

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

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## Abstract

Cooperative intergroup contact, originally designed as a tool for prejudice reduction, offers a promising means to resolve group conflict. Evidence for contact-based interventions, however, is sparse, and violence may nullify the effects of 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 evaluate the program with surveys, direct observation of *good* behavior, and a behavioral game. We find that participation in the program increases intergroup contact, intergroup trust, and perceptions of physical security among group members; many of the program's effects also diffuse to group members who did not directly participate in the program but who lived alongside participants. 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.

*Very nicely done intro*

1971; Slavin and Cooper 1999) in the United States. More recent studies demonstrated the prejudice-reducing effects of contact by leveraging random assignment to college dorms (Marmaros and Sacerdote 2006), college roommates (Boisjoly et al. 2006; Burns, Corno, and La Ferrara 2015; Van Laar et al. 2005), schools (Rao 2019.), U.S. Air Force groups (Carrell, Hoekstra, and West 2015), mixed sports teams (Ditlmann and Samii 2016; Mousa 2018), job training programs (Scacco and Warren 2018), and medical doctors (Weiss 2019). The contact hypothesis also increasingly motivates policy interventions, especially peacebuilding programs (Ditlmann, Samii, and Zeitzoff 2017; Lemmer and Wagner 2015).

Despite contact's many successes, scholars know little about the effects of contact-based interventions for people actively participating in and personally victimized by a violent conflict, or even for interventions directed at improving adults' attitudes towards racial or ethnic groups (Paluck, Green, and Green 2019). Cooperative intergroup contact has only recently been tested in the field, and never programmatically with communities who are currently perpetrating violence against each other. If one of the goals of cooperative contact is to mitigate violent conflict, then contact-based interventions must be tested between participants and victims in violent conflict.

Ongoing violence poses the most difficult test for contact and could interfere with mechanisms through which contact improves relations. The mechanisms through which contact improves relations assume that negative attitudes result from unfamiliarity, and that "familiarity breeds[es] liking" (Pettigrew and Tropp 2006, 766). We posit that familiarity through cooperative contact allows groups to identify latent shared interests by providing positive interactions towards achieving a common goal. Those positive interactions

costs \$13 billion annually in lost economic productivity (Akinwotu 2018; Daniel 2018; Harwood 2019; McDougal et al. 2015). In our sample, some members of each community had been killed by members of the other community in the year before the project began. Ongoing violence, occupational and ethnic differences, and fighting over resources necessary for livelihoods all make this context a hard test for contact theory.

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; committees then collaboratively chose and constructed infrastructure projects.<sup>2</sup> 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 effects of the intervention, we conducted pre- and post-intervention surveys, a post-intervention natural public goods behavioral game,<sup>3</sup> and twelve months of systematic observations in markets and social events during the intervention. *nice*

*Would the cooperation have occurred in the absence of funds?*

We find that the program increased intergroup affect, intergroup contact outside of the intervention, and perceptions of physical security. We see signs of the positive effects in fieldwork as well as in data: in one of the treatment sites, farmers defended pastoralists  
*nice*

*What does mean*

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

<sup>3</sup> In 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.

attitudes worse, not better (Barlow et al. 2012; Paolini, Harwood, and Rubin 2010; Stark, Flache, and Veenstra 2013).

Even if contact succeeds in providing positive experiences with outgroup members, the resulting cognitive dissonance may not be resolved by embracing positive attitudes.

Participation in and victimization by violence motivates group members to justify their existing attitudes (Kunda 1990). Existing attitudes are harder to reject once an individual has acted on them (Festinger 1962; Tavris and Aronson 2008). Once an attitude is acted upon, rejection of the attitude threatens an individual's self-identity because the individual must come to terms with his or her own immoral behavior. Likewise, individuals are less likely to reject existing attitudes when they have personal experiences that reinforce those attitudes. In the case of prejudice, prejudiced attitudes are least likely to be rejected when an individual has harmed or been harmed by the outgroup. Instead of rejecting negative attitudes, violent experiences can lead individuals to resolve cognitive dissonance by justifying previous attitudes (Gubler 2013) or, at best, by differentiating "good" outgroup members from typical outgroup members (Doosje, Spears, and Koomen 1995).

Beyond past violent, ongoing group violence provides negative experiences with outgroup members that counter the positive experiences provided by cooperative contact. These negative experiences bolster the psychological barriers to groups' identifying their shared interests. Rather than dispelling stereotypes and alleviating feelings of threat, negative experiences reinforce negative stereotypes and justify feelings of threat. Taking the perspective of the other side will not improve cross-group relations if taking their

Want to be very careful about your use of terms on last page and here

So all of the above (last page) are mechanisms? Or are there the mechanisms by which the 4 conditions have a positive impact?

Allport argued that contact works by enhancing knowledge and overriding negative stereotypes about the outgroup, and subsequent scholarship has identified three additional mechanisms through which contact improves attitudes. First, contact reduces the feelings of threat and anxiety that arise from fear of the unknown (Page-Gould, Mendoza-Denton, and Tropp 2008; Stephan and Stephan 1985). Second, contact enables perspective-taking so that ingroup members empathize with the outgroup (Batson et al. 1997; Broockman and Kalla 2016). And third, contact makes salient a shared identity based on the groups' similarities and interests (Gaertner and Dovidio 2014; Gaertner et al. 1993). Through these mechanisms group members can experience positive cross-group interactions, which triggers cognitive dissonance against the preexisting negative attitudes. Attitudes improve when that dissonance is resolved by rejecting, rather than justifying, negative attitudes towards the outgroup (Gubler 2013).

These mechanisms support the reduction of group-based prejudice for individuals involved in the intergroup interaction, but the positive effects of contact must diffuse to individuals not involved in the interaction for intergroup contact to meaningfully improve intergroup relations. This diffusion to other group members occurs through changing social norms about cross-group interaction (Christ et al. 2014; Paluck 2009) and through the knowledge that other ingroup members had positive contact with outgroup members (Wright et al. 1997). Norms and awareness of cross-group cooperation shows that cross-group interaction is safe and socially encouraged. It also creates the expectation of future interaction with outgroup members, which motivates individuals to see the outgroup more positively (Klein and Kunda 1992; Van Dessel, Hughes, and De Houwer 2019). Through

Is this the bottom line?  
8

I think it might help if you diagrammed your discussion of the past few pages. I generally do not like diagrams; this might be an exception. Good for you to do for yourself.

perspective reveals incentives for belligerence (Kertzer, Brutger, and Quek 2018). And far from revealing common identities and interests, group violence perpetuates opposing group identities and interests (Fearon and Laitin 2000). To overcome preexisting negative beliefs, individuals need strong and consistent information that counters those existing beliefs – a signal that the object of their belief has changed (Nickerson 1998). For that reason, some scholars believe group reconciliation cannot begin until conflict is resolved (Bar-Tal 2000).

*how does this fit with the last paragraph on  
p. ??*

Social norms are a potent means to change attitudes and behavior, but in contexts of group violence social norms prevent rather than facilitate attitude change (Bar-Tal 2007; Bar-Tal and Avrahamzon 2017). These pre-existing norms self-perpetuate by discouraging ingroup members with positive attitudes from displaying those attitudes, either through talking about or engaging in cross-group interaction publicly. Group members who do not conform to these norms risk being branded as traitors (Bornstein 2003). With no opportunities to hear about or observe positive cross-group interaction, the effects of contact cannot extend to ingroup members without contact.

But these barriers do not mean that contact cannot improve intergroup relations for groups in violent conflict. Conflicting groups share an interest in obtaining peace because fighting is costly (Fearon 1995), and cooperative contact can make that shared interest salient. Though existing norms likely support negative attitudes, successful cross-group cooperation can generate cooperative social norms because cooperation and peace are in the interest of both groups. Cooperative contact also shows that the outgroup is composed

been rare. The Middle Belt came to be known as Nigeria's "food basket" due to the abundance of foodstuffs coming out of the region, like beef, dairy, yam, and cassava<sup>4</sup>.

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). As the number of farmers, pastoralists, and mouths to feed increased, the amount of land available to produce food declined. These factors also pushed pastoralists southward, towards farming communities with whom the pastoralists had no pre-existing 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). Sharing land is easier when people are scarce and land is plentiful; it is not so easy when land is scarce and people are plentiful.

Government policies exacerbated the issues caused by demographic and geographic changes. Land privatization encouraged farmers to plant crops that occupy land continuously, like orchards, and effectively nullified farmer-pastoralist land sharing agreements (Bassett 2009). Official cattle reserves for moving herds are rarely enforced by the government, leading farmers to plant crops in once-protected areas and further limiting pastoralists' available grazing space. The "indigene versus settler" policy limits

<sup>4</sup> <https://qz.com/africa/1315749/nigeria-herdsmen-farmer-attacks-are-damaging-agriculture-economy/>

the local conflicts. First, cattle graze on farmland.<sup>5</sup> 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." (McDonnel 2017).

*very!*

Farmer-pastoralist conflict poses a tough test for intergroup contact to improve group relations. The material, social, and psychological incentives of these groups are opposed. *good learning statement*

They want the same land for different purposes and their livelihoods depend on that land. The groups are involved in a bloody, violent, and escalating conflict for land in which thousands of farmers and thousands of pastoralists have been killed by members of the other group. Within an individual's community, several people will have been attacked or killed; several others will have attacked or killed members of the other side. To justify killing, groups create collective myths about the retaliatory/defensive nature of their belligerent action and the iniquity and inhumanity of the other side. Despite their physical proximity, the groups have little to bond over; they are distinct culturally, ethnically, linguistically, and often religiously. And finally, government favoritism of farmers over pastoralists creates a power disparity between the groups.

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<sup>5</sup> 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). With no agreed upon compensation and no authority to punish illegal grazing or illegal cattle rustling, groups take justice into their own hands.

## Intervention: Engaging Communities for Peace in Nigeria

To address farmer-pastoralist conflict, peacebuilding NGO Mercy Corps implemented a four-year, USAID-funded program titled Engaging Communities for Peace in Nigeria (ECPN) in Middle Belt sites 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. Mercy Corps implemented the project in two Middle Belt states, Benue and Nassarawa, which have been focal points for farmer-pastoralist conflict.

The intervention formed mixed-group 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. It started with separate farmer and pastoralist community meetings to avoid negative contact experiences. These intra-community meetings eventually built up to joint decision-making meetings with the two groups together. Each joint project committee included an even number of farmers and pastoralists, as well as women and youth representatives, and totaled between 12 and 15 members. Each committee received two grants, one for quick-impact projects, of approximately \$2,000, and one for joint projects, of approximately \$25,000.

how can this be  
even # of two groups?

The quick-impact projects were conceived as a trust-building initiative, intended to let community members see that cooperation was possible. Projects, managed by both farmers and pastoralists, included hand pumps; construction or rehabilitation of market stalls, schools, and health centers; and construction of fences along grazing routes to protect

pastoralists. The communities within a site engaged in deadly clashes within one year of our scoping exercise.<sup>6</sup> We identified fifteen sites eligible for the study and surveyed ~50 randomly selected respondents per community. We then randomly selected the communities in ten of fifteen sites to receive the ECPN program, blocking by state so that an equal proportion of sites in Benue and Nassarawa received the program. After 18 months, we surveyed another ~50 randomly selected respondents and ~10 respondents from the baseline survey per community. In between the surveys, we monitored farmer-pastoralist interactions in markets and at social events.<sup>7</sup>

This designs gives us two datasets to analyze. First, we aggregate the randomly-sampled individuals to compare communities before and after ECPN. The main goal of this analysis is to learn about the effect of implementing ECPN in a community. Communities were randomly assigned to receive ECPN or function as a control group, which allows us to determine the causal effect of ECPN at a community-level. This comparison between communities that received or did not receive ECPN is our main analysis.

Second, we supplement the community-level analysis by creating a dataset of ~10 respondents per community before and after ECPN. The main goal of this analysis is to

*can you speak to (speculate a bit)  
communities where leaders did not agree*

<sup>6</sup> To identify eligible sites, we undertook a scoping exercise to determine if the two communities in an implementation site had a demonstrated need for a peacebuilding program and were willing to participate in one. We defined “demonstrated need” as the communities engaging in violent clashes within one year of the scoping exercise.

Willingness to participate in the program was obtained through conversations with community leaders, none of whom refused the program.

<sup>7</sup> This experimental design was pre-registered with Evidence in Governance and Politics (EGAP) under ID 20150716AA. The preregistration can be found at <http://egap.org/registration/1242>.

## Estimation

Here we describe our estimation procedure for the community-level analysis and the individual-level analysis. For both analyses we estimate one-tailed tests because 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.<sup>8009</sup>

Both analyses also use randomization inference for p-values and bootstrapping for standard errors. The specifics of each procedure are described in Appendix A.

We use two estimators to estimate the treatment effect of ECPN. 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. 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.<sup>9</sup>

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

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.  $\delta$  is a fixed effect for the state  $j$  in which the community belongs.

$$Y_{i,j} = \beta_0 + \beta_1 Z_{i,j} + \delta_j + \epsilon_{i,j}$$

<sup>9</sup> We 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>.

*OK-  
that's  
my an-  
swere  
quest*

contact and willingness to engage in intergroup contact, and reduced insecurity due to violence. We also measured three mechanisms from the contact literature through which contact could affect outcomes: (1) empathy/perspective-taking, (2) perceived threat, and (3) ingroup expansion. Lastly, we measured a placebo outcome that may be affected by social desirability: attitudes about violence. We measured these outcomes with survey self-reports, survey experiments, a natural-field behavioral game, and monitoring of farmer-pastoralist interaction in markets and social events.

*90%*

For most survey self-reports, we combine together several survey questions to create an index. We create both additive indices and inverse-covariance weighted indices. Inverse-covariance weighting constructs an index by down-weighting index questions that are correlated with other index questions and up-weighting those that are uncorrelated with other questions. This approach maximizes the amount of unique information the index takes from each question and prevents "double counting" when two questions measure the same underlying concept. We report results using inverse-covariance weighted indices, but results hold with additive indices. Results with additive indices are included in Appendix A.

## Primary outcomes

**Intergroup affect:** Our first outcome is affect towards the other side. A primary goal of our contact intervention, and of much previous contact research, was for individual's attitudes to improve. Changing attitudes towards the other side is one pathway towards improving intergroup relations and changing behavior, though not the only pathway (Paluck 2009; Scacco and Warren 2018).

with the other group in the past month. The respondents are asked about interaction in markets, at public social events, in the respondent's own home, at the home of a member of the other group, and in any other way. The responses are then ranked, scaled from 0-1, and combined into an index. The behavioral observations provide a measure of contact independent of response biases.

*do you have results for the individual measures?*

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 create a farmers index and a pastoralist 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 anything<sup>12</sup>, both in absolute numbers and as a percentage of total attendees. We then create measures for the number of farmers and pastoralists attending social events and the number of farmers and pastoralists eating at social events.<sup>13</sup>

A survey experiment, which we are calling the percent experiment, tells us about respondents' willingness to engage in contact. It asks respondents two questions about their willingness to interact with members of the other side. We asked respondents if they would (1) join a group and (2) live in a community with some percentage of the other

<sup>12</sup> Taking 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

<sup>13</sup> 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.

We combined these ten insecurity questions into an index, with high values indicating low perceptions of insecurity and low values indicating high perceptions of insecurity.

*900* | **Violence Placebo:** Several of our outcomes are survey self-reports, and all self-reports could be affected by social desirability bias. Our survey results are suspect if respondents in treatment communities learned the "correct" answers better than respondents in control communities. If social desirability accounts for the effect in survey self-reports, we would also expect differences between treatment and control for other normatively desirable attitudes. To test social desirability effects, we conduct a placebo analysis (using attitudes about violence as a placebo). Attitudes about violence are a good candidate for a placebo because intergroup contact should not affect general attitudes about violence, but respondents may feel social pressure to answer violence questions in a desirable way. We measure attitudes about violence with a six question index asking respondents if it is always, sometimes, rarely, or never justified to use violence in certain situations, such as retaliating against violence or bringing criminals to justice.

### Mechanisms *900*

*Ordering of your questions  
in the survey?*

The primary outcomes of intergroup affect, intergroup contact, and insecurity tell us if ECPN worked but provide no evidence for how the program worked. Previous work on contact specified three mechanisms through which contact affects attitudes: empathy/perspective-taking, threat/anxiety, and ingroup expansion (Al Ramiah and Hewstone 2013; Dovidio et al. 2017; Pettigrew and Tropp 2008). We do not manipulate these mechanisms directly, and so cannot make causal claims about the mediating role of these variables for ECPN. But we can provide exploratory evidence that these mechanisms

the groups working together on specific goals, such as repairing a road or solving a water supply problem.

*not totally close to me*

*incentive?*  
*willingness?*

We also used a natural-field public goods game to measure the ability of the groups to cooperate to achieve a common goal. If ECPN causes respondents to incorporate the former outgroup into their ingroup, then we expect those communities to better cooperate in a public goods game. Compared with lab-based behavioral games, whose choice-making situations are necessarily artificial, the choice-making situation of a natural-field game is akin to the choices people make in their lives (Harrison and List 2004); Winking and Mizer (2013)]. Because these communities often decide how to contribute to some public good, such as repairing a borehole or a market, we chose to use a natural-field public goods game (PGG) as a realistic behavioral measure of cooperation.<sup>17</sup>

These designs and measurements put us in a strong position to identify effects if effects exist. First, we have data at the community-level and individual-level. If the two analyses show similar relationships, we can be more sure that those relationships are not spurious. Second, both community and individual-level analyses use a baseline/endline + control group design to differentiate a secular trend from a treatment effect. Many changes occurred in the social environment between the beginning and the end of ECPN that could change intergroup relations, such as the economic downturn in Nigeria and the anti-grazing law in Benue. By comparing the change in the treatment group to the change in the control, we are more certain that differences are due to ECPN and not other factors. Third,

<sup>17</sup> This game is similar to the one implemented by Fearon, Humphreys, and Weinstein (2009) as part of a similar study on community-driven development in Liberia.

## Intergroup Affect

tables, figures?

ECPN bolstered intergroup affect in treatment communities. Compared to control communities, respondents in treatment communities report more trust in the other group and are more comfortable engaging in various relationships with the outgroup, such as trading goods and intermarriage. Intergroup affect as measured by the endorsement experiment also improves more in the treatment group than the control group, though the difference is not statistically significant at conventional levels. *Wow*

Figures 3 and 4 show the descriptive change in affect for treatment and control communities. Affect in control communities decreased from baseline to endline, while intervention communities improved over the same time period. As measured by the endorsement experiment, affect declines in both treatment and control communities, but declines more in control communities. Both measures suggest that ECPN improved affect towards the outgroup.

## Contact

need to be more clear  
specific - and  
convincing?

The effect of ECPN on contact is substantial. Respondents in treatment communities report more contact and more willingness to engage in contact at all levels of the percent experiment; we also observe more pastoralists in markets interacting with farmers. Since the markets are all located in the farming community, the sustained presence of pastoralists there suggests that (1) farmers were accepting/tolerant of pastoralists in their community and (2) pastoralists felt comfortable spending time in the farmer community.

community, not just respondents involved in ECPN committees. These improvements in treatment communities are especially powerful because other survey questions show that ECPN increased awareness of the conflict – respondents in ECPN communities are more likely than the control to know that violence between groups has occurred recently, yet they feel more secure.

Figures 7 and 8 show the descriptive change in insecurity for treatment and control communities. The insecurity of control communities declines slightly from baseline to endline but insecurity in treatment communities declines substantially more. ECPN communities initially felt more insecure than control communities but were more secure at the end of the program. ECPN substantially improved the security of people in intervention communities.

## Placebo: attitudes about violence

To provide evidence that these survey results are due to intergroup contact and not due to social desirability bias, we analyze the effect of ECPN on attitudes about violence. If ECPN affects attitudes about violence, then we worry that other self-reports were affected by social desirability bias. If ECPN has no effect on attitudes about violence, then it is unlikely that other self-reports were affected by social desirability bias.

ECPN has no effect on attitudes about violence in the community-level data or the individual-level data. The lack of an effect on this placebo outcome, plus our use of survey experiments and behavioral observation to corroborate survey self-reports, suggests that

the treatment group was not better at coordinating than the control group. Treatment communities donated less to the shared community fund than control communities. At the individual-level, ECPN participants donated less than nonparticipants who donated less than respondents in the control group. This is the opposite pattern of what we would expect if intergroup contact caused the communities to think of each other as part of one ingroup. Reduced threat and ingroup expansion are still plausible psychological mechanisms – each correlated strongly with at least one outcome – though neither was increased by ECPN.

More details about the mechanisms analyses can be found in Appendix B.

## Discussion

This paper provides evidence that intergroup contact can improve intergroup relations, even in dire circumstances. We tested the effects of a programmatic contact intervention in an active and escalating conflict between farmers and pastoralists in Nigeria. The extreme violence of this context and personal involvement of the research subjects poses a tough test for contact to improve intergroup relations. The violence provides grievances that feed outgroup animosity and reinforce group differences, strengthen social and psychological barriers to improving attitudes, and reinforces the perception that groups' material incentives are opposed. Despite the difficult context, the program improved intergroup affect, fostered more intergroup contact, and decreased feelings of insecurity in these communities. Methodologically, this study demonstrates the benefits of measuring

that researchers at Armed Conflict Location & Event Data Project called it “ethnic cleansing” (Economist 2019). Understanding how to prevent violent conflict between Fulani and settled peoples can help prevent violence that targets other nomadic, semi-nomadic, and itinerant peoples, such as the Tuaregs in West Africa, Kochi in Afghanistan, Khoisan of Southern Africa, and Romani of Europe. Preventing such violence could help preserve a dying way of life.

There remain several opportunities to learn about the effects of contact in conflict environments. First, this study employed a design to test the hypothesis that contact would improve group relations in an active conflict. It also provided exploratory evidence of the mechanisms through which contact affects group relations, showing that contact may have worked through increased empathy. Future studies can bring more causal evidence to the question of *how* contact improves group relations. Second, our program was designed, implemented, and randomized at the community-level because conflict between farmers and pastoralists occurs at the community level. Future studies should randomize individual community members’ participation in a contact-based intervention. Such studies could learn much about the affect of contact on individuals, including the dosage of contact necessary to improve attitudes, as well as how social norms and interpersonal discussion diffuse the positive effects of contact to individuals without outgroup contact.

Third, contact interventions, explicitly or implicitly, involve the groups cooperating to *achieve* a joint goal. ECPN 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

can you elaborate a bit  
more on the inconsistencies  
in your study?

begin to associate cross-group cooperation with good outcomes? In a similar vein, are Allport's conditions necessary for contact to achieve its aims, or are they only needed insofar as they ensure the intergroup cooperation generates positive outcomes for both groups? Future studies should determine the necessity of Allport's conditions and attempt to differentiate the fact of contact from the outcomes that group cooperation produces.

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