Reducing Conflict in Nigeria's Middle Belt

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Questions for EGAP:

1. Theoretical and practical underpinnings: Are there other literatures we could be drawing from? Right now the theoretical aspects are from a somewhat dated social psychological, organizational behavior and behavioral economics literature.
2. Modeling treatment spillover/interference between units/fact that participants will interact with non-participants. We've tried to model this by having a true control group of subjects in non-program sites and the non-participants in program sites (who should receive some treatment spillover). Are there options? Specifically how might we use social network analyses to model spillover effects?
3. Observational measures: Any suggestions beyond the traditional trust games?
4. Effective 'participation incentives' in this context: There are some program concerns with using lotteries. Besides the ones we've outlined (financial incentives, social pressure upfront commitment), any other ideas?

Summary: Much of peacebuilding work is based on the assumption that if you bring conflicting parties together over mutual interests, then people's attitudes towards the outgroup will improve, trust will improve and violence will decrease. The proposed evaluation design examines both the practical significance of these types of interventions as well as the theoretical underpinnings in the Middle Belt of Nigeria, where farmer-pastoralist conflict, which is also along religious lines, costs the Nigerian economy nearly $14 billion a year.[1]

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[1] McDougal, T., Hagerty, T., Inks, L., Dowd, C. & Conroy, S. (2015). Maco-economic benefits of farmer-pastoralist peace in Nigeria's Middle Belt: An input-output analysis approach. *The Economics of Peace and Security Journal,* v. 10 (1), pp. 66-77. Mercy Corps (2015). "The Economic Costs of Conflict and the Benefits of Peace: Effects of Farmer-Pastoralist Conflict in Nigeria's Middle Belt on State, Sector, and National Economies." Accessed 17 Oct 2015 at https://www.mercycorps.org/research-resources/economic-costs-conflict-nigeria.

Program Description

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.

Mercy Corps' Engaging Communities for Peace in Nigeria (ECPN) is a two-year, $1,2000,000 program that aims to reduce violence in the Middle Belt states by peacefully preventing and resolving farmer-pastoralist conflicts. This will be achieved by a holistic, people-to-people approach that: 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.

The program is built on three core theories of change that stress the importance of bringing people together to peacefully resolve shared challenges and build relationships and economic interdependence. They are: 1) If key farmer and pastoralist leaders are given the skills and support needed to peacefully and collaboratively manage disputes, then inter-communal violence will decrease; 2) If farmers and pastoralists collaborate on quick impact projects that demonstrate the benefits of cooperation and economic projects that address underlying tensions, then there will be an increase in positive interactions, thereby building trust and economic interdependence, while reducing violence; and 3) If farmers and pastoralists work together with surrounding communities and local government authorities to monitor and prevent conflict, then these communities will be better able to absorb and adapt to shocks and stressors in their environment.

This evaluation will examine the second Theory of Change in greater depth. The logic behind it is that violent conflicts between farmer and pastoralist communities negatively affect their social and economic relationships, which then limit these groups' mobility and access to surrounding land and water sources. In these highly tense communities, an integrated and phased approach—in which participants begin interacting in small ways and move up the continuum to deep interaction that causes mutual economic gains—will be more effective for

addressing drivers of conflict. Creating face-to-face interactions around quick impact projects will build trust as communities see how addressing shared interests provides immediate mutual gains. Such positive interactions then offer an entry point for divided communities to jointly implement projects that strengthen existing economic ties, while building beneficial interdependence.

The description of these activities is as follows:

Activity 2.1: Community-led resource & conflict mapping and needs assessment. Mercy Corps and partner Pastoral Resolve (PARE) will train trusted community members to lead needs assessments after leaders are identified and trained under Objective 1, using Mercy Corps' participatory mapping tools. We learned through our other conflict management programming in the region that by having local community members who already have communities' trust conduct the assessments, we can gather sensitive conflict and resource mapping information much more quickly. Through these needs assessments, communities will take the lead in analyzing the linkages between resources and conflict and identify economic opportunities and gaps in services that they can collaboratively address with conflicting groups. Once the information is gathered, Mercy Corps and PARE will facilitate discussions between conflicting communities to identify shared needs and opportunities and lay the groundwork for joint economic and social impact projects.

Activity 2.2: Quick Impact Projects. The conflict and resource mapping exercise will highlight localized social and/or economic issues that can address immediate concerns related to the common service needs of both communities at a low cost. For example, many communities in Mercy Corps Middle Belt assessment expressed a need for improved school facilities. In one site a primary school shared by both farmer and pastoralist communities had no chairs or desks. With Mercy Corps facilitating a small project in which carpenters from farmer and pastoralist communities build the necessary school furniture, jointly implemented solutions can lead to agreements over the sharing of new resources, thus reducing tensions over lack of services, building trust and demonstrating how cooperation benefits all.

Activity 2.3: Joint economic development and natural resource projects. Competition for farm and pasture, lack of clarity around the demarcation of pasture, and access to land and water points are underlying causes of tension that drive many of farmer-pastoralist disputes. Using the information about context-specific tensions gathered as part of Activity 2.1, Mercy Corps and our partners will work with farmer and pastoralist communities to identify local intervention points. To reduce resource demand, increase economic opportunity, and promote cooperation, the team will work with community members and local leaders, including youth leaders, to identify and implement 10 economic and resource management initiatives. Particular focus will be placed on women's involvement in these projects, whose roles and responsibilities put them in a leadership

position and who also are in need of livelihood diversification opportunities. These economic development projects will be linked to a USAID-funded market development program and its efforts to strengthen value chains such as rice and cassava.[2]

The program model described above has deep roots in both development programming as well as social psychology. Below we describe the evaluation's practical and theoretical significance.

Practical Importance

The evaluation will both contribute to practical knowledge as well as theoretical knowledge. On the practical side, many peacebuilding organizations implement activities that bring people together across lines of division in order to build trust. For example, Congress provides $26M a year to fund People-to-People Reconciliation projects (half of which are for West Bank/Gaza), almost all of which have some component of bringing people together across lines of division to change attitudes, reduce prejudice and build trust. However, there is little evidence that these activities are effective. The Conflict Management and Mitigation (CMM) office within USAID hired Social Impact to do a desk review of 30 of these programs and a meta-analysis of 10 of these. While there was some evidence of attitude change in pre-post tests, most only conducted a post-test with project participants.[3] Very few included comparison groups and there were no complete randomized trials reported. A longitudinal study of Seeds for Peace—a camp that brings Israelis and Palestinian youth together—was recently published, showing positive attitude change that withstood time.[4] Similarly, though, there was no comparison group.

The process in which groups come across conflict lines to decide on a joint project is similar to many Community Driven Development (CDD) models. There is a participatory process to decide on the community needs and then people work together to implement the project at various levels. CDD purports that this process will lead to social cohesion.[5] Recent studies on the benefits of these programs have shown little evidence that the process builds social cohesion or social capital between groups.[6] CDD models are multi-faceted, and there are a number of pathways that hypothetically could increase social cohesion. In the proposed

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[2] Mercy Corps is already in regular contact with Francis Iketaku, team leader for the Middle Belt at MARKETS II and will coordinate more closely for this current project

[3] http://pdf.usaid.gov/pdf_docs/pbaaa370.pdf

[4] http://gpi.sagepub.com/content/early/2014/07/25/1368430214542257

Chase, Robert, and Michael Woolcock. (2005). Social Capital and the Micro-Institutional Foundations of CDD Approaches in East Asia: Evidence, Theory and Policy Implications. Arusha Conference 'New Frontiers of Social Policy', December.

[6] King, E. (2013). A Critical Review of Community Driven Development in Conflict-Affected Contexts. DFID & IRC.

study, we are examining one pathway—the changing of attitudes, based on the contact hypothesis.

Another commonality between both CDD programming and reconciliation programming is that projects ostensibly should help the whole community and not only the decision makers or the few community members most directly involved in the project (i,e., direct participants). 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.

Theoretical Importance
This above intervention is based on a number of social psychological and behavioral economics 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[7]

Pettigrew and Tropp (2006) conducted a meta-analysis of 515 studies and found that intergroup contact reduces prejudice. Moreover, Paluck and Green (2008), 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.

We recognize that we are making a bit of leap from the contact hypothesis, which theorizes reducing prejudice to trust building and willingness to engage with members of an outgroup. Other literatures demonstrate a link between less prejudicial attitudes and willingness to engage with an outgroup. For example, the Implicit Association Test on prejudicial attitudes towards an outgroup is predictive of discriminatory behavior.[8] And while there is some controversy on

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[7] To note, in the condition of *personal interaction,* Allport notes that there needs to be informal interaction. The structure of some of these interventions may not be conducive to that. As one expert has noted, CDD isn't very social.

[8] Greenwald, Anthony G.; Poehlman, T. Andrew; Uhlmann, Eric Luis; Banaji, Mahzarin R. (2009). Understanding and using the Implicit Association Test: III. Meta-analysis of predictive validity. *Journal of Personality and Social Psychology,* Vol 97(1), Jul 2009, 17-41.

the replicability of priming studies, in one classic study on priming where when people were primed with negative information about an outgroup, people would put more social distance between them and the outgroup, illustrating that they were those with more prejudicial beliefs were less willing to engage with an outgroup.[9]

While contact may reduce prejudice and increase positive feelings about an outgroup and increase willingness to engage with an outgroup, does it increase trust? In a conflict setting, there are often acts committed by the parties involved or members of the group that break trust. Lewicki and colleagues have outlined steps for how to rebuild trust, using a combination of developing a shared understanding (which would occur during the dialogues that help identify joint projects), and taking small steps (quick impact projects) and gradually engage in more interdependent activities (economic projects).[10] Much of this literature is theoretical. Additionally, there is the behavioral economics and psychology literature regarding trust that shows that people favor ingroups; only under certain conditions, usually with the right incentives, will people trust outgroups.[11] However, much of it is based on the minimal group paradigm, with few real group differences. In one rare study with Catholics and Protestants in Northern Ireland, they did disentangle liking versus trust, and found that trust is more predictive of behavior.[12]

In this field experiment, we will examine whether a blended model based on contact theory and trust development will lead to greater trust and social cohesion.

Research Design and Methodology
In this section we describe the experimental design that we believe to be both analytically strong and feasible to implement. We demarcate the experimental design section into three parts: (1) treatment and experimental groups, (2) subject participation and incentives, and (3) assessment of outcomes.

Treatment Assignment and Control Groups
Individual participation in quick-impact and joint-economic projects are the "intervention" or "treatment" in this experiment. Based on the ECPN program,

[9] Bargh, J. A., Chen, M., & Burrows, L. (1996). Automaticity of social behavior: Direct effects of trait construct and stereotype activation on action. Journal of Personality and Social Psychology, 71, 230-244.; Macrae, C. Neil; Bodenhausen, Galen V.; Milne, Alan B.; Jetten, Jolanda. Out of mind but back in sight: Stereotypes on the rebound. *Journal of Personality and Social Psychology*, Vol 67(5), Nov 1994, 808-817.

[10] Lewicki, R. J., McAllister, D. J., & Bies, R. J. (1998). Trust and distrust: New relationships and realities. *Academy of Management Review, 23,*438-458.

[11] Bornstein, G. (2003). Intergroup Conflict: Individual, Group and Collective Interests. *Personality and Social Psychology Review*. vol. 7no. 2 **129-145**

[12] Tam, T. , Hewstone, M., Kenworthy, J & Cairns, E. Intergroup Trust in Northern Ireland. *Personality and Social Psychol Bulletin* January 2009 35: 45-59,

community members will be be randomly assigned roles as direct participants, indirect participants, or non-participants. Within treated conflict sites, this yields (1) Direct Participants, (2) Indirect Participants, and (3) Non-Participants. The direct participants consistently work together to design and implement quick-impact and joint-economic projects; the indirect participants attend a one-off event related to the projects; and the non-participants neither work on the projects nor attend the event. Non-participants can be further disaggregated into "exposed non-participants" and "unexposed non-participants", though we will not control that. Exposure occurs if the non-participants discuss the treatment with participants or witness participation in the treatment. Though participation in a project is on an individual level, individuals are likely to discuss their participation in the projects with other community members, leading to exposure of non-participants, or "treatment spillover". If these communities are not very large, it is likely that there will be no unexposed non-participants, and thus every member of the community will be affected by the treatment in some way. Even if there are unexposed non-participants, exposure/non-exposure to treatment spillover is not likely to be random. For example, those who are not even exposed to discussion about the treatment might have very ingrained prior attitudes that lead them to avoid those discussions. This complicates the analysis and interpretation of the findings, because we have no clear control group to compare treated individuals within treated conflict zones.

To provide a clear control group, we will survey community members of at least three untreated conflict zones.[13]

The activities of the indirect participant group will determine whether we consider them to have received a lower dose of the treatment or to have a received a different treatment altogether. A full dose of treatment goes to direct participants, who will be participating in joint economic-development projects and quick-impact projects. We would consider indirect participants to have received a lower dose of treatment if they participate in one event or a very small number of events, including resource and conflict mapping and needs assessment (Activity 2.1), community forums (Activity 3.1), or quick-impact projects (Activity 2.2). In that case, the direct participants are the most likely to have an effect from treatment, the indirect participants the second most likely, the non-participants in the treated villages the third most likely, and the members of non-treated villages the least likely.

If indirect participants were to participate in a *series* of community forums or a *series* of some other activity, we could no longer say that they have received

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[13] There will be 10 treated conflict sites. According to the power analysis, with 1,000 subjects spread evenly through 10 treated conflict sites and 3 control conflict sites (780 treated subjects and 220 control subjects), we have a 90% probability of seeing a treatment effect if the treatment effect is 0.25 standard deviations strong.

"less" treatment than the direct participants; they would have a full dose of a different treatment. In that case, the direct and indirect participants are equally likely to have an effect from treatment, the non-participants are the second most likely, and the members of non-treated villages the least likely. Because direct and indirect participants are not being compared to one another, we gain less statistical power from the doses concept, but we still gain statistical power. The doses concept will employ ordered hypothesis testing (Rosenbaum 2008). With four distinct treatment groups (group 4 getting no dose and group 1 getting max dose), we will have 4 groups being tested against each other. The statistical tests will be completed as follows: (1) test if group 4 is has smaller effects than groups 1-3; (2) if group 4 shows smaller effects than groups 1-3, test if group 3 shows effects greater than group 4 but less than groups 1-2; (3) if group 3 shows effects between group 4 and groups 1-2, test if group 2 shows effects greater than group 3-4 but less than group 1. This analysis will determine if increasing the amount of treatment increases the treatment effect.

In summary, the experiment should have four experimentally assigned groups: (1) direct participants, (2) indirect participants, (3) non-participants in treated conflict sites, and (4) non-participants in non-treated conflict sites. Subjects in treated conflict sites will be randomly assigned to one of the first three groups, and a baseline survey will be used to ensure pre-treatment similarity between subjects in treated and non-treated conflict sites.

Subject Activities
Subjects will be randomly assigned to treatment conditions. However, if certain community members are deemed integral to the success of projects, we will assign them to direct participation and randomly assign from other community members. This assignment changes the definition of the treatment from "participation in a quick-impact or joint-economic project" to "participation in a quick-impact or joint-economic project with community leaders", but if ECPN's theories of change imply that the involvement of community elites is important, then the design should reflect that. In this case, we will conduct analyses separately for the community leaders and the rest of the community. Community leaders should be considered less members of an experimental group and more part of the experimental intervention. They can be compared to themselves over time (baseline to endline changes) but not to other groups.

Incentives and Attrition
Subjects will need to remain in their assigned experimental group for the course of the experiment, so we will take steps to reduce attrition (subjects leaving the experiment) and contamination (subjects moving into groups to which they were not assigned). In addition to the typical problems of attrition experiments encounter - people move or no longer wish to participate in the study - we'll also

encounter atypical attrition as leave the community after a violent event. These are distinct challenges and must be handled in different ways.

We can diminish typical attrition/contamination with compliance incentives for all participants. First option is a two-track lottery incentive structure, with one track containing relatively cheap items distributed frequently (eg: monthly), and a second track containing more expensive items distributed more infrequently (eg: every 6 months). Participants are eligible to win only if they remain in their assigned group. Potential items to win from the lottery can be (1) household items such as sugar, soap, and non-perishable food, (2) cloth, garments, shoes and hats, and (3) mobile phone credit. Second, we rely on "social pressure" incentives wherein community leaders encourage subjects to participate in the experiment. Since local community leaders are the focus of much of the program and receive the negotiation training, they should have the authority and desire to assist the project by encouraging community members to remain in their assigned groups.

Atypical attrition as a consequence of violence poses a more serious problem. If the fled-to community is distinct from the fled-from community, we cannot equate them in our analysis. The myriad tensions that could result from integrating one community with another will confound our outcomes of interest (intergroup trust and tolerance, economic integration). If the project is to continue in the new community, the only comparison from the new community should be the community's outcomes at different time points. We should not compare the relocated community's subjects with the stable communities' subjects.

We can mitigate the problems caused by atypical attrition in three ways. First, we can assume in the experimental design that at least one conflict site will be lost due to relocation and begin the experiment with more sites than is necessary if all of the sites remain in the experiment. Second, since we have baseline survey data from conflict sites that were not selected for the experiment, we can substitute one of the unused sites for the site discarded due to mass relocation. This works best if the site discarded due to relocation was a control site, because the unused sites and the control sites are similar in that they have received no treatment. Lastly, relocation due to violence can be leveraged as an outcome measure. If the treatment is having the desired effect, we should be more likely to observe relocation due to violence in the control conflict zones than in treated conflict sites.

In summary, subjects will be monitored over the two-year period of the ECPN project. To encourage subjects to remain in their assigned experimental group, we should use a lottery incentive structure and social pressure incentives from other community members. In the case of mass community relocation due to violence, we can (1) plan this type of attrition into the design by monitoring extra conflict

sites, (2) substitute unused conflict sites from the baseline survey in for the site lost due to relocation, and (3) consider the event an outcome .

Outcome Measurements
Goals of the experiment are known to the subjects and that may bias the reporting of outcomes via survey responses. We will mitigate this somewhat through the use of both list and endorsement experiments. To complement the surveys, we should want to develop observational outcome measures that do not rely on subject reporting. Other options include:

1) Using the number of joint economic development and natural resource projects participants implement beyond the grants allocated through the program.

2) Conducting focus groups and measure how members of the two communities interact; namely, how often people from each community speak and how they handle disagreement. For the direct participants who meet frequently, we can monitor their meetings to evaluate the same criteria;

3) Behavioral games (e.g. trust games) or other measures;

4) Spillover to individuals in the social network of participants can be considered a measurable outcome of receiving the treatment. We would expect the direct participants to discuss the treatment most often and so the non-participants in their social network should be the most exposed non-participants. We can compare these exposed non-participants to other non-participants who should receive less spillover from their social environment. We would also expect the direct/indirect participants with the most direct participants in their social networks to be more affected by treatment, since their social network is densely packed with people receiving a treatment. Measuring spillover via social communication shows one way that treating a few individuals in a community can affect the entire community.

Comparisons and Statistical Power
There are multiple outcome comparisons we can make in this experiment, and all comparisons teach us something different about the ECPN project. First, we can see the effect of receiving any treatment vs. receiving no treatment, and the effect of participating directly vs. being in any other category. These comparisons use all of the data and consequently have the most statistical power. We can also make comparisons between subjects in different groups, which will tell us the difference between participating in every project (direct participation) and only participating in a one-off event (indirect participation), for example. Or comparisons within groups over time, which will tell us the effect of a particular treatment assignment on an individual, such as direct participation. These group-

specific comparisons have less statistical power, meaning a lower chance of seeing a statistically significant effect even if one is present.

If low statistical power becomes a concern for the project due to small sample size, high intra-cluster correlations, non-equivalent treatment and control groups, participant attrition, or treatment spillover, there are multiple ways to deal with low statistical power. First, we can use the concept of treatment "doses", which are differences in treatment effect based on the magnitude of treatment. Our treatment conditions are not random, they are different amounts of treatment. Direct participation as the maximum dose, indirect participation as the medium dose, non-participation in treated conflict sites as the small dose, and non-participation in control conflict sites as no dose. We expect the magnitude of the treatment effect to increase with the magnitude of the treatment, and modeling that increases our likelihood of finding a statistical effect. Instead of our multiple treatment conditions posing a problem to statistical power, we can leverage the doses inherent in different treatment conditions to gain statistical power.

Doses assist us in finding a treatment effect overall, but does not help us determine the difference in effect between groups. While the simplest way to determine the difference in effect between groups is to increase the number of subjects in each group, we can also employ coherent hypothesis testing. Coherent hypothesis testing can increase statistical power when the treatment is expected to affect multiple outcomes. Instead of analyzing the treatment's effect on each outcome individually, we would analyze its effect on the outcomes together. The logic of coherent hypothesis testing is as follows: we are less likely to see three outcomes correlated with our treatment by chance than we are of seeing one outcome correlated with our treatment by chance. For example, if we predict that direct participation will cause (1) trust towards farmers/pastoralists, (2) positive interactions between farmers/pastoralists, and (3) individuals engaging in more community projects, we increase statistical power but testing those three hypotheses together instead of separately. In essence, by decreasing the likelihood of the seeing a correlation if there is no effect, we increase the likelihood of statistical significance if there is an effect.