ECPN Final Evaluation PAP

Christopher Grady, Rebecca Wolfe, Lisa Inks, Dawop Saidu, John Daudu, Tefera Mekonnen, Rahama Baloni, and Nate Crossley

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Introduction

Clashes between farmer and pastoralist communities in Nigeria's Middle Belt states are increasingly violent and taking on religious and ethnic overtones that divide these communities even further. Due to the effects of climate change, underdevelopment, and massive displacement caused by extremist groups in the North, communities that traditionally interacted over land and natural resources are fast becoming polarized from each other. Farmer and pastoralist communities in the Middle Belt region face limited access to natural resources and land, negatively affecting their livelihood options and causes grievances to fuel more violence and instability in an environment where widespread poverty, poor governance and high corruption levels are already pervasive.

To address this, Mercy Corps is implementing the two-year Engaging Communities for Peace Nigeria 2-year program in the Middle Belt of Nigeria that aims to prevent violence and conflict between farmer and pastoralist communities. More specifically, the program 1) strengthens the capacity of farmer and pastoralist leaders to resolve disputes in an inclusive, sustainable manner; 2) leverages social and economic opportunities to build trust across lines of division; and 3) fosters engagement among farmer-pastoralist communities, local authorities and neighboring communities to prevent conflict.

To understand the impacts of the program, we conducted a two-level evaluation: 1) A RCT at the community level to understand how the overall intervention affects communities and 2) individual analyses to understand how one aspect of the program–joint project committees that foster contact–affects attitudes and behaviors toward outgroups.

Theory and Hypotheses

Th ECPN intervention is based on a number of social psychological theories that this study will field test. The main theory is the contact hypothesis. The contact hypothesis is the basis of many social interventions—from integrating classrooms to preventing intercommunal conflict. According to Allport's original theory, the conditions under which intergroup attitudes will improve include:

- Equal Status
- Common goals
- Intergroup cooperation
- Support of law, authorities or customs
- Personal interaction

Pettigrew and Tropp (2006) conducted a meta-analysis of 515 studies and found that intergroup contact reduces prejudice. Moreover, Paluck and Green (2009), who reviewed the contact hypothesis as well as other prejudice reducing interventions in both the lab and the field, found that actually there have been few field experiments examining the contact hypothesis, and the ones they note involve intense living arrangements during a camp or a dorm. A more recent review by Paluck, Green, and Green (2017) focused on field experiments did find an overall significant effect of contact, but this was largely in smaller studies. Additionally, studies focused on racial and ethnic prejudice had weaker effects.

In Kaduna–part of the Middle Belt of Nigeria– Scacco and Warren (2016) tested the contact hypothesis with Christian and Muslim youth in a computer training program. Youth were divided into either heterogeneous

or homogeneous classrooms, and within the heterogeneous classrooms, youth were either in homogeneous pairs or heterogeneous pairs. The main findings of the study were that while attitudes did not change due to contact, cooperative behavior did as measured by dictator and destruction games. This effect appears driven not so much that contact made people more cooperative towards the outgroup, but that those in homogeneous groups tended to favor their ingroup more.

Contact and creating opportunities for cooperation is the implicit theory behind many community-driven development programs (CDD). Yet, the effects of such programs are minimal, particularly on social outcomes. CDD purports that this process will lead to social cohesion Chase, Woolcock, and others (2005). Recent studies on the benefits of these programs have shown little evidence that the process builds social cohesion or social capital between groups in a range of contexts, from DRC, the Philippines, and Afghanistan King (2013). However, Fearon, Humphreys, and Weinstein (2009) found in Liberia that a CDD did improve cooperation among community members that were in mixed-gender groups as compared to all women's groups.

One potential reason that CDD programs may have little impact on social outcomes at the community level is that a small group of people make a decision for the whole community on a project that will benefit the whole community, but may not foster interaction between people. The hope is that if community leaders or a small group of people cooperate, others will see it and/or benefit from cooperation, thus changing their opinions of other member of the community or "outgroup." However, it is unclear if those spillover effects do occur and, if so, to what extent.

This study builds off this previous work in the following ways: 1) we examine contact within an ongoing conflict; 2) contact is sustained over multiple years rather than at most one year, and usually much less; 3) the CDD intervention is combined with more intentionality to bring communities from different groups in conflict together (i.e., there could be a ceiling effect in other CDD studies on social outcomes); and 4) communities receive a tangible, material benefit from working together. Below we specify our hypotheses at the community and individual level.

Community-level hypotheses:

1. For communities that receive the ECPN program, which includes a combination of mediation support, projects that a committee of farmers and pastoralists jointly decide on and implement, and community fora that prevents conflict from escalating,[1] we will find increased trust and positive affect between farmers and pastoralists than in control communities.
2. For communities that receive the ECPN program, we will find farmers and pastoralists are more likely to cooperate and interact with the outgroup than control.
3. For communities that receive the ECPN program, we will find farmers and pastoralists have more positive attitudes about the outgroup than control.
4. Communities that receive the ECPN program will have increased security compared to control communities.
5. Communities that receive the ECPN program will resolve disputes more peacefully compared to control communities.
6. Communities that receive the ECPN program will have fewer community members donate nothing during the Public Goods Game than communities that do not receive the ECPN program.[2]

Individual-level hypotheses:

1. Individuals involved in the planning and implementing of projects that benefit both farmers and pastoralists will cooperate with the outgroup more than individuals who did not participate in these activities.

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[1] After the first hypothesis, we use "ECPN" to refer to all the various ECPN activities: mediation, joint projects, community fora, etc.

[2] We also have a weak hypothesis about changes in the variance of donations, but we do not feel strongly about this hypothesis and believe it could go in either direction. We could observe lower variance because the treatment decreases the size of an individual's decision space – after treatment, people know about how much everyone in their area will give, and they conform to that amount. But we could observe higher variance because treatment changes the perception of the social norm for *some* people, leading to a distribution of donation amounts drawn from two distributions: the old social norm and the new social norm. If those norms are sufficiently far apart, it would increase variance. Unlike most of our tests, our test of variance will be two-tailed because are do not have expectations about the direction of the effect.

2. Individuals involved in the planning and implementing of projects that benefit both farmers and pastoralists will trust each other more and feel more positively towards the outgroup than individuals who did not participate in these activities.

3. Individuals involved in the planning and implementing of projects that benefit both farmers and pastoralists will interact more with the outgroup than individuals who did not participate in these activities.

4. Individuals involved in the planning and implementing of projects that benefit both farmers and pastoralists will have more positive attitudes about the outgroup than than individuals who did not participate in these activities.

5. Individuals involved in more ECPN activities will cooperate more, trust each other more, have more positive attitudes about the outgroup, and interact more than those who participated in fewer ECPN activities. More specifically, we will see the most change in participants who participated in the joint projects, followed by the non-participants in the treatment communities, and then the control participants who we expect to have no change.

Mechanisms Through Mediating Variables

We are interested in knowing not just *if* ECPN affected communities and individuals, but *how* ECPN affected communities and individuals. We believe ECPN may work through four possible mechanisms that we are able to test through our survey data. Those mechanisms are: (1) intergroup contact, (2) involvement with ECPN, (3) perceptions of benefit from ECPN and intergroup cooperation, (4) perceptions of outgroup threat.

ECPN might work through increasing intergroup contact. Farmers and pastoralists may more frequently interact due to ECPN, and this could decrease prejudice. This mechanism is the classic contact theory story: more contact caused each group to redefine the other in ways that are more positive and less discriminatory. This mechanism can work with the others – for example, an individual may find the other group less threatening through contact. But contact may also work through updating attitudes about the outgroup in ways we have not measured, and the other psychological mechanisms (threat and benefit) could work without an increase in contact.

ECPN might also work through involvement with ECPN. Full ECPN participants worked with members of the outgroup to implement joint-resource and quick-impact projects[3] and that intergroup contact might affect attitudes even if it did not increase contact in other contexts. Partial ECPN participants, though they did not work directly with members of the outgroup, attended an intergroup "conflict forum" where members of both groups aired grievances, proposed solutions, and discussed points of agreement. Through these specific types of structured intergroup contact, full or partial participation in ECPN may have facilitated attitude change.

ECPN may also work through two psychological intervening variables: decreased outgroup threat or increased perceptions of benefit from cooperating with the outgroup. ECPN may only affect other attitudes (such as prejudice and trust) if it first reduced perceptions of threat from the outgroup. When fear and threat decrease, cooperation may replace the "us vs. them" perception of many situations. Likewise, other attitudes may improve only when an ingroup member believes she and her group have benefited tangibly from working with the outgroup. Through ECPN, cooperating with the outgroup led to construction of a borehole and another development project. People who perceive personal or ingroup benefit from those projects may associate benefit from those projects with intergroup cooperation, thereby decreasing prejudice.

Overall Study Design of Final Evaluation

We evaluate the effects of ECPN with a combination of a RCT at the community level and pre-post test analysis of individuals without random assignment. Initially the plan was to randomize at both the community

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[3]The joint resource projects were always boreholes, and the quick-impact projects were small construction projects. For example, renovating or constructing a school building or a health care center that both groups may use.

and individual level as follows: (1) Communities are randomly selected to be "treated" with the ECPN program or remain "control" communities, and then (2) community members in treated communities are randomly selected to participate in the programs. Among participants selected to participate in ECPN programs, some people participate "fully" in every ECPN activity and others participate "partially" in just one ECPN activity. This yields four experimental groups: (1) full participants in treated communities, (2) partial participants in treated communities, (3) non-participants in treated communities, and (4) non-participants in control communities.

However, due to the low compliance[4] among those randomly selected to participate, we changed our study design to (1) a community-level RCT and (2) pre-post analysis of individuals we surveyed at baseline and endline. In Study 1, the community-level RCT, we randomly sample 50 people from 10 treated and 5 control sites, where each site contains one farmer and one pastoralist community. These samples are aggregated at the community level and our analysis is between communities in a difference-in-differences framework. In Study 2, the individual-level pre-post analysis, we resurvey about 100 individuals who participated in the joint project committees and were randomly assigned to do so, about 100 individuals who were assigned to participate in the joint committee but did not because they were not located at the time the committees formed,[5] and about 100 individuals in control sites. Henceforth we will refer to those people who participated as "participants", those people who did not as "non-participants", and those people in control communities as "control." Study 2 will contain multiple analyses comparing these groups with a difference-in-differences frameworks.

Study1 tells us about community-level change due to ECPN. Since communities were randomly assigned to receive ECPN, we can causally attribute community-level differences to ECPN. Study 2 has three analyses. The first analysis, comparing the baseline-endline change of the participants to the baseline-endline change of the control, tells us how participation in joint project committees with ingroup and outgroup members changed the individuals who participated. The second analysis, comparing the baseline-endline change of non-participants to the baseline-endline change of the control, tells us about the social diffusion of the effects of ECPN to community members who did not directly participate in joint project committees. The third analysis, comparing the baseline-endline change of participants to the baseline-endline change of non-participants, tell us if the participants changed more than the non-participants.

Communities are randomly assigned to receive or not receive ECPN, so control communities function as a counterfactual to treatment communities. Since the control group should have parallel trends with the treatment group if the treatment group was not treated, the community-level analysis of Study 1 uses a difference-in-differences design to compare the baseline-endline change in treatment communities to the baseline-endline change in control communities. In the analyses of study 2, individuals are in randomly assigned communities but are not themselves randomly assigned. As a counterfactual for the amount of change we would expect in a world where the participants were not treated with ECPN and the non-participants were not in close contact with people treated with ECPN, we will use the average change from baseline to endline of individuals in control communities from the same state as the treatment communities.

The average change of people in these control communities may not be a perfect counterfactual for how these individuals in the treatment communities would have changed absent ECPN. Though these groups were initially randomly selected, we are only resurveying the ~20% of each group's baseline respondents who we can locate and identify. The 20% of participants and 20% of nonparticipants we resurvey could be different types than the 20% of control participants we resurvey. To lend credibility to the claim that participants and nonparticipants in the ECPN communities are not "different types" than control group respondents (i.e. that respondents in control communities function as a counterfactual for respondents in treatment communities), we will (1) compare demographic balance of the participants, non-participants, and controls, and (2) provide evidence for parallel trends by conducting a placebo test on outcomes we do not expect ECPN to change.

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[4]In this case, lack of compliance was mainly our inability to locate these respondents at the time the joint project committees were being formed, not participants actively refusing to participate; it's that they could not be found for participation. These "passive non-compliers" were located before the endline survey with the assistance of the communities themselves.

[5]Since this group of people did not participate through assigned because they were not located at the time of assignment, we feel better about this group representing non-participating community members than we would if they had actively selected out of participation.

We measure impact in three ways: (1) survey data, (2) observational monitoring, and (3) a natural-field public goods behavioral game. The survey measures affect towards the farmer/pastoralist outgroup and many other attitudes that may influence affect towards that outgroup, such as trust. Surveys also measure self-reported interaction with the outgroup. Observational monitoring quantifies qualitative evidence regarding interaction between farmers and pastoralists in these communities. And the public goods game offers a behavioral measure of cooperation between farmers and pastoralists.

Research Design and Data Sources

This section summarizes the data sources and analytic strategy for evaluating ECPN with these data sources.

Research Design

We are interested in the change in attitudes, levels of interaction, cooperation, and perceptions of security from baseline to endline. More specifically, we are interested in the *difference* in the amounts of change between treatment communities and control communities, as well as those that participated fully in the joint project committees and those who did not. This is called a "difference-in-differences" (DID) design. A basic DiD design is: separate subjects into a treatment and a control group, measure pre-treatment outcomes for both groups, administer treatment only to the treatment group, measure post-treatment outcomes for both groups. The ECPN community-level study is exactly this design. The main individual-level study for ECPN differs in that individuals are not randomly assigned into treatment or control.

A difference-in-differences analysis is vital when comparing across time. From 2015-2017 many things change for the survey respondents. One glaring difference is the state of the Nigerian economy, which fell deeply into recession in 2016. Another is the institution in late 2017 of an anti-grazing law and the resulting violence in Benue state, one of the states for the intervention. Changes over time could lead to changes in how respondents answer survey questions, and we would not want to confuse those "time changes" with changes that are due to ECPN.[6] The difference-in-differences analysis lets us capture all changes *not* due to ECPN, and observe if ECPN causes additional changes.

Since we have hypotheses that the program will *improve* attitudes, we conduct one-tailed tests. We are not predicting that the ECPN program *changes* the mean (or other test statistic), we are predicting that ECPN will *improve* the test statistic. This type of strong hypothesis is represented by a one-tailed "greater than" test.

Analysis – Community Level Difference-in-Differences

We will use the DID framework to estimate the effect of ECPN with our survey data. The DID regression equation, without covariate adjustment, is:

$$Outcome = \beta_0 + \beta_{ECPN} + \beta_{2018} + \beta_{ECPNx2018} + \epsilon$$

In that equation the $ECPNx2018$ coefficient is our outcome of interest, representing the amount of change in ECPN sites above the amount of change in control sites. This is identical to a $T-test$ comparing the difference in change of two group means. For a concrete example, consider outcome values from 0-10 where high numbers are desirable. Imagine that ECPN sites score 6 in 2015 and 9 in 2018, and that Control sites score 7 in 2015 and 6 in 2018. The ECPN sites improve three points, but the overall effect is *four* points because the Control sites regress one point:

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[6]Other changes can also lead to a shift in survey responses and other outcome measures, even if underlying attitudes remain the same. For example, different enumerators generally lead to slightly different survey responses. In the USA this is especially pronounced with respect to questions about racial tolerance. We would not want to confuse enumerator differences for a treatment effect.

$$Effect = (ECPN_{2018} - ECPN_{2015}) - (Control_{2018} - Control_{2015})$$

$$Effect = (9 - 6) - (6 - 7)$$

$$Effect = 3 + 1 = 4$$

These equations are for a site-level analysis.

The data for the community-level analysis comes from aggregating together the survey responses from 50 randomly selected respondents in each community. The survey responses will be made into indices to increase measurement precision and reduce error. The questions used in each index are included as an appendix. To construct the index for each concept we will generally use inverse covariance weighting[7], since we believe each index measures an underlying concept and the questions inform us about that concept to the extent that they provide new information about the respondent.

Since we have individual-level data and covariates, the next step is to produce outcomes that control for individual-level noise through covariance adjustment (Rosenbaum and others 2002). Since treatment at the community level is assigned randomly, in expectation covariance adjustment will not affect the relationship between treatment and outcomes, but will reduce the error around the outcome and increase statistical power. After individual-level covariance adjustment, each community will be assigned their community mean value for each index.[8]

The final step is compare treatment to control communities across the range of outcomes that we expect to be impacted by ECPN. Rather than testing each outcome sequentially, we will test them simultaneously using the NPC procedure presented by (Caughey, Dafoe, and Seawright 2017).[9] By testing each outcome simultaneously, we better represent our hypothesis that ECPN will affect all of these outcomes and we gain statistical power to the extent that these outcomes are uncorrelated with each other. Because multiple outcomes are less likely to be randomly correlated with our treatment variable than a single outcome, we can gain statistical power by testing multiple outcomes simultaneously. This level will include a state-level blocking variable because our treatment assignment was block-randomized by state. Lastly, we plan to use one-tailed tests because we only predict that our intervention will improve attitudes and behaviors.

In addition to the DiD analysis, we will use our observational data to examine any of the trends we see in the treatment sites. These will be panel data for each treatment community, beginning shortly after the baseline and ending shortly before the endline.[10] This data will lend itself to some descriptive statistics and qualitative understanding of change.

Analysis – Individual Level Panel Data

The data for our individual-level analyses comes from surveying the same ~300 respondents at baseline and endline. At baseline respondents were randomly sampled from communities and about 20% of those respondents will be surveyed again at endline.[11] These 300 contain respondents in control communities, respondents who were assigned to participate in ECPN committees and did, and respondents who were assigned to participate in ECPN committees but were not located in time to include them on committees. The data for these respondents is constructed in the same way as the data for people randomly selected at endline (i.e. we will construct the same indices in the same way).

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[7]Inverse covariance weighting upweights questions that are uncorrelated with other questions of the index and downweights questions that are highly correlated. Compared with other ways of constructing indices, it should maximize precision if all index questions measure the same concept but drastically reduce precision if a question used to create the index is unrelated to the underlying concept.

[8]An alternative analysis strategy would be to more directly account for community-level uncertainty around the community mean. This would be done by bootstrapping within communities to obtain community-level confidence intervals and then conducting permutation tests across villages.

[9]We have also considered testing hypotheses about different outcomes in order as in Rosenbaum (2008), but we do not have strong beliefs that some outcomes will be affected more strongly than others.

[10]Due to funding issues, the observational data has a several month gap in the middle.

[11]Due to resource constraints, we and the field team have only attempted to locate and identify respondents assigned to the full or partial participant group in our original study design, and respondents in control communities.

With these data, we will make three comparisons: (1) participants compared to controls, (2) non-participants compared to controls, and (3) participants compared to non-participants. Our primary interest is the average amount of change in outcome indices from baseline to endline in each of the treatment groups. The average amount of change in the participants group relative to controls represents the effect of participating in ECPN, and the average amount of change in the non-participants group relative to controls represents the effect of ECPN on people in the community who did not participate directly in ECPN. Instead of covariates to absorb error, individual-level error should be minimized by analyzing the *change* from baseline to endline. Since we are only looking at an effect within individuals, characteristics of those individuals are controlled for. As with the community-level study, we plan to use one-tailed tests with the individual-level studies because we only predict that our intervention will improve attitudes and behaviors.

We hypothesize that the participants will display greater attitude change than the nonparticipants, and that the nonparticipants will display greater attitude change than the control group. We can leverage our expectations about this order of effects using Ordered Hypothesis Testing (Rosenbaum 2008). With ordered hypothesis testing we gain power by formalizing our belief that that some coefficients will be larger than others. The probability of seeing multiple coefficients in a specific order through random chance is lower than the probability of seeing multiple coefficients in any order; thus we gain statistical power if our hypotheses are in the order we expect. In this case, we expect the coefficient describing the participant-control comparison to be larger than the coefficient describing the nonparticipant-control comparison, and we expect the coefficient describing the nonparticipant-control comparison to be larger than zero.

While we have baseline and endline data for our attitudinal and self-report measures, we do not have baseline data for our behavioral measure, the Public Goods game. This will make it difficult to interpret that measure, as we won't know if differences in the level of cooperation is due to that the people who participated were more cooperative types, and therefore that's why they agreed to participate, rather than the engagement in project committees and learning how to work with the outgroup made the more cooperative. However, by looking at this measure in parallel with the other measures, if we see similar trends, we can intuit that the program may have contributed to more cooperative behavior, though any findings will be inconclusive.

Participant vs. Control Differences and Differences

We are interested in the amount of change in the participants group that is attributable to ECPN. The primary issue with an endline-baseline comparison of participants is that we do not know how these individuals would have changed over time in the absence of ECPN. That analysis assumes no systematic changes from baseline-endline except for ECPN (i.e. that we would detect no group-level differences from baseline to endline if we had not conducted ECPN). This may be a faulty assumption. To establish what the participant group's over-time trend would have been without ECPN, we should compare the change we see in the participants group to the change in a group that is similar except that they were not exposed to ECPN. We have two strategies for establishing a trend: (1) the average change of respondents in control communities and (2) a synthetic control of respondents from control communities.

We can compare the change in individuals from the treatment group with the average change in individuals from the control group *if* the average change in control group is the average change we'd expect in these treatment groups in a world where the treatment group did not receive treatment. Since the baseline sampling strategy for each group was the same (random selection) but groups have attrition such that only 20% participated and are re-contacted at endline, we cannot assume parallel trends. We can show evidence for parallel trends in two ways: (1) a placebo test in which we establish parallel trends on an outcome not predicted to change through ECPN, and/or (2) establish that attrition was random and so the recontacted group members are as good as randomly selected.

A placebo test requires us to have baseline and endline data for an outcome or outcomes that should not be affected by ECPN but that represents how ECPN outcomes would have changed in the absence of ECPN. Our primary candidates for this are questions about radio listening frequency. To establish parallel trends on this outcome, we would need to see a non-significant interaction coefficient in the following linear model:

$$Outcome = \beta_0 + \beta_{TR} + \beta_{2018} + \beta_{TRx2018} + \epsilon$$

A non-significant interaction coefficient indicates no difference in the treatment group's amount of change relative to the control group's amount of change. This model would need to be run comparing the control group to both treatment groups – those who participated and those who did not participate.

Another way to establish parallel trends is to confirm that attrition from our original sample of individuals to the 20% of identifiable individuals was random. If those 20% are not more different from their original samples than you'd expect when taking 20% at random, attrition was random and the 20% represent the random sample. If the identified individuals are as good as a random sample of their original random samples, then it may be reasonable for us to assume parallel trends – that change over time is not correlated with any group membership except through ECPN. If the parallel trends assumption is satisfied we can use the average change of respondents in control communities as the baseline amount of change we'd expect in the treatment group if ECPN did not exist.

The other strategy is the construct a synthetic control group to simulate what would have happened with the treatment groups in the absence of ECPN. This would be similar to the method described in Xu (2017) article entitled "Generalized Synthetic Control Method: Causal Inference with Interactive Fixed Effects Models." In the synthetic control method, often described as a difference-in-differences design plus matching, the treatment group is compared to a weighted average of control group observations. Due to limited pre-treatment data, we may not have enough data to establish a synthetic control.

Non-Participant vs. Control Difference-in-Differences

We are also interested in the amount of change in the non-participants group that is attributable to ECPN exposure. This analysis, and the tests to establish parallel trends between the non-participants and the control group, are identical to the analysis and tests described for the participant vs. control comparison. If we find evidence for parallel trends in our placebo test and attrition analysis, we will use the control group as a counterfactual for the amount of change we'd expect in non-participants if non-participants were not exposed to ECPN.

Participant vs. Non-participant Heterogeneity

The third analysis in Study 2 tells us if participants in treated communities changed more than non-participants in treated communities; if there was a benefit to their participation in ECPN committees. To answer this question we compare participants-nonparticipants using the difference-in-differences design described to compare participants-controls and nonparticipants-controls. If we find evidence that respondents who did not participate in ECPN projects because we could not locate them are a good counterfactual for respondents who did participate in ECPN projects, then we can treat participants who did not participate as the "baseline change" group in a within-community difference-in-differences design. We would interact "Year" with "Group" with a blocking variable for "Community" to obtain one coefficient estimate per group for the baseline-endline change. This difference can be causally attribute to participation in ECPN committee if these two groups would have changed at the same rate without one having participated in ECPN committees. Without this parallel trends assumption, we can still describe the difference in the amount of change between groups, but we cannot say definitively that the difference is due to participation in ECPN.

Survey Data

We conduct a baseline and an endline survey for ECPN. We measured several outcomes relevant to peace-building under the various umbrellas: 1) Trust and outgroup affect includes (a) social cohesion in terms of values, (b) social cohesion in terms of actions, (c) affect and social distance with outgroups, specifically with the farmer/pastoralist conflict group, and (d) perceptions of outgroup threat; 2) Interaction includes social contact between groups; 3) Violent conflict history; 4) Perceptions of insecurity due to the conflict; 5)

Perceptions of the acceptability of violence; 6) Cooperation includes a) sharing of resources and b) perceptions of economic benefit of cooperation; and 7) Dispute resolution includes a) perceptions of dispute resolution success and b) perceptions of actors involved in dispute resolution. Specific questions for these topics can be found in the Question Appendix. Some of these concepts are exploratory for the ECPN impact evaluation. Specifically, while certain outcomes may be of interest to the program team, the impact evaluation team does not include hypotheses about ECPN changing perceptions of the acceptability of some types of violence, about inter-religious trust, or perceptions of the actors involved in dispute resolution (i.e. state and local governments, traditional leaders, etc. . .).

Each of these outcomes is measured through multiple survey questions asked on both the endline survey and the baseline survey. We will combine these survey questions into indices that summarize a respondent's attitude about each topic of interest.[12] An index is a better measure of an attitude than one question. Responses to individual questions are part concept measurement and part measurement error, and the measurement error washes out when combining multiple questions. The error is by definition random, whereas the concept measurement is directed. Even if the questions are 50% error and 50% concept measurement, that error is overwhelmed by the use of multiple questions to measure a single concept.

We also use three survey experiments to measure specific aspects of outgroup attitudes and circumvent social desirability bias. First, a survey list experiment asking the number of items that possibly could make respondents upset to measure general affect towards the outgroup. Second, a randomization experiment to measure tolerance for interacting with the outgroup. And third, an endorsement experiment to measure politicization of outgroup affect.

Our primary analysis predicts effects for the survey experiments, attitudes towards outgroups, social cohesion, perceptions of outgroup threat, social contact between groups (i.e., interaction), violent conflict history, insecurity due to conflict, the acceptability of retaliatory violence, sharing resources, perceptions of the success of dispute resolution, and perceptions of economic benefit.

Survey Implementation and Sampling Strategy

We will survey two groups during enumeration. First, we survey respondents identified from the baseline. Second, we survey new randomly selected respondents in these communities. To avoid randomly selecting the respondents from the baseline, we will survey them before surveying the randomly selected respondents. Our survey protocol is: (1) map the community and select households, (2) survey respondents from baseline at their homes, and (3) survey randomly selected respondents at their homes, moving to the next house if a respondent from the baseline was randomly selected. After each survey is conducted, we will announce the result of the public goods game to the community.

Community-level sampling: Our baseline survey randomly sampled individuals from each community. First our enumeration teams mapped each community, beginning in the community center and extending up to 50 households roughly in each cardinal direction. Enumerators worked in male-female pairs, with each pair taking one cardinal direction. Once a pair had mapped 50 households, they randomly selected 10 total households using a random number generator installed on their survey tablets. Each enumerator took 5 households, randomly selecting a respondent within each household by having each select a number from 1 to n, where n is the number of adults in the household. If the selected respondent was available, the enumerator conducted the survey. If the selected respondent was not available, the enumerator set up an enumeration time for the following day.

In some communities the households were too few and far apart to map 50 in each direction.[13] In this case, the enumerators created a map with the assistance of the community leaders and then walked to as many households as they could reach in 30 minutes. They then randomly selected households from that list of "reachable" households. The within-household randomization remained the same.

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[12]As discussed above in the analysis sections, we could score indices in multiple ways. A simple way is to add together the variables that make up the index. More sophisticated methods could use factor analysis or inverse covariance weighting.

[13]This was frequently the case for pastoralist settlements.

At endline, we will use a similar method to randomly select participants. We will sample 50 people per community with this method.

Individual-level sampling: At baseline, we randomly assigned people to engage in the joint committees. Because we desired gender equity in group assignment, we blocked each community into males and females and randomly assigned to experimental groups such that each community had about 10 males and 10 females assigned to be full participants,[14] 8 males and 8 females assigned to be partial participants, and the rest assigned to be non-participants.

However only approximately 20% of those selected to participate actually did participate. This was not due to refusal by the respondent (typical non-compliance), but rather because we could not locate the respondents when the teams went to the communities to establish the ECPN committees. In addition to that 20% of full participants, we have now identified more respondents from baseline, for a total of approximately 300 respondents. We will survey each of these approximately 300 respondents. This gives us individual-level baseline and endline data to use in the individual-level analyses we describe above in the "Analysis – Individual Level Panel Data" section.

Observational Monitoring

We monitor market and social behavior in the communities under study. We want to know if ECPN is increasing social interaction between farmers and pastoralists, particularly in their shared marketplace and with social events. We therefore attempt to measure: (1) cross-group interaction at the market, including purchasing of market goods, (2) cross-group social event attendance, and (3) cross-group food sharing at weddings.

The observers attended the market at the same time each month and attended social events roughly once per month at the first event that occurred each month.

We also monitor meetings of our project committees in treatment sites. We want to know if these farmer-pastoralist meetings become more collaborative over time and if women are more involved in the decision-making process over time. We measure: (1) the number of farmer and pastoralist men and women in attendance, (2) the number of times each group speaks, separating males and females, (3) any disagreements/issues discussed in the meetings, (4) if those issues were resolved, and (5) how those issues were resolved.

These data attempt to quantify qualitative information; they are difficult to collect and will likely to be noisy measures. They are also extremely valuable for documenting changes in how ECPN participants *interact*, which we may miss in survey responses.[15] We expect ECPN participants to interact more with their farmer/pastoralist outgroup than non-ECPN participants. We also expect more equal participation in meetings and more issues resolved successfully over time.

In the analysis we will consider these observational monitorings as panel data. Each community has data points from the beginning of the project and the end of the project. Due to funding issues, there is no data in the middle of the project. We expect more positive change in the ECPN communities than the control communities, and the greatest change in observed behavior for full ECPN participants.

Observational data was only collected for treatment sites, so it will help understand potential differences but we cannot use for comparisons.

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[14]In some communities, there were not enough women sampled to assign 10 women to be full participants. In these places we increased the number of men from the community assigned to the full participants group so that each side (farmers and pastoralists) had twenty members randomly assigned to be full participants.

[15]Several studies, including Scacco and Warren (2016) and Paluck (2009) demonstrate that intergroup contact programs can affect behavior without necessarily affecting attitudes.

Natural-Field Public Goods Behavioral "Game"

We use a natural-field behavioral game to measure attitudes towards the farmer/pastoralist outgroup. Behavioral games create a strategic choice-making situation for participants, and researchers observe participants' behavioral choices. In a typical behavioral game, participants make this strategic choice in a lab with full knowledge that they are participating in an experiment. Due to these artificial conditions, which are not present in real-world choices, results from lab behavioral games may not conform to similar real-world behaviors (Winking and Mizer 2013; Galizzi and Navarro-Martínez 2017). Natural-field experiments solve this problem by creating a choice-making situation in the participants' natural environment where participants are not aware that an experiment is taking place (Harrison and List 2004; Winking and Mizer 2013).

Natural-field behavioral games are especially useful for measuring the tangible, behavioral effects of an intervention. A behavioral game displays an individual's real behavior in an artificial situation; a natural-field behavioral game displays an individual's real behavior in a real situation. As Grossman and Baldassarri (2012) found in Uganda, the cooperation displayed through a public goods game was correlated with observational data on similar cooperative behaviors. Our natural-field public goods game as a measure of behavior change complements our survey responses as a measure of attitudinal change.

We use the natural-field public goods "game" to measure intergroup cooperation and social cohesion.[16] Our game is similar in form to the game implemented in Fearon, Humphreys, and Weinstein (2009). In the "game", we observe participants' contributions to a community fund that will be used to fund a development project that benefits them *and* their paired conflict community. Participants are told that any contribution they make to the community fund will be matched, so that their giving 100 Naira to the community fund becomes 300 Naira for the community fund. The socially desirable behavior – contributing to a community fund – is costly, but it generates more overall money than the selfish behavior. Thus, participants must make a difficult trade-off between their own interests and the interests of the broader community. In this case, that broader community contains both members of their ingroup and members of the outgroup they are or were in conflict with.

We expect people in treated communities to contribute more money to the community fund than control communities.

Game Details

In our "game", the fifty randomly selected members from each community and the respondents identified from the baseline survey receive 1,000 Naira as part of a development project. They are told that Mercy Corps has money that is to be given directly to people in communities where Mercy Corps works. The money is for these people to do with as they please – they can keep the money or contribute it to a joint farmer-pastoralist project committee that will use the money for a collective good that will help both communities. Participants are also told that Mercy Corps also found someone who will match all donations to these project committees, so if participants donate 1 Naira it becomes 3 Naira for the project committee, and if they donate all 1000 Naira the project committees will receive 3,000 Naira.

Following Fearon, Humphreys, and Weinstein (2009), we will go to the communities before implementing the game. Both treatment and control communities are told that receipt of funds depends on completing a form that tells us: (1) the community members who will form a committee to manage the money, and (2) plans for how the funds will be spent.

More contributions towards the community fund by people in treatment villages would show behavioral change in a real situation regarding the use of funds. It shows that people in the treatment villages are willing to cooperate across community lines and sacrifice their own money so that both communities can benefit. 1,000 Naira is not an inconsequential amount of money in this area. According to our baseline survey, the

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[16]We also believe this game could measure intergroup trust, since participants with more trust that their outgroup will donate to the community fund should donate more than participants who do not trust their outgroup to donate to the community fund. It is certainly weaker as a measure of trust than as a measure of cooperation, and a trust game would more explicitly measure intergroup trust.

average annual income in these communities is around 100,000 Naira. 1,000 Naira amounts to about half a week of personal income. Willingness to contribute that money to a community fund that helps the outgroup demonstrates powerfully that the program has affected a significant change in intergroup relations.

Similar to Fearon, Humphreys, and Weinstein (2009), we will run the game as a one-time trial as opposed to a repeated game. We are interested in how the participants play, not in how participants learn to play and change their behavior after repeated exposures to the same game.

One concern is that a member of the research team is a foreigner, and could prime communities to give differently than if he was not present (Cilliers, Dube, and Siddiqi 2012). This difference is exacerbated with status differentials. As the communities we are working in are quite poor, this is a concern. To minimize the possibility that his presence will affect people's responses, the researcher will stay at a central point and not go out with the enumerators to the households. The researcher will also balance his presence in treatment and control communities.

Implementation of the Public Goods Game

The natural-field public goods game will be conducted in all communities, both treatment and control. An advance team will visit each community to secure their consent to receive funds for development one week before we conduct the public goods game.[17] We should explain the conditions of these development funds to the community leaders and other people important to community consent. They should know: (1) that we can provide 1,000 Naira to fifty farmers and fifty pastoralists of their community; (2) that the community members to whom we give the funds can keep the money or donate it to a project committee containing an equal number of farmers and pastoralists; (3) that we found another donor who will match every contribution to the project committee at a 2:1 rate, such that an individual giving 100 Naira to the project committee results in the project committee receiving 300 Naira; and (4) that receipt of funds depends on completing a form that tells us who in the community will form a committee to manage the money and the plans for how the funds will be spent. The communities should have the form completed when we return for the endline survey, and a project committee and plan for use of funds should be ready when we present the keep/donate option to the participants.

The public goods game will be conducted immediately after the respondent completes the endline survey at their home. Enumerators will survey the community members identified from the baseline and 50 randomly selected respondents from each community. The enumerators will survey the respondent, describe the public goods game, provide each respondent with an envelope with their unique ID number that contains five 200 Naira notes, allow the respondent to privately select the amount they wish to keep and the amount they wish to donate, and collect the donation envelopes in a large sealed manila folder.

The participants will be told the same thing we told the community leaders: (1) that we can provide 1,000 Naira to about 50 members of their community, including the respondent themselves, and about fifty people in the other community, for about 100,000 Naira in total funds given to individuals; (2) that they can keep the money or donate it to the joint-community project committee that contains an equal number of farmers and pastoralists; and (3) that we found another donor who will match every contribution to the project committee such that 1 Naira donated = 3 Naira received by the committee and 1,000 Naira donated = 3,000 Naira received by the committee.

We should then give the participants an envelope with their unique participant ID on it. This will allow us to know their contribution, but will keep it anonymous to anyone who does not have the participant ID-Name key. We must be sure to give each participant the correct envelope. We then tell the group that each envelope contains five 200 Naira in bank notes[18], and that the enumerator has a donation envelope to collect donations. The respondent may go into their home, take whatever money they want to keep, leave whatever amount they want to donate in the envelope, and come back out to place their envelope in the donation envelope. We

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[17]It is possible that knowledge that we are coming to bring "funds for development" could affect survey responses about farmers/pastoralists. Ideally, we would conduct the public goods game months after the survey to avoid this issue.

[18]It is important that every respondent receives the same mix of bank notes

tell them that we will tally the money and announce how much money their community has raised for the project committee within three days, on our last day doing the survey in their community.

Also to ensure that people do not learn what other people donate to the public good we will 1) have people determine their donations in the privacy of their own home, 2) inform participants that they should not tell others what they donated as it is critical for the research, 3) only announce the whole pool of funds, not what communities or individuals gave. If we find that people do tell others, that data will be excluded from the analysis. Additionally, we will match all contributions as if everyone gave the full amount.

Scripts

Game

Great, thank you very much for participating in our survey. Before I go, there is one last thing. As you may have heard, we have development funds to use in this community. We have randomly selected you as one of the 50 people to receive these funds. These funds are not for a Mercy Corps project, but rather for you to keep personally or to donate to a community fund. We have 1,000 Naira to give to you. It is yours, and you can use it either way–for yourself or for a community good.

Your community and [joint farmer/pastoralist community] have created a project committee to whom you can donate this money so that it may be used to help both communities. The project committee has 4 people from each community. We have found a donor that will match the funds that you all contribute to the project committee, so that if you donate 100 Naira the project committee receives 300 Naira, and if you donate all 1,000 Naira the project committee receives 3,000 Naira. You are welcome to donate none, some, or all of the money to the project committee.

Give participant their ID labeled envelopes with five 200 Naira notes in them.

These are your individual donation envelopes. All the donations will be private – only you will know how much money you donated. It essential that you keep how much you give private – please do not tell anyone. I have with me a donation envelope to collect donations. Please go into your home, put however much of the 1,000 Naira you wish to donate to the project committee in the envelope, take whatever amount you want to keep for yourself, and come back to place your envelope in the donation envelope. You are welcome to donate none, some, or all of the money to the project committee. After that we are finished and you may continue your day. We will come back and publicly announce how much money your community's project committee will receive.

Thank you very much for participating and have a great day.

Result Announcement

Your community did very well and is one of the most generous communities we surveyed! You will receive $X amount towards the project committee.

Behavioral Game Options

We chose the public goods game, and this specific variation, after considering other variations and even other natural-field behavioral games. The public goods game most closely mimics the type of cooperation that ECPN is intending to foster. Fearon, Humphreys and Weinstein (2009) used public goods games as well to test how a similar intervention affected levels of cooperation.

While there is some concern that what we find will be more of a practice effect in the treatment communities than actual cooperation, since they already have similar project committees working this way, after looking at other games, which were largely dyadic (i.e., dictator game, with or without punishment; trust games) we elected to use the public goods since having trust in joint institutions (i.e., the project committee) is

important in of itself. We will use other attitudinal measures to triangulate on why we see or do not see cooperative behavior.

Analytical Strategy: Combining Outcomes and Ordering Hypothesis Tests

We will use three analytic strategies to increase statistical power in our studies. The first technique is testing multiple hypotheses simultaneously, described in Caughey, Dafoe, and Seawright (2017). We predict that ECPN changes multiple outcomes: survey responses on multiple attitude dimensions and donations in the public goods game. Since our prediction is about this *collection* of attitudes and behaviors we should test the hypothesis that ECPN affected *all* of them against the null hypothesis that ECPN did not affect *any* of them. This comparison should increase our statistical power – even if we observe only small changes on all these outcomes, the probability of observing *multiple* small changes due to random noise is much lower than the probability of observing *one* small change due to random noise. If we observe an overall change, we can then look within this global collection of outcomes to assess which outcomes are driving this overall change.

We can further increase our power by testing hypotheses in order. In the same way that randomly observing multiple outcomes correlated with treatment is less likely than randomly observing one outcome correlated with treatment, predicting and observing some outcomes more strongly correlated with treatment than others increases power relative to predicting all outcomes equally correlated with treatment. To the extent that we believe ECPN will more strongly affect certain outcomes than others, we can gain power by testing hypotheses about outcomes in an ordered fashion.

We can also increase power by combining the result of multiple studies in the same way that we combine the results of multiple outcomes. We predict that ECPN will affect outcomes (1) at the community-level, (2) among participants in ECPN committees, and (3) among non-participants in treated villages. We are less likely to randomly observe small p-values in multiple analyses about the same intervention than to randomly observe small p-values in one analysis about an intervention. To leverage the p-values we have from multiple analyses, we will first record and combine the p-values from each of the three analyses described above (community-level, participants, and nonparticipants). We will then simulate the possible results we could have observed in each of those studies if there was no effect. To simulate these studies with no effect, we break the relationship between treatment and outcomes by shuffling the treatment indicator (for the community-level study) or the outcome scores (for the individual-level studies). We then have the distribution of p-values we would see if ECPN had no effect in each study. Our outcome of interest is the proportion of this "no effect distribution" that is smaller than our observed group of p-values.[19]

Conclusion

This document summarizes our research designs and analytic strategies for assessing the impact of Mercy Corps' program *Engaging Communities for Peace in Nigeria* (ECPN). Our research designs compare (1) the effects of ECPN between communities that received ECPN and control communities that did not, and (2) the effect over time for people who did and did not directly participate in ECPN activities. We measure impact in three ways: (1) survey data, (2) observational monitoring, and (3) a natural-field public goods behavioral game. Each type of data tells us something unique about farmer-pastoralist relations. The survey data tells us about individual-level attitude change and social norms, the observational monitoring tells us about cross-group interaction, and the public goods game tells us about cooperation and social cohesion with the outgroup community. We will use statistical techniques to test the hypothesis that ECPN affects all of these diverse outcomes simultaneously.

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[19]The combined p-values could be weighted if we believe some analyses provide more information than other analyses. After consulting with other methodologists we may want to weigh the community-level analysis, though at the current time we do not have plans to weight any of the analyses.

Question Appendix

Social Cohesion

- People in this area are willing to help their neighbors across ethnic and religious lines
- People in this area can be trusted
- People in this area generally do not get along together
- People in this area do not share the same morals
- People in this area see the benefits of working together to achieve common goals
- 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?
- 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?
- 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?
- 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?
- 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?

Outgroup Trust

- On a range from 1-5, how much do you trust people from an ethnic group different than your own? On this range, 5 means "trust completely" and 1 means "do not trust at all."
- From 1-5, how much do you trust people from a different religion than your own?
- From 1-5, how much do you trust people from X GROUP in your area?

Social Distance

- With regards to someone from X group, would you feel comfortable:
- if they worked in your field?
- paying them to watch your animals?
- trading goods with them?
- sharing a meal with them?
- with a close relative marrying a person from X group?

Perceptions of Economic Benefit

Please tell me how strongly you agree/disagree with each of the following statements:

- You currently benefit economically from cooperating with members of X GROUP.
- You would benefit economically more than you currently do if there was peace between your communities and X group.
- You would benefit economically if there were peace between your community and X group.

- You would personally commit to peace with X group, even if members of X group used violence against your group.

Randomization Experiment

Think about groups that you might join in your leisure time. Would you join a group that had **5%/25%/50%/75%** X Group members?

Think about the community you live in. Would you live in a community that had **5%/25%/50%/75%** X Group members?

List Experiment

"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.

- When your football team loses a match
- Increases in the price of gasoline
- Lack of rainfall
- **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.

Endorsement Experiment

Imagine that there is a proposal [**by a Farmer's Cooperative Society/MACBAN**] for action to enhance access to clean water in rural areas. Though expensive, the proposal aims 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?

Threat Perception

- Some people say X group is responsible for most of the violence in this community, while others say that both groups are responsible for the violence here. Which is closer to your view?
- You see X group as a threat to your community
- You think X group have too much influence on your community
- You think that people from X group have different morals than people from your group

Social Contact

- Think of the market you go to most frequently. During the past month, have members of X GROUP gone to that market too?

- In the past month, how many times did you interact with X group in the market?

- How did you interact? (Economic, Social, Both)

- Were the market interactions mostly... (Very Positive - Very Negative)

- In the past month, have you interacted with members of X group outside the market?

- Joined a member of X group for a social event outside the home?

- How often?

- Hosted a member of X group for a ceremony in your home?

- How often?

- Gone to the home of a member of X group for a ceremony?

- How often?

- Have you interacted with members of X group in any other way in the past month?

- How else have you interacted with members of X group?

- Overall, would you say your interactions with X group are: (Very Positive - Very Negative)

Violent Conflict History

- To your knowledge, in the last year, were there any violent clashes or disputes in your community?
- How many were there?
- About how long ago was the most recent clash?
- And in that clash, about how many people died?
- In the most recent clash, what was the main cause of violence?

Insecurity due to Conflict

- In any clash that occurred in the last year, were you or anyone in your family negatively affected by an attack caused by X group?
- In the last year, to what extent have people's abilities to work/earn a living in your community been affected by violent clashes or disputes with other groups? Would you say...
- In the last year were there any areas that you avoided going to or through because of insecurity during the night?
- In the last year were there any areas that you avoided going to or through because of insecurity, during the day?

In the last year, did insecurity ever prevent you from...

- Working when you wanted to work?
- About how many days were you unable to work?
- Going to the market?
- Getting water for the household?
- Going to your field/farm?
- Moving your animals to grazing areas?
- Moving your animals to water?
- Earning money or going to work?
- Going to school?

Shared Resources

- Does your community share markets with X group? **OR** Earlier you said your community shared markets with X group, is that right?

- Does sharing the market with X group cause tension?

- Does sharing the market with X group cause disputes?

- Would you say disputes between your community and X group in or over markets are resolved peacefully...

- 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.

- Does your community share pasture areas with X group?

- Does sharing the pasture with X group cause tension?

- Does sharing the pasture with X group cause disputes?

- Would you say disputes between your community and X group in or over pastures are resolved peacefully. . .

- 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.

- Does your community share farmland areas with X group?

- Does sharing the farmland with X group cause tension?

- Does sharing the farmland with X group cause disputes?

- Would you say disputes between your community and X group in or over farmland are resolved peacefully. . .

- 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.

Dispute Resolution

Note: Also includes questions above asking "who usually resolves disputes".

I'm going to ask some questions about how disputes are resolved in your community.

- How often do disputes over one issue persist after an attempt at resolving them?
- During the past year, who usually resolved disputes between members of your community and members of other communities? Please name the three types of people who resolve disputes the most frequently.

Please tell me how much you agree or disagree with each of the following statements. - Local government officials try hard to manage conflict in my area.
- How often are local government officials successful in resolving clashes nonviolently? Is It..

- State government officials try hard to manage conflict in my area.

- How often are state government officials successful in resolving clashes nonviolently?

- Traditional leaders try hard to manage conflict in my area.

- How often are traditional leaders successful in resolving clashes nonviolently? Is It..

- Religious leaders try hard to manage conflict in my area.

- How often are religious leaders successful in resolving clashes nonviolently? Is It..

- Security officials try hard to manage conflict in my area.

- How often are security officials successful in resolving clashes nonviolently? Is It..

- Women try hard to manage conflict in my area.

- How often are women successful in resolving clashes nonviolently? Is It..

- Youth try hard to manage conflict in my area.

- How often are youth successful in resolving clashes nonviolently? Is It..

- What type of leader in your community has the most influence on dispute resolution?

- To what extent is the local government addressing the security concerns of this community?

Acceptability of Violence

Now I am going to ask you some questions about the use of violence.

Is it always, sometimes, rarely, or never justified to use violence to do each of the following:

- Retaliate against violence
- Defend one's group
- Maintain culture and traditions
- Defend one's religion
- Bring criminals to justice
- Force the government to change their policies

Power Appendix

Community-level power analysis (presented below) suggests we can detect an effect between 0.3 - 0.4 SD with 80% power.

Individual-level power analysis forthcoming.

Questions in Indices

```
# should use inverse covariance weighting for some of these.  Should reconsider the variables in some o

### Social Cohesion Index

#### SC1 -- Cohesion of values
ecpn$sc1.index=ecpn$sc_help + ecpn$sc_trust + ecpn$sc_getalong + ecpn$sc_values + ecpn$sc_work

#### SC2 -- Cohesion of actions
ecpn$sc2.index=ecpn$sc_you_contribute + ecpn$sc_x_contribute + ecpn$sc_water + ecpn$sc_misfortune_x + ec


### Outgroup Attitudes Index
ecpn$out_ethnic_trust+ecpn$out_rel_trust+ecpn$out_x_trust+ecpn$out.field+
  ecpn$out.animals+ecpn$out.trade+ecpn$out.meal+ecpn$out.marry

#### Outgroup Survey Exps
Randomization exp.
List exp.
Endorsement Exp.

### Outgroup Threat Index
ecpn$threat.index=ecpn$threat_threat+ecpn$threat_influence+ecpn$threat_values

### Social Contact Index: note that this really includes one measure of the positivity of the contact.
```
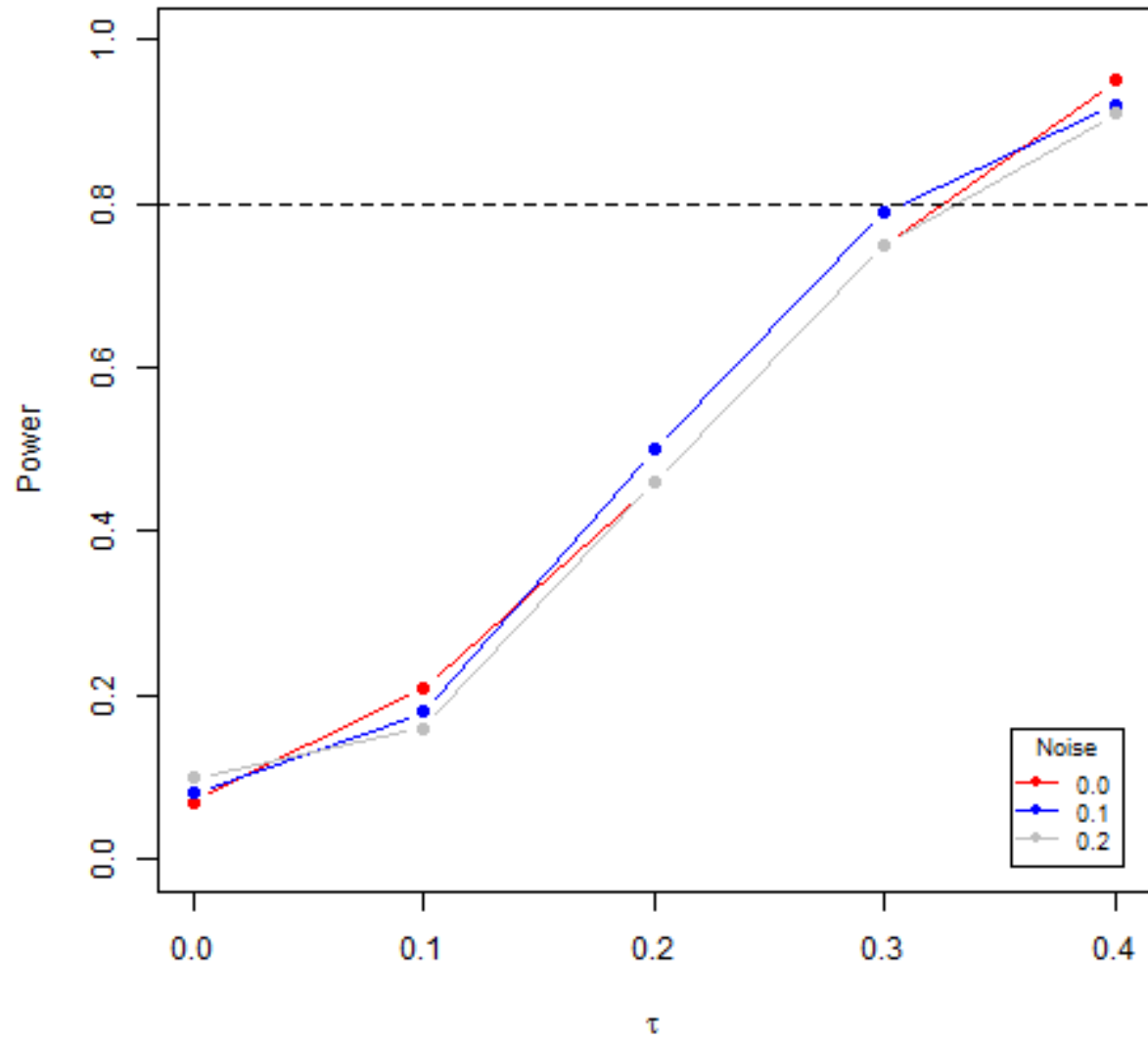
Figure 1: Community-Level Power Analysis

```
ecpn$contact.index=ecpn$cont_market+ecpn$cont_market_num+ecpn$cont_event+
  ecpn$cont_home+ecpn$cont_home_x+ecpn$cont_other+ecpn$cont_pos

### Clash History Index
ecpn$clash.index=ecpn$clash_num+ecpn$clash_when+ecpn$clash_killed+ecpn$clash_personal+ecpn$clash_work

### Insecurity Index
ecpn$insecurity.index=ecpn$insecurity_night+ecpn$insecurity_day+ecpn$insecurity_work+
  ecpn$insecurity_market+ecpn$insecurity_water+ecpn$insecurity_field+
  ecpn$insecurity_graze+ecpn$insecurity_animals_water+ecpn$insecurity_money+
  ecpn$insecurity_school

### Violence Index
vio.vars <- c('violence_retaliate','violence_defend_gr','violence_culture',
  'violence_defend_r','violence_criminals','violence_government', 'peace_commit')

### Shared Resources Index: note that this really has two concepts: total sharing of resources and if th
ecpn$share.index=ecpn$share_market+ecpn$share_market_tens+ecpn$share_market_disp+
  ecpn$share_market_disp_resolve+ecpn$share_pastures+ecpn$share_pastures_tens+
  ecpn$share_pastures_disp+ecpn$share_pastures_disp_resolve+ecpn$share_farm+
  ecpn$share_farm_tens+ecpn$share_farm_disp+ecpn$share_farm_disp_resolve+
  ecpn$share_peace

### Perceptions of Economic Benefit
eb.vars <- c("econ_benefit", "more_benefit", "would_benefit")

### Dispute Resolution: note this should probably absorb the questions about dispute resolution from the

How often do disputes over one issue persist after an attempt at resolving them? Is it...

### Perceptions of Conflict Management
Local government officials try hard to manage conflict in my area.
How often are local government officials successful in resolving clashes nonviolently? Is It..


State government officials try hard to manage conflict in my area.
How often are state government officials successful in resolving clashes nonviolently? Is It..


Traditional leaders try hard to manage conflict in my area.
How often are traditional leaders successful in resolving clashes nonviolently? Is It..


Religious leaders try hard to manage conflict in my area.
How often are religious leaders successful in resolving clashes nonviolently? Is It..


Security officials try hard to manage conflict in my area.
How often are security officials successful in resolving clashes nonviolently? Is It..


Women try hard to manage conflict in my area.
How often are women successful in resolving clashes nonviolently? Is It..
```

```
Youth try hard to manage conflict in my area.
How often are youth successful in resolving clashes nonviolently? Is It..

To what extent is the local government addressing the security concerns of this community?
```

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