Nigeria—Corinna Robbins

DOES PEACEBUILDING WORK IN THE MIDST OF CONFLICT?

Impact Evaluation of a Peacebuilding Program in Nigeria

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MERCY CORPS

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FROM THE AMERICAN PEOPLE

Executive summary

Persistent violence between farmers and pastoralists in Nigeria has contributed to more than 7,000 deaths in the past five years and costs the Nigerian economy $13 billion a year. Communities in the Middle Belt that once cooperated over natural resources are competing for increasingly scarce land and water as climate change intensifies, sparking migration further south in search of available resources. Underdevelopment and poor governance further contribute to a breakdown in traditional agreements, and farmer and pastoralist communities are fast becoming polarized as clashes take on religious and ethnic overtones.

In response, Mercy Corps and our local partner, Pastoral Resolve (PARE), implemented the USAID-funded Engaging Communities for Peace in Nigeria (ECPN), from 2015 to 2019 in the Middle Belt states of Benue and Nasarawa. The program sought to prevent violent conflict between farmer and pastoralist communities through three main interventions: (1) strengthening the capacity of local leaders to resolve disputes inclusively and sustainably, including training and coaching them in interest-based negotiation and mediation; (2) building trust by facilitating opportunities for people to collaborate across conflict lines on quick-impact projects and natural resource management initiatives that addressed shared needs; and (3) fostering engagement among community leaders and local authorities to prevent conflict through joint violence prevention planning as well as information sharing around conflict triggers and violent incidents.

Because of a demand among policymakers and practitioners for more evidence on the impact of peacebuilding investments, we conducted a randomized impact evaluation of the first phase of the program, which lasted approximately two years. With this evaluation, we test a central tenet of ECPN and many other peacebuilding programs: do mediation and contact over shared interests change attitudes and increase cooperation among conflicting groups?

Research Design

We hypothesized that, compared with those not participating in the program, communities and individuals involved with the ECPN program would experience improved intergroup attitudes, including trust and intergroup cohesion; increased perceptions of security; and increased peaceful behaviors, including higher levels of intergroup interaction, dispute resolution success, and cooperation. To test the program's effects on these outcomes, we used a randomized controlled trial (RCT) at the community level to examine the overall impact of the program on communities. We triangulated the results of the community-level RCT with a pre-/ post-program analysis of individuals within communities to see how outcomes among direct participants— those most engaged in program activities—differed from outcomes among indirect participants—those living in intervention communities who were merely exposed to program activities. We also compared direct and indirect participants with those in control communities with no exposure to the program at all. We measured attitudes, perceptions and behaviors, and used a combination of surveys, behavioral games and observational monitoring tools to assess these various outcomes.

Key Findings

Overall, findings indicate that ECPN improved the conditions for peace in the communities in which it operated and among the individuals who participated most in activities that brought farmers and pastoralists together regularly. On most measures, ECPN communities either improved or stayed the same, while control communities stayed the same or deteriorated. That intervention sites and ECPN participants' attitudes improved or stayed steady is especially noteworthy, given heightened tensions and a regionwide uptick in violence during the final round of data collection, due at least in part to the implementation of a new Benue state law prohibiting open grazing.

Intergroup contact and trust between farmer and pastoralist communities increased or deteriorated significantly less in ECPN sites than in control sites, even as regional tensions increased. Contact between farmers and pastoralists in the control sites decreased by approximately 15 percent, while contact in ECPN sites stayed the same. Further, though trust between farmers and pastoralists in control sites decreased, trust within ECPN sites increased, leading to a difference of 13 percentage points in the overall level of trust between intervention and control sites.

Perceptions of security increased significantly more in ECPN communities than in control communities. By the end of the first phase of the program, perceptions of security in ECPN communities had improved by 15 percentage points more than in control sites. The increase in perceptions of security across all sites—both intervention and control—was surprising, given rising violence in the region. However, the added presence of security forces, including the military, in all communities due to recent events, and dialogues hosted by ECPN that incorporated control communities due to the immediacy of the situation, may explain this overall trend.

Among individuals, as a result of the program, direct participants' attitudes and behaviors improved more than those of indirect participants in ECPN communities, who in turn improved more than individuals in control communities. One criticism of peacebuilding programs is that they often only work with the "converted," those who want to participate, and do not affect the wider community. We found that the benefits to those who participated did spread to the wider community. These trends were strongest for intergroup trust and perceived security.

Perceptions of the effectiveness of dispute resolution mechanisms did not improve in ECPN communities. While direct participants' perceptions of dispute resolution slightly improved compared with those of indirect and control individuals, ECPN communities' perceptions related to dispute resolution decreased slightly more than the perceptions of control sites. One potential explanation for the difference in trends between the individual-level and the community-level results is that direct participants may have been more knowledgeable about the mediators' activities due to their level of engagement in the program. Those randomly sampled in the community may not have known about the more than 500 disputes resolved over the study period unless they had a dispute themselves or knew someone who used the mediators. However, these resolved disputes may have indirectly affected people's perceptions of security, attitudes, and behaviors, because fewer disputes erupted into violence. Or it is possible that the pathway to reaching the outcomes above did not go through dispute resolution. How dispute resolution does or does not affect peacebuilding outcomes requires further examination.

Recommendations

1 **Increase investments in programs that facilitate positive contact between groups in active conflict.** This study demonstrates that contact theory–based peacebuilding programs can support communities to maintain or improve relationships despite a broader escalation of violence. Donors should increase their investments in these relatively low-cost interventions, in this case, less than $60 per direct participant, to build communities' resilience to being drawn into violence during periods of intense conflict.

2 **Pair community-level interventions with robust advocacy campaigns to promote policies conducive to peace.** ECPN either improved peace outcomes despite the policy and conflict environment, or at least kept communities from being pulled into the broader conflict. Because donor-funded program impacts can go only so far if government policies sow divisions, peacebuilding investments should incorporate strategies for strengthening policies that will facilitate sustainable peace.

3 **Design interventions to maximize the ripple effect from direct participants to the broader community.** This study showed that people-to-people activities that facilitate close cooperation between members of communities in conflict had a positive effect beyond the specific individuals engaged. Unfortunately, the mediation component had less of a ripple effect. These results make it clear that program interventions' logic should clearly articulate the intended ripple effect and that program activities should be designed to accentuate this effect, such as publicizing successes due to cooperation or mediation through community forums or messaging.

4 **Invest in larger-scale, rigorous impact evaluations of peacebuilding programming.** Absent a rigorous impact evaluation with a comparison group, the program would have appeared to have little impact on peace outcomes in light of the overall deteriorating security conditions. When possible, more rigorous impact evaluations of peacebuilding programs are needed in order to (1) increase learning among practitioners and donors about how to implement peacebuilding programs effectively and which approaches present the best return on investment, and (2) support increased evidence-based investments in peacebuilding interventions.

Abbreviations

ADR	Alternative Dispute Resolution
CONCUR	Conciliation in Nigeria through Community-Based Conflict Management and Cooperative Use of Resources
CDD	Community-Driven Development
ECPN	Engaging Communities for Peace in Nigeria
FDR	False Discovery Rate
FWER	Family Wise Error Rate
ODA	Overseas Development Assistance
PARE	Pastoral Resolve
PGG	Public Goods Game
RCT	Randomized Controlled Trial

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Introduction

While most attention on conflict in Nigeria focuses on the northeast, where Boko Haram has ravaged people's lives, persistent violence between farmers and pastoralists in Nigeria's Middle Belt has contributed to more casualties (Fulton and Nickels 2017) and costs the Nigerian economy $13 billion a year (McDougal et al. 2015). Clashes between farmer and pastoralist communities in Nigeria's Middle Belt states are becoming increasingly violent and taking on religious and ethnic overtones that divide these communities even further. Due to the effects of climate change, underdevelopment, and a population boom, communities that traditionally cooperated over land and natural resources are finding that (1) fewer resources are available and (2) more people are needing to use what resources are available. Consequently, farmer and pastoralist communities are fast becoming polarized from each other. The government exacerbates this polarization by instituting policies that negatively affect pastoralists. Recently, clashes between farmers and pastoralists have become more intense, especially after an antigrazing law was passed in Benue state in late 2017. As a result of farmer-pastoralist clashes in the Middle Belt, 1,205 people died in 2014; in 2017, the number went down to 314 deaths, but it then spiked again in 2018, with 1,476 deaths (Harwood 2019).

To address this situation, Mercy Corps, in cooperation with local partner Pastoral Resolve (PARE), implemented the first two years of its USAID-funded Engaging Communities for Peace in Nigeria (ECPN) project in two states within the Middle Belt of Nigeria, Benue and Nasarawa.[1] ECPN aims to prevent violence and conflict between farmer and pastoralist communities. More specifically, the program was designed to (1) strengthen the capacity of farmer and pastoralist leaders to resolve disputes in an inclusive, sustainable manner; (2) build trust among farmers and pastoralists by creating opportunities for contact and collaborations to achieve common goals; and (3) foster engagement among farmer and pastoralist communities, local authorities, and neighboring communities to prevent conflict.

Because many peacebuilding programs utilize similar activities, we conducted a rigorous evaluation to answer a question that is central to the peacebuilding field: do mediation and contact over shared interests change attitudes and increase cooperation among conflicting groups? To answer this question, we conducted a two-level impact evaluation: (1) a randomized controlled trial (RCT) at the community level to understand how the overall intervention affected the communities and (2) individual-level analyses to understand how one aspect of the program—joint project committees that foster contact—affected individuals' attitudes and behaviors toward people from other groups.

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1   Mercy Corps received a cost extension and continued these activities in additional locations. This evaluation informs the extended program but does not include data from it, except to the extent that many of the control communities became intervention communities as part of the cost extension. These communities, however, were not informed that they would be part of the program until after the endline survey was completed.

Ezra Millstein—Nigeria

Policy and Theoretical Motivations

Overall, a small proportion of development funds are focused on peacebuilding. In 2016, for example, only 2 percent of overseas development assistance (ODA) funds were spent in fragile contexts, and $1.7 billion out of $31 billion was directed toward conflict, peace, and security programming (OECD 2018). Though a small proportion of overall ODA spending, $1.7 billion is still a considerable amount of money, making it an important policy priority to use these funds as effectively as possible.

Recent reviews of peacebuilding interventions show limited results, mostly because few robust evaluations have been conducted. A systematic review of peacebuilding programs across various domains (e.g., transitional justice, land reform, dispute resolution) noted how few impact evaluations there had been. For certain areas, such as natural resource management, the systematic review found no evaluations (3ie 2015). Another review looked specifically at the USAID-funded People-to-People interventions,[2] often referred to as *contact interventions* (Social Impact 2014). The People-to-People program has funded interventions across numerous conflict contexts, including Burundi, Somalia, Kenya, Uganda, Nepal, Myanmar, Yemen, and Nigeria. The review concluded that too few People-to-People programs used rigorous enough evaluations to be able to draw conclusions on the effectiveness of the interventions.

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2   People-to-People is an authorization by the US Congress that commits $25 million per year to these types of programs.

Many of the People-to-People interventions reviewed included a mix of various activities to bring conflicting groups together—sports and cultural events, joint trainings (e.g., vocational training, civic engagement training), producing stories for media, and community service. The underlying theory of most of these interventions is the contact hypothesis (Allport 1954), which we describe below. These interventions also often include two other, likely overlapping, components. The first is based on the idea of superordinate goals (Sherif 1958) that is the basis of many community-driven development (CDD) programs. The second is mediation, a tool for helping people resolve conflicts more peacefully. The Engaging Communities for Peace in Nigeria (ECPN) intervention, which was funded through the USAID People-to-People program, includes all three of these elements—contact, superordinate goals, and mediation. Next, we review the relevant literature to clarify the assumed links between these factors and improved peace and security.

Contact Hypothesis

An underlying theory for many, if not most, peacebuilding programs is the contact hypothesis, which states that intergroup attitudes will improve and prejudice will decrease if people get to know one another under defined conditions. Intergroup contact dismantles the negative stereotypes and misperceptions that drive prejudice, replacing them with positive experiences and mutual understanding. The contact hypothesis inspires many social interventions—from integrated classrooms to public housing. While the original theory specified a number of conditions that had to be met for contact to be effective (i.e., equal status; common goals; intergroup cooperation; support of law, authorities, or customs; and personal interaction), few studies have included all of these conditions or tested which ones are necessary. In the current study, we meet three of these conditions: equal status, intergroup cooperation, and personal interaction.

Pettigrew and Tropp (2006) conducted a meta-analysis of 515 studies and found that, overall, intergroup contact reduces prejudice. The meta-analysis encompassed field studies, surveys, experiments, and quasi-experiments, and included racial and ethnic groups as well as other types of groups (e.g., people with disabilities). Paluck and Green (2009) also reviewed studies of the contact hypothesis and other theories about prejudice-reduction in both the lab and the field. They found that the strongest results involved intense living arrangements at a summer camp or in a dorm; most other interventions based on the contact hypothesis involved much less actual contact between people, and the results of those interventions tended to be limited. However, few of the studies in the either review were field-based RCTs, so it is difficult to identify whether contact caused the reduction in prejudice, or whether the people who self-selected into situations involving intergroup contact were already more open to changing their views. A more recent review by Paluck, Green, and Green (2018) specifically examined field RCTs to see if contact that was experimentally manipulated in naturally occurring environments changed attitudes. They found an overall significant effect of contact, but larger studies and studies that focused on racial and ethnic prejudice or on groups with a history of violent conflict had weaker effects than smaller studies and those involving groups without strong intergroup animosities (e.g., persons with disabilities).

One exception was a study in Kaduna city, an urban environment in Nigeria with a relatively recent history of riots between Christians and Muslims (Scacco and Warren 2018). As part of the study, youth participated in a computer training program. The participants were divided into either heterogeneous (i.e., mixed-religion) or homogeneous (i.e., same-religion) classrooms, and within the heterogeneous classrooms, youth were either in homogeneous pairs (Christian-Christian, Muslim-Muslim) or heterogeneous pairs (Christian-Muslim). The main finding of the study was that contact did not change attitudes but did improve cooperative behavior as measured by behavioral games. However, this effect appeared to be driven less by increased cooperation with

the other group, as the contact hypothesis would predict, than by decreased in-group favoritism—that is, those in the mixed classrooms did not favor people like them as much as people in the same-religion classrooms.

A relevant feature of the Scacco and Warren (2018) study's design was that it was not explicitly a peacebuilding intervention; participants became involved to receive computer training. It is unclear how an intervention that was explicitly about changing attitudes and behavior through contact would fare, since few field-based RCTs using the contact hypothesis have been conducted with people in the midst of *active* violent conflict. Additionally, though many peacebuilding interventions are based on the hypothesis, as shown above, there is little understanding of how contact affects people amid active violence.[3] In violent contexts, contact may have the intended effect of improving attitudes. Alternatively, however, some research suggests that contact in violent contexts may instead backfire, increasing competition, negativity, and feelings of exclusion and competition (Amir 1976; Paolini, Harwood, and Rubin 2010; Gubler 2011; Enos 2014). Or contact could also cause mixed effects, depending on a group's status. For example, the high-status group often benefits by learning about the low-status group, but the low-status group, which already knows quite a bit about the high-status group, often feels that attitude change without systemic change is worth little (Barnhardt 2009; Ditlmann and Samii 2016). Because contact theory is the basis of numerous interventions in conflict environments, it is critical to know how these interventions work—how they help or potentially hinder improvement of relations—and how practitioners can improve the design of these interventions to address these strengths and weaknesses. The current study addresses this gap by examining whether contact between farmers and pastoralists, via their involvement on various project committees, changes attitudes and behaviors.

Superordinate Goals and Community-Driven Development

Another feature of many peacebuilding programs is that groups in conflict work together to achieve a common goal, which the social psychology literature refers to as a *superordinate goal* (Sherif 1958). The belief is that this form of cooperation will build social cohesion, making people less likely to engage in future conflict because they see the benefits of working together. This theory is also implicitly behind many CDD programs, in which community leaders come together to work on a project that will benefit the whole community. However, a recent systematic review found no evidence that CDD programs contribute to social cohesion (White, Menon, and Waddington 2018) as well as other social outcomes, though they do have impact on economic outcomes (King 2013; Casey 2018). While not focusing on social cohesion per se, Fearon, Humphreys, and Weinstein (2009) did find that a CDD intervention in Liberia improved cooperation among community members who were in mixed-gender groups, as compared with all-women groups.

One potential reason that CDD programs may have little impact on social outcomes at the community level is that these programs have a small group of people make decisions for the whole community, rather than getting the wider community involved in the decision-making process. The hope is that other members of the community will change their attitudes by observing or benefiting from the cooperation that occurs within that small group of people. However, it is unclear whether those ripple effects do occur and, if so, to what extent. Additionally, while many of the CDD programs reviewed took place in conflict or post-conflict contexts, none of them were explicitly designed to bring groups together across conflict lines to build peace.

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3   An exception is in the Israeli-Palestinian conflict, where numerous contact studies have been conducted. However, only one, to our knowledge, was a randomized experiment (Ditlmann and Samii 2016).

Therefore, more intentionality about increasing social cohesion between specific groups may, in fact, allow CDD projects to increase social cohesion between those groups (Bennett and D'Onofrio, 2015).

This study addresses the research gaps on CDD, social cohesion and ripple effects by (1) intentionally bringing together groups with a history of conflict (i.e., farmers and pastoralists) to make a joint decision about a project to benefit both communities and (2) examining differences in impacts based on varying levels of exposure to the program—specifically, between people on the various project committees (direct participants), people in the intervention communities but not on the committees (indirect participants), and people in control communities (control). This design allowed us to see if there is a ripple effect (spillover) between people on the project committees and the wider community.

Mediation and Dispute Resolution

Another common component of peacebuilding programs is a form of alternative dispute resolution (ADR), particularly in areas where formal institutions, such as courts, are weak. The most common form of this are mediation programs. The theory is that where ADR is present, attitudes between groups will at least not get worse if conflicts do not escalate into violence. Additionally, community members may change their perceptions of social norms, attitudes, or both if they see groups with a history of conflict peacefully resolving disputes. Yet while much has been written on the theoretical value of these approaches (Bercovitch 1996; Menkel-Meadow 2001; Moffit and Bordone 2005) and numerous qualitative studies and case studies published (Bercovitch and Kadayifci-Orellana 2009; Kolb 2001; Lieberman and Henry 1986), few experiments exist that look at the effectiveness of ADR or mediation programs. An exception is in Liberia, where communities that had trained mediators were found to have a lower incidence of violence, higher resolution of land disputes, and stronger norms related to non-violence one year after the program (Blattman, Hartman, and Blair 2014). Three years post-program the incidence of violent disputes remained low, and norms related to non-violent dispute resolution persisted, though weakened to some degree (Hartman, Blair, and Blattman 2018). Although Liberia had previously suffered from a devastating civil war, the intervention studied did not intentionally try to bridge ethnic cleavages and therefore did not look at how the mediation program reshaped attitudes between groups. Our current study in Nigeria addresses this gap.

Attitudes versus Behaviors

While it is often assumed that if attitudes change, behavior will also change, numerous studies have shown that this often is not the case (Ajzen and Fishbein 2005). For one, there are often measurement issues, whether because of self-reporting biases, in which people report more positive attitudes than they actually feel, or because attitudes are often more abstract and diffuse than behaviors (Ajzen and Fishbein 2005). Second, social norms may compel people to behave in a certain way despite how they actually feel (Paluck 2009; Tankard and Paluck 2016). Third, often people infer their attitudes from their behaviors, rather than vice versa (Bem 1972; Olson and Stone 2005). Consequently, whether contact theory–based interventions change attitudes first and then behavior, or whether the situation of encouraging people to interact (i.e., their behavior) changes their attitudes is an open question. In previous studies, sometimes contact changed behavior but not attitudes (Scacco and Warren 2018) and sometimes the opposite (Paler, Marshall, and Atallah 2018). Therefore we examined both attitudes and behaviors in this study.

Context and Program

Nigeria's Middle Belt divides the country between north and south, and houses a blend of various ethnic groups, with no clear majority. The south comprises primarily Christian farmers from various ethnic groups, while Muslims from the Hausa and Fulani ethnic groups—including the mostly Fulani pastoralists—dominate the north of the country. These religious, ethnic, and occupational identities intersect and create deep fault lines between communities. Historically, these communities interacted through trade and access to land. However, in recent years, a number of interconnected factors have made these interactions more contentious: (1) climate change, (2) government policies that are perceived to favor some groups over others, and (3) the breakdown of traditional agreements.

Climate change has affected relationships between farmers and pastoralists in two ways. The first is that there is less arable land as well as less land and water conducive to grazing. Therefore farmers and pastoralists need to make do with fewer resources to share. Second, climate change is pushing pastoralists to migrate further south, increasing the number of pastoralists using already scarce land and water resources in the Middle Belt (Unah 2018) and leading to an increase in tensions between the groups.

Grievances related to access to and use of land and water points are compounded by the "indigene versus settler" policy, which limits land ownership and other rights, including political representation, to certain ethnic groups in each state (Nigeria Research Network 2014). Certain communities—often, though not always, pastoralists, who are seen as "settlers"—are denied the right to run for public office, limiting the incorporation of their views into local policies. Official cattle routes and reserves for moving herds are rarely enforced by the government, leading farmers to plant crops in once-protected areas, which further limits pastoralists' available grazing space. Compounding matters, the government of Benue State enacted an anti–open grazing law in November 2017, which many pastoralists viewed as biased against them, sparking more violence in the area.

Exacerbating these tensions is that many of the increased numbers of pastoralists migrating further south are unaware of traditional agreements that have managed tensions for decades. Once-respected traditional agreements over seasonal land sharing and compensation for damage to crops by livestock are less likely to be implemented by pastoralists not involved in forging these agreements. Their use of land beyond what "local" pastoralists have agreed upon has ignited new tensions (ICG 2017). The government and security forces generally have not gotten involved in resolving these local disputes, even when there are outbreaks of violence. Additionally, in many cases, community members have viewed security forces as biased along ethnic or political lines. Lack of an institutional response has intensified grievances, leading communities to manage justice themselves, including exercising vigilante justice. While security responses have increased since the recent uptick in violence, there are some questions related to their legitimacy: the arrests of those involved in the violence receive quite a bit of media attention, but later those suspects are quietly released (Campbell 2018).

This persistent violence continues to have debilitating effects on Nigerians and the economy. First, it has taken many lives. In 2013 alone, Plateau, Kaduna, Nasarawa, and Benue states registered more than 100 incidents of violent conflict, accounting for more than 1,050 deaths (Mercy Corps 2018).[4] The more recent violence left 300,000 displaced (Akinwotu 2018) and more than 1,476 dead in 2018 (Harwood 2019). The Middle Belt is considered Nigeria's "food basket" and is central to key value chains throughout the country, including beef, dairy, and cash crops such as cassava. This violence has impeded food production and threatens to create a food shortage (Hailemariam 2018). Before the latest surge in violence, the conflict was costing the Nigerian economy $13.7 billion a year (McDougal et al. 2015).

To address these issues, in 2015, Mercy Corps began implementing a four-year, USAID-funded program titled Engaging Communities for Peace in Nigeria, initially targeted at over 9,000 people at 10 Middle Belt sites. The main objective of the first phase of the program was to foster positive contact between farmers and pastoralists, with the aim of improving their attitudes toward and relationships with each other. The program included three main interventions. The first intervention was training community leaders in how to mediate disputes so that conflict did not escalate into violence. To help alleviate violence, two joint committees were formed: peace[5] and early warning / early response. Over the course of the two years, 120 people were trained in mediation, and they went on to resolve 528 disputes around local grazing routes, seasonal access to water points, crop damage, cutting down of trees, and water pollution by animals.

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4   This is likely a gross underrepresentation of the actual number, given that over 10 percent of these conflicts were reported not through media outlets but rather firsthand by community members with whom we had established relationships.

5   In some venues, a peace committee was already in place. In those instances, we worked with the preexisting peace committee to ensure that it was balanced in terms of farmers and pastoralists, as well as gender.

The second part of the intervention involved creating a third type of committee, the joint project committee, in which farmers and pastoralists agreed upon ways to address tensions through the implementation of projects. The process for selecting projects was similar to that of many community-driven development programs. It started with a series of community meetings, beginning with separate farmer and pastoralist meetings that 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. To inform the selection of projects, the joint project committees conducted a participatory needs assessment to gather the opinions of various demographic groups. The committees used this information, along with a conflict and resource mapping exercise, to identify resource-based drivers of conflict and select which projects to pursue with the grant money. 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 and schools, rehabilitation of health centers, and construction of fences along grazing routes to protect farmlands. 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 received a new borehole well, with farmer and pastoralist youth helping to construct the wells.

The last main intervention was conflict prevention forums for the larger community, in which farmers and pastoralists came together to discuss issues and policies that were affecting them. Government officials also attended these events.

Hypotheses

Based on the theories and evidence described above, which were used to develop the Engaging Communities for Peace in Nigeria (ECPN) program, we constructed the following hypotheses, which fall into four categories: attitudes, perceived security, behaviors, and dosage.

Attitudes

As described above, contact through the joint projects was largely expected to reduce individuals' prejudice—that is, negative feelings toward people from other groups. Based on that expectation, we hypothesized the following:

> ❭ Attitudes between farmers and pastoralists will have improved to a greater degree among communities and individuals involved with the ECPN program, compared with those not involved with the program.

Perceived Security

The program was theorized to change how safe people felt, as a result of a projected reduction in the incidence of violence. The violence reduction could come about because of stronger relationships between the two sides due to working together, because of the mediation program, or both. The resulting hypothesis may be stated as follows:

> ❭ Perceived security among farmers and pastoralists will have increased to a greater degree for communities and individuals participating in the ECPN program, compared with those not participating in the program.

Behaviors

Because behaviors may not necessarily follow from attitudes, we also wanted to see if the program changed how much people interacted and cooperated with one another, as well as how they resolved disputes. We hypothesized the following:

> ❭ The amount farmers and pastoralists interact with one another will increase to a greater degree for communities and individuals involved with the ECPN program than for those not involved with the program.

> ❭ The amount farmers and pastoralists cooperate with one another will be greater for communities and individuals that receive the ECPN program than for those not in the program.

> ❭ Disputes will be resolved more peacefully to a greater degree for communities and individuals involved in the ECPN program than for those not involved in the program.

Dosage

We also examined why the program may have had these various effects, specifically in terms of how intensive the program was for any individual (i.e., dosage, as described by whether the individual was a direct participant, an indirect participant, or a member of the control group). We hypothesized the following:

> ❭ Direct participants will change to a greater degree than indirect participants, who in turn will change more than those not exposed to the program at all (i.e., the control group).

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6   Unfortunately, there was no way to disentangle which component of the program was contributing most to the reduction in violence. While all of the outcomes relied to some degree on both of the interventions, perceived security is likely most closely tied to both mediation and the joint projects.

Research Design

We evaluate the effects of Engaging Communities for Peace in Nigeria (ECPN) with a randomized controlled trial (RCT) at the community level to examine the overall impact of the program on communities. We triangulate the results of the community-level RCT by using a pre-/post-intervention analysis of the program's effect on (1) the individuals most engaged in program activities (committee members/direct participants), (2) those who were merely exposed to program activities (non–committee members in communities that participated in the interventions/indirect participants), and (3) those with no exposure to the program at all (a control group). See Figure 1 for a diagram of our sampling strategy. The baseline survey was conducted between September and December 2015, and the endline was conducted between January and April 2018.

**1539 participants
randomly sampled at baseline**

↙   ↘

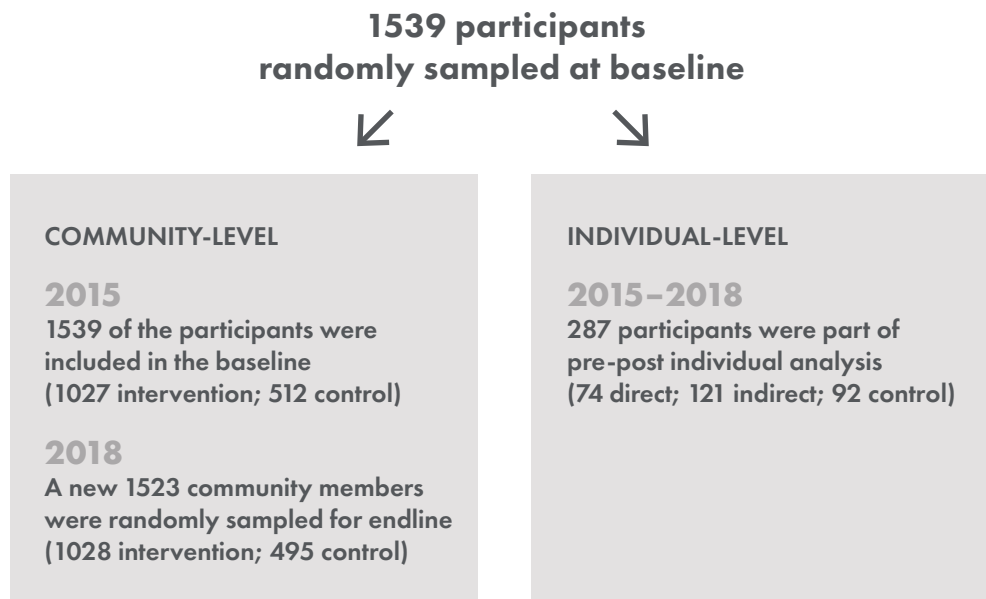| COMMUNITY-LEVEL	INDIVIDUAL-LEVEL
2015 1539 of the participants were included in the baseline (1027 intervention; 512 control)	**2015–2018** 287 participants were part of pre-post individual analysis (74 direct; 121 indirect; 92 control)
2018 A new 1523 community members were randomly sampled for endline (1028 intervention; 495 control)	

FIGURE 1: Sampling diagram

Community-Level RCT

For the community-level RCT, we first established a list of sites eligible for the ECPN intervention, where each site contained one farmer and one pastoralist community. To identify eligible sites, Mercy Corps undertook a scoping exercise to determine whether 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' having engaged in violent clashes within one year of the scoping exercise. Willingness to participate in the program was assessed through conversations with community leaders, none of whom refused the program. The scoping exercise initially led to the identification of 30 eligible sites; through further visits and interviews, we narrowed the list to 15 sites (30 communities).[7] We then randomly selected 10 of these 15 sites to receive the program and monitored 5 of the sites as a control group.[8]

To collect data within the 30 communities, we took a random sample using a "listing exercise" procedure, whereby all houses in a community were numbered, and then, from that list, 50 were randomly chosen for the survey.[9] This procedure yielded over 1,500 survey respondents at baseline and endline.

Individual-Level Pre-/Post-intervention Analysis

For the individual-level analysis, we surveyed 287 individuals at baseline and endline. Using the initial list of 1,539 baseline respondents, we identified a subset we would survey again at endline. We selected about 10 individuals per community, ensuring equal numbers of farmers and pastoralists.[10] We also made sure that we surveyed an equal number of males and females. In intervention sites, this was a simple random sample from two baseline groups: (1) people who joined ECPN committees after the survey was conducted and (2) people who did not participate in ECPN committees but had responded to the survey at baseline. In control sites, we surveyed a simple random sample from all baseline respondents. Overall, there were three groups of respondents (1) ECPN committee participants (i.e., direct participants), (2) ECPN indirect participants (i.e., indirect participants), and (3) the control group.

As discussed in the "Context and Program" section, we set up three committees at each intervention site: (1) a joint project committee, (2) an early warning system committee, and (3) a peace committee. We initially randomly assigned baseline survey respondents to be part of these committees, but random assignment proved difficult. Many people who were not selected wanted to be on the committees, and some people who were selected were not able to participate or could not be located when the committees were launched. As a result, in most cases, people self-selected into committees. Despite this self-selection, committee members and non–committee members we resurveyed were not statistically different on observable attributes at baseline, before ECPN began. Their baseline similarity increases our confidence that having served on a committee, not preexisting dispositions, explains differences in attitudinal, perceptual and behavioral change between committee members and non–committee members from baseline to endline.

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7   For further details on the scoping exercise, please see Appendix 1.
8   The control sites received the program after this impact evaluation was conducted; at the time of the endline survey, however, they were not aware that they would receive the program.
9   For more details on the listing exercise procedure, see Appendix 2.
10   Since we were unable to locate many people from the baseline sample, the endline sample resembles a convenience sample more than a random sample. In addition, to ensure that we maintained a random sample, we did not remove from the community-level baseline respondents who were in the individual-level analysis. Therefore, p-values from the two sets of analyses are not independent.

Our final sample included 74 individuals from intervention communities who participated in ECPN project committees, 121 individuals from intervention communities who did not participate in any ECPN activities, and 92 individuals from control communities. We refer to those people who participated in the committees as "direct participants," those from intervention sites who did not participate in committees as "indirect participants," and those in control communities as "controls."

Within the individual-level analysis, we make three comparisons to investigate differential effects of ECPN. The first comparison is between participants (both direct and indirect) and controls, to assess how direct exposure to the program affected participants, compared with people who had no exposure. The second comparison is between indirect participants and controls, to assess whether individuals at intervention sites without direct exposure to the program benefited, compared with people in control communities who were not exposed to the program at all (i.e., to observe any ripple or spillover effects of the committees). The third comparison is between direct participants and indirect participants in intervention communities, to assess the added value of being on a project committee, compared with being exposed to the program indirectly. Overall, if ECPN had an effect on people in intervention communities, we would expect more positive change in direct participants than in indirect participants, and more positive change in indirect participants than in individuals from control communities (direct participants > indirect participants > controls).

Estimation

Below, we describe our estimation procedure for the community-level analysis and the individual-level analysis. For both analyses, we estimate one-tailed greater-than tests, unless otherwise noted, because our hypotheses are that the change in outcomes for treated units will be *greater than* those for the controls, not that the change in outcomes for treated units will be *different from* those for the controls. The tests are adjusted for these multiple comparisons using the false discovery rate (FDR) and the Family Wise Error Rate (FWER)[11] method. In the "Results" section, we report the unadjusted *p*-values in the text and note the adjusted *p*-values in the footnotes.

Community-Level RCT Estimation

For instances in which baseline values are balanced between intervention and control sites (i.e., within 0.2 standard deviations), we compare endline values. Our regression equation is as follows:

$$Y_{ij} = \beta_0 + \beta_1 Z_{ij} + X_{ij} + \delta_j + \varepsilon_{ij}.$$

In this equation, Y is the outcome at endline, β_0 is the average change in the outcome in control communities, $\beta_1 Z_{ij}$ is the difference in change between intervention communities and control communities, X is the outcome at baseline, i is the community in state j, δ is a fixed effect at the level of the state (j) because we randomized the intervention to communities within state-level blocks, and ε is an error term.

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11   When conducting many hypothesis tests, there is a chance that a result might be significant even if there is not a true effect. For example, at a .05 threshold for significance, we would expect a spuriously significant result in 5 percent of tests—that is, if we conduct 20 tests of variables that are unrelated to each other, 1 of them would likely be significant anyway. One way to prevent running a number of tests, seeing what is significant, and reporting those results is to adjust for the number of tests conducted (another way is to include a preanalysis plan, which we also did; it is preregistered with Evidence in Governance and Politics: http://egap.org/registration/1242). Here, we account for the number of tests conducted with a multiple hypothesis correction. The FDR correction for multiple hypotheses controls for the expected *proportion* of false discoveries (< .05 *p*-values even when there is no effect). This method is in contrast to familywise error rate (FWER) procedures, which control for the probability of *at least one* false discovery. FDR procedures have greater statistical power but are more likely to make false discoveries, whereas FWER procedures are less likely to make false discoveries but have much lower power. We therefore report both.

For the public goods game, we have no baseline value, so we use a similar equation without the outcome at baseline, X:

$$Y_{ij} = \beta_0 + \beta_1 Z_{ij} + \delta j + \varepsilon_{ij}.$$

Where baseline values were not balanced, we use a difference-in-difference analysis to estimate the effect of ECPN on measures for which we have baseline and endline data. In this model, each community has one observation, the *change* in outcome from baseline to endline, represented by Y. In this formulation, our regression equation is as follows:

$$Y_{ij} = \beta_0 + \beta_1 Z_{ij} + \delta_j + \varepsilon_{ij}.$$

The above equations describe ECPN's impact on a single outcome. We also hypothesized that ECPN will simultaneously affect *all* our outcome measures, in that the likelihood of seeing the same pattern of results if the program had no or little effect on each of the outcomes is small. To test whether ECPN affected all of the outcomes, we use simultaneous hypothesis testing, as described in Caughey, Dafoe, and Seawright (2017).[12] This procedure summarizes the effect of an intervention across several outcomes and helps increase statistical power when the intervention affects all outcomes and sample sizes are small.[13] The procedure described by Caughey, Dafoe, and Seawright (2017) accounts for the number of outcomes affected by an intervention. Observing many low p-values across several distinct outcomes by chance is very unlikely, even if those low p-values are not statistically significant. If the individual outcomes generate many low but not statistically significant p-values, the nonparametric combination (NPC) procedure may generate a statistically significant cumulative p-value that summarizes the program's effect across all outcomes. The NPC procedure is ideal to test the ECPN program's impact because it tests the effect on multiple outcomes, and ECPN is hypothesized to affect several outcomes, not just one.[14]

Individual-Level Pre-/Post-intervention Estimation

We use similar equations to estimate the individual-level effect of participating in ECPN committees and of being in intervention communities without participating in an ECPN committee. The data contain one observation per individual: either the person's endline value or the difference between that individual's baseline value and endline value.

When we control for baseline values, using the same criteria identified above, the regression equation for a single outcome is the following:

$$Y_{ij} = \beta_0 + \beta_1 Z_{ij} + \beta_1 P_{ij} + X_{ij} + \delta_j + \varepsilon_{ij}.$$

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12   Simultaneous hypothesis testing determines the joint probability of observing several outcomes to be correlated with an intervention. This method is in contrast to a typical hypothesis test, which determines the probability of one outcome's being correlated with the intervention. Using the nonparametric combination procedure, we generate p-values for each outcome of interest using the above equation, correct the p-values for multiple hypothesis tests, combine those p-values, and then ask how often we would see a combined p-value of that size if ECPN did not have an effect.

13   The nonparametric combination package, from Caughey, Dafoe, and Seawright (2017), uses randomization inference to generate p-values. To ensure that our null distribution is created by randomizing the intervention between exchangeable units, we mimic our randomization process by randomizing the intervention to communities in site-level clusters and within state blocks. This means that both communities in an implementation site (farmers and pastoralists) will always be treated together and that assignment to the intervention is conducted separately in Nasarawa and Benue, just as the intervention was assigned in this study.

14   We identified this strategy in our pre-analysis plan.

The symbols in the equation represent similar variables as in the community-level equation. The addition is $\beta_1 P_{ij}$, which is the difference between the average change among direct participants and that among indirect participants. Errors are clustered at the community level. We are interested in $\beta_1 P_{ij}$.

When we do not have baseline values, the regression equation for a single outcome is as follows:

$$Y_{ij} = \beta_0 + \beta_1 Z_{ij} + \beta_1 P_{ij} + \delta_j + \varepsilon_{ij}.$$

The difference-in-differences regression equation for a single outcome is the following:

$$Y_{ij} = \beta_0 + \beta_1 Z_{ij} + \beta_1 P_{ij} + \delta_j + \varepsilon_{ij}.$$

Again, in this last equation, Y represents the *change* from baseline to endline.

As with the community-level analysis, we hypothesize that ECPN will affect all outcomes. Additionally, we also hypothesize an ordered effect in which direct participants show the greatest baseline-to-endline change, followed by indirect participants, followed by the control group. To represent this hypothesis, we create a combined test statistic that represents the amount of change in both groups, and then use that test statistic across outcomes with a simultaneous hypothesis-testing procedure similar to the procedure described above.[15]

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15   To ensure that our null distribution is created by randomizing the intervention and committee membership between exchangeable units, we mimic our randomization process by (1) randomly assigning the intervention within state blocks, (2) randomly assigning the intervention to all individuals within site-level clusters, and (3) randomly assigning ECPN committee participation to a proportion of individuals in these new "intervention" communities such that the proportion of people on committees in the new "intervention" communities matches the proportion in one of the real intervention communities. Our combined test statistic is the sum of the coefficients for *ECPN* and *Participant*.

Outcomes and Measures

We measured three outcomes to estimate the impact of Engaging Communities for Peace in Nigeria (ECPN): (1) intergroup attitudes (feelings and thoughts about farmers by pastoralists and vice versa), (2) perceptions of security, and (3) behaviors. We used a complementary set of measures to best triangulate on each of these outcomes, including survey questions,[16] a natural-field behavioral game, and observational monitoring.

We provide more details about these measures below.

Intergroup Attitudes

We measured two attitudinal outcomes: (1) trust and (2) intergroup cohesion. We measured each outcome with multiple survey questions, both directly and indirectly, to overcome social-desirability bias. For the direct questions, individual survey questions were aggregated into indices using inverse covariance weighting to increase measurement precision and reduce error. The indirect questions were two single-question survey experiments.

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16   A complete list of survey questions is available in Appendix 3.

Trust

Direct questions. We asked six survey questions to measure trust, ranging from trust in the other group to comfort with engaging in various activities with members of the other group, such as trading, attending a wedding, and having a family member marry someone from the other group. Among farmers, the questions referred to the nearby pastoralists, and among pastoralists they referred to nearby farmers, rather than the group writ large.

Indirect Endorsement experiment. In an endorsement experiment, the survey respondent was asked about her support for a hypothetical policy. Half the respondents were told that a group she may have an opinion about (either positive or negative) endorses the policy; the other half were not told about any group's endorsement of the policy. The difference in support between the endorsed policy and the unendorsed policy is a measure of bias for or against that group. In this case, we measured bias against the other group. Our endorsement experiment 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 group (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 Cohesion

Direct. To measure intergroup cohesion between the farmer and pastoralist communities, we asked 10 survey questions, which fell into two types: abstract and concrete. The abstract cohesion questions asked about farmers and pastoralists in the area generally, for example, "Are people in this area willing to help their neighbors across ethnic and religious lines?"[17] The concrete cohesion questions asked about hypothetical situations that directly relate to the other group, such as "If something unfortunate happened to someone from your group in this community, such as a serious illness or the death of a parent, how likely is it that some people in the community from the other group would get together to help them?"

Indirect list experiment. Our list experiment included two questions. One of them was a three or four item list and asked the respondent how many of the items would make him or her upset. The only difference between the three-item version and the four-item version was the addition of an item related to the attitude of interest. The three-tem list included "when your football team loses a match," "increases in the price of gasoline," and "lack of rainfall." To measure intergroup attitudes, we added the following item to the four-item list: "when I have to interact with [farmers/pastoralists] in the market." The difference in the average number of items that upset people is interpreted as the proportion of people within a group who are upset by this additional item. The other one was similarly set up, but was a five or six item list.

Perceptions of Security

We measured perceptions of physical security with 13 direct survey questions. These questions asked whether respondents had felt insecure in the past year when engaging in activities such as grazing their animals, working on their farms, fetching water for their families, and working for wages. We combined the security questions into an index, with high values indicating high perceptions of security and low values indicating low perceptions of security.[18]

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17   Before we ask these questions, we say, "I am going to ask you about people in this area, including people from the other group."

18   We measured violent incidents in ECPN communities using Mercy Corps' Violent Incident Tracker, developed in our previous peacebuilding program in the Middle Belt, Conciliation in Nigeria through Community-Based Conflict Management and Cooperative Use of Resources (CONCUR). However, we were not able to use the tracker in ECPN control sites, and therefore the data were not included in the analysis of the ECPN project.

Behavior

We measured behaviors in four ways: (1) survey self-reports of interactions with members of the other group and intentions to interact with them, (2) perceptions of dispute resolution success, (3) a public goods game, and (4) observational monitoring of behavior in shared markets and at social events. Below we describe each in more detail.

Intergroup Interaction

We measured intergroup interaction with five survey questions. We asked respondents if and how often they had interacted with the other group over the past month in (1) the market, (2) their own home, (3) the home of a member of the other group, (4) a social event outside the home, and (5) other locations or activities. For each question, if respondents did not interact with the other group, the question was coded as 0 for "no contact." If respondents interacted with the other group, the question was coded in quantiles representing low (1), medium (2), or high (3) frequency of contact.

Additionally, respondents were asked about their willingness to (1) join a community group and (2) live in a community that included a certain percentage of members of the other group. The percentage of members of the other group was randomized to be 5 percent, 25 percent, 50 percent, or 75 percent, to see if the proportion of members of the other group affected the respondent's willingness to interact.[19]

Dispute Resolution Success

We measured success in dispute resolution with five questions, which asked about the resolution of disputes concerning farmland, pastures, and markets. If the respondent reported that any of these resources were shared by farmers and pastoralists,[20] the respondent was also asked if sharing caused disputes and if those disputes were resolved successfully. If the shared resource did not cause disputes, or if its disputes were always resolved successfully, the respondent received the maximum score for that question. Unsuccessful dispute resolution attempts received lower scores, with the minimum score given if the respondent said disputes were never resolved successfully.

Natural-Field Public Goods Game

We used a natural-field behavioral game based on a public goods dilemma to measure intergroup cooperation.[21] 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.[22]

In the PGG, participants were given an envelope with a cash gift of 1,000 Nigerian naira (in the form of 10 bills, 100 naira each)[23] and asked whether they wanted to contribute part of that gift to a community fund to finance

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19   Note that this is different from a randomized-response survey experiment.
20   Any respondent who reported that these resources were not shared by farmers and pastoralists received "NA" ("not applicable") for this index and was not included in the data.
21   We also believe this game could measure intergroup trust, since participants with more trust that the other group will donate to the community fund may donate more than those who do not trust the other group to donate.
22   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.
23   1,000 Nigerian naira is about US$2.77.

a development project that would benefit their own community and the other community.[24] Participants were told that any contribution they made to the community fund would be tripled, so a gift of 100 naira to the community fund would become 300 naira for the community fund. The game asked participants to make a difficult trade-off between their own interests and the interests of the broader community. The cooperative behavior (contributing to the community fund) generates less money for the individual but more money for the community than the selfish behavior (keeping the gift for oneself).

We measured two outcomes of the public goods game: (1) whether the individual gave any amount or not, and (2) if so, the amount contributes d to the community fund.

Observational Data

To triangulate with the self-reported data and the PGG, our local partner, Pastoral Resolve (PARE), monitored market and social behavior in the study sites. We wanted to know if ECPN increased social interaction between farmers and pastoralists beyond the program activities. 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. At social events, we measured the number of members of the other group in attendance and the number who ate or drank anything,[25] both in absolute numbers and as a percentage of total attendees.

Observations were made in two periods: July 2016–February 2017 and September–December 2017. Events that occurred before February 2017 were considered baseline; events occurring in September 2017 and later were considered endline.

Limitations

Small Number of Communities

The main limitation of the community-level randomized controlled trial is the number of communities we were able to include in the study. With 30 communities clustered at 15 sites, we have relatively low power to detect an effect of ECPN. We try to increase power by testing multiple hypotheses simultaneously (following Caughey, Dafoe, and Seawright 2017) and by using inverse-covariance-weighted outcome indices,[26] which should measure our outcomes of interest more precisely than indices constructed using other methods.

Self-Selection at the Individual Level

We also initially planned to randomize participation on ECPN committees within intervention communities. However, as discussed above, we had low compliance with the individual-level randomization. As a result, many of the people on the committees self-selected into participation. If we see positive change among committee participants, therefore, it is possible that the type of people who participated would have changed more positively even without ECPN, making it difficult to attribute the change to ECPN. It is also possible that

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24   In many ways, this is similar to what the joint project committees were already doing: making decisions on what to do with a grant from Mercy Corps to the committee.

25   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.

26   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 thing. This type of index maximizes measurement precision when the researcher wants to learn about the general concept being measured by a set of survey questions.

ECPN is effective only on the type of people who elected to participate and would not be as effective on people less interested in the program, making it difficult to generalize the effects of ECPN to the wider population in these areas.

We try to address these concerns in three ways. First, we illustrate that the respondents we resurveyed are not statistically different from baseline respondents on baseline measures. Since the people we resurveyed are an as-if-random sample of all baseline respondents, effects we see in this sample should generalize to other respondents. Second, we demonstrate that on most measures, there are no measurable baseline differences between direct participants, indirect participants, and controls. When there are differences, the control sites start out more positively than intervention sites, which would make it more difficult for us to see an effect (i.e., the differences work against us). Third, we present evidence that these groups do not differ in their baseline-to-endline changes on two placebo outcomes,[27] suggesting that they have similar trajectories in the absence of ECPN. The results of these balance and placebo tests are presented in Appendix 4.[28]

Displacement

An additional limitation of both analyses was the significant displacement in Benue state at the time of the endline. Widespread violence between farmers and pastoralists had forced many of the communities in Benue to flee to safer locations. While we chose randomly among the people we could find, we do not know whether the community members we could locate were somehow different from the broader population in these communities.[29] Appendix 1 presents evidence that on measured variables, resurveyed respondents in the individual-level analysis are representative of all people from the baseline; we are not able to conduct a similar analysis with the community-level sample. In the discussion section, we provide further explanations for how the interpretation of our results would change if our sample is unrepresentative due to displacement.

Program Adaptations

Finally, due to the fluid nature of conflict dynamics and the need to adapt the program when necessary, we were not able to maintain separation between intervention and control sites (i.e., there was contamination). For example, the team conducted an intercommunity peace forum in one intervention site, but community leaders requested that leaders from a neighboring site—which happened to be a control site—attend the forum because of a recent conflict event that had spread across the area. The program team decided to risk contamination of the research by including the control site in that one forum, for the sake of the program's success. This type of contamination was limited as much as possible, and to the extent that it may affect results of the study, it would attenuate the results, working against our hypotheses rather than in favor of them.

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27   The placebo outcomes are radio listening and acceptance of violence against the government.

28   A final way we address these concerns is to show in our individual-level analysis that the pattern of attitude change is the way we would expect ECPN to change attitudes (direct participants > indirect participants > controls), and we are unlikely to observe this pattern by chance if ECPN was not effective.

29   This concern is especially acute for Benue pastoralists, who were the most strongly displaced.

Results

Overall Community-Level Results

The community-level analysis displays a general trend in which intervention communities changed more positively than control communities. Trust, perceptions of security, and intergroup contact were statistically significant at $p < .1$. For only three of our measures—the public-good donation, dispute resolution, and the list experiment—were the results in the opposite direction than we predicted, though none of these was statistically significant. The cumulative p-value was .04 shows that it is unlikely we would see this pattern of results if the program did not have an effect.

Community-level Effects
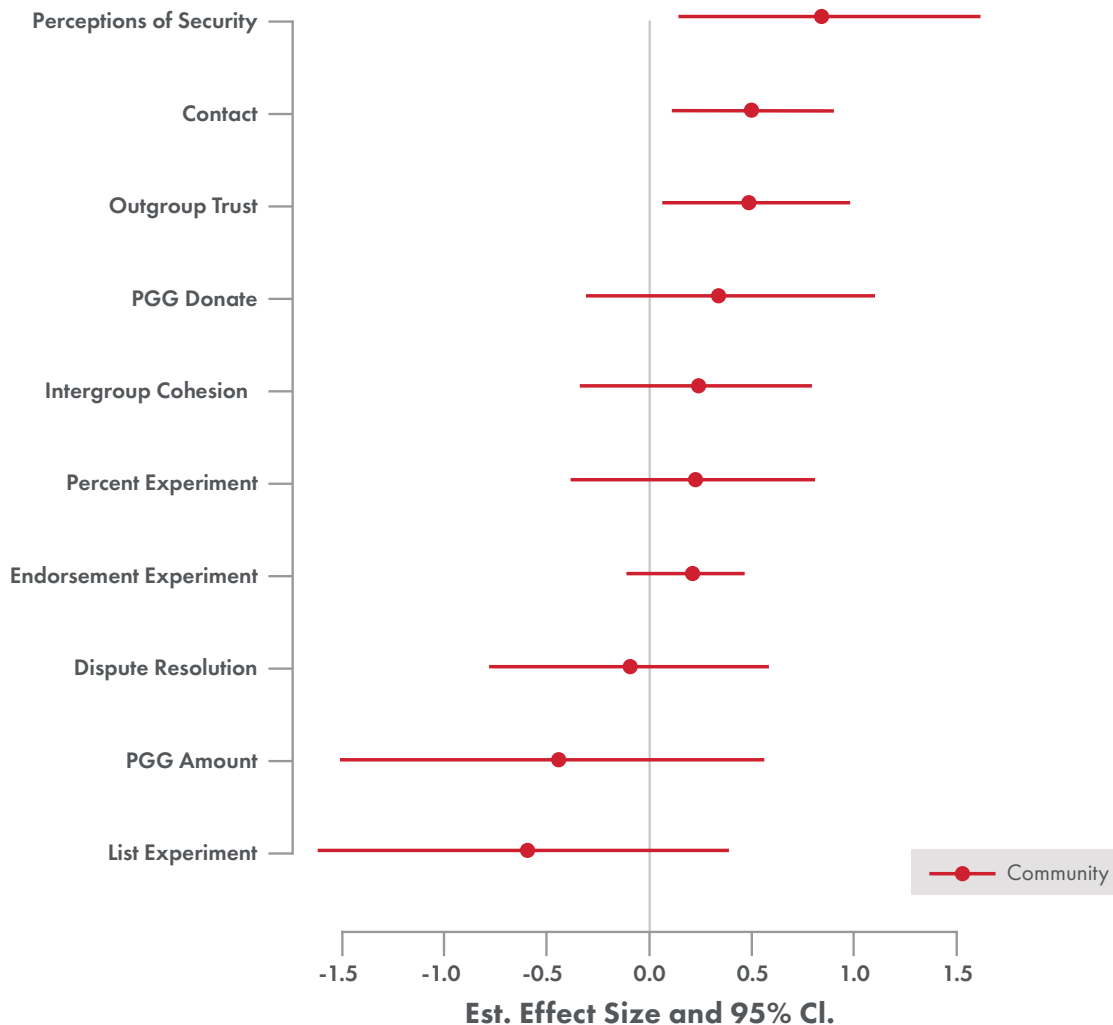
FIGURE 2: Community-level effects   NOTE: PGG = public goods game; CI = confidence interval.

The coefficient plot in Figure 2 shows the effect of Engaging Communities for Peace in Nigeria (ECPN) for all outcomes. In line with the different estimation models explained above, the vertical line represents either the control group's endline value equaling zero or the control group's change from baseline to endline equaling zero. In accordance with the different estimation models, the points represent either how much greater the differences—either positive or negative—between intervention and control groups are at endline, or how much more, positively or negatively, the intervention group changed relative to the control. The horizontal lines represent the bootstrapped 95 percent confidence interval. The further right of zero a point lies, the larger the estimated effect of ECPN; the smaller the horizontal lines, the more certainty we have about the size of the effect. Below we discuss these effects in greater detail.

Overall Individual-Level Results

The individual-level results, in Figure 3, show that for most measures, those who directly participated in the program or were indirectly exposed to the program improved more than the control group. As with the community-level effects, these results were statistically significant at *p* < .1 for intergroup contact, trust, and
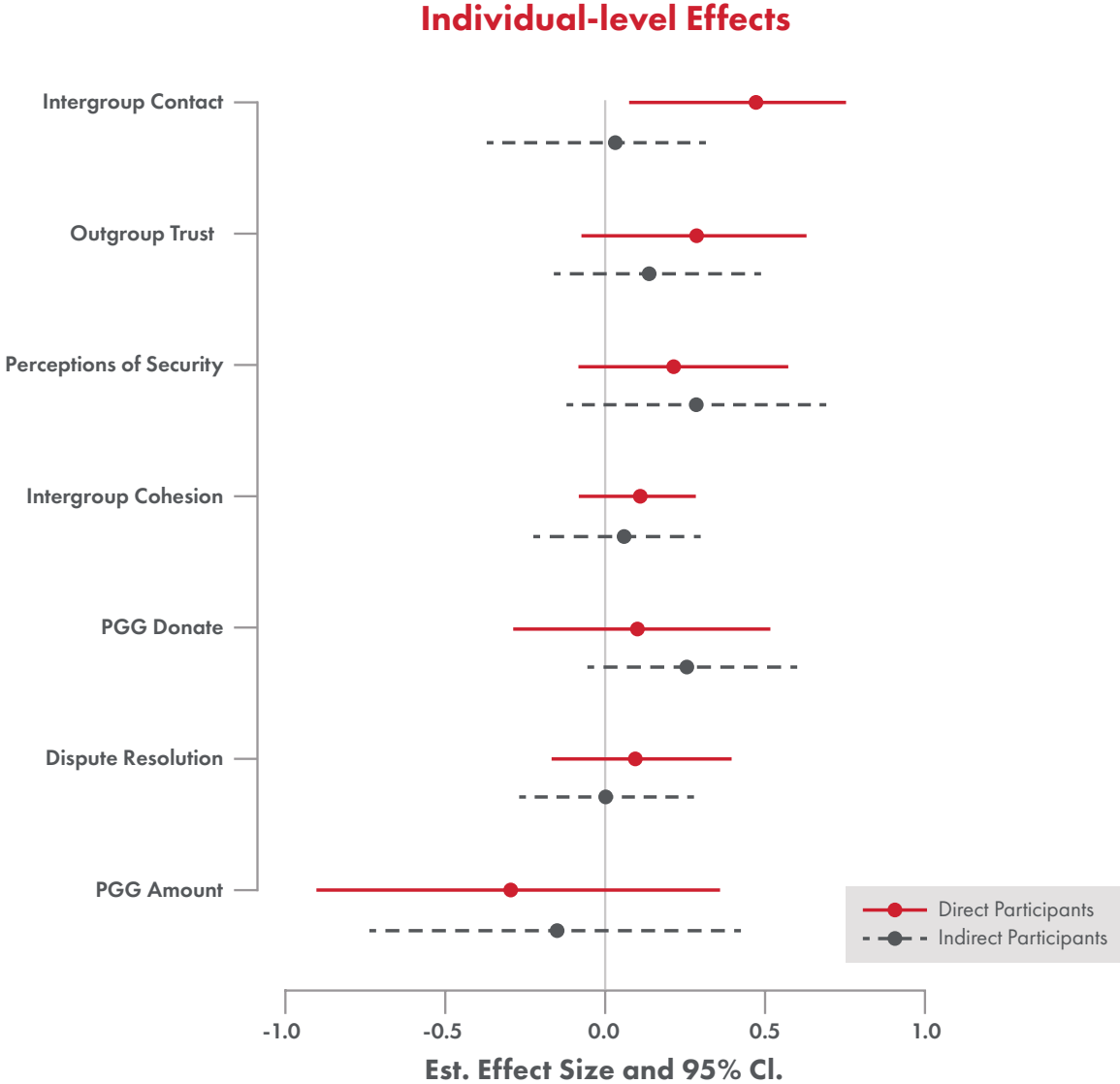
FIGURE 3: Individual-level effects   NOTE: PGG = public goods game; CI = confidence interval.

perceptions of security. For most outcomes, we also observe the expected pattern of effects: direct participants changed more positively than indirect participants, and indirect participants changed more positively than controls. Our cumulative *p*-value was .08, which shows that it is unlikely we would see this pattern of results if the program did not have an effect. To illustrate the pattern of results, Figure 3 includes separate lines for direct and indirect participants. However, since our hypothesis was about the linear relationship between the three types of participants—direct, indirect, and control—and not pairwise comparisons, we report only one effect size in the detailed results below. For the pairwise comparisons, see Appendix 5.

Attitudes

Trust

With regard to trust—how comfortable people are with the other group—the results (in Figure 4) suggest that ECPN communities were bolstered by the program. Control communities became less trusting from baseline to endline, while intervention communities improved slightly over the same time period. The effect size was 0.49, which translates to a 13 percent difference in the ways in which the intervention and control sites changed, with an unadjusted *p*-value of .06.[30] The negative change in the control group illustrates that ECPN may have protected people from developing negative attitudes despite high levels of intergroup violence in the broader area.
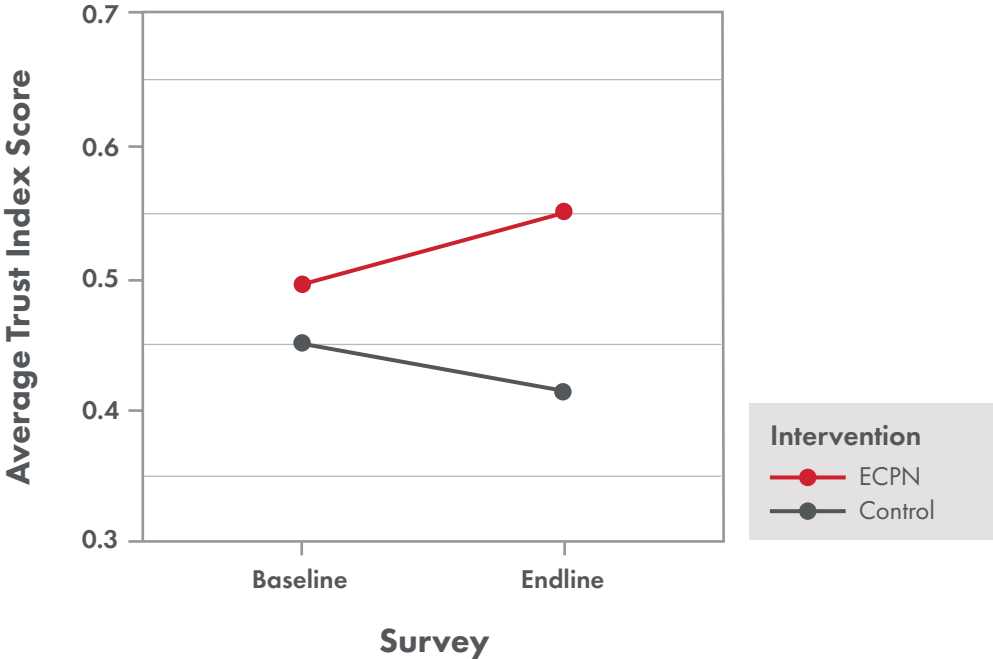
FIGURE 4: Community-level effects on trust

When trust is measured indirectly, through the endorsement experiment, we find the pattern we would expect—people in ECPN communities are more willing to endorse a policy advocated by the other group than are those in the control group—however, this result did not reach statistical significance.

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30   The adjusted *p*-values for multiple hypothesis correction are .20 (FDR) or .47 (FWER).

We find similar results for the direct measure of trust at the individual level: direct participants and indirect participants in intervention communities changed more positively than the control group on our measure of trust (Figure 5).[31] More than that, the data follow our predicted pattern of change based on the level of participation within ECPN: direct participants improved more than indirect participants, and indirect participants improved more than the control group. The effect size for this pattern of results is .15, translating to a 5 percent difference in the degree to which direct, indirect, and control participants changed on our measure of trust. This pattern has an unadjusted *p*-value of .072.[32]
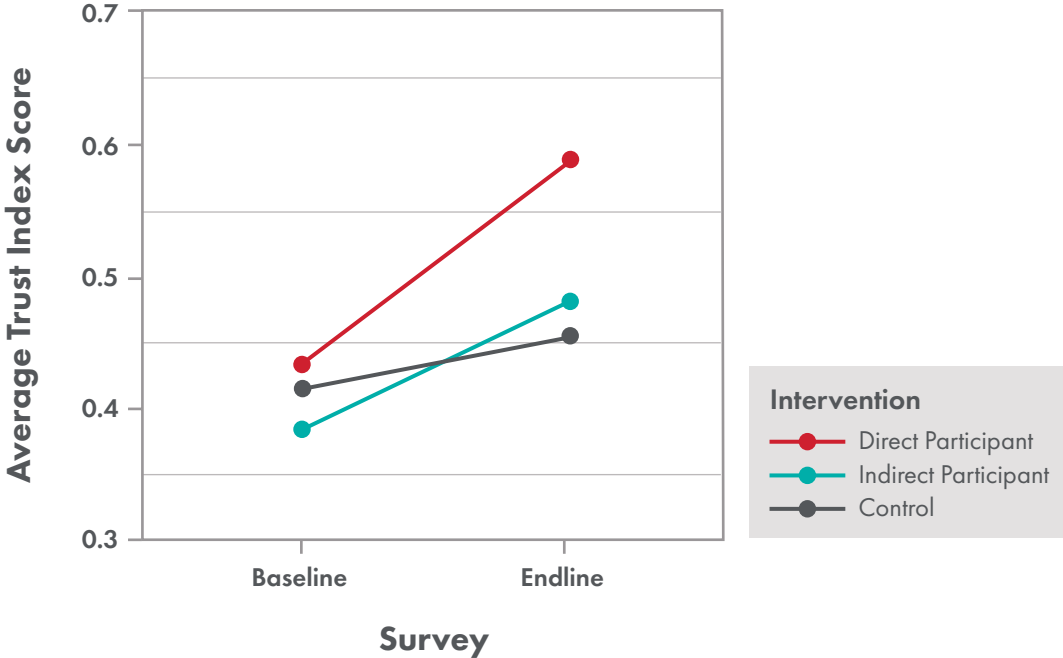
FIGURE 5: Individual-level effects on trust

Intergroup Cohesion

Our other attitudinal measures examined intergroup cohesion. When we asked about cohesion directly, the results trended as we expected at both the community and individual levels. However, intergroup cohesion was not statistically significant for either set of analyses. We found the results to be somewhat stronger for the concrete questions—those about a specific situation—than for the abstract questions, but results were still not statistically significant, even when looking only at the concrete questions.

When asked about intergroup cohesion indirectly through the list experiment, we found that results at the community level trended in the opposite direction from what we would expect, with people in ECPN communities more likely to say trading with a farmer/pastoralist would upset them than those in control communities. Again, though, this result was not significant.

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31   The indirect survey experiments can be conducted only at the community level, in order to maintain anonymity.
32   The adjusted *p*-values are .24 (FDR) and .43 (FWER).

Perceptions of Security

Another key outcome for ECPN was to increase perceptions of security among both farmers and pastoralists. The results of our surveys suggest that ECPN was successful in this regard: perceptions of security increased in intervention communities far more than in control communities (Figure 6). Of note here is that ECPN communities initially felt less secure than control communities yet ended up feeling more secure at the end of the program. The effect size was 0.84, which translates into a 15 percent difference in the degree to which intervention and control sites changed, with an unadjusted p-value of .01.[33]
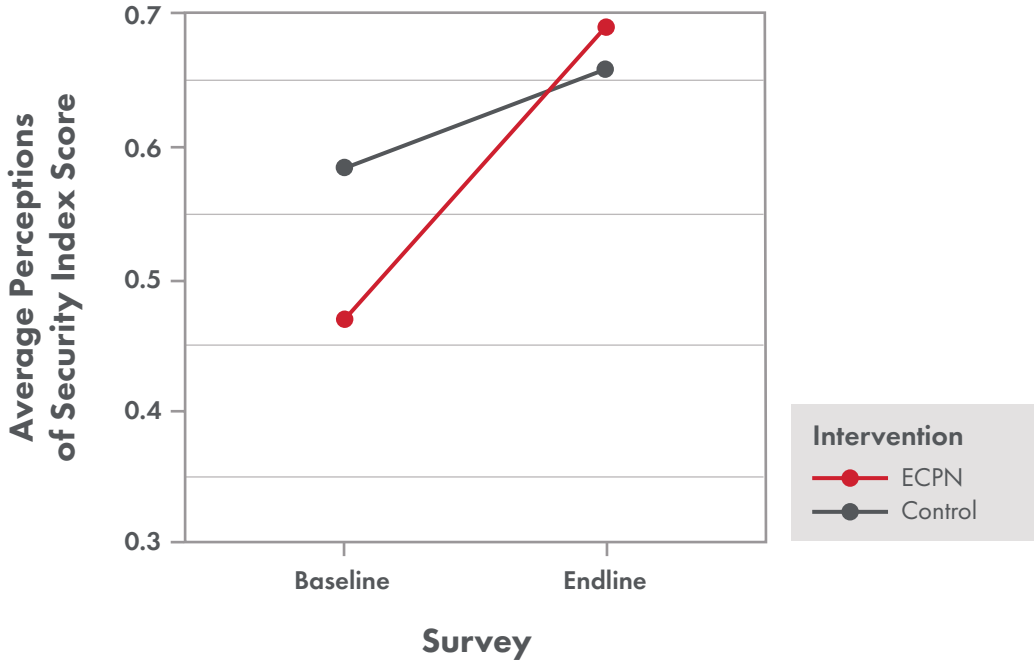
FIGURE 6: Community-level effects on perceptions of security

The individual-level data also suggest that ECPN improved respondents' perceptions of their security. Perceptions of security increased more from baseline to endline among direct participants and indirect participants than among controls (Figure 7). Though this measure increased relative to the control group for both ECPN groups, we do not observe the hypothesized pattern that direct participants would increase more than indirect participants. Rather, they increased by about the same amount. The effect size is 0.12, translating to a 3 percent difference in the degree to which direct, indirect, and control participants changed on our measure of perceptions of security. This pattern has an unadjusted p-value of .10.[34]

One potential explanation for this pattern of results is that through the committees, particularly the peace committees, direct participants became more aware of various incidents in the communities. Examining the results by state brings further evidence for this interpretation: the only group whose perceptions of security decreased were the pastoralists who were direct participants in Benue. These were the people who would most likely be involved in helping with any security incidents.

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33   The adjusted p-values are .12 (FDR) and .15 (FWER).
34   The adjusted p-value is .31.
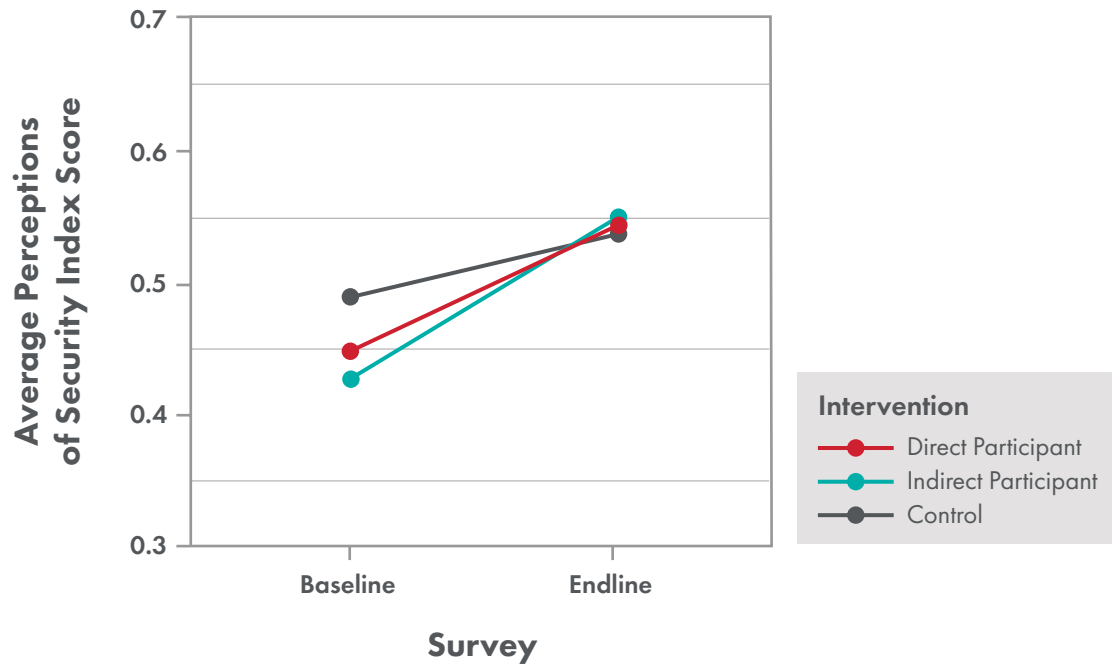
FIGURE 7: Individual-level effects on perceptions of security

Behavior

Intergroup Contact

ECPN was designed to increase interaction between farmers and pastoralists. The community-level results strongly support the idea that ECPN encouraged intergroup contact between farmers and pastoralists (Figure 8). When measured by direct questioning, intergroup contact declined sharply in control communities
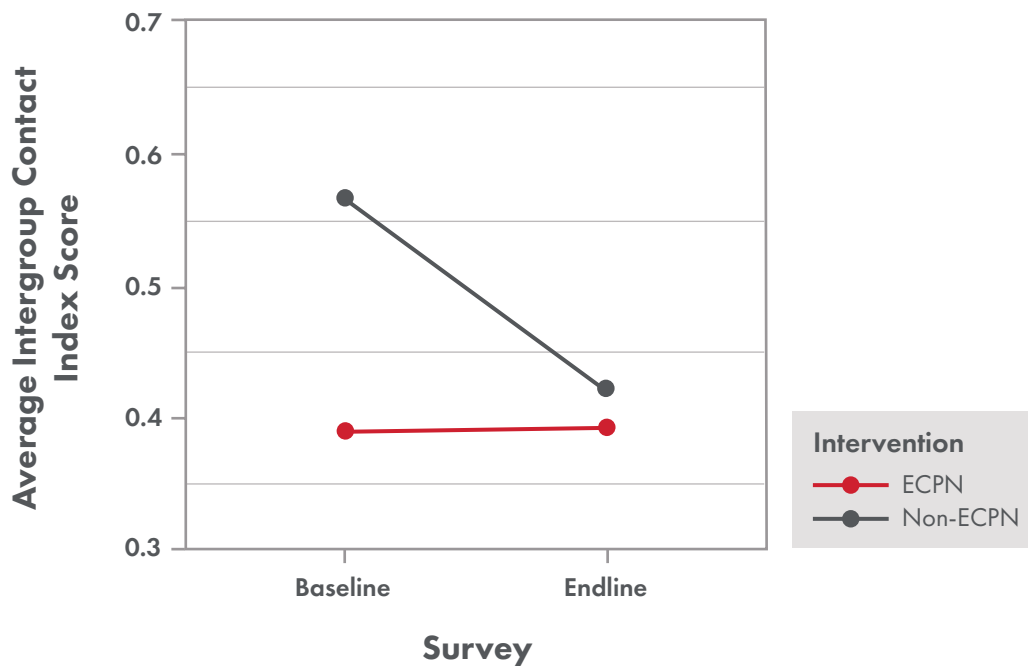
FIGURE 8: Community-level effects on intergroup contact

and remained steady in intervention communities. This result seems largely due to the displacement in Benue, where intergroup contact went down for every group, especially pastoralists. In Nasarawa, intergroup contact increased for pastoralists, though more so in ECPN communities. For farmers, we found that intergroup contact remained the same in ECPN communities but decreased in control communities. The effect size was 0.50, which translates to a 15 percent difference in the degree to which the intervention and control sites changed, with an unadjusted *p*-value of .03.[35]

We also asked about willingness to join in an activity or live with people from the other group, and tested whether the concentration of people from the other group would affect respondents' answers (i.e., the percent experiment). We found the expected trends, whereby people in intervention communities were more likely to be willing to join a group or live in an area with members of the other group than those in the control, even at higher concentrations (50 percent and 75 percent members of the other group). These results, however, were not significant.

At the individual level, intergroup contact increased for direct participants but stayed largely the same for indirect participants and controls (Figure 9). The effect size was 0.22, which translates to a 7 percent difference in how much direct, indirect, and control participants changed. This pattern has an unadjusted *p*-value of .01.[36] For non-ECPN communities, it appears the overall level of violence in the area may have decreased contact between farmers and pastoralists. ECPN helped direct participants overcome this negative environment and increase their interaction with one another. However, unlike trust, there was not a ripple effect for intergroup contact. Additionally, in relation to specific types of contact that were part of the index, direct participants' reports of hosting members of the other group in their homes and of visiting the homes of people in the other group increased from baseline to endline. Thus the increased contact among direct participants expanded beyond the required activities, illustrating that ECPN helped build relationships that extended beyond the program.
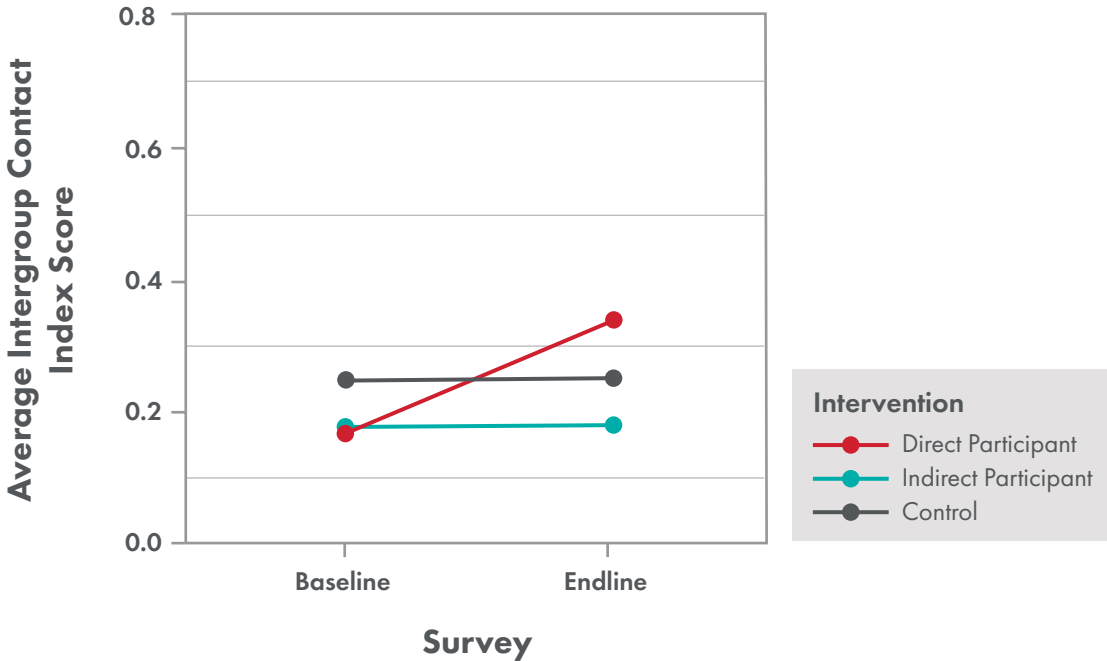
FIGURE 9: Individual-level effects on intergroup contact

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35   The adjusted *p*-values are .13 (FDR) and .24 (FWER).
36   The adjusted *p*-value is .07 (both FDR and FWER).

Our local partner, PARE, also observed intergroup contact in markets and at social events. The data were not distributed normally—that is, the data contained some extreme outliers—so we rank scored the data for this analysis, with sites with the largest number of the other group present ranked as 1, the second-largest ranked as 2, and so forth.

These observational monitoring data also support the idea that intergroup contact increased between farmers and pastoralists in intervention sites. Consistent with several other results, this effect is primarily due to a decrease in market contact in control communities coupled with a stable rate of intergroup contact in intervention communities. Relative to control sites, market contact increases in intervention sites on 7 of our 8 observational categories (see the "Outcomes and Measures" section for descriptions of the categories). Two of our categories, related to pastoralists selling in the markets, each increased to a statistically significant level. Specifically, for the number of pastoralists selling, the effect size was 1.26, translating to a 19 percent difference in rank between the intervention-site markets and those in control sites (p = .08). For the number of pastoralist women selling milk, the effect size was 1.2, translating to a 13 percent difference in rank between the number of pastoralist women selling milk in intervention sites and those in control sites (p = .07). Since the markets are all located in the farming community, the sustained presence of pastoralists there suggests that (1) farmers were tolerant of pastoralists in their community and (2) pastoralists felt comfortable spending time in the farmer community. We observe no significant effect on contact at social events. Six of 10 categories were positive but insignificant, 3 of 10 were negative but insignificant, and 1 outcome did not change at all.

Dispute Resolution Success

A major component of the program was developing the skills of both farmers and pastoralists to resolve disputes. At the community level, contrary to expectations, we found that people thought local dispute resolution processes were less likely to be successful at endline than at baseline in both ECPN and control communities. The decrease was slightly bigger for ECPN communities, though not statistically significant.

At the individual level, however, direct participants' perceptions of local dispute resolution processes improved over the course of the program; indirect and control participants' perceptions stayed the same. Again, these results were not statistically significant.

One potential reason for these differences in the community and individual analyses is that as with many community-driven development programs, only the beneficiaries who are most closely attached to the program are aware of what the mediators are doing. In this case, these would be the direct participants. Additionally, because the need to share (fewer) resources went up over the course of the program, the number of potential disputes increased. People may have been aware of the disputes, but not of their resolution, if any. We discuss these results further in the next section.

Public Goods Game

As described above, we included a behavioral measure for cooperative behavior in an attempt to overcome any self-presentation biases. We used a public goods game in which players decided (1) to contribute or not contribute to a common pool of money for a shared project, and (2) how much money to contribute. The public goods game was played only at endline. At the community level, we find that ECPN did not affect a community's likelihood of cooperating with the other community. Out of the 1,000 naira that participants were given, those in intervention communities donated 35 naira less, on average, toward shared projects than those in control sites. At the same time, participants in ECPN communities were 2.2 percent more

likely than the control group to donate something rather than nothing. Neither of these differences was statistically significant.

The individual-level results of the public goods game also suggest that ECPN did not impact program participants' likelihood of cooperating with the other group. Out of the 1,000 naira they were given, indirect participants donated about 27 naira less than those in the control group, and direct participants donated about 27 naira less than indirect participants. But again, people in intervention communities were more likely to donate something than nothing: indirect participants were about 5 percent more likely to donate than controls, and direct participants were about 2 percent more likely to donate than controls. As in the community-level analysis, neither of these differences was statistically significant.

One possible reason for these results is that those in the control communities were trying to show their desire to cooperate (i.e., signaling), whereas those in ECPN communities did not need to. However, the reason we see more consistent giving in ECPN communities is that there were stronger norms to give something where the intervention took place; those norms hadn't developed in control communities. In the next section, we further discuss possible reasons for these differences between attitudes and behaviors, as well as differences among the various behavioral measures.

Nigeria—Corinna Robbins

Discussion

Overall, it appears that Engaging Communities for Peace in Nigeria (ECPN) communities and the individuals who participated most in the program saw improvements due to the intervention. The overall trends show that ECPN communities were able to maintain or improve trust, intergroup contact, and perceived security, while these qualities stayed the same or deteriorated in places where ECPN was not present. Similarly, we found general trends that those who participated in the program to the greatest extent saw the greatest benefit, particularly related to trust and intergroup contact. Individuals within ECPN communities who participated to a lesser extent (i.e., indirect participants) benefited less than direct participants, but generally had better outcomes than people in communities that did not receive the ECPN program. At the same time, however, many of the results were nonsignificant at conventional levels, and none were significant when corrected for multiple hypothesis testing. Below we delve further into these results and what they tell us about the central theories on which many peacebuilding programs are based.

Strengths and Limitations of Contact-Based Approaches to Peacebuilding in the Face of Growing Conflict

Many community-based peacebuilding programs are implemented post-conflict—that is, after the violence has subsided—to help communities reconcile and prevent renewal of violence. People often worry that conducting such programs in the midst of violence may either not help or, at worst, be counterproductive. Here, we implemented a community-based peacebuilding program in the midst of violence. In fact, in the months leading up to endline data collection, violence in the Middle Belt was steadily increasing.

Against this background environment, we found that ECPN communities for the most part stayed the same or improved on most outcomes. Control communities stayed the same or became worse on most outcome measures. One potential reason for this pattern of results is the way intergroup contact between communities changed over the course of the program. Intergroup contact in control communities decreased substantially, largely driven by communities in Benue state, where the anti–open grazing law was instituted. The violence that resulted from the law caused significant displacement, particularly among pastoralist communities, making it more difficult for the two communities we studied—farmers and pastoralists—to interact on a practical level.

In contrast, ECPN participants overcame this pressure and continued to interact frequently with members of the other group. The continued or increased interaction among ECPN participants was not solely in the context of formal ECPN activities. For example, ECPN participants reported more frequently hosting members of the other group in their own homes and going to the home of a member of the other group. Finding a way to maintain interactions despite the overall violence helped keep these communities from backsliding into conflict.

People's perception of security was the one outcome at the community level that did not follow this pattern. There are a number of potential explanations for why perceptions of security improved for both ECPN and control communities, though significantly more so for ECPN communities. (1) All of our other measures were specific to relationships. When answering questions about intergroup contact, trust, and intergroup cohesion, those surveyed were asked about the specific farmer or pastoralist group with whom they had been in conflict. However, when asked about their perception of security, they were asked about security in the surrounding area, not security in relation to the other group. (2) Due to recent events in Benue, there was more of a security presence in both Benue and Nasarawa states, where the program was implemented. (3) Again due to the recent events, ECPN held large forums with local government and security actors to address the uptick in violence. Although these were held in intervention communities, because of displacement and the physical proximity of some of the communities, there may have been spillover from these events to control communities.

Looking at these results all together—trust, intergroup contact, and perceptions of security—also shows the limitations of community-level peacebuilding efforts. Clearly such efforts support community conflict management capacities in areas where tensions can easily escalate into violence, helping residents withstand negative influences from larger societal events and, in some cases, helping conditions improve despite these larger events. But without government policies in place to secure these gains, such efforts will be difficult to scale. Our results point to the need to match community-level and more macro-level efforts so that local interventions are supported by a conducive policy environment.

Community-Driven Development: Creating a Ripple Effect

One question we wanted to answer with this project is whether such efforts change only the direct participants or also contribute positive ripples to the wider community (i.e., spillovers). A potential limitation of many peacebuilding efforts based on contact theory and community-driven development (CDD) is the expectation within such programs that intense work with a few people, such as community leaders, will have effects that spread to the larger community. The Reflecting on Peace Practice framework, for example, tries to address the specific question of how to create peace writ large (Reflecting on Peace Practice Program 2014). We designed the present study to test ripple effects directly: would changes in direct participants' influence similar changes in the wider community?

The results suggest that while those who were directly engaged in the ECPN program through one of the various committees gained the most benefit, those who were not directly involved (i.e., indirect participants) also saw improvements due to the activities being implemented in their community. Indirect participants saw particular improvement on outcomes related to trust and perceptions of security, compared with people in the control communities. Since indirect participants did not increase their intergroup contact from baseline to endline, it does not seem that the example of direct participants' interacting with the other group inspired indirect participants to interact with members of the other group. Rather, it is more likely that indirect participants were influenced by community-wide changes in social norms (Paluck 2009), by social discussion and persuasion from direct participants and community leaders, or by vicarious contact through observing in-group members interacting with members of the other group.[37] Future research should explore which mechanisms or combination of mechanisms—social norm changes, individual persuasion, vicarious contact, or others—accounts for the increased outcomes among indirect participants in relation to the control group.[38]

Dosage and Self-Selection

One reason we were able to test for the ripple effect is that we designed this study to see how an individual's level of engagement in the program—whether fully committed and very active or occasionally was "touched" by the program—had differing impacts, and to what degree. One potential reason for null findings on CDD and similar programs is that the research has examined only community-level effects (White, Menon, and Waddington 2018). As a result, many of the people surveyed in such programs likely were not deeply involved in the activities. While these null results are important, since most implementers of and donors to these programs expect larger dispersion of program effects throughout the community, it is also important to understand how these programs may differentially affect those directly involved versus those not directly involved.

We had hoped to examine this question through random assignment of people to the project committees to avoid any self-selection biases (the latter would mean that people involved in the project committees were somehow different from the general population in these communities). Unfortunately, as described above, our random assignment to the committees did not work. However, on the variables we examined, we found few differences at baseline among those who were involved in the committees, those in ECPN communities

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37   These mechanisms should not be thought of as mutually exclusive. Observing positive contact and hearing discussions about the other group can signal social norm changes. Likewise, realizing that social norms are changing could encourage an individual to engage in social discussions about the other group or interpret observed contact more positively.

38   The change in perceptions of security may not be due to spillover, since indirect participants' perceptions of security improved more than direct participants'. As we explain above, direct participants may have been more aware of security incidents through the peace committees, causing perceptions of security to be lower for direct than indirect participants.

who did not participate in a committee, and those in non-ECPN communities. Any differences that did exist largely argued against a self-selection bias, with people in ECPN communities—both direct and indirect participants—having less intergroup contact at baseline than those in non-ECPN communities.

As a result, we feel relatively confident in our findings related to ways in which participation in the program benefited those who participated most. Across most factors, we see that direct participants often changed the most positively on factors such as trust, social cohesion, perceptions of security, and effectiveness of dispute resolution, while indirect participants largely stayed the same or improved somewhat, and individuals in control communities stayed the same or deteriorated. We cannot rule out that there is something different about these committee members compared with others—perhaps their willingness to be involved in the committees meant they were more open to being influenced by the program. However, we believe that examining dosage by measuring the level of participation in these programs helped us better identify whether such programs work, for whom, and under what conditions.

The Role of Mediation

Unexpectedly, we did not find direct effects for mediation. At the community level, we found the opposite of what we expected. At the individual level, direct participants' opinions of the success of dispute resolution processes rose more from the beginning to the end of the program than did the opinions of indirect and control participants, the latter of which showed no difference from beginning to end.

As explained above, one reason for these trends is the same reason behind the null results for CDD programs— only those who directly benefit from the mediation are aware of its success. While there were over 500 successful mediations across the 10 intervention sites during the program, many people may not have been aware that disputes were being resolved by community mediators. This may be why we see improvement in the opinions of direct participants but not those of others.

However, mediation may still have indirectly affected the various outcomes, such as trust and perceptions of security. Hartman, Blair, and Blattman (2018) found that while there were fewer violent disputes in the communities where their alternative dispute resolution program was active, people in the community did not report increased mediation skills. For ECPN, the mediators may have prevented disputes from erupting into violence, the lack of outbreaks of violence may have partially contributed to people's perceptions of security, but people were unaware of why disputes did not erupt, so when asked, they did not credit the mediation program. Because our interventions were bundled with the various other components, we are not able to attribute any change (or lack thereof) to any one component of the program. Future research should more explicitly explore the effects of mediation programs on direct and indirect participants so we can better identify the degree to which the mediation component contributed to various outcomes, and for whom.

Behavior versus Attitude Change

To address the differences between attitudes and behaviors, and to overcome the challenges surrounding self-reported measures, we examined both sets of outcomes. Overall, we do see shifts in attitudes in the expected direction, except for the list experiment. However, for behaviors, our results are more mixed. In the public goods game (PGG), which measured willingness to cooperate, ECPN communities and participants contributed less of their gift to the public fund than did controls. This result is in contrast to our other measures of behavior change—survey and observations of interactions at the market—where we do see an increase in interactions within ECPN communities.

Why did the shift in behavior as measured by the PGG not correspond to the shift in attitudes measured by survey responses? Scacco and Warren (2018) found the opposite in their contact study with youth in the urban areas of the Middle Belt: behavior shifted but attitudes did not, though it was the behavior of homogeneous groups that changed, not that of mixed groups. Our study does not have a similar comparison, so we do not know if we would have seen more giving if the PGG had benefited only one's own group, which is likely, given the spike in violence (Bauer et al. 2016).

Anecdotal evidence suggests that the PGG did not measure the willingness of these communities to cooperate. The community that donated the most money per person—a control community—had such poor farmer-pastoralist relations that neither group trusted the other to hold onto the money while their intercommunity project was decided and implemented. Mercy Corps had to keep the money and distribute it as the communities were implementing the project. Moreover, an intervention community where the farmers had defended the pastoralists from the anti–open grazing "livestock guards" was among the lowest contributors to the PGG. While others have found more of a correlation between PGGs and real world behavior, including within farming communities in Uganda (Grossman and Baldassari 2012), it seems not to be the case here.

One theory is that control communities may have wanted to signal their ability to focus money on community-wide needs in order to attract even more money from Mercy Corps. Intervention communities and direct participants would not feel this pressure because they (1) were already receiving a Mercy Corps program and (2) had already proven they could successfully implement a joint-community project. Further, separating the PGG measure from the endline survey may have made it easier to detach the PGG from Mercy Corps, removing the incentive for people to signal financial responsibility to Mercy Corps. Also, given the amount of variance in the measure, it could be that people were somewhat confused and did not fully understand what was being asked, although the protocol was piloted in two communities before we began the data collection.

That said, we also need to think about how these types of interventions shape attitudes and behavior separately. Paler, Marshall, and Atallah (2018) found shifts in attitudes but not behaviors as a result of intersectarian dialogue in Lebanon. Although that study used one-time meetings, compared with our two-year program, it is possible that these explicit peacebuilding programs change attitudes more consistently than behavior since people are aware of the socially desirable answer and are less able to control their behaviors (though this is debated). In contrast, Scacco and Warren's (2018) study was not advertised as a peacebuilding program. Whether our findings on attitudes are demand effects from peacebuilding programs or true attitude change needs to be further studied, as well as how to design programs that simultaneously address behaviors and attitudes.

Recommendations

As debates about the effectiveness of community-level peacebuilding programs continue, this evaluation demonstrates that programs that bring people into contact with one another, across lines of division, even in the midst of violent conflict, are effective at changing attitudes, perceptions, and behaviors. While the results are somewhat mixed—not all outcomes were statistically significant, and some trended in a direction opposite the one predicted—overall the trends showed that the peacebuilding intervention had a positive effect. As donors decide how to invest their resources to foster peace and implementers decide how to enact peacebuilding programs based on theories similar to the ones behind ECPN, we recommend they consider the following:

1   **Increase investments in programs that facilitate positive contact between groups in active conflict.** This study demonstrates that contact theory–based peacebuilding programs can support communities to maintain or improve security despite a broader escalation of violence. On multiple measures, the ECPN program had a positive effect on peacebuilding outcomes compared with outcomes at control sites. That intervention sites and ECPN participants' attitudes improved or stayed steady is especially noteworthy, given heightened tensions and a regionwide uptick in violence during the period of final evaluation data collection. Donors should increase their investments in these relatively low-cost interventions (i.e., approximately $60 per direct participant—and much less per person given the ripple effects in the communities) to build communities' resilience to being drawn into violence during periods of intense conflict.

2   Pair community-level interventions with robust advocacy and government engagement campaigns to promote policies conducive to peace. Evidence from this study indicates that ECPN either made an impact on peace in intervention communities despite the policy environment and broader conflict context, or at least kept communities from being pulled into the broader conflict. For example, pastoralists' contact with farmers in Benue decreased far less in ECPN communities than in control communities. Donor-funded programs can go only so far in effecting change if government officials enact policies that sow divisions, such as the anti–open grazing law in Benue, or poorly implement policies that support peaceful relations, such as the establishment of representative peace committees. Peacebuilding program investments should therefore go beyond contact-focused, intercommunity-level efforts and incorporate strategies for strengthening policies that promote peace. Activities for staff and partners might include actively engaging with government officials to generate and share evidence on drivers of and responses to conflict, sharing with government officials ways in which successful peace policies have worked elsewhere, and building coalitions with other civil society actors to advocate for policy change. Combining local interventions, such as community-based peacebuilding programming, with state-level policy-oriented programming can help to both stem the negative impacts of the current conflict and prevent future conflict.

3   Design interventions to maximize the ripple effect from direct participants to the broader community. Encouragingly, the study showed that People-to-People activities had a positive effect beyond the specific individuals engaged, creating positive ripple or spillover effects into the broader community. This finding bolsters the case that contact theory–based programming presents high value for the money. At the same time, programmatic ripples do not happen automatically, as we saw from the mediation results, and therefore should be deliberately incorporated into the program design. While it is unclear how these ripple effects happened—through changing norms or role modeling, for example—future programs should be sure to examine direct and indirect participants separately to understand the extent to which change spreads. Additionally, to intensify change in the broader community, more deliberate activities need to be included in the program design, such as direct participants' publicizing instances of cooperation between groups or of successful dispute resolution. From our results, it is clear that program interventions' logic should clearly articulate the intended ripple effects and program activities should be built accordingly.

4   Invest in larger-scale, rigorous impact evaluations of peacebuilding programming. Absent a rigorous impact evaluation with control communities and participants—creating a counterfactual of what would have occurred without ECPN—the program would have appeared to have little or no impact on peace outcomes. A simple baseline-to-endline comparison would have yielded small or no gains across intervention groups on most measures. Only in contrast with the counterfactual is it clear that the program substantially improved or helped maintain relationships between communities. This lesson learned from our study means that more investment is needed to rigorously measure the impact of peacebuilding programs in order to (1) increase learning among practitioners and donors on how to implement peacebuilding programs effectively and which approaches present the best return on investment, and (2) make the case for continued and increased funding for peacebuilding interventions.

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Appendix 1: Details of Sample Creation

The scoping exercise initially identified more than 30 potential implementation sites with a history of violence. The Engaging Communities for Peace in Nigeria (ECPN) implementation team visited these sites to establish community need and obtain community consent for potentially becoming part of the ECPN program. From these visits, we narrowed down the list of implementation sites to 24. At these 24 sites, we conducted a preliminary survey of 10 individuals per farmer and pastoralist community to further identify need. This survey revealed 1 site that was too close geographically to a larger site, 1 site that was too remote for feasible implementation, and 4 sites that did not fulfill our "demonstrated need" criteria. Of the remaining 18 sites, 3 were lost before random assignment or program implementation. From the 15 remaining sites, we randomly assigned 10 to intervention—that is, implementation of ECPN—and 5 to control—that is, no ECPN.

Appendix 2: Listing Exercise

We used a listing exercise to create a sampling frame from which to randomly select survey respondents. The specific details of the listing exercise differed depending on the context of the community we were surveying, as explained below.

Once we selected a household, we randomly selected one respondent, 18 or older, from within the household for the survey. Respondents within households were selected by (1) allowing each potential respondent to choose a number from 1 to n, where n was the number of potential respondents in the household, and (2) using a random number generator to select one of those numbers.

Farming Towns/Villages

In the farming towns/villages, enumerators created a numbered list of every household in the town and labeled each household with chalk.

The enumerators started in the center of a town and were grouped into four pairs. Each pair moved in one of the four cardinal directions, mapping the area as they walked. The groups labeled each house they passed with chalk, drew each house on a map, and labeled the map with other landmarks, such as shops, trees, mosques/churches, and schools. Once each team had labeled every house in the assigned direction, they moved toward the right and continued labeling until they reached houses that had already been labeled by another team.

Through this procedure, we created a list of every household in a town.[39] We then randomly selected 50 households to be surveyed. If a selected household refused, enumerators sampled the next household that had not been selected. Refusals were very rare.

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39   Towns generally encompassed about 250 households. We listed only households living inside the town, not those outside but near the town, even if they frequently visited the town market.

Pastoralist *Rugas*

In the pastoralist *rugas* (encampments), enumerators created a numbered list of every household affiliated with the *ruga* and labeled each household with chalk.

Unlike farming villages, pastoralist *rugas* generally include several settlements. The settlements are affiliated with the *ruga*, refer to the *ruga* by a common name, and pay allegiance to the same traditional leader (*ardo*). The settlements, however, are separated by open land and connected by walking paths.

To map the *rugas*, the enumeration teams first visited the main pastoralist settlement and learned from the *ardo* the locations of the other settlements. Enumerators, in pairs, then went to the settlements listed by the *ardo* and listed, labeled, and mapped all of the households in each settlement.

Through this procedure, we created a list of every household in a *ruga*.[40] We then randomly selected 50 households to be surveyed. If a selected household refused, enumerators sampled the next household that had not been selected. Refusals were very rare.

Internally Displaced Peoples' Camps

Some of our communities had been displaced by violence by the time of the endline survey. To locate these communities, we first identified the internally displaced peoples' camp or camps to which the respondents had fled. We then visited these camps and created a list of every adult respondent we could identify from the community. We randomly selected 50 of these potential respondents directly, rather than in the two-step procedure we used for farming villages and pastoralist *rugas* (i.e., selecting the household and then selecting respondents from within the household).

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40   *Rugas* were generally smaller than farming towns, the largest *ruga* containing only 192 households. We listed only settlements associated with the target *ruga*, not households from other *rugas*, even if they interacted with people from our *ruga* of interest.

Appendix 3: Survey Tool

Part 1: Pre-interview data: The interviewer should fill this out with the team leader before the interview.

Part 2: Informed consent: The interviewer should read this aloud exactly as it is below.

❯ **Hello. My name is** _____ **and I work with Mercy Corps. Mercy Corps is an international nongovernmental organization that works for economic development and natural resource management in this state. We are conducting a survey of households and have randomly selected yours.** Members we spoke to came from diverse backgrounds: some had jobs, and others did not; some had attended secular school, others Islamic school, and others had dropped out. Profiling in youth interventions based on demographics is unlikely to be successful. The purpose of the survey is to learn more about your community so that we can improve our programs in the area. Participation in the survey is voluntary and you are free to decline to answer any or all questions. The results will be kept confidential—your responses are private and will only be used by the research team to help Mercy Corps do better work in this area. You will be given a participant identification number and your responses will be linked to that number so that your responses are not linked to your name. This survey usually takes about 30 minutes to complete.

To help Mercy Corps improve our programs in your area, we'd also like to ask you to participate in a research study. The study will ask you to participate in a Mercy Corps program for the next 18 months. Participation will entail answering survey questions again in 18 months and possibly once more between now and then. Participation may also entail monthly meetings with other members of your community and working with those members of your community on community projects. In return for participation, all participants are included in a lottery to win things such as cell phone credit or household goods. If at any point during this survey or the subsequent study you would no longer like to participate, you are free to opt out with no consequences.

If you have any questions or comments, please contact Claire Ugo-Ike at [phone] or [email]). Here is a card with that information for you.

Will you participate in this survey?   YES   NO

Signature of interviewer _____

Part 3: Demographic information: The interviewer should read aloud: I'd like to start by learning a little bit about you. Please remember that your responses will be kept confidential.

#3	Category/Question	Answers (and Coding)	Instructions
3.1	Respondent's sex	Male .... 1 Female 2	
3.2	How old are you?	_____ Years Don't Know (Provide range.)................... 2 Refused 3	
3.3	What is your ethnic group? *Do not read the list of options.* *Circle one only.*	Fulani .... 1 Hausa .... 2 Tiv.. 3 Idoma.. 4 Gwandara.................................. 5 Other *(Specify.)* 6	
3.4	What is your religion?	Muslim .... 1 Christian..................................... 2 Other *(Specify.)* 3	
3.5	What is the primary way you make a living? *Do not read the list of options.* *Circle one only.*	Crop farming............................ 1 Pastoralism................................ 2 Crop farming and pastoralism equally .... 3 Trading...................................... 4 Other *(Specify.)* 5	
3.6	How often do you listen to the radio? Do you listen daily, a few times a week, at least once a week, at least once a month, or not even every month?	Daily.. 1 A few times a week.................. 2 Once a week............................. 3 Once a month .... 4 Less than monthly..................... 5	
3.7	What was your approximate total household income last month, in naira? *If respondent can only provide a yearly income, please divide by 12 and write in the box.*		
3.8	What was your approximate total household income last year, in naira?		

Part 4: Intergroup Cohesion: The interviewer should read aloud: Thank you for answering those questions about yourself. Now I have some questions about your community.

#4	Category/Question	Answers (and Coding)	Instructions
4.1	I understand your community has violent clashes or disputes with another group from time to time. What is the name of the primary (main) group your community has clashes or disputes with? *Elicit the name of one group (i.e., Fulani, Muslim, Christian, Hausa, Berom, Tiv, farmer, herder), and write down exactly the word they use.*		
	Now I'm going to ask you questions about your community here in Benue/Nasarawa, including X group. Please answer honestly and remember that your responses will remain confidential.		
4.2	Please tell me how strongly you agree/disagree with each of the following statements:		
4.2a	People around here are willing to help their neighbors across ethnic and religious lines.	Strongly agree ....1 Somewhat agree......................2 Somewhat disagree.................3 Strongly disagree....................4	
4.2b	People in this area can be trusted.	Strongly agree ....1 Somewhat agree......................2 Somewhat disagree.................3 Strongly disagree....................4	
4.2c	People in this area generally do not get along together.	Strongly agree ....1 Somewhat agree......................2 Somewhat disagree.................3 Strongly disagree....................4	
4.2d	This is a close-knit area.	Strongly agree ....1 Somewhat agree......................2 Somewhat disagree.................3 Strongly disagree....................4	

#4	Category/Question	Answers (and Coding)	Instructions
4.2e	People in this area do not share the same values.	Strongly agree ....1 Somewhat agree......................2 Somewhat disagree.................3 Strongly disagree....................4	
4.2f	People in this area see the benefits of working together to achieve common goals.	Strongly agree ....1 Somewhat agree......................2 Somewhat disagree.................3 Strongly disagree....................4	
4.2g	What proportion of your group in this area contribute time or money toward common development goals, such as building a levy or repairing a road?	Very likely ....1 Somewhat likely......................2 Neither likely or unlikely..........3 Somewhat unlikely...................4 Very unlikely............................5	
4.2h	What proportion of X group in this area contribute time or money toward common development goals, such as building a levy or repairing a road?	Everyone...................................1 More than half ....2 About half................................3 less than half............................4 no one......................................5	
4.2i	If there was a water supply problem in this community, how likely is it that people from your group and people from X group would cooperate to try to solve the problem?	Very likely ....1 Somewhat likely......................2 Neither likely or unlikely..........3 Somewhat unlikely...................4 Very unlikely............................5	
4.2j	Suppose something unfortunate happened to someone in this community from X group, such as a serious illness, or the death of a parent. How likely is it that some people in the community from your group would get together to help them?	Very likely ....1 Somewhat likely......................2 Neither likely or unlikely..........3 Somewhat unlikely...................4 Very unlikely............................5	
4.2k	Suppose something unfortunate happened to someone in this community from your group, such as a serious illness, or the death of a parent. How likely is it that some people in the community from X group would get together to help them?	Very likely ....1 Somewhat likely......................2 Neither likely or unlikely..........3 Somewhat unlikely...................4 Very unlikely............................5	

Part 5: Trust: I'm going to ask you some more questions about groups in your area.

#5	Category/Question	Answers (and Coding)	Instructions
	Trust		
5.1	On a scale from 1–5, how much do you trust people from an ethnic group different than your own? On this scale, 5 means "trust completely" and 1 means "do not trust at all."	Do not trust at all......................1 2 3 4 Trust completely.......................5 Refuse to answer......................9	
5.2	On a scale from 1–5, how much do you trust people from a different religion than your own? On this scale 5 means "trust completely" and 1 means "do not trust at all."	Do not trust at all......................1 2 3 4 Trust completely.......................5 Refuse to answer......................9	
5.3	On a scale from 1–5, how much do you trust people from X group in your area? On this scale 5 means "trust completely" and 1 means "do not trust at all."	Do not trust at all......................1 2 3 4 Trust completely.......................5 Refuse to answer......................9	
5.4	In regard to someone from X group, would you feel comfortable:		
5.4a	❭ If they worked in your [field]? *Contextualize per country or situation context. Broad category is livelihood location.*	Completely comfortable...........1 Comfortable ....2 Neither comfortable nor uncomfortable ....3 Uncomfotable ....4 Completely uncomfortable .....5 Not applicable...................... 99	*Only for respondents who grow crops*
5.4a2	❭ If you had to say, would you feel:	Somewhat comfortable............1 Somewhat uncomfortable.......2	*If respondent gives neutral option*
5.4b	❭ Paying them to [watch your animals]? *Contextualize per country or situation context. Broad category is livelihood location.*	Completely comfortable...........1 Comfortable ....2 Neither comfortable nor uncomfortable ....3 Uncomfotable ....4 Completely uncomfortable .....5 Not applicable...................... 99	*Only for respondents who raise animals*

#5	Category/Question	Answers (and Coding)	Instructions
5.4b2	❭ If you had to say, would you feel:	Somewhat comfortable .... 1 Somewhat uncomfortable 2	
5.4c	❭ ...trading goods with them?	Completely comfortable .... 1 Comfortable .... 2 Neither comfortable nor uncomfortable .... 3 Uncomfotable .... 4 Completely uncomfortable ..... 5 Not applicable 99	
5.4c2	❭ If you had to say, would you feel:	Somewhat comfortable .... 1 Somewhat uncomfortable 2	*If respondent gives neutral option*
5.4d	❭ ...sharing a meal with them?	Completely comfortable .... 1 Comfortable .... 2 Neither comfortable nor uncomfortable .... 3 Uncomfotable .... 4 Completely uncomfortable ..... 5 Not applicable 99	
5.4d2	❭ If you had to say, would you feel:	Somewhat comfortable .... 1 Somewhat uncomfortable 2	*If respondent gives neutral option*
5.4e	❭ ...with a close relative marrying a person from X group?	Completely comfortable .... 1 Comfortable .... 2 Neither comfortable nor uncomfortable .... 3 Uncomfotable .... 4 Completely uncomfortable ..... 5 Not applicable 99	
5.4e2	❭ If you had to say, would you feel:	Somewhat comfortable .... 1 Somewhat uncomfortable 2	*If respondent gives neutral option*

#5	Category/Question	Answers (and Coding)	Instructions
5.5	Please tell me the extent to which you agree or disagree with each of the following statements.		
5.5a	❱ You currently benefit economically from cooperating with members of X group. Do you: *Read the list of options, except "neutral," again if needed.*	Strongly agree ....1 Agree ....2 Neutral......................................3 Disagree ....4 Strongly disagree....................5	
5.5b	❱ You would benefit economically more than you currently do, with members of X group, if there were peace between your communities. Do you: *Read the list of options, except "neutral," again if needed.*	Strongly agree ....1 Agree ....2 Neutral......................................3 Disagree ....4 Strongly disagree....................5	
5.5c	❱ I would personally commit to peace with X group, even if members of X group used violence against my group. *Read the list of options, except "neutral," again if needed.*	Strongly agree ....1 Agree ....2 Neutral......................................3 Disagree ....4 Strongly disagree....................5	
5.6a *(Randomize)*	Think about groups that you might join in your leisure time. Would you join a group that had [Randomize: 5%/25%/50%/75%] X group members? *If clarification needed, "like a group that meets to play football every week."*	Yes...1 No..2	
5.6b *(Randomize)*	Think about the community you live in. Would you live in a community that had [Randomize: 5%/25%/50%/75%] X group members?	Yes...1 No..2	

#5	Category/Question	Answers (and Coding)	Instructions
5.7	❯ Now I'd like to ask you a few more questions about your feelings toward X group.		
5.8	❯ Some people say X group is responsible for most of the violence in my community, while others say that both groups are responsible for the violence here. Which is closer to your view?	X group is responsible .... 1 Both groups responsible 2	
5.9	I see X group as a threat to my community.	Strongly agree .... 1 Agree .... 2 Neutral .... 3 Disagree .... 4 Strongly disagree 5	
5.10	I think X group has too much influence on my community.	Strongly agree .... 1 Agree .... 2 Neutral .... 3 Disagree .... 4 Strongly disagree 5	
5.11	I think that people from X group have different values than people from my group.	Strongly agree .... 1 Agree .... 2 Neutral .... 3 Disagree .... 4 Strongly disagree 5	

Part 6: Intergroup Contact: Read aloud: Now I'm going to ask you questions about your contact with X group in your area.

#6	Category/Question	Answers (and Coding)	Instructions
6.1a	Think of the market you go to most frequently. During the past month, have members of X group gone to that market too?	Yes .... 1 No .... 2 Don't know 2	
6.1b	In the past month, how many times did you interact with X group in the market?	Numeric	

#6	Category/Question	Answers (and Coding)	Instructions
6.1c	How did you interact? *Encourage and record a brief response, but be specific on types of interaction (if economic, ask whether they bought/sold goods, worked together, etc.)*	Economic/trade......................1 Socializing..............................2 Other *(Specify.)* 3	
6.1d	❯ Were the interactions mostly: *Read the list of options, except "Neither positive nor negative."*	Very positive............................1 Somewhat positive..................2 Neither positive nor negative..3 Somewhat negative................4 Very negative 5	
6.2	In the past month, have you interacted with members of X group outside the market?	Yes...1 No..2	*If no, skip to 6.4.*
6.3	Have you:		
6.3a	❯ Joined a member of X group for a social event outside the home? How often?	Numeric	
6.3b	❯ Hosted a member of X group for a social event in your home? How often?	Numeric	
6.3c	❯ Gone to the home of a member of X group for a social event? How often?	Numeric	
6.3d	❯ Interacted with members of X group in any other way in the past month?	Yes...1 No..2	*If no, skip to 6.4.*
6.3d1	How else have you interacted with members of X group? *Encourage and record a brief response.*		
6.4	Overall, would you say your interactions with X group are:	Very positive............................1 Somewhat positive..................2 Neither positive nor negative..3 Somewhat negative................4 Very negative 5	

Part 7: Perceptions of Security: Read aloud: I'm going to ask some questions about peace and security in your community.

#7	Category/Question	Answers (and Coding)	Instructions
	Perceived Threat		
7.1a	To your knowledge, in the last 6 months, were there any violent clashes or disputes in your community?	Yes..1 No...2	*If no, skip to 7.3.*
7.1b	❭ How many were there? *Press for a number.*		
7.1c	❭ About how long ago was the most recent clash?		
7.1d	❭ And in that clash, about how many people died? *Press for a number.*		
7.1e	❭ In the most recent clash, what was the main cause of violence? *Encourage and record a brief response.*		
7.1f	In any clash that occurred in the last 6 months, were you or anyone in your family negatively affected by an attack caused by X group? *Await Reply. Check all that apply.*	No..1 Yes, my work/business was interrupted/closed ....2 Yes, I was forced to leave my home/migrate.......................3 Yes, my property was damaged.......................4 Yes, a family member was injured.............................5 Yes, a family member was killed ....6 Refused .... 98 Don't know 99	

#7	Category/Question	Answers (and Coding)	Instructions
7.2	In the last 6 months, to what extent have people's ability to work/earn a living in your community been affected by violent clashes or disputes with other groups? Would you say: *Read the list of options to the right, except for "Neither small nor great extent."*	To a very great extent..............1 To a great extent ....2 Neither small nor great extent 3 To a small extent.......................4 To a very small extent ....5 Refuse to answer......................7	
7.3	In the last month, were there any areas that you avoided going to or through because of insecurity during the night?	Yes..1 No..2	
7.4	In the last month, were there any areas that you avoided going to or through because of insecurity, during the day?	Yes..1 No..2	
7.5	In the last 6 months, did insecurity ever prevent you from:		
7.5a	❭ Working when you wanted to work?	Yes..1 No..2 Not applicable.........................3	
7.5a1	❭ About how many days were you unable to work? *Solicit a number.*		
7.5b	❭ Going to the market.	Yes..1 No..2 Not applicable.........................3	
7.5c	❭ Getting water for the household.	Yes..1 No..2 Not applicable.........................3	
7.5d	❭ Going to your field/farm.	Yes..1 No..2 Not applicable.........................3	
7.5e	❭ Moving your animals to grazing areas.	Yes..1 No..2 Not applicable.........................3	

#7	Category/Question	Answers (and Coding)	Instructions
7.5f	❯ Moving your animals to water.	Yes..............................1 No................................2 Not applicable.........................3	
7.5g	❯ Earning money or going to work. *If same as market, field/farm, or moving animals select "Not applicable."*	Yes..............................1 No................................2 Not applicable.........................3	
7.5h	❯ Going to school	Yes..............................1 No................................2 Not applicable.........................3	

Part 8: Dispute Resolution: Read aloud: I'm going to ask some questions about shared resources. [A shared resource is anything that people need in order to make a living or provide for their families.]

#8	Category/Question	Answers (and Coding)	Instructions
8.1a1	Do you share markets with X group? *Use response from 4.4 to fill in name of group.*	Yes..............................1 No................................2	*Asked if no to market interaction (Q 7.1) If no, skip to 5.2a.*
8.1a2	Earlier you said you shared markets with X group, is that right?	Yes..............................1 No................................2	*Asked if no to market interaction (Q 7.1) If no, skip to 5.2a.*
8.1b	Does sharing the market with X group cause tension?	Yes..............................1 No................................2	
8.1c	Does sharing the market with X group cause disputes?	Yes..............................1 No................................2	
8.1d	❯ Would you say disputes between your community and X group in or over markets are resolved peacefully:	All of the time...........................1 Most of the time.......................2 Sometimes ....3 Rarely...4 Never..5	

#8	Category/Question	Answers (and Coding)	Instructions
8.1e	❭ Who usually resolves disputes between your community and X group in or over markets? Please name up to **three** types of people who resolve disputes the most frequently. ***Do not read the list.*** *Let the respondent speak; circle the first three people/institutions the respondent mentions. If the respondent does not name up to three, you may circle fewer than three responses.*	Nobody ....1 Friends or family members ......2 Elders/traditional leaders ....3 Local peacebuilding organizations ....4 Religious leaders....................5 Local government officials ......6 Law enforcement officials........7 Women ....8 Youth ....9 Other *(Specify.)* 10	
8.2a	Do you share pasture areas with X group?	Yes..1 No..2	*If no, skip to 5.2a.*
8.2b	Does sharing the pasture area with X group cause tension?	Yes..1 No..2	
8.2c	Does sharing the pasture area with X group cause disputes?	Yes..1 No..2	
8.2d	❭ Would you say disputes between your community and X group in or over markets are resolved peacefully: *Read the list of options, except for "Don't know" and "No disputes/ not applicable."*	All of the time...........................1 Most of the time........................2 Sometimes ....3 Rarely......................................4 Never.......................................5	
8.2e	❭ Who usually resolves disputes between your community and X group in or over pastures? Please name up to three types of people who resolve disputes the most frequently. ***Do not read the list.*** *Let the respondent speak; circle the first three people/institutions the respondent mentions. If the respondent does not name up to three, you may circle fewer than three responses.*	Nobody ....1 Friends or family members ......2 Elders/traditional leaders ....3 Local peacebuilding organizations ....4 Religious leaders....................5 Local government officials ......6 Law enforcement officials........7 Women ....8 Youth ....9 Other *(Specify.)* 10	
8.3a	Do you share farmland areas with X group?	Yes..1 No..2	*If no, skip to 8.4.*

#8	Category/Question	Answers (and Coding)	Instructions
8.3b	Does sharing farmland with X group cause tension?	Yes..............................1 No................................2	
8.3c	Does sharing farmland with X group cause disputes?	Yes..............................1 No................................2	
8.3d	❭ Would you say disputes between your community and X group in or over farmland areas are resolved peacefully: *Read the list of options, except for "Don't know" and "No disputes/ not applicable."*	All of the time..........................1 Most of the time.......................2 Sometimes ....3 Rarely.......................................4 Never..5	
8.3e	❭ Who usually resolves disputes between your community and X group in or over farmland? Please name up to three types of people who resolve disputes the most frequently. ***Do not read the list.*** *Let the respondent speak; circle the first three people/institutions the respondent mentions. If the respondent does not name up to three, you may circle fewer than three responses.*	Nobody....................................1 Friends or family members ......2 Elders/traditional leaders ....3 Local peacebuilding organizations........................4 Religious leaders.....................5 Local government officials ......6 Law enforcement officials........7 Women ....8 Youth ....9 Other *(Specify.)* 10	
8.4	To what extent do you agree or disagree with the following statement? "In general, my community manages shared natural resources, such as farmland and pasture areas, or water, peacefully." Do you: *Read the list of options, except for "neutral."*	Strongly agree ....1 Agree ....2 Neutral.....................................3 Disagree ....4 Strongly disagree....................5	

Part exp: Survey experiments: Read aloud: I'm going to ask you just a few more questions, and then we'll be done. Thank you!

#exp	Category/Question	Answers (and Coding)	Instructions
Exp1	I'm going to read you a list of items that anger or upset some people. I'd like you to tell me how many of these things upset you. Please don't tell me which items upset you, just how many of them upset you. 1 When your football team loses a match 2 Increases in the price of gasoline 3 Lack of rainfall 4 When you have to interact with a member of X group in the market Remember, don't tell me which items upset you, just how many.	Numeric	
Exp2a	Imagine that there is a proposal **[by a farmers' cooperative society/MACBAN[41]]** for action to enhance access to clean water in rural areas. Though expensive, the proposal aims to build water pipelines and dig wells to bring fresh, clean water to hundreds of areas without access to it, including this one. If this were proposed, how would you feel about it?	Strongly support.......................1 Somewhat support...................2 Indifferent.................................3 Somewhat oppose...................4 Strongly oppose.......................5	
Exp2b	Imagine that there is a proposal for action to enhance access to clean water in rural areas. Though expensive, the proposal aims to build water pipelines and dig wells to bring fresh, clean water to hundreds of areas without access to it, including this one. If this were proposed, how would you feel about it?	Strongly support.......................1 Somewhat support...................2 Indifferent.................................3 Somewhat oppose...................4 Strongly oppose.......................5	

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41   MACBAN is the Miyetti Allah Cattle Breeders Association of Nigeria.

Appendix 4:
Balance and Placebo Tests

Individual-Level Balance and Placebo Checks

Since our individual-level endline data were based on a convenience sample of our baseline data (i.e., those we could find again, a sample that was limited due to displacement), one concern with these data is that the results might apply only to the type of person we resurveyed at endline. Another concern is that the type of person we were able to resurvey in each group (direct participants, indirect participants, and controls) may be different from the type of person we were able to resurvey in the other groups, i.e., direct participants we resurveyed are some way different than resurveyed indirect participants. To address these concerns, we conducted several analyses.

First, we compared the people we were able to resurvey with those we were not on demographics and baseline outcomes to ensure that our resurveyed respondents were representative of their respective overall groups on measured traits. Second, we compared baseline outcomes for participants, indirect participants, and controls to ensure that these respondents were comparable at baseline. And third, we compared all groups' baseline-endline change on two placebo outcomes to demonstrate that their baseline-endline trends would be the same in the absence of ECPN.

Differences between Individuals Resurveyed and Those Not Located at Endline

The types of people we could resurvey were not substantially different from the types of people we could not contact at endline. Compared with the average person surveyed at baseline, recontacted respondents were statistically different on only the age variable, by 2.15 years. No other group-level difference neared statistical significance—the preselected respondents scored the same on all of the study outcomes as well as on gender, and were only slightly older. An omnibus test of balance on all variables cannot reject the hypothesis that these groups are similar (p-value = .961).

Baseline Similarity of Direct Participants, Indirect Participants, and Controls

Recontacted respondents in each group were similar across baseline outcomes and on the two demographic traits we analyzed, age and gender. Though we saw that there was some selection on age into who was recontacted and who was not, that selection seems identical in each group. Two statistical differences do appear: controls had more intergroup contact at baseline relative to the other two groups, and direct participants were slightly less likely to be female than indirect participants.[42] Omnibus tests of balance on all variables cannot reject the hypothesis that the direct and indirect participants were similar at baseline (p-value = .151) or that the indirect participants and controls were similar at baseline (p-value = .242). With the test comparing direct participants with controls we can reject that the groups are similar, but only by a slim margin (p-value = .057) because the controls had more baseline intergroup contact than either group of participants. This difference actually works against us in finding changes; we were worried those selecting into the program already had more contact than controls, and therefore were open to interacting.

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42   The p-values describing these differences are not adjusted to account for the number of comparisons we made, since we are not trying to determine if these differences are greater than we would expect by chance.

Overall, all respondents in this baseline-endline analysis were very similar at baseline; in other words, there are few baseline differences between the groups.

Placebo Outcomes

For placebo tests, we chose two outcomes that ECPN should not affect: (1) radio listening and (2) using violence to force the government to change policies. All groups changed in the same way on these placebo outcomes. For radio listening, the p-value comparing controls with indirect participants is .585, the p-value comparing controls with direct participants is .813, and the p-value comparing direct with indirect participants is .654. For antigovernment violence, the p-value comparing controls with indirect participants is .614, the p-value comparing controls with direct participants is .932, and the p-value comparing direct with indirect participants is .487.

Overall, the respondents in each group changed in the same way on outcomes we would not expect ECPN to affect.

Appendix 5: Full Set of Analysis for Community- and Individual-level Effects

Community-Level

	var	coef	sd	ll	ul	size	ll_size	ul_size
1	Trust	1.264733e-01	0.259890516	1.710934e-02	0.253192121	0.48664063	0.06583286	0.97422609
2	Cohesion	5.530516e-02	0.223542011	-7.592486e-02	0.177184146	0.24740387	-0.33964471	0.79262124
3	PGG amount	-3.512350e+01	80.063813954	-1.202477e+02	44.209923441	0.43869382	-1.50189789	0.55218358
4	PGG donation	2.241997e-02	0.066647514	-2.040035e-02	0.073294821	0.3363617	-0.30609321	1.09973827
5	Contact	1.469304e-01	0.291078216	3.483853e-02	0.259838747	0.50477972	0.11968786	0.89267672
6	Percept security	1.471905e-01	0.175621488	2.599772e-02	0.283522277	0.83811219	0.14803271	1.61439400
7	Dispute res	-2.182983e-02	0.256410461	-2.009761e-01	0.149876265	-0.08513628	-0.78380618	0.58451697
8	List_exp	-1.172797e-01	0.198973007	-3.2022006e-01	0.077468580	-0.58942534	-1.60936723	0.38934216
9	Per exp	8.828630e-05	0.003506368	-1.406602e-03	0.001748275	0.02517885	-0.40115642	0.49860014
10	End_exp	1.225162e-01	0.559731514	-5.940775e-02	0.261071475	0.21888386	-0.10613615	0.46642268

Individual—Linear Effect

	var	sd	coef_ind	ll_ind	ul_ind	size_ind	size_ll_ind	size_ul_ind
1	Trust	0.3201090	0.047753009	-0.002952516	0.08507171	0.14917734	-0.009223472	0.2657586
2	Cohesion	0.1856819	0.010331885	-0.007822959	0.02576471	0.05564292	-0.042130965	0.1387572
3	PGG amount	186.2611776	-26.877687015	-80.254185769	28.52739342	-0.14430107	-0.430869099	0.1531580
4	PGG donation	0.1923211	0.012230931	-0.031001938	0.04905250	0.06359641	-0.161198842	0.2550552
5	Contact	0.2924184	0.065670335	0.014065424	0.11113410	0.22457663	0.048100341	0.3800517
6	Percept security	0.2281849	0.026518640	-0.017812301	0.05742978	0.11621556	-0.078060808	0.2516809
7	Dispute res	0.1985650	0.009126935	-0.018080214	0.03629786	0.04596447	-0.091054375	0.1828009

Individual—Separate Effects

var	sd	coef_ind	coef_dir	ll_ind	ul_ind	ll_dir	ul_dir	size_ind	size_ll_ind
1 Trust	0.3201090	0.044669633	0.09316147	-5.028565e-02	0.15429921	-8.706906e-03	0.17559482	0.139545073	-0.15708915
2 Cohesion	0.1856819	0.011103265	0.02058065	-4.032824e-02	0.05444691	-1.495646e-02	0.05155314	0.059797232	-0.21718989
3 PGG amount	186.2611776	-27.022870337	-53.73975316	-1.366453e+02	79.07092016	-1.671007e+02	66.85014788	-0.145080530	-0.73362191
4 PGG donation	0.1923211	0.050166101	0.02038026	-9.869252e-03	0.11653537	-5.483682e-02	0.09834371	0.260845551	-0.05131654
5 Contact	0.2924184	0.009115920	0.13742559	-9.450945e-02	0.09159235	2.266777e-02	0.21923634	0.031174237	-0.32319942
6 Percept security	0.2281849	0.065542481	0.04883854	-2.654145e-02	0.15755807	-1.735692e-02	0.12906491	0.287234036	-0.11631552
7 Dispute res	0.1985650	0.001442908	0.01908806	-5.203723e-02	0.05812379	-3.197649e-02	0.07793525	0.007266679	-0.26206646

	size_ul_non	size_part	size_ll_part	size_ul_part
1	0.4820208	0.29103047	-0.02719982	0.5485470
2	0.2932268	0.11083822	-0.08054880	0.2776422
3	0.4245164	-0.28851827	-0.89713103	0.3589054
4	0.6059417	0.10596997	-0.28513160	0.5113517
5	0.3132236	0.46996222	0.07751827	0.7497351
6	0.6904841	0.21403053	-0.07606515	0.5656154
7	0.2927192	0.09613001	-0.16103788	0.3924923

NOTE: dir = direct participant; ind = indirect participant.

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Mercy Corps is a leading global organization
powered by the belief that a better world is possible.
In disaster, in hardship, in more than 40 countries
around the world, we partner to put bold solutions
into action — helping people triumph over adversity
and build stronger communities from within.
Now, and for the future.