

Promoting Peace Amid Intergroup Conflict: An Intergroup Contact Field Experiment in Nigeria

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This manuscript was compiled on January 5, 2023

Intergroup contact, originally designed as a tool for prejudice reduction, offers a promising means to resolve intergroup conflict. Evidence for contact-based interventions to improve intergroup relations is sparse, however, with most studies focusing only on the individuals who directly engage in contact. We test the ability of a contact-based intervention to promote peace between conflicting groups with a field experiment in Nigeria, where farmer and pastoralist communities are embroiled in a deadly conflict over land use. We examine the effectiveness of the contact interventions using surveys, direct observation of behavior in markets and social events, and a behavioral game. We find those who lived in the communities that received the intervention had more positive intergroup attitudes and feelings of physical security, as well as were more likely to engage in voluntary intergroup contact measured through self-reports and observed behavior in markets. Exploratory analyses show that those who directly participated in the program and those who were only exposed to it by living in the communities where activities were taking place changed similarly with regard to attitudes and perceptions of security, but not with regard to behaviors, indicating the spread to the wider community was likely due to norm change. These results suggest that reducing barriers to peace between conflicting groups is possible, and that structured intergroup contact is a promising method to do so.

Contact Theory | Intercommunal Conflict 2 | Peacebuilding 3 |

How can groups in conflict improve intergroup relations? Violent intergroup conflict has caused 2 million deaths since the year 2000 (Sundberg and Melander 2013)(1), forcibly displaced over 70 million people from their homes in 2018 (UNHCR 2019)(2), threatens food supplies in numerous countries (Verwimp, et al. 2012)(3), and extracts a psychological toll on participants and victims (Rigterink and Schomerus 2019)(4). Improving intergroup relations, therefore, is vital to stem the human, economic, social, and psychological costs of violent intergroup conflict.

Scholars and practitioners consider intergroup contact to be one of the most effective tools for improving individuals' attitudes and behaviors towards an outgroup (Allport 1954)(5). Cooperative contact is contact wherein members of two groups (1) cooperate (2) with equal status (3) to achieve shared goals (4) with support of local authorities.¹ Contact also makes salient the benefits of peace, reminding groups that fighting is costly (Fearon 1995)(6).

Contact-based interventions in the lab and in the field generally change attitudes and/or behaviors of individuals who directly participate in contact interventions (Pettigrew and Tropp 2006; Paluck, Green and Green 2019 for reviews)(7, 8), even among groups with deep-seated conflicts (Ditlmann and

Samii 2016; Mousa 2020, Paler et al 2020 Scacco and Warren 2018)(9–12). Yet it is unclear if and how these interventions affect the wider community involved in conflict. While contact interventions are often implemented at the individual level, the hope is that it improves relations between groups, not just individuals. To improve intergroup relations, the positive effects of contact must diffuse from the individuals involved in contact to other group members (Ditlmann, Samii, and Zeitzoff 2017)(13). This diffusion to other group members can occur through a number of channels, including changing social norms about cross-group interaction (Christ et al. 2014; Paluck 2009)(14, 15) and through the knowledge that other ingroup members had positive contact with outgroup members (Wright et al. 1997)(16). Through social diffusion, contact improves attitudes even for ingroup members with no cross-group contact. Cooperative contact also shows that the outgroup is composed of differentiated individuals (Rimé et al. 2011)(17), opening the possibility that past negative experiences with a few outgroup members do not characterize the entire outgroup.

Yet, there are a number of forces, particularly when there is a history of conflict or in the midst of ongoing conflict that limit this diffusion. Individuals need strong and consistent information to overcome preexisting negative beliefs – a signal that the object of their belief has changed (Nickerson 1998)(18). In contexts of intergroup violence, existing norms against the outgroup may discourage ingroup members with

Significance Statement

Bringing people from different backgrounds together through contact is a common peacebuilding intervention and is used globally to reduce intercommunal conflicts. Yet few experimental field studies of contact theory examine whether it is an effective strategy in ongoing violent conflicts. In this research in the Middle Belt of Nigeria where there is persistent violence between farmer and herder communities, we find that contact led communities to have more positive attitudes about and more contact with the outgroup than control, and that these attitudes diffused to the wider community where the intervention took place. This research illustrates how contact can support peace between communities despite ongoing violence.

CG led the research design and data analyses and co-wrote the paper with RW. RW, LI and DD led the intervention design and contributed to the research design. DD and LI ensured implementation of the experimental treatment.

CG, RW, DD, and LI were employed by Mercy Corps, the implementer of the program, during the research. The study was partially funded by a USAID grant to Mercy Corps for both research and programming activities. CG was not employed by USAID while conducting the research.

¹Note that equal status does not mean that the groups must have the same status in society, but that the groups share equal status in the cooperative situation.

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positive attitudes from displaying those attitudes (Bar-Tal 2007; Bar-Tal and Avrahamzon 2017)(19, 20), lest they be branded as traitors (Bornstein 2003)(21). Even when people do witness contradictory information, individuals often resolve cognitive dissonance by justifying negative previous attitudes (Gubler 2013)(22) or, at best, by differentiating “good” outgroup members from typical outgroup members (Doosje, Spears, and Koomen 1995)(23). Individuals also more readily store and recall confirmatory negative interactions than positive interactions that are dissonant with preexisting negative attitudes (Nickerson 1998; Ward et al. 1997)(18, 24). Due to these combinations of forces, it is difficult for contact to improve attitudes towards a group, particularly in the midst of violence when people are receiving confirmatory negative information.

To investigate the question of whether or not contact-based interventions can extend to the wider community, we study one such intervention in the Middle Belt of Nigeria, where the most recent conflict escalation between farmers and pastoralists has caused 7,000 deaths from 2014-2019 and displaced hundreds of thousands of people from their homes. We randomly assigned communities with ongoing farmer-pastoralist violence to receive a contact-based intervention or serve as a control group. The intervention formed mixed-group committees and provided them with funds to build infrastructure that would benefit both communities.² The program also provided mediation training to each community’s leaders and held forums where the groups discussed the underlying drivers of conflict. To measure the effect of the intervention, we used pre- and post-intervention surveys, a post-intervention natural public goods behavioral game,³ and twelve months of systematic observations in markets and social events during the intervention.

We find that the program increased intergroup attitudes, intergroup contact outside of the intervention, and perceptions of physical security, though it did not increase contributions in the public goods game. Our results also show that the intervention affected communities as a whole, not just community members directly involved in the intergroup contact.

This study brings evidence to the question on whether contact-based interventions can create wider societal change. We identify our intervention’s duration and publicness as potential explanations for why we observe wider change when other interventions have not (Mousa, 2020)(10). Most contact interventions last a relatively short time and the contact is only visible to those directly involved in the intervention. Our intervention lasted eighteen months and the interaction between group members could be observed by the wider community. We suspect the publicness of the interventions shifted perceptions of norms of acceptable behavior and common knowledge among community members (Adida et al. 2020; Arias 2019; Grossman and Michelitch 2018; Levy Paluck 2009)(15, 25–27).

Farmer-pastoralist conflict in Nigeria’s Middle Belt. Nigeria’s Middle Belt is plagued by violent conflict over land use. Farmers, who claim land for agricultural production, and pastoralists, who claim land for animal grazing, increasingly clash over

claims to the same land. Both groups depend on land for their livelihoods, but their divide is also cultural, ethnolinguistic, and, in some locations, religious. The pastoralists are almost homogeneously of the Fulani ethnic group, speak Fulfulde as their primary language, and practice Islam. They maintain a semi-nomadic way of life, belonging to a home community but traversing vast distances to secure access to pastureland and water as seasons change. The farmers live in sedentary villages and cultivate land for agriculture. The ethnic group, language, and religion vary by village. In our study, farmers came from more than a dozen ethnic groups, often residing side-by-side with one another.

Historically, these communities cooperated through trade and sharing land that was abundant relative to populations. In recent years, this relationship has been stressed by population booms and climate change. Nigeria’s population at independence in 1960 was about 50 million; Nigeria’s population in 2019 is estimated around 200 million. At the same time, the Sahara’s size expanded over 10%, decreasing land available for farming and grazing (Okpara et al. 2015; Thomas and Nigam 2018)(28, 29). Climate change has also pushed pastoralists southward, towards farming communities with whom the pastoralists had no preexisting relationship. Land scarcity and new migrants jeopardize traditional cooperative agreements that have managed farmer-pastoralist interactions for decades (Cotula et al. 2004; Kuusaana and Bukari 2015)(30, 31). Exacerbating these issues are government policies on land privatization, which encouraged farmers to plant crops that occupy land continuously, like orchards, and effectively nullified farmer-pastoralist land sharing agreements (Bassett 2009)(32). Additionally, “indigene versus settler” policies limit economic and political rights to certain ethnic groups in each state, often denying the “settler” pastoralists the ability to own land and run for political office (Network 2014)(33).


These stressors have sparked violent conflict between farmers and pastoralists in recent years (Ilo, Ier, and Adamolekun 2019)(34). Several state governments have responded to the conflict by enacting anti-grazing laws, compounding matters. These laws spark more violence because many pastoralists viewed these laws as biased against their way of life. In the state of Benue, the government mobilized state-sanctioned vigilante groups called “livestock guard” to enforce the law, but the livestock guard have often gone beyond guarding farmland and instead acted aggressively and offensively against pastoralists (Duru 2018)(35).

Though we have discussed the conflict as between two large and cohesive groups (“Farmers” and “Pastoralists”), the conflict occurs between numerous small, independent farming and pastoral groups. The groups typically reside a couple miles from each other – like people from the next town over. These independent groups are aware of the broader context of farmer-pastoralist conflict, but their concerns are local and mostly unrelated to what happens in distant villages. Different versions of the same story initiate and sustain the local conflicts. First, cattle graze on farmland.⁴ Next, a farmer retaliates by stealing cattle from the pastoralists (because the farmer does not know which herd grazed on his land, the stolen cattle do not necessarily come from the transgressing herd). This cycle continues and eventually explodes when a member of one side physically attacks a member of the other side. From there, a little war often breaks out. As one reporter noted, “The

²The communities built boreholes, market stalls, and primary health care facilities, for example.

³In a public goods game (PGG), research subjects are given money and told they can keep the money or donate it to a public fund. Money donated to the public fund is multiplied by some amount and then shared with all subjects. Our PGG is natural because it was conducted in a natural setting, rather than a lab. The funding for the PGG came from the National Science Foundation under Grant No. 1656871.

168 countryside is littered with the charred ruins of homes, schools,
169 police stations, mosques and churches.” (McDonnell 2017)(36).

170 
171 Despite the forces of land scarcity and discriminatory poli-
172 cies that push these groups into cycles of retaliatory violent
173 conflict, their interests are not completely misaligned. The
174 conflict has destroyed billions of dollars in agricultural produce,
175 animal products, and physical infrastructure. Moreover, the
176 groups formerly maintained mutually beneficial trade agree-
177 ments: farmers trade the crop residue left on their fields for
178 animal manure/urine to replenish soil; farmers trade grains
179 and vegetables in exchange for the pastoralists’ milk and meat.
180 That these groups have regular contact, engage in trade as
181 equals, and have common goals despite their differences makes
182 this an apropos context in which to test the applicability of
183 contact theory.

184 **Intervention: Engaging Communities for Peace in Nigeria.** To
185 address farmer-pastoralist conflict, Mercy Corps, an inter-
186 national humanitarian and development organization, imple-
187 mented a two-year, USAID-funded program titled Engaging
188 Communities for Peace in Nigeria (ECPN) in Benue and Nas-
189 sarwa, two Middle Belt states embroiled in violent conflict.
190 The main objective of the program was to foster positive
191 contact between farmers and pastoralists, improve attitudes,
192 improve intergroup relations, and ameliorate conflict.

193 The intervention was designed with contact theory in mind.
194 Specifically, groups (1) cooperated with (2) equal status to
195 achieve (3) shared goals with (4) support of local authorities.
196 Below we describe how these conditions of contact theory were
197 met in the program design.

198 The intervention included mixed-group project committees
199 with equal numbers of farmers and pastoralists and provided
200 them with funds to build infrastructure that would benefit
201 both communities; committees then collaboratively chose and
202 constructed infrastructure projects. Each joint project com-
203 mittee consisted of about 16 members, half from the farmer
204 community and half from the pastoralist community. The
205 committees also included women and youth representatives
206 from both sides.

207 Each project committee received two grants, one for quick-
208 impact projects, of approximately \$2,000, and one for joint eco-
209 nomic projects, of approximately \$25,000. The quick-impact
210 projects were conceived as a trust-building initiative, intended
211 to let community members see that cooperation was possible.
212 These projects, managed by both farmers and pastoralists,
213 included hand pumps; construction or renovation of market
214 stalls, schools, and health centers; and construction of fences
215 along grazing routes to protect farmlands and avoid accidental
216 crop damage. The joint economic development projects aimed
217 to address an underlying issue related to the conflict: shar-
218 ing of resources that impact livelihoods. Pollution of water,
219 affecting both farming and livestock, was the primary issue
220 people raised. As a result, each site chose to build a new
221 borehole well, with members of both farmer and pastoralist
222 communities helping to construct the wells.

223 To ensure support of authorities, the program involved
224 community leaders from both sides in all aspects of the projects,

225 with community leaders serving on the committees for all
226 projects. Mercy Corps provided mediation training to each
227 community’s leaders to ensure that conflict did not escalate
228 into wider spread violence. Community-wide forums where
229 the groups discussed the underlying drivers of conflict also
230 included community leaders.

231 With these conditions met, we developed a research design
232 to test whether the contact-based components of the program
233 shifted behavior and attitudes of program participants and
234 the wider community.

235 Research Design

236 We evaluate the effects of Engaging Communities for Peace
237 in Nigeria (ECPN) with a site-level field experiment. Each
238 site contains two communities, one of farmers and one of pas-
239 toralists, who had engaged in deadly clashes within one year
240 of the intervention’s start date. We identified fifteen sites
241 (thirty communities) eligible for the study and surveyed ap-
242 proximately 50 randomly selected respondents per community
243 in a baseline survey. We then randomly selected ten of fifteen
244 sites to receive the intervention, blocking by state so that an
245 equal proportion of sites in Benue⁵ (four) and Nassarawa (six)
246 received the program.⁶ After 18 months, we surveyed another
247 approximately 50 randomly selected respondents in an endline
248 survey. In the months immediately after the baseline survey
249 and immediately before the endline survey, we collected ob-
250 servational data on farmer-pastoralist interactions in shared
251 markets and at social events. We categorized these as baseline
252 observations and endline observations to match our survey
253 data.⁷


254 This design gives us two datasets to analyze. First, we
255 create community-level survey data by aggregating the survey
256 respondents within each community at baseline and endline.
257 Second, we have observational data for social and market
258 behaviors for each site at baseline and endline. In total, we
259 randomly sampled 1539 respondents at baseline in 2015. 1027
260 of those respondents were in intervention sites and 512 were
261 in control sites. At endline, we also randomly sampled 1523
262 respondents, 1028 in intervention sites and 495 in control sites.
263 We conducted 71 baseline and 39 endline market observations;
264 we conducted 54 baseline and 38 endline event observations.

265 **Estimation.** We use linear regression to estimate the effect of
266 the ECPN intervention in the survey and behavioral data. We
267 use randomization inference for p-values and bootstrapping
268 for standard errors, and we estimate one-tailed tests since our
269 hypotheses are that the change in outcomes for treatment units
270 will be greater than control, not that the change in outcomes
271 for treatment units will be different than control. The specifics
272 of each procedure are described in Appendix A. We use one

⁵ The violence in Benue drove some of our treatment and control communities out of their traditional settlements. We describe the steps we took to survey those communities in the appendix.

⁶ Mercy Corps had the budget to treat ten sites regardless of the number of eligible sites. This led to more than 50% of eligible sites being treated because we could not identify 20 sites far enough away from each other and both eligible for and interested in the program.

⁷ Observations were made in two periods: July 2016 – February 2017, immediately after the project commenced but before joint project committees convened, and September 2017 – December 2017, after project committees convened but before the endline survey began. Events that occurred February 2017 or earlier are baseline measurements; events occurring September 2017 or later are endline measurements.

⁸ This experimental design was pre-registered with Evidence in Governance and Politics (EGAP) under ID 20150716AA.  registration can be found at <http://egap.org/registration/1242>. Between preregistration and analysis, we received feedback and modified the analysis plan. Our modifications can be found here: [to be linked](#)

⁴ In past decades, compensation for crop damage would have been standardized, but these traditional agreements have fallen apart in recent years (Cotula et al. 2004; Kuusaana and Bukari 2015)(30, 31). With no agreed upon compensation and no authority to punish illegal grazing or illegal cattle rustling, groups take justice into their own hands.

of two statistical models to estimate the treatment effect of the intervention. When treatment groups are balanced on the baseline outcome, we use the baseline outcome as a covariate to predict the endline outcome, as seen in equation 1.

$$Y_{i,j} = \beta_0 + \beta_1 Z_{i,j} + X_{i,j} + \delta_j + \epsilon_{i,j}, \quad [1]$$

Where i is the community in state j , Z is the treatment indicator, X is the outcome at baseline, and Y is the outcome at endline. δ is a fixed effect for the state j in which the community belongs.

When treatment groups are not balanced on the baseline outcome, we use the change score of the outcome as Y , as seen in equation 2.

$$Y_{i,j} = \beta_0 + \beta_1 Z_{i,j} + \delta_j + \epsilon_{i,j}, \quad [2]$$

Where i is the community in state j , Z is the treatment indicator, and Y is the change in outcome from baseline to endline. δ is a fixed effect for the state j in which the community belongs. For the observations of market behavior, the model is the same except i is the observation in site j because we conducted multiple observations per site and clustered them at the site-level.

Outcomes. We measured four outcomes to estimate the effect of the intervention: (1) intergroup contact, (2) perceptions of physical security, (3) intergroup attitudes, and (4) intergroup cooperation. These multiple measures provide us with attitudinal, perceptual and behavioral measures.

Intergroup contact: Our main outcome is intergroup contact that occurs outside of the intervention. We measured intergroup contact with behavioral monitoring of farmer-pastoralists interactions in markets and social events, a survey index, and a survey experiment.

The behavioral observations in markets and at social events provide a measure of contact independent of response biases. Enumerators attended markets and social events on randomly-selected days and noted the number of interactions between farmers and pastoralists. In the markets, we measured interactions related to buying and selling market goods, such as the number of farmer and pastoralist sellers present and the number of farmer and pastoralist buyers. We then created a farmers index and a pastoralists index to measure the presence of farmers and pastoralists in the market. At social events, we measured the number of members of the other group in attendance and the number who ate or drank what was offered.¹¹ We then created measures for the number of farmers and pastoralists attending social events and the number of farmers and pastoralists eating at social events.

The survey index included questions asking if and how often the respondent interacted with the other group in the past month. The respondents were asked if they interacted with the other group in markets, at public social events, in the respondent's own home, at the home of a member of the other

group, or in any other way. A survey experiment, which we call the percent experiment, informed us about respondents' willingness to engage in contact, depending on the presence of outgroup members. We asked respondents if they would (1) join a group and (2) live in a community with 5%, 25%, 50%, or 75% outgroup members. The percentage was randomized so that the percentage was the same for those two questions but varied across individuals. We took the mean response so that a respondent saying yes to both was assigned a 1, a respondent saying yes to one was assigned a 0.5, and a respondent saying no to both was assigned a 0.

Perceptions of physical security: We measured respondents' perceptions of physical security with a survey index. Because the disaggregated and diffuse nature of the conflict makes obtaining an objective measure of violent conflict extremely difficult, we measured the effect that violent conflict had on individuals' perceptions rather than attempting to measure the frequency and intensity of violence.¹³ We asked respondents if they avoided any areas during the day or night due to insecurity and if insecurity prevented them from engaging in various activities, such as grazing their animals, working on their farms, and fetching water for their families. We combined these ten questions into an index, with high values indicating security and low values indicating insecurity.

Intergroup attitudes: We measure intergroup attitudes with a survey index and an endorsement experiment. The survey index includes two measures of intergroup trust and a five item social distance scale created for the farmer-pastoralist context.

In an endorsement experiment, respondents are asked how much they support a hypothetical policy. We asked respondents how much they would support a water policy if it was endorsed by a farmer organization (asked of pastoralists), if it was endorsed by a pastoralist organization (asked of farmers), or if no endorsement was mentioned (the control condition posed to both pastoralists and farmers). Support was measured on a 5-point scale, where high values indicated support and low values indicated opposition.

Intergroup Cooperation: We measure intergroup cooperation with donations in a natural-field public goods game, where respondents are put into a choice-making situation akin to the choices they make in their lives and should elicit more realistic behavior (Harrison and List 2004; Winking and Mizer 2013) (37, 38).

Because individuals in these communities often decide how to contribute to public goods in the form of community development projects, such as repairing a borehole or building a market stall, we offered respondents the opportunity to participate in a development project. Respondents contributed none, some, or all of 1000 Naira (~ \$3) to a development project committee that comprised an equal number of farmers and pastoralists. Respondents participated in this public goods game in their own homes. One thousand Naira is about half a

⁹We use two different equations because the effectiveness of each equation depends on the correlation between treatment assignment and baseline outcomes. The "controlling-for" method of equation 1 is more precise but is biased when treatment assignment correlates with baseline outcomes. The "differencing" method of equation 2 is unbiased but less precise. For a comparison between these methods, see <https://declaredesign.org/blog/2019-01-15-change-scores.html>.

¹⁰Some of the self-reports and the observations are overdispersed count data. We rank recode all count data. We include analysis of raw (unranked) data in Appendix X

¹¹Taking food or beverages at a social event is a sign of closeness and intimacy in these contexts. Casual attendees would not take food or beverages

¹²This experiment was based on a question from the General Social Survey (GSS) asking respondents if they would favor or oppose living in a neighborhood that was half white/black.

¹³Asking respondents to recount the number of violent events does not accurately measure the scale of the conflict because those answers are determined by the awareness and memory of the community members. Awareness of individual violent events is low because many of the violent events occur in fields and grazing routes far from the town center and residential areas. Additionally, neither ACLED nor Nigeria Watch disaggregated data to the geographic level in which this intervention took place.

¹⁴The public goods game our respondents participated in is similar to the one implemented by Fearon, Humphreys, and Weinstein (2009) (39) as part of a study on community-driven development in Liberia.

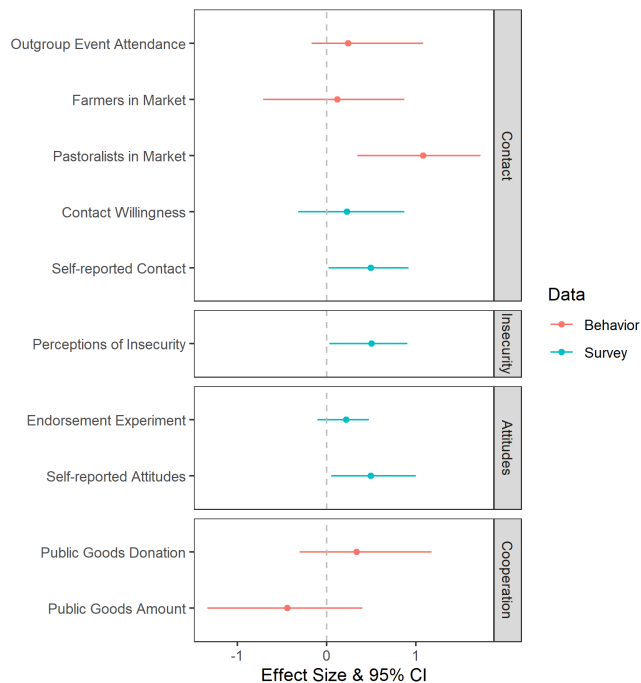


Fig. 1. The ECPN intervention generally had a positive effect on contact, security and attitudes, for both survey measures (in red) and behavioral measures (in blue). We present effect sizes with 95% CIs.

week of work for the median respondent in our survey. A major advantage of having multiple data sources and outcome measurements is if patterns are similar across multiple measures, it increases confidence in the reliability of the effects, even with our limited sample. If we observe similar relationships across multiple measures and data sources we can be more certain that the relationship is not spurious.

Results

Our major finding is that the intervention spurred voluntary intergroup contact, increased feelings of physical security, and improved intergroup attitudes. The intervention, however, did not affect donations in the public goods game. We use coefficient plots to report average treatment effects in our survey data and in our behavioral monitoring data. All coefficient plots show 95% confidence intervals and standardized coefficients.

Figure 1 shows the intervention's effect on survey and behavioral outcomes. From top to bottom, the first five outcomes correspond with the intergroup contact measures; the next two outcomes correspond with the intergroup attitudinal measures; and the last outcome corresponds with perceptions of security.

Intergroup Contact: Our first major outcome is intergroup contact. The results show increases in observed and self-reported intergroup contact. More pastoralists in treatment sites were present buying and selling goods at the local market than in control sites. Since the markets were all located in the farming community, the sustained presence of pastoralists there suggests that (1) farmers were accepting of pastoralists in their community and (2) pastoralists felt comfortable spending time in the farmer community. We do not see a change in the number of farmers present in the

markets in either group because the markets were inside the farming community. The survey corroborates these behavioral results: respondents in treatment communities also reported more contact and more willingness to engage in contact at all levels of the percent experiment.

Perceptions of Physical Security: The intervention substantially increased feelings of security in the treatment group. Compared to respondents in control communities, respondents in treatment communities became more secure engaging in a variety of activities, such as working their fields, grazing their animals, and going out at night. Treatment communities initially felt less secure than control communities but felt more secure by the end of the program.

Intergroup Attitudes: The intervention also bolstered farmers' attitudes towards pastoralists and pastoralists' attitudes towards farmers. Compared to control communities, respondents in treatment communities reported more trust in the other group and were more comfortable engaging in various relationships with the outgroup, such as trading goods and intermarriage. Intergroup attitudes as measured by the endorsement experiment also improved more in the treatment group than the control group, though the difference is not statistically significant at conventional levels.

Intergroup Cooperation: The results of the PGG show that the intervention did not increase respondents' willingness to donate to a fund that helps both groups. Respondents in treatment communities were slightly more likely to donate any amount, but had a lower average donation than control communities. Our fieldwork suggests that the public goods game was not an effective measure of intergroup cooperation in this instance and that respondents did not perceive donations to the public fund as a way to coordinate with the other group. For example, the communities that donated the most money to the public fund had such problematic farmer-pastoralist divisions that the farmers and pastoralists could not agree on who would hold the money in the community fund. The community fund had to be held at Mercy Corps' Abuja office until the communities decided how to spend it.

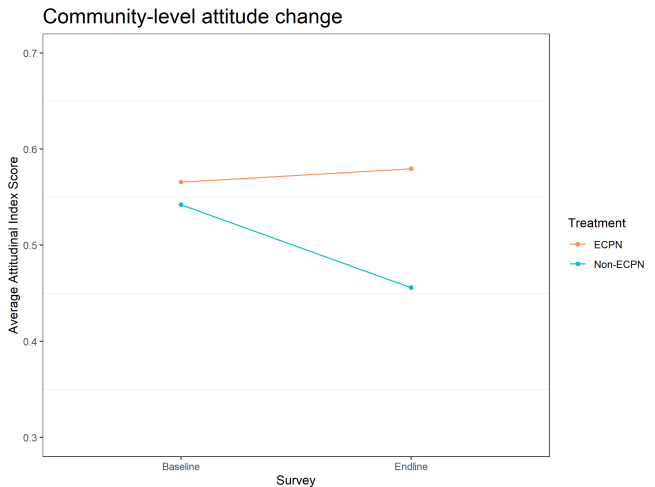


Fig. 2. The ECPN intervention helped communities maintain their attitudes (red line), while attitudes worsened in control communities (blue line). The vertical axis represents the Average Attitudinal Index Score.

Exploring these effects: Intervention Participation. We conducted exploratory analyses of respondents' level of participation in the intervention. Individuals fell into one of three groups: (1) a control group who had no exposure to the intervention, (2) a nonparticipant group who did not participate but lived in communities where the intervention occurred, and (3) participants who directly participated in the intervention's joint-project committees. Comparing individuals in these three groups, though purely observational, informs us about the extent of the effect the intervention had on direct participants relative to their wider community. As explained above, there are a number of potential channels through which changes due to contact can spread between those who are actively engaged in contact to those in the wider community.

To learn about the effect of direct intergroup contact, we resurveyed baseline respondents from three groups: control, non participants and participants.¹⁵ We then compared the change of participants and nonparticipants in intervention sites to the change in controls. We observed that the attitudes and perceptions of participants and nonparticipants improve equally, but voluntary intergroup contact only increases for full participants. The full individual-level analysis is included in the appendix, but Fig X shows the descriptive change in voluntary intergroup contact, perceptions of physical security, and intergroup attitudes for participants, nonparticipants, and controls.¹⁶ That perceptions and attitudes shift for both participants and nonparticipants, but not behavior, indicates that the spread to the wider community is likely due to changes in community norms and/or observing others cooperating across conflict lines.

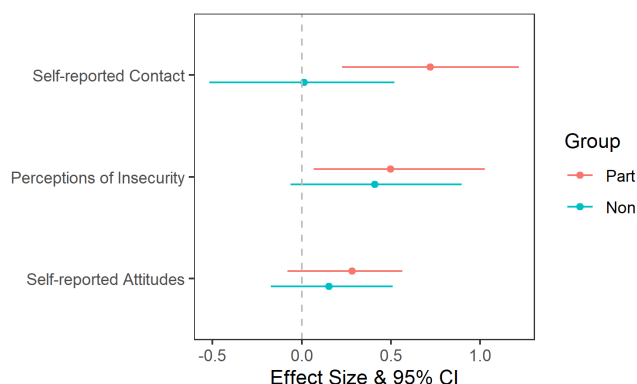


Fig. 3. The ECPN intervention had a positive effect on self-reported contact, security and attitudes for participants (in red) and non participants (in blue) compared to control. We present effect sizes with 95% CIs.

Social desirability bias. To provide evidence that these survey results are due to intergroup contact and not due to social desirability bias, we analyze the effect of the intervention on attitudes about violence. Attitudes about violence are a good candidate for a “placebo outcome” because intergroup contact should not affect general attitudes about violence, but respondents may feel social pressure to answer violence questions

¹⁵At endline, we resurveyed 287 of the baseline respondents to form an individual-level dataset. 74 of those respondents directly participated in ECPN, 121 were in intervention sites but did not participate, and 92 were in control sites. We resurveyed, on average, ~ 10 respondents per community before and after the intervention.

¹⁶We are not able to test intergroup cooperation for this analysis as we did not include the public goods game at baseline.

in a desirable way. If the intervention affects attitudes about violence, then we worry that other self-reports were affected by social desirability bias. If the intervention has no effect on attitudes about violence, then it is unlikely that other self-reports were affected by social desirability bias. Analysis of this placebo outcome, presented in Appendix C, shows that the intervention has no effect on attitudes about violence in the community-level data ($p=0.691$) or the individual-level data ($p=0.556$). The lack of an effect on this placebo outcome, plus our use behavioral observation to corroborate survey self-reports, suggests that our self-report results for primary outcomes are likely not due to social desirability bias.

Discussion

This paper provides evidence that intergroup contact can improve intergroup relations, even in dire circumstances. We tested the effects of a contact intervention in an active and escalating conflict between farmers and pastoralists in Nigeria. The persistent violence in this context poses a stringent test for contact to improve intergroup relations. The violence generates grievances that feed outgroup animosity, reinforce group differences, strengthen social and psychological barriers to improving attitudes, and support the perception that each groups' interests are opposed. Despite the difficult context, the intervention improved intergroup attitudes, fostered more intergroup contact, and decreased feelings of insecurity in these communities. This study also provides suggestive evidence that the effects of contact interventions, which typically involve only a small subset of a community, can spillover to others in the community. Respondents from intervention communities who did not directly participate in our intervention felt more positively toward the other side and felt more physically secure from violence than respondents from control communities. One reason we may see this spillover is the public nature of the contact. In other studies using vocational training, sports and dialogues, the contact was contained and not broadcasted to the larger community. Our treatment was much more public, with community leaders holding open fora and the construction of community infrastructure as a result of joint project committees. Several recent studies suggest that public information has a greater impact on attitudes and behaviors than private information (Adida et al. 2020; Arias 2019; Grossman and Michelitch 2018)(25–27). In some cases, maintaining the confidentiality of contact is a necessary security measure, as was likely the case for Christian and Muslim soccer players in Mosul (Mousa 2020)(10). In those contexts, those who are willing to meet with the other side may be considered traitors and targeted by less tolerant ingroup members. However, by keeping the contact private, there are fewer opportunities to shift norms of appropriate and accepted behavior between groups. This could be one reason why we see behaviors change outside the confines of the intervention – increased contact in markets – while there is little evidence of a change in behaviors off the sports field in Mosul.

Contact in treatment communities did increase more than contact in control communities, but only for intervention participants. Contact by nonparticipants did not change relative to control respondents, but attitudes and perceptions of

¹⁷Although Mousa (2020)(10) found no average change in off-field behaviors, the paper found indicative evidence that off-field cooperative behaviors improved where the soccer leagues were more public and had more community support.

physical security increased similarly for nonparticipants and participants. As a result, we believe that some of the change in attitudes and perceptions of security are due to a spillover effect.¹⁸ By examining both participants and nonparticipants, we are able to address a main critique of many contact-based and peacebuilding interventions: that even if these interventions change individuals, it is often not clear whether this change is scalable and will lead to societal change (Ditlmann, Samii, and Zeitzoff 2017)(13).

We are not able to determine how this spillover from participants to nonparticipants occurred, but we speculate that spillover occurred through three mechanisms that stem from the publicness of the intervention. First, nonparticipant community members may have observed members of both groups cooperate to address shared issues, shifting beliefs that cooperation is possible. Second, the outcome of cooperation (i.e., the borehole) could have shifted beliefs that cooperation with the other group can benefit the individual and their group. Third, and, we think, most importantly, the publicness of the intervention may have caused norms of cooperation—and what was appropriate behavior between groups—to diffuse through each community. Our fieldwork also suggests that the publicness of the intervention contributed to learning and cooperation between communities. For example, our research partners on the ground noted that treatment communities were often able to resolve their disputes because pastoralists became more aware of the financial value of the crops destroyed by cows and farmers became more aware of the difficulty of controlling and corralling thousands of cows; no such learning occurred in control communities.¹⁹ In another of the treatment sites, farmers defended pastoralists from a group of anti-pastoralist vigilantes, rather than assist the vigilantes in removing the pastoralists and claiming their land.

This paper also contributes to the growing number of field experiments testing contact theory. One of the major questions emerging from this literature is whether these interventions shift attitudes, behaviors, or both. While Scacco and Warren (2018)(12) and Mousa (2020)(10) find changes in behaviors but not attitudes, Paler et al. (2020)(11) find changes in attitudes, but not behaviors. One difference between these interventions is whether the peacebuilding elements of the program were explicit or implicit. Like Paler et al. (2020)(11), we test an explicit peacebuilding intervention. We find some changes in attitudes (e.g., outgroup affect) and some changes in behaviors (e.g., in contact—both self-reported and observational, but not in the public goods game). Unlike these other contact-based interventions which ranged from a one-shot meeting (Paler et al. 2020(11)) to sixteen weeks (Scacco and Warren 2018(12)), ours lasted eighteen months. That we were able to provide a stronger “dosage” of contact may be one potential explanation why we were able to see changes in both attitudes and behaviors.

There remain several opportunities to learn about the effects of contact in conflict environments. Contact interventions, explicitly or implicitly, involve groups cooperating to achieve a joint goal. This intervention was designed to benefit all

communities by having the conflicting communities cooperate successfully. But what if contact is not successful and the goal is not achieved? Does contact itself still improve attitudes, or does contact work because groups begin to associate cross-group cooperation with good outcomes?

Contact interventions are also meant to change both attitudes and behavior. Future work should more deliberately study the dosage of contact necessary to improve attitudes and behaviors, or if other forms of contact, such as public contact, are more successful in shifting both.

Lastly, while this study illustrates that larger societal change is possible with contact interventions, we are unsure how and why it occurred. Future studies should examine how social norms and interpersonal discussion diffuse the positive effects of contact to other ingroup members without outgroup contact.

While there remain these important theoretical and practical questions regarding how, why and when contact reduces conflict between groups, these results do illustrate the promise of contact not only affecting those directly involved in an intervention, but that under certain circumstances, has the power to shift the broader community towards the belief that cooperation between groups with a history of violence is possible.

ACKNOWLEDGMENTS. We are grateful to Jake Bowers, Jim Kuklinski, Justin Rhodes, Cara Wong, Caglayan Baser, Ekrem Baser, Nuole Chen, Alice Iannantuoni, Betsy Rajala, Chagai Waess, and Donald Beaudette for useful conversations and feedback on the many drafts of this paper. Nicholas Tint Zaw provided critical assistance in preparing the manuscript. We thank the Mercy Corps Nigeria team for their work implementing the peacebuilding project. We also thank Tahiru Ahmadu, Ibrahim Hassan, and the enumeration teams for their excellent work interviewing farmers and pastoralists. Hadiza Nuhu and Israel Okpe provided crucial support as the main contact people and mobilizers in farming and pastoral communities. Lastly, we acknowledge the support of Evidence for Governance and Politics for this research, in helping to arrange the collaboration and its members for providing invaluable design input and feedback on the paper.

Funding information: This research was funded by the United States Agency of International Development (USAID) through a cooperative agreement to Mercy Corps, the implementer of the program. Additional research support was provided by the National Science Foundation under Grant No. 1656871 and the Carnegie Corporation of New York through the Peacebuilding Evaluation Consortium. All data are available through [data.worldbank.org](#).

Conflict of Interest: Conflict of interest declaration: CG, RW, DD, and LI were employed by Mercy Corps, the implementer of the program, during the research. The study was partially funded by a USAID grant to Mercy Corps for both research and programming activities. CG was not employed by USAID while conducting the research.

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¹⁹ We are especially grateful to Israel Okpe for this and other observations about farmer-pastoralist conflict dynamics.

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