PNAS RESEARCH ARTICLE | SOCIAL SCIENCES OPEN ACCESS

How contact can promote societal change amid conflict: An intergroup contact field experiment in Nigeria

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Edited by David Laitin, Stanford University, Stanford, CA; received March 24, 2023; accepted September 18, 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 intervention on the wider population—not just those directly engaged in contact—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 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 contact interventions can have wider societal change and reduce the barriers to peace between conflicting groups.

contact theory | intercommunal conflict | peacebuilding

Significance

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 and whether contact spreads beyond those directly participating in the intervention. 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.

How can groups in conflict improve intergroup relations? Violent intergroup conflict has caused 2.8 million deaths since 1989 (1) and forcibly displaced over 100 million people from their homes (2). It threatens food supplies in numerous countries (3) and extracts a psychological toll on participants and victims (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 toward an outgroup (5). Intergroup contact is when members of two groups 1) cooperate, 2) with equal status, 3) to achieve shared goals, and 4) with support of local authorities. Contact also makes salient the benefits of peace, reminding groups that fighting is costly (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 (7, 8), even among groups with deep-seated conflicts (9–12). Yet it is unclear whether 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 (13). This diffusion to other group members can occur through a number of channels, including changing social norms about cross-group interaction (14, 15) and through the knowledge that other ingroup members had positive contact with outgroup members (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 (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 (18). In contexts of intergroup violence, existing norms against the outgroup may discourage ingroup members with positive attitudes from displaying those attitudes (19, 20), lest they be branded as traitors (21). Even when people do witness

Author contributions: C.G., R.W., D.D., and L.I. designed research; C.G., R.W., D.D., and L.I. performed research; C.G., R.W., and L.I. contributed new reagents/analytic tools; C.G. analyzed data; and C.G. and R.W. wrote the paper.

Competing interest statement: C.G., R.W., D.D., and L.I. 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. C.G. was not employed by USAID while conducting the research.

This article is a PNAS Direct Submission.

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This article contains supporting information online at https://www.pnas.org/lookup/suppl/doi:10.1073/pnas.2304882120/-/DCSupplemental.

Published October 19, 2023.

contradictory information, individuals often resolve cognitive dissonance by justifying their preexisting negative attitudes (22) or, at best, by differentiating "good" outgroup members from typical outgroup members (23). Individuals also more readily store and recall confirmatory negative interactions than positive interactions that are dissonant with preexisting negative attitudes (18, 24). Due to these combinations of forces, it is difficult for contact to improve attitudes toward 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 recent conflict escalation between farmers and pastoralists caused 7,000 deaths from 2014 to 2019 and displaced hundreds of thousands of people from their homes. The conflict in the Middle Belt is similar to other intercommunal conflicts globally where different ethnic groups or tribes fight over scarce resources at a local level, such as in Kenya, Uganda, Mali, South Sudan, Yemen, and Afghanistan.

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, such as boreholes, market stalls, and primary health care facilities. 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 on the wider community (i.e., not just those engaged in direct contact), we used pre- and postintervention surveys, a postintervention natural public goods behavioral game, and systematic observations in markets and social events during the intervention.

We find that the program improved intergroup attitudes, intergroup contact outside of the intervention, and perceptions of physical security of the wider community, though it did not increase contributions in the public goods game.

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 (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 18 mo 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 (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, practice Islam, and have a distinct style of dress. They maintain a seminomadic 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. Their 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 2023 is estimated to be over 210 million. At the same time, the Sahara's size expanded over 10%, decreasing land available for farming and grazing (28, 29). Climate change has also pushed pastoralists southward, toward 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 (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 (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 (33).

These stressors have sparked violent conflict between farmers and pastoralists in recent years (34). Several state governments have responded to the conflict by enacting antigrazing laws, which sparked 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 (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 of 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. In past decades, compensation for crop damage would have been standardized, but these traditional agreements have fallen apart in recent years (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. 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 countryside is littered with the charred ruins of homes, schools, police stations, mosques and churches" (36).

Despite the forces of land scarcity and discriminatory policies that push these groups into cycles of retaliatory violent conflict, their interests are not completely misaligned. The conflict has destroyed billions of dollars in agricultural produce, animal products, and physical infrastructure. Moreover, the groups formerly maintained mutually beneficial trade agreements: Farmers trade the crop residue left on their fields for animal manure/urine to replenish soil; farmers trade grains and vegetables in exchange for the pastoralists' milk and meat. That these groups have regular

contact, engage in trade as equals, and have common goals despite their differences makes this an apropos context in which to test the applicability of contact theory.

Intervention: Engaging Communities for Peace in Nigeria. To address farmer–pastoralist conflict, Mercy Corps, an international humanitarian and development organization, implemented a two-year, USAID-funded program titled Engaging Communities for Peace in Nigeria (ECPN) in Benue and Nassarawa, two Middle Belt states embroiled in violent conflict. The main objective of the program was to foster positive contact between farmers and pastoralists, improve attitudes, improve intergroup relations, and ameliorate conflict. The intervention was designed with contact theory in mind. Specifically, groups 1) cooperated with, 2) equal status to achieve, 3) shared goals with, and 4) support of local authorities. Below we describe how these conditions of contact theory were met in the program design.

The intervention included mixed-group project committees with equal numbers of farmers and pastoralists and provided them with funds to build infrastructure that would benefit both communities; committees then collaboratively chose and constructed infrastructure projects. Each joint project committee consisted of about 16 members, half from the farmer community and half from the pastoralist community. The committees also included women and youth representatives from both sides. As informal governance structures, such as development committees, are common across much of Nigeria and sub-Saharan Africa generally (37), Mercy Corps' role was to help ensure the committees were more representative by incorporating women and youth. To incentivize cooperation between farmer and pastoralist committees, which had become less common in recent years, Mercy Corps provided grants for joint projects (*SI Appendix, Intervention Details*).

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

To ensure support of authorities, the program involved community leaders from both sides in all aspects of the program. Some of the leaders participated on project committees, but they only made a small proportion of the membership. Mercy Corps provided mediation training to each community's leaders to prevent conflict from escalating into wider-spread violence. Community-wide forums where the groups discussed the underlying drivers of conflict also included community leaders.

Materials and Methods

We evaluated the effects of Engaging Communities for Peace in Nigeria (ECPN) with a site-level field experiment. Each site contains two communities, one of farmers and one of pastoralists, who had engaged in deadly clashes within one year of the intervention's start date. We identified 15 sites (30 communities) eligible for the study and surveyed approximately 50 randomly selected respondents per community in a baseline survey (see *SI Appendix, Intervention Details* for site selection details). We then randomly selected ten of 15 sites to receive the intervention, blocking by state so that an equal proportion of sites in Benue (4) and Nassarawa (6) received the program.* After 18 mo, we surveyed another approximately 50 randomly selected respondents per community in an endline survey. In the months immediately after the baseline survey and immediately before the endline survey, we collected observational data on farmer–pastoralist interactions in shared markets and at social events. We categorized these as baseline observations and endline observations to match our survey data. In intervention sites, community members who did not participate directly in the contact interventions composed the vast majority of the sample.

This design gives us two datasets to analyze. First, we create community-level survey data by aggregating the survey respondents within each community at baseline and endline. Second, we have observational data for social and market behaviors for each site at baseline and endline.

In total, we randomly sampled 1,539 respondents at baseline in 2015. A total of 1,027 of those respondents were in intervention sites and 512 were in control sites. At endline, we randomly sampled 1,523 respondents, 1,028 in intervention sites, and 495 in control sites. We conducted 71 baseline and 39 endline market observations; we conducted 54 baseline and 38 endline social event observations.† Local enumerators informed all randomly selected respondents that they were not required to participate in the study. They were also informed that they could stop the survey at any time and they would still receive full compensation. All study methods and materials, including informed consent procedures, were approved by the University of Illinois, Urbana-Champaign IRB #16054.

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

We use one 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 Eq. **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 Eq. **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 and social events, 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 on the wider community: 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

*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.

†This experimental design was preregistered with Evidence in Governance and Politics (EGAP) under ID 20150716AA. The preregistration can be found at https://osf.io/p9a5f/. Between preregistration and analysis, we received feedback and modified the analysis plan. Those changes are cataloged in the preregistration.

measures. A major advantage of having multiple data sources and outcome measurements is that, if we observe similar relationships across multiple measures and data sources, we can be more certain that the relationship is not spurious, even with our limited sample (see *SI Appendix*, Tables S29–S32 for balance tests across outcomes; see *SI Appendix*, Tables S37–S40 for survey outcome 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. Nigerian enumerators familiar with farmers and pastoralists but unaware of the study's hypotheses 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 vendors in the market. We then created a farmer index and a pastoralist index to measure the presence of farmers and pastoralists in the market. At social events, we recorded which group hosted the event and measured the number of members of each group in attendance and the number who ate or drank what was offered. We then created an outgroup index to measure the number of outgroup members attending and eating at social events.[‡]

The survey index included questions asking whether and how often the respondent interacted with the other group in the past month. The respondents were asked whether 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 whether 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.[§] We asked respondents whether they avoided any areas during the day or night due to insecurity and whether 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 than lab-based games (38, 39).

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 1,000 Naira (∼$3) to a development project committee that comprised an equal number of farmers and pastoralists. One thousand Naira is about half a week of work for the median respondent in our survey. Respondents participated in this public goods game in their own homes. Our outcomes are the proportion of individuals who donated to the public good and the average donation amount in each community.

Results

Our major finding is that the intervention spurred voluntary intergroup contact, increased feelings of physical security, and improved intergroup attitudes among the wider community, not just those who directly participated in the intervention. The intervention, however, did not affect donations in the public goods game (PGG). 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.[¶]

Fig. 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 is our measure of perceptions of security; the next two outcomes correspond with the intergroup attitudinal measures; and the last outcomes correspond with intergroup cooperation.

Intergroup Contact. Our first major outcome is intergroup contact. The results show increases in observed and self-reported intergroup contact. Farmers and pastoralists were not statistically more likely to attend outgroup events in treatment sites, but 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, likely because the markets were inside the farming community. The survey corroborates these behavioral results: Respondents in treatment communities also reported more contact. However, they were not statistically more willing to engage in contact as per 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 toward pastoralists and pastoralists' attitudes toward

[‡] Due to inconsistent observations of social events, we only have both baseline and endline data for six intervention sites.

[§] Asking 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.

[¶] Effects are not statistically different in Benue or Nassarawa (presented in *SI Appendix*, Table S27) or between farmers and pastoralists (presented in *SI Appendix*, Table S28).

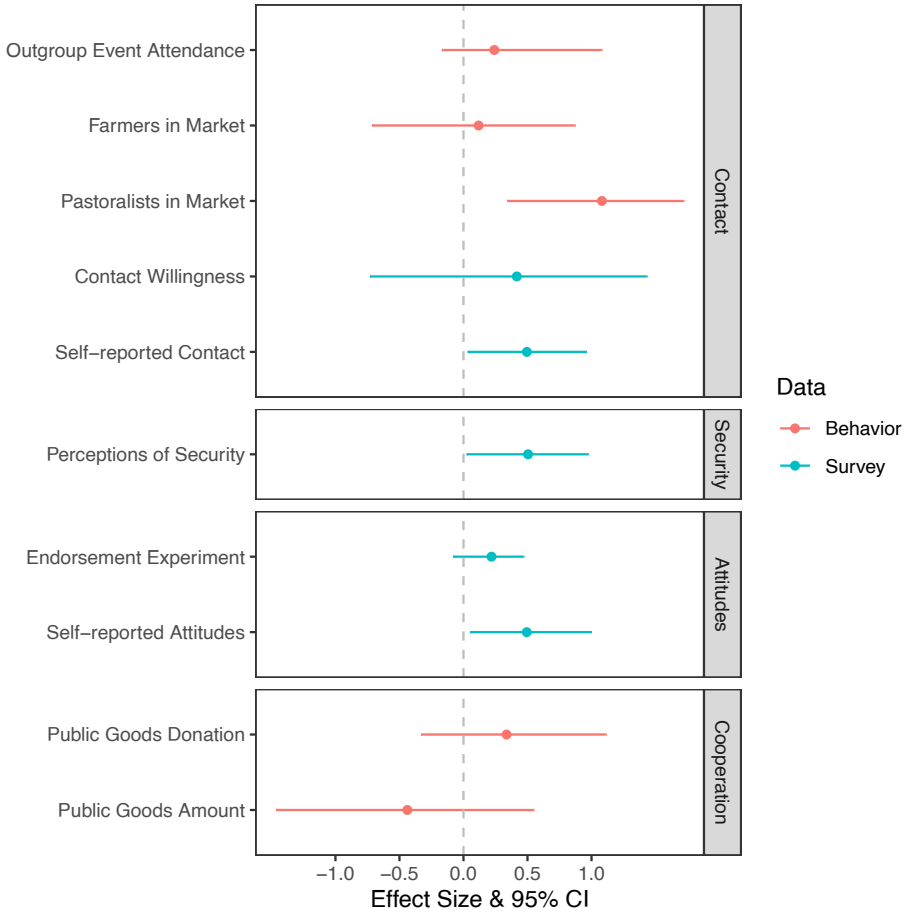

Fig. 1. The ECPN intervention generally had a positive effect on contact, security, and attitudes, for both survey measures (in blue) and behavioral measures (in red). The intervention did not affect cooperation. We present effect sizes with 95% CIs.

farmers. Compared to control communities, respondents in treatment communities reported more trust in the outgroup and were more comfortable engaging in various relationships with the outgroup, such as trading goods and intermarriage, though we do not find the same shift in attitudes as measured by the endorsement experiment. As shown in Fig. 2, the intervention appears to have helped groups maintain or increase trust marginally; however, for areas without the intervention, trust decreased substantially.

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.

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 is observational and while we can account for baseline differences between groups by comparing their change over time,

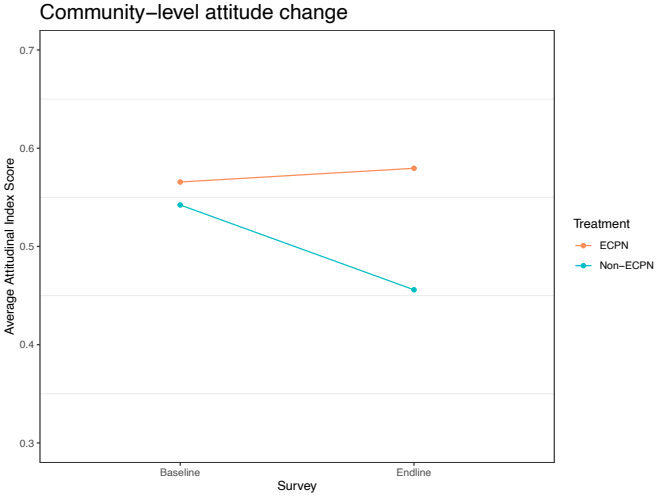

Fig. 2. The ECPN intervention helped communities maintain their attitudes (red line), while attitudes worsened in control communities (blue line). The vertical axis represents the Average Attitudinal Index Score.

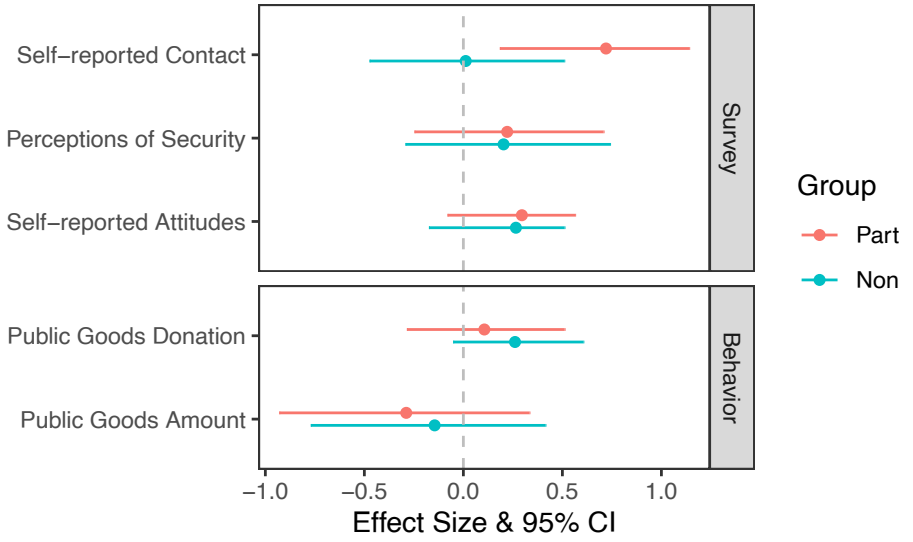

Fig. 3. The ECPN intervention descriptively increased perceptions of security and attitudes for participants (in red) and nonparticipants (in blue) compared to control, though the difference is not statistically significant. Self-reported contact increased significantly, but only for participants. We present effect sizes with 95% CIs.

we cannot account for different propensities to change. Despite this limitation, we believe the analysis provides insight into the effect the intervention had on direct participants relative to those from the 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, nonparticipants, and participants. At endline, we resurveyed 287 of the baseline respondents—approximately 10 per community—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 then compared the change of participants and nonparticipants in intervention sites to the change in controls. We observed a positive trend in the attitudes and perceptions of participants and nonparticipants, though the trend is not statistically significant. In contrast, voluntary intergroup contact increases significantly for full participants. The full individual-level analysis is included in *SI Appendix*, Tables S23–S26, but Fig. 3 shows the differential changes in voluntary intergroup contact, perceptions of security, intergroup attitudes, and cooperation for participants and nonparticipants compared to controls. 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.

Social Desirability Bias and Other Alternative Explanations. To provide evidence that these survey results are due to intergroup contact and not due to other factors (e.g. social desirability bias), we analyze the effect of the intervention on three placebo outcomes: attitudes about violence generally; trust toward religious outgroups, who were not part of the intervention; and radio listening. We chose these as placebo outcomes because they should not be affected by the intervention but could be affected by social desirability, a general increase in tolerance, or access to information that could also affect attitudes and perceptions. If we see "treatment effects" on these placebo outcomes, it would raise doubts about our findings.

SI Appendix, Tables S1–S8 shows that the intervention has no effect on these placebo outcomes. Treatment and control groups do not differ in attitudes about violence ($P = 0.69$ in the community-level data and $P = 0.55$ in the individual-level data); trust in religious outgroups ($P = 0.35$ in the community-level data and $P = 0.59$ individual-level data); or radio listening ($P = 0.43$ in the community-level data and $P = 0.20$ the individual-level data). The lack of effects on these placebo outcome, plus our use behavioral observation to corroborate survey self-reports, suggests that our self-report results for primary outcomes are likely due to the effects of the intergroup contact intervention.

Additionally, we recognize that due to the bundled nature of the intervention, the cause for these shifts in attitudes and behaviors may be the result of the mediation training or the development projects (e.g., boreholes) rather than contact per se. Analyses indicate that these alternative explanations are unlikely. While we cannot rule out that the effect was due to the meditation training, only 52 of the over 1,000 respondents in treatment sites had any exposure to the mediation intervention (*SI Appendix*, Table S13). It is also not the case that treatment effects were significantly larger for communities where larger proportions of people were aware of, used, and perceived benefit from the projects (*SI Appendix*, Tables S9 and S10). These analyses provide us with further confidence that these changes were primarily due to contact.

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, bolster 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, not only for those who directly participated, but also for the wider community.

This study also provides evidence that the effects of contact interventions, which typically involve only a small subset of a community, can spillover to others in the community. Our main results show community-wide effects among a random sample of people who largely were not involved in the intervention. Additional exploratory analyses show that 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 plausible reason we 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 broadcast 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 (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 (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. Although Mousa (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.

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 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. Another possibility is that nonparticipants materially benefited from the infrastructure projects completed by the project committees, though our analyses of respondents' awareness, use, and perceived benefit of the projects do not suggest that material benefits from the projects drove changes in outcomes. As a result, 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 was 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. 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 that these interventions change groups (13).

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.[#] In another of the treatment sites, farmers defended pastoralists from a group of antipastoralist 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 knowledge, attitudes, behaviors, or all three. Some prior research finds changes in behaviors but not attitudes (10, 12), while other research finds changes in knowledge but not behaviors (11). One difference between these interventions is whether the peacebuilding elements of the program were explicit or implicit. Like Paler et al. (11), we test an explicit peacebuilding intervention. Whereas Paler et al. (11) only find changes in knowledge, we find some changes in attitudes (e.g., trust) and some changes in behaviors (e.g., self-reported and observational contact, but not in the public goods game). Unlike other contact-based interventions which ranged from a one-shot meeting (11) to 16 wk (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? For example, Lowe (40) found collaborative contact produced more cross-caste behaviors; however, adversarial contact produced the opposite.

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 determine whether the publicness of contact helps to successfully shift both.

Additionally, there are questions about the generalizability of this type of contact intervention to other contexts. Contact interventions are likely not suitable for all types of conflicts, particularly interstate conflicts where the underlying cause of conflict is a commitment problem (6, 41, 42). In contrast, we see these findings generalizing to other intercommunal conflicts characterized by competition over scarce resources, groups living side-by-side, and a history that has contributed to negative beliefs and attitudes about one another. These traits describe numerous conflicts across the Sahel and the Horn of Africa, sectarian and tribal conflicts in the Middle East and South Asia, and continued flare-ups in the Balkans. Climate change and population growth will likely make these types of conflicts more prevalent by increasing resource competition. Future research should consider the ways that conflict does or does not ameliorate different causes of conflict.

Last, 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

[#]We are especially grateful to Israel Okpe for this and other observations about farmer–pastoralist conflict dynamics.

interpersonal discussion diffuse the positive effects of contact to other ingroup members for whom intergroup contact did not increase.

While there remain these important theoretical and practical questions regarding how, why, and when contact reduces conflict between groups, these results illustrate that contact has the power to go beyond affecting those individuals directly involved in an intervention. Under certain circumstances, intergroup contact can shift the broader community toward the belief that cooperation between groups with a history of violence is possible.

Data, Materials, and Software Availability. Anonymized data are available through our GitHub repository (https://github.com/cdgrady21/ecpn_submission) (43).

ACKNOWLEDGMENTS. We are grateful to Jake Bowers, Jim Kuklinski, Justin Rhodes, Cara Wong, Caglayan Baser, Ekrem Baser, Nuole Chen, Alice Iannantuoni, Betsy Rajala, Quinn Waeiss, and Donald Beaudette for useful conversations and feedback on the many drafts of this paper. Nicholus 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. Last, 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. Portions of the paper were developed from the thesis of C.G. This research was funded by the United States Agency of International Development through a cooperative agreement to Mercy Corps, the implementer of the program. Additional research support was provided by the NSF under Grant No. 1656871 and the Carnegie Corporation of New York through the Peacebuilding Evaluation Consortium.

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