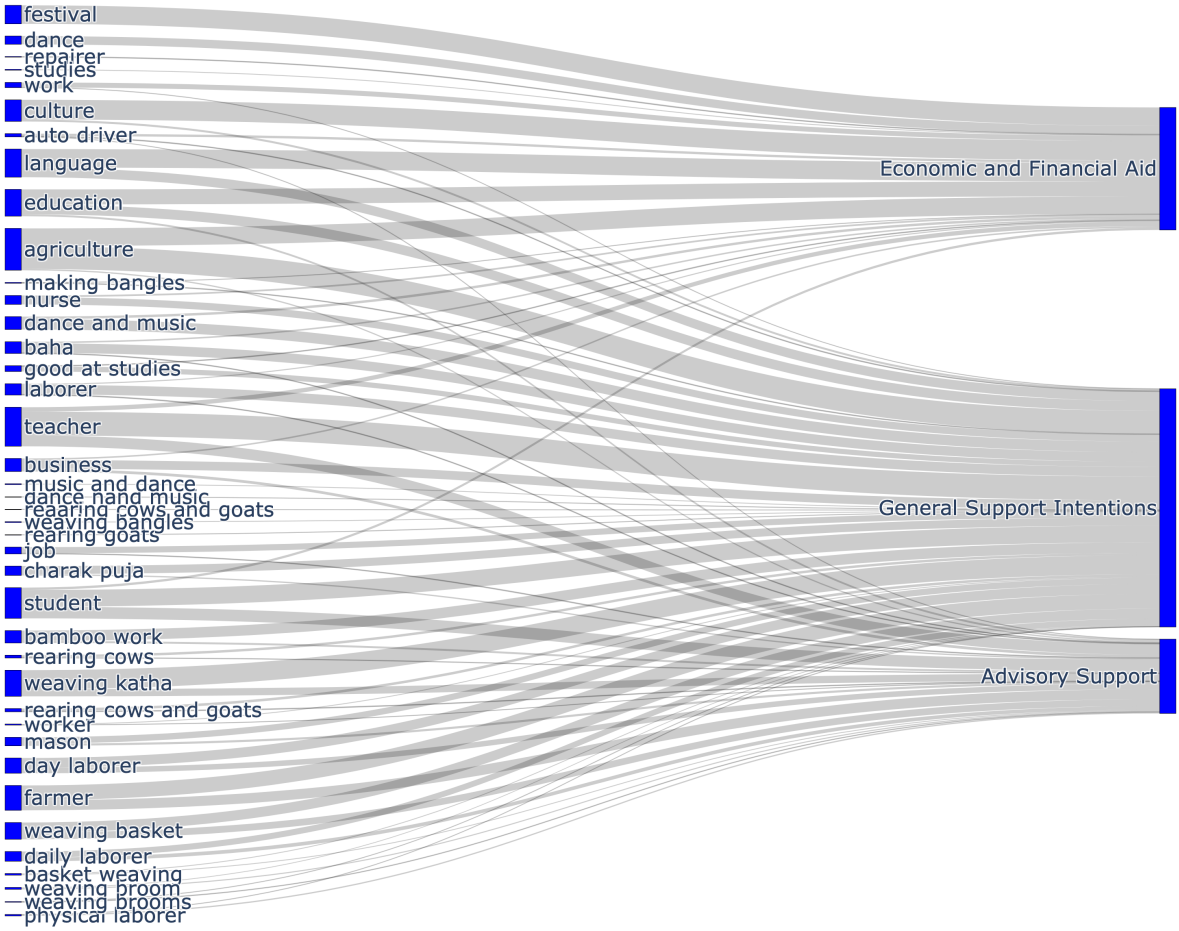
Note: Sankey diagram showing the mapping of new information received through the documentary film (using all unique answers) to Bengalis' intentions to help Santals.

Figure C5: Mapping new information into reasons behind the intentions to help Santals



Note: Sankey diagram showing the mapping of new information received through the documentary film (using all unique answers) to Bengalis' reasons behind the intentions to help Santals.

Figure C6: Mapping new information into the intentions to encourage neighbors when Santals are suffering



Note: Sankey diagram showing the mapping of new information received through the documentary film (using all unique answers) to Bengalis' intentions to encourage neighbors when Santals are suffering.

D Appendix: Categorization using GPT-4

We use GPT-4 to perform the categorization in a process similar to human labeling. GPT-4 is a distinct service from ChatGPT. GPT-4 allows for programmatic queries over a dataset using common programming languages such as Python. This is performed using an Application Programmatic Interface (API). An API is a method for computers to programmatically access a service. OpenAI makes the 'model weights' available to query through this process. The process involves sending a 'prompt' with categories and the survey response you want to categorize to their model through an API, the model predicts the category for each response and returns it to your computer. To ensure the answers are consistent, we run the query five times and take the most frequently predicted category for each survey response.

A standard approach in survey analysis involves using third-party human labelers for data categorization and analysis (Bochkay et al., 2023). ChatGPT and GPT-4 (the model behind it) have emerged as a viable alternative to human labeling. It significantly outperforms mechanical Turk workers across a variety of labeling tasks and is up to 30 times more cost-effective than using human labelers (Gilardi et al., 2023). Moreover, Veselovsky et al. (2023) report that 33 to 46 percent of crowd workers utilize Large Language Models (LLMs) for labeling tasks, indicating a shift towards automation in data processing.

To ensure our categorization is reproducible we use the beta feature 'seed', which is a set seed parameter that aims to make the output of the model more deterministic.[1] This outputs a system fingerprint if the analysis is rerun holding the input text, the prompt, and the temperature constant as a change in system fingerprint indicates that there may have been a change in the model itself. To access this feature, we use the 'gpt-4-1106-preview' model which has the most consistent and reproducible output. This ensures that no observations are missed because of errors in the structured output, additionally, we set the script to try again if there is any error so that all comments are given a category in each iteration. We also set the temperature parameter equal to zero. This means that the most likely outputs are given a higher weighting in the softmax layer. Finally, we asked for the language models to provide reasoning and spreadsheets with this information so that it is possible to identify if the models use the same key information or whether they are focusing on different keywords in determining their category.

Below we provide the prompts used for categorization.

D.1 Prompt system for categorizing 'New lessons learned' answers

The Prompt

You are a highly detailed and analytical assistant.
Your task is to categorize comments based on the following specific categories, which cover a wide range of topics from cultural festivals to economic challenges and educational barriers.
The comment is in response to the question "If this is your village what would you suggest your neighbours to do?"

[1] https://platform.openai.com/docs/api-reference/chat/create

Each category is outlined below along with a concise example that reflects the style of answers we're working with:

Categories and Definitions:

- Education and Capacity Building (c1): Covers formal and informal education, training, and any activity aimed at improving knowledge, skills, and competencies. Example: Conducting workshops on environmental education for community leaders.
- Livelihood and Artisanal Crafts (c2): Encompasses economic activities related to craftsmanship, artisan skills, and professions outside the agricultural sector, focusing on income generation and cultural heritage. Example: Organizing a local market for handmade pottery and ceramics.
- Cultural, Artistic, and Community Engagement (c3): Includes activities that promote cultural preservation, artistic expression, and community participation, strengthening social bonds and cultural identity. Example: Facilitating a community mural project that reflects local history and values.
- Agriculture and Rural Development (c4): Specifically targets agricultural activities, including farming and related rural development practices, emphasizing sustainability and environmental stewardship. Example: Initiating a community-supported agriculture (CSA) program to connect local farmers with urban consumers.

When analyzing an answer:

1. Identify any explicit mentions or clear implications related to the categories.
2. If the answer is general or lacks specific details, infer the most likely category based on broad definitions of keywords in the answer and categories.
3. Provide a reasoned analysis for your categorization, including how certain words or phrases led you to associate the answer with specific categories.
4. Look back at all the suggested categories if there are more than two, do the analysis again focusing on economic definitions of the keywords, and be more precise with categorization.
5. If there are still multiple categories, select only the most directly relevant.

For each answer, provide the following as a single YAML format:

- **comment**: (original comment)
- **category_index**: [list the category codes that apply, e.g., c1, c2, ...]
- **reasoning**: [provide a detailed explanation for each category chosen]

Your analysis should aim to capture the nuance and breadth of each comment's potential relevance to the categories, especially when direct information is limited.

D.2 Prompt system for categorizing 'What to do', 'Why', and 'How to encourage neighbors' answers

This prompt will take each answer and compare it to the categories. The best approach is to provide step by step instructions of what you want the tool to do, similar to how you would set up a data labelling task for a research assistant. This helps to ensure that the output is consistent for all answers. Where the prompt asks for YAML output this is a format that can be extracted directly into a dataframe. The bolded words such as **answer** and **keywords** refer to columns in the final dataframe that we would like to output. **category_index** returns an id for the category instead of rewriting the entire category.

The prompt

You are a highly detailed and analytical assistant.
Your task is to categorize comments based on the following specific categories, which cover a wide range of topics from cultural festivals to economic challenges and educational barriers.
Each category is outlined below along with a concise example that reflects the style of answers we're working with:

Enter the Categories here

When analyzing an answer:

1. Identify any explicit mentions or clear implications related to the categories.
2. If the answer is general or lacks specific details, infer the most likely category based on broad definitions of keywords in the answer and categories.
3. Provide a reasoned analysis for your categorization, including how certain words or phrases led you to associate the answer with specific categories.
4. Look back at all the suggested categories if there are more than two, do the analysis again focusing on economic definitions of the keywords, and be more precise with categorization.
5. If there are still multiple categories select the single most relevant category.

For each answer, provide the following as a single YAML format:

- **answer**: (original answer)
- **category_index**: [list the category codes that apply, e.g., c1, c2, ...]
- **keywords**: [list keywords used to inform the categorization]
- **definitions**: [list of keyword/key terms definitions e.g., keyword; definition]
- **reasoning**: [provide a detailed explanation for each category chosen]

Your analysis should aim to capture the nuance and breadth of each comment's potential relevance to the categories, especially when direct information is limited.