Supporting Information for

How Contact Can Promote Societal Change Amid Conflict: An Intergroup Contact Field Experiment in Nigeria

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Datasets S1 to S4

Supporting Information Text

Intervention Details. To address the rising conflict in the Middle Belt in Nigeria, nonprofit organization Mercy Corps implemented a four-year, USAID-funded program titled Engaging Communities for Peace in Nigeria, targeted at over 9,000 people at 10 Middle Belt sites (where a "site" is a location that contains two conflicting communities). The main objective of the program was to foster positive contact between farmers and pastoralists, with the aim of improving their attitudes toward and relationships with each other. The program included three interventions.

The first intervention involved creating joint project committees in which farmers and pastoralists worked together to design and implement development projects that would address intergroup tensions. The process for selecting projects was similar to that of many community-driven development programs. It started with a series of community meetings, beginning with separate farmer and pastoralist meetings that built up to joint decision-making meetings with the two groups together on the project committees. Project committees met weekly during the dry season and bi-weekly during farming and harvest season. Each joint project committee included an even number of farmers and pastoralists, as well as women and youth representatives, and totaled between 12 and 15 members.

Each committee received two grants, one for quick-impact projects, of approximately $2,000, and one for joint economic projects, of approximately $25,000. To inform the selection of projects, the project committees conducted a participatory needs assessment to gather the opinions of various demographic groups within their communities. The project committees then used this information, along with a conflict and resource mapping exercise, to identify resource-based drivers of conflict and select projects to implement with the grant money. The quick-impact projects were conceived as a trust-building initiative, intended to demonstrate that cooperation was possible and in the interest of both groups. Quick-impact projects, managed by both farmers and pastoralists, included hand pumps, construction or rehabilitation of market stalls and schools, rehabilitation of health centers, and construction of fences along grazing routes to protect farmlands. The joint economic projects aimed to address an underlying issue related to the conflict: sharing of scarce resources that impact livelihoods. Pollution of water, affecting both farming and livestock, was the primary issue people raised. As a result, each project committee decided to build a new borehole well, with the committee managing the project and with farmer and pastoralist youth helping to construct the wells. For most communities, the borehole was erected near the end of the 18 month program.

The second intervention was training community leaders in how to mediate disputes so that conflict did not escalate into violence. To help alleviate violence, two joint committees were formed: peace and early warning / early response. In some venues, a peace committee was already in place. In those instances, we worked with the preexisting peace committee to ensure that it was balanced in terms of farmers and pastoralists, as well as gender. Over the course of the two years, 120 people were trained in mediation, and they went on to resolve 528 disputes around local grazing routes, seasonal access to water points, crop damage, cutting down of trees, and water pollution by animals. That said, only 52 of over 1000 respondents in the treatment group had exposure to mediation at endline.

The third intervention was conflict prevention forums for the larger community, in which farmers and pastoralists came together to discuss issues and policies that were affecting them. Government officials also attended these events. Due to time, this aspect of the program was only lightly implemented.

Site Selection. The scoping exercise initially identified more than thirty potential implementation sites with a history of violence. The ECPN implementation team visited these sites to establish community need and obtain community consent for potentially becoming part of the ECPN program. From these visits we narrowed down the list of implementation sites to 24. These 24 sites received a preliminary survey of 10 individuals per farmer and pastoralist community to further identify need. This survey revealed one site that was too close geographically to a larger site, one site that was too remote for feasible implementation, and four sites did not fulfill our "demonstrated need" criteria. Of the remaining 18, three were lost before random assignment or implementation of the program. From the fifteen remaining sites, we randomly assigned ten to intervention, where ECPN would be implemented, and five to control, where ECPN would not be implemented.

Randomization Inference. Our analysis uses randomization-based p-values and confidence intervals (1). These are nonparametric methods to generate p-values (randomization inference) and confidence intervals (bootstrapping). With *randomization inference*, we first shuffle the treatment variable to break the relationship between treatment and outcomes. Next we regress outcomes on treatment using our regression equation and store the resulting coefficient. Lastly, we repeat that process 10,000 times to create the distribution of coefficients we would observe if treatment had no effect on outcomes – the null hypothesis. Our p-value is the proportion of the null distribution that is greater than or equal to our observed coefficient.

Bootstrapping for standard errors is similar, but instead of shuffling the treatment indicator we resample units with replacement. By resampling with replacement, we create the empirical distribution of our data and the range of possible treatment effects we might observe if we repeated the experiment 10,000 times. The treatment effect at the 2.5th percentile and at the 97.5th percentile are equivalent to a 95% confidence interval.

In each of these procedures, we mimic our randomization process by randomizing/resampling the intervention to communities in site-level clusters and within state blocks. This means that both communities in an implementation site (farmers and pastoralists) will always be treated/sampled together and that assignment to the intervention/resampling are conducted separately in Nassarawa and Benue, just as the intervention was assigned in this study. This procedure ensures that our null distribution (for p-values) is created by randomizing the intervention between exchangeable units and that our empirical distribution (for confidence intervals) is created by resampling units as they were sampled.

Power analysis. At community level, we could detect effects of ~0.60 SDs with 0.80 power. We expected better power for our actual analysis because our power analysis does not use randomization inference to generate true p-values. Our community-level power analysis is presented in Figure S1.

At individual level, we could detect effects of ~0.40 SDs with 0.80 power. We expected better power for our actual analysis because our power analysis does not use randomization inference to generate true p-values. Our individual-level power analysis is presented in Figure S2.

We used the EGAP power calculator for the individual-level power analysis. The parameters were: Alpha = 0.05, Tau = 0.40 SD, ICC=0.05, 15 clusters per arm, up to 300 respondents per cluster.

Placebo Tests. Several of our outcomes are survey self-reports, and self-reports could be affected by factors other than the intervention. For example, our survey results are suspect if respondents in treatment communities learned the "correct'' answers better than respondents in control communities (social desirability bias). If social desirability accounts for the effect in survey self-reports, we would also expect differences between treatment and control for other normatively desirable attitudes.

To test social desirability effects, we conduct a placebo analysis using attitudes about violence as a placebo. Attitudes about violence are a good candidate for this placebo because intergroup contact should not affect general attitudes about violence, but respondents may feel social pressure to answer violence questions in a desirable way. We measure attitudes about violence with a six question index asking respondents if it is always, sometimes, rarely, or never justified to use violence in certain situations, such as retaliating against violence or bringing criminals to justice.

Respondents in treatment communities might also express more positive attitudes towards the outgroup if attitudes were becoming more tolerant in treatment villages in a way that was unrelated to the intervention. If attitudes towards any outgroup were becoming more tolerant in treatment communities compared to control communities, we would expect attitudes towards religious outgroups to improve more in treatment communities than control communities. The contact intervention should not affect attitudes towards people from other religions because the farmers and pastoralists are often the same religion.

Respondents in treatment communities also might have had better access to information, and that information changed their attitudes/perceptions. To measure access to information, we use frequency of radio listening. If the treatment communities increased their amount of radio listening significantly more than control communities, it is possible their attitudes/perceptions changed due to information and not the contact intervention.

Estimation for the placebo outcomes mimics estimation for the paper's main outcomes. We also conducted robustness checks on the placebo outcomes as described in this appendix – the "base" column is a 1 for the estimation procedure we would have used in the paper. The results are presented in Tables S1-S8. There was no effect on placebo outcomes.

Alternative Explanations. It is possible that the effects of our intervention are due to impact of the development project around which the contact was organized, rather than intergroup contact itself. The effect could also be due to mediation provided to some community leaders. We ran three analyses to disentangle the effects of contact from the effects of the development projects and mediation. The results are presented in Tables S9-S13.

Effect of contact vs. effect of development projects: The first analysis was to determine if treatment effects were significantly larger for communities where larger proportions of people were aware of, used, and perceived benefit from the projects. In treatment communities, we predicted outcome change with (1) awareness of boreholes, (2) use of boreholes, (3) awareness of quick-impact projects, and (4) perceived benefit from quick-impact projects. If treatment effects are due to development projects and not contact, we would expect larger effects where a greater proportion of respondents were aware of, used, and benefited from the development projects.

Our analysis shows that awareness, use, and benefit from the projects is not significantly related to any outcome (the mean p-value is ~0.32). It's also not the case that any of those variables separately (awareness, use, and benefit) are related to improvements on any outcome. Though the p-values are insignificant, the mean t-statistic for the awareness/use/benefit variables is positive, possibly suggesting that the development projects, while not explaining the bulk of the treatment effect, may have increased the effect. The tables show the mean coefficient, p-value, and t-statistic for each outcome being predicted by each "benefit" variable.

The second analysis looked at whether pastoralists in Benue differed from the rest of the sample. Pastoralists in Benue benefited least from the development projects (especially the main project that built boreholes) because they became displaced from where the boreholes were constructed prior to the endline survey. Here we (1) confirm that pastoralists in Benue were less likely to be aware of, use, or benefit from the development projects and (2) see no significant outcome differences between pastoralists in Benue and the rest of the sample (mean p-value ~0.58).

Effect of contact vs. effect of mediation:

The third analysis concerned mediation. While we cannot rule out that the effect was due to the meditation training, only 52 of the over 1000 endline respondents in treatment sites were aware of the mediation intervention.

Family-Wise Error Rate (FWER). To control the FWER, we conducted within-family hypothesis corrections using the Holm correction. The results are presented in Table S14. For two of our outcomes (pastoralists in market and perceptions of security), the findings remain statistically significant. For the two other outcomes (self-reported contact and attitudes), significance levels shift from statistically significant to marginally significant (.04 to .08).

The exploratory individual-level data has only one outcome per family (except for the Cooperation family, for which no p-values were significant without correction). We therefore do not show "corrected" individual-level p-values. If we combine all individual-level hypotheses into a single family and use the Holm correction, the outcome that was statistically significant in the individual-level data (*Self-reported contact*) becomes marginally significant(p=0.088).

Robustness checks. We conducted several robustness checks for our results. The results are presented in Tables S15-S26.

For behavioral outcomes, we confirmed that the results did not depend on the inclusion of two senior enumerators who interacted with the research team frequently and may have intuited the study's hypotheses. We first checked the change over time in outcomes they reported. Descriptively, the two senior enumerators observed *less* interaction in treatment sites over time, suggesting that they were not influenced by knowledge of the study's hypotheses. We then repeated the main analysis but removed observations from these two enumerators. Coefficients with their data removed are substantively similar to coefficients with their data included; p-values are higher mainly as a result of fewer observations (and therefore degrees of freedom).

For survey data, we shows different ways of making indices (additive vs inverse-covariance weighted), different models for estimating effects (differencing vs controlling-for), and different ways of coding count variables (raw vs ranked). The method used in the paper is denoted in the "base" column. The base index-creation method is always inverse-covariance weighted indices. The base estimation method is differencing unless the baseline difference between the treatment and control groups is below 0.20 standard deviations (specified in our pre-analysis plan). When baseline differences are large, the controlling-for method is biased (2).; The base method of handling count variables is dense rank (only applicable to contact outcomes). Where a method is not applicable, it is not shown. For example, we do not show the controlling-for method when baseline differences are greater than 0.20 standard deviations. The results are substantively similar. With differencing and additive indices, p-values tend to be higher due to lower precision.

We also shows regressions using demographic characteristics as control variables to confirm that accounting for these variables does not change the study's results.

Heterogeneous effects. We looked for heterogeneous effects by state and by group (farmers and pastoralists). Coefficients and p-values estimating heterogeneous effects were calculated with robust OLS regression using site-level clusters. The regression interacted the treatment indicator with the state indicator or the group indicator.

The results for states are presented in Table S27. Benue was the reference category so Table S shows differences for Nasarawa. There are no significant differences in treatment effect by state.

The results for group (farmers/pastoralists) are presented in Table S28. Farmers were the reference category so this table shows differences for pastoralists. There are no significant differences in treatment effect by group.

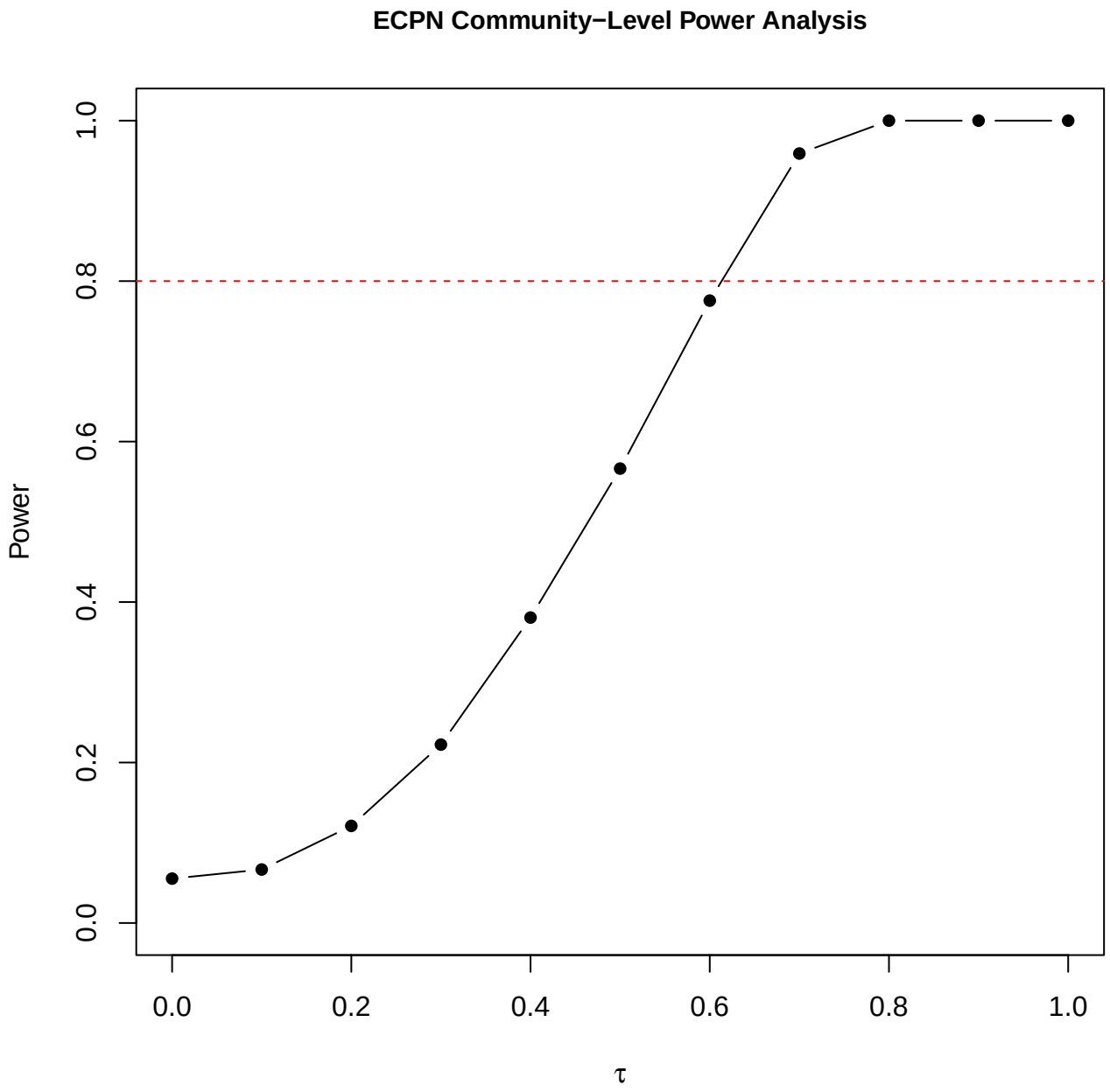

Fig. S1. Community-level power analysis.

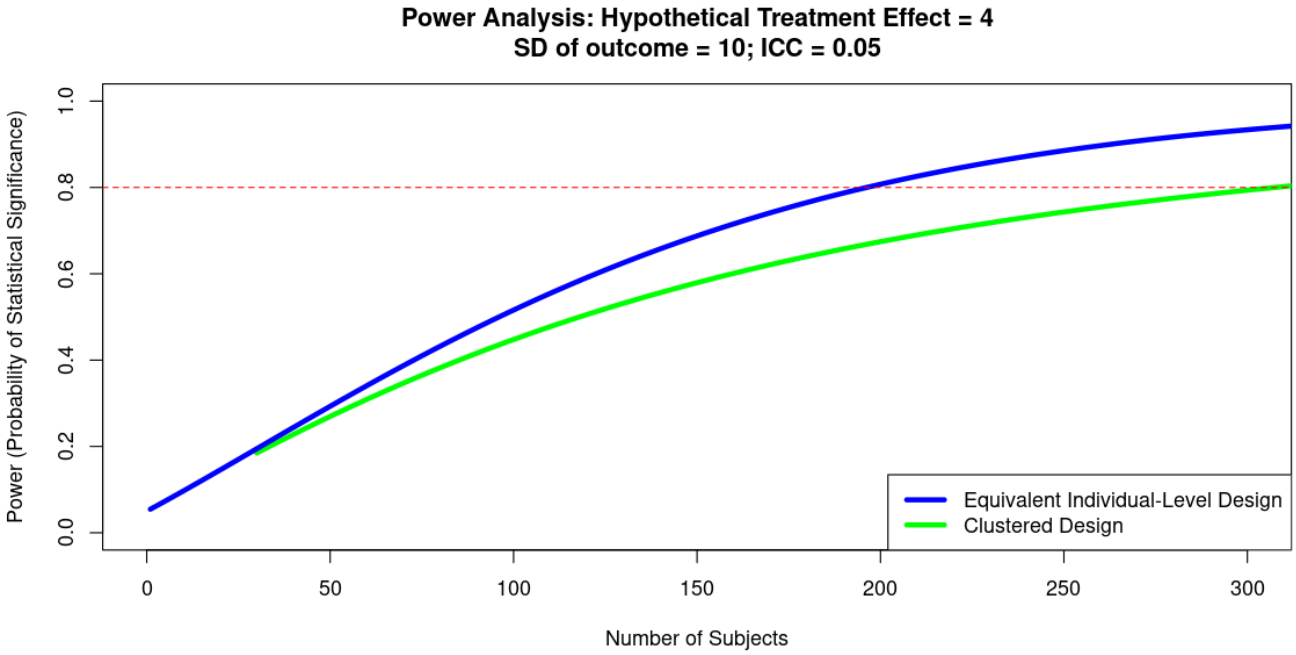

Fig. S2. Individual-level power analysis.

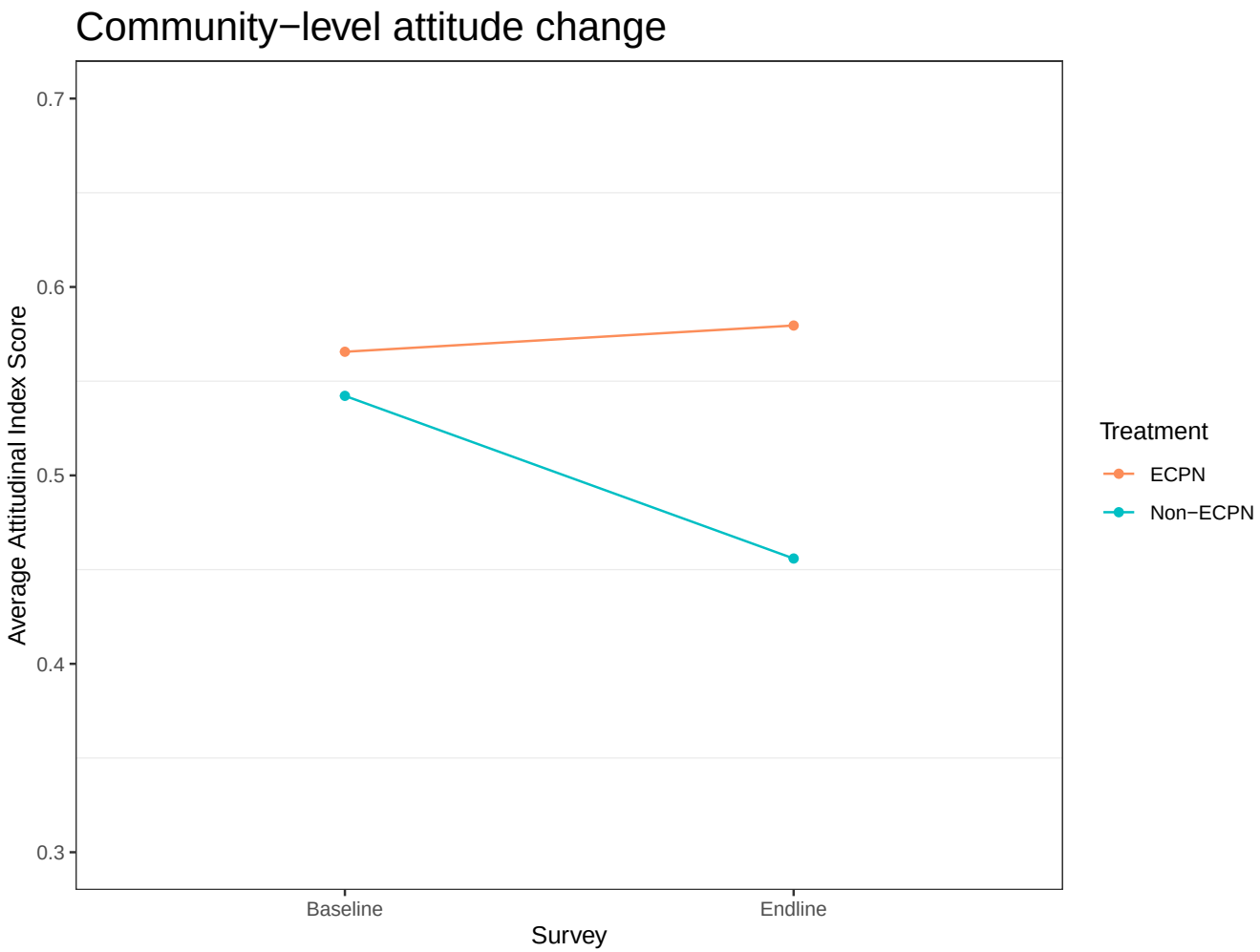

Fig. S3. Difference-in-differences plot comparing change in treatment to change in control.

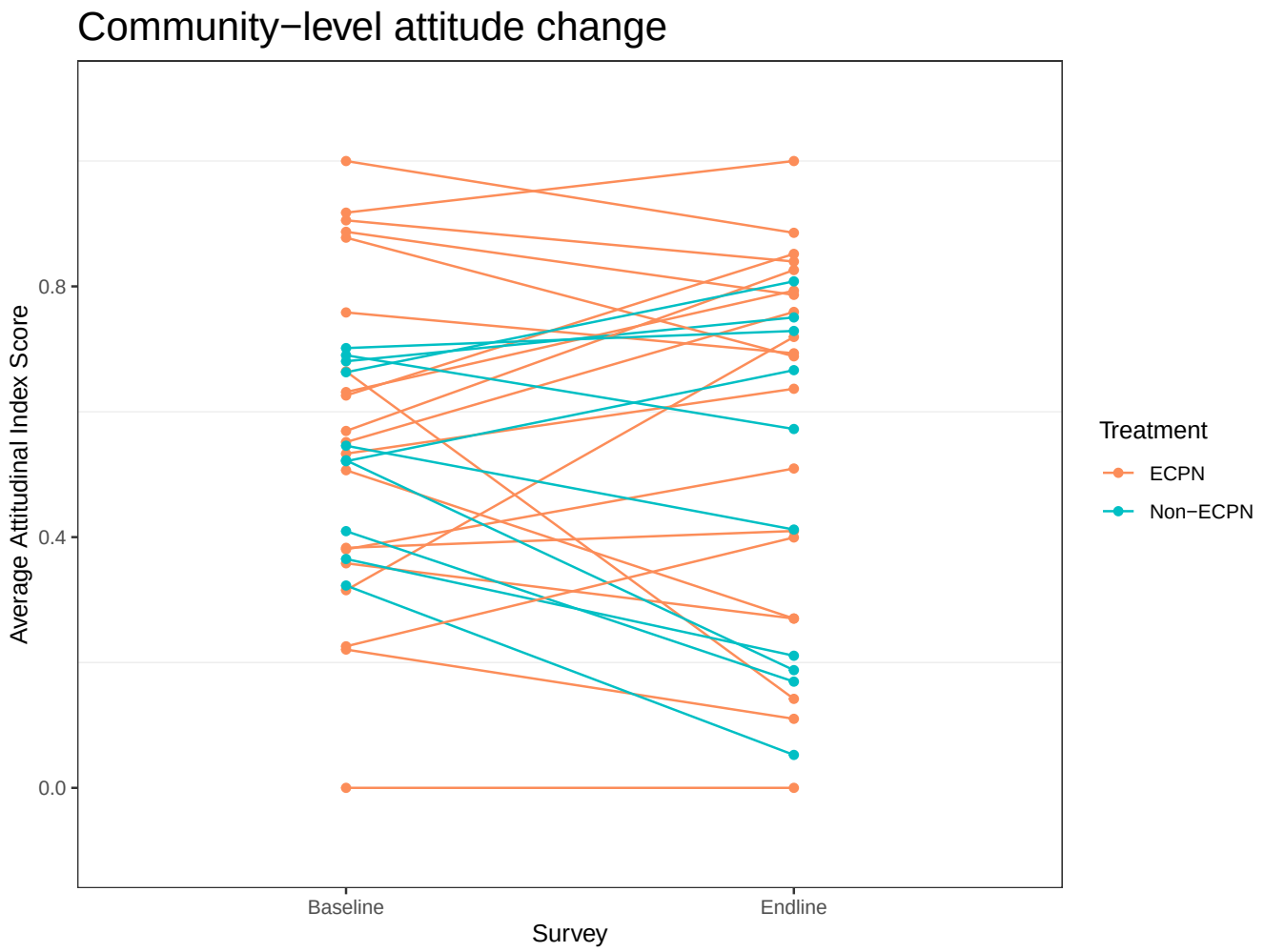

Fig. S4. Each point represents a community. This graph shows (1) that overall changes are not driven by a large change in a single community and (2) that the overall change does not reflect a ceiling or floor effect.

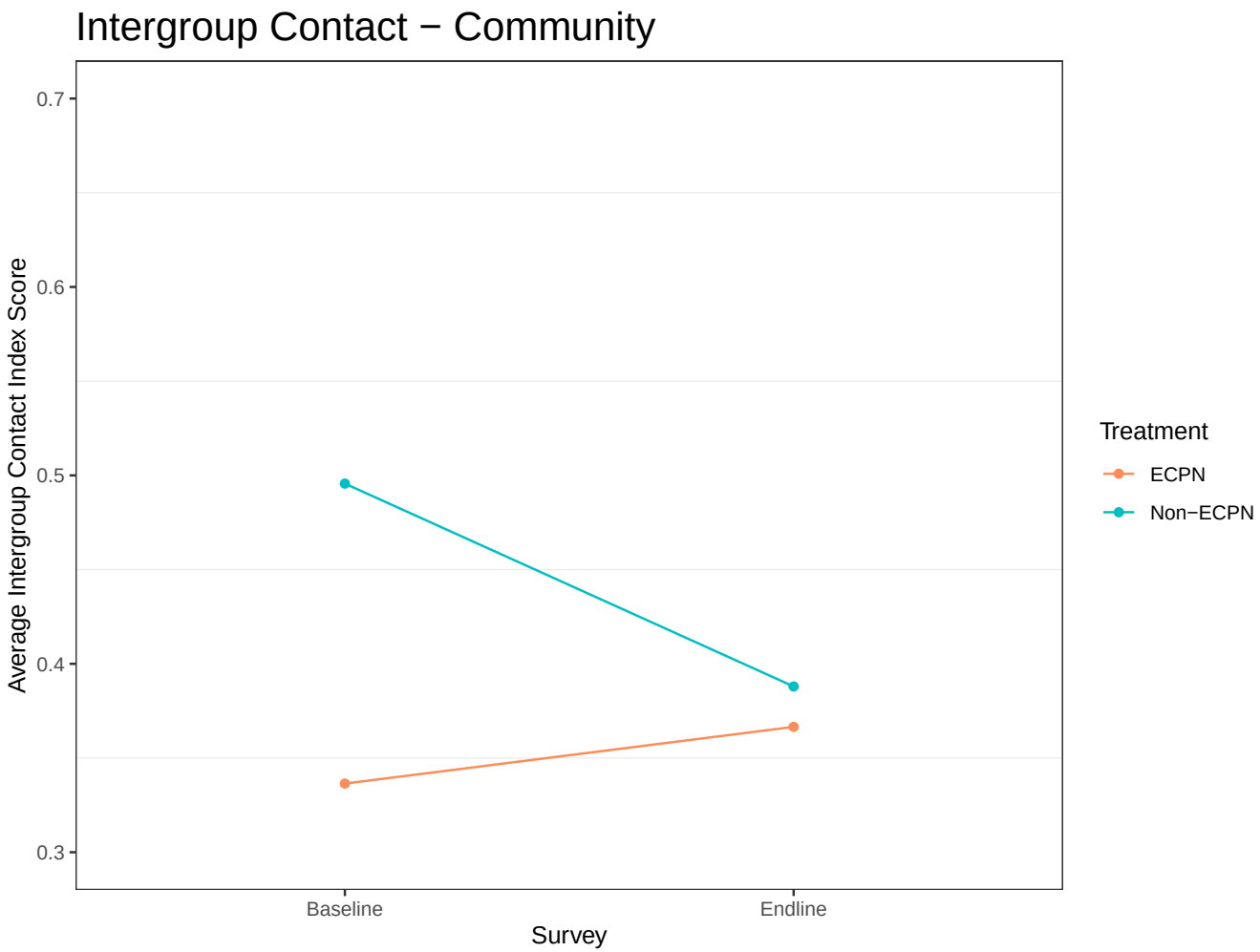

Fig. S5. Difference-in-differences plot comparing change in treatment to change in control.

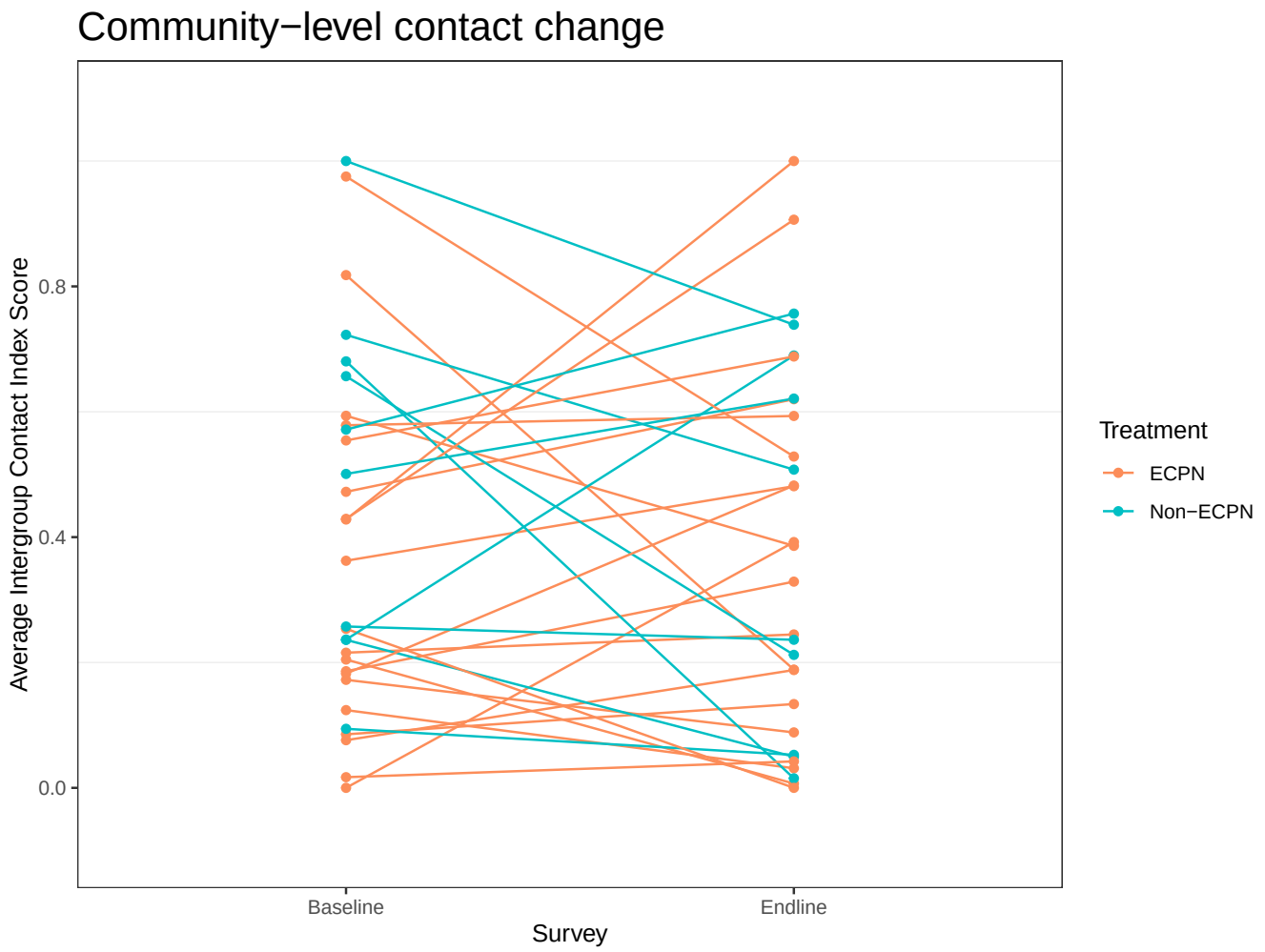

Fig. S6. Each point represents a community. This graph shows (1) that overall changes are not driven by a large change in a single community and (2) that the overall change does not reflect a ceiling or floor effect.

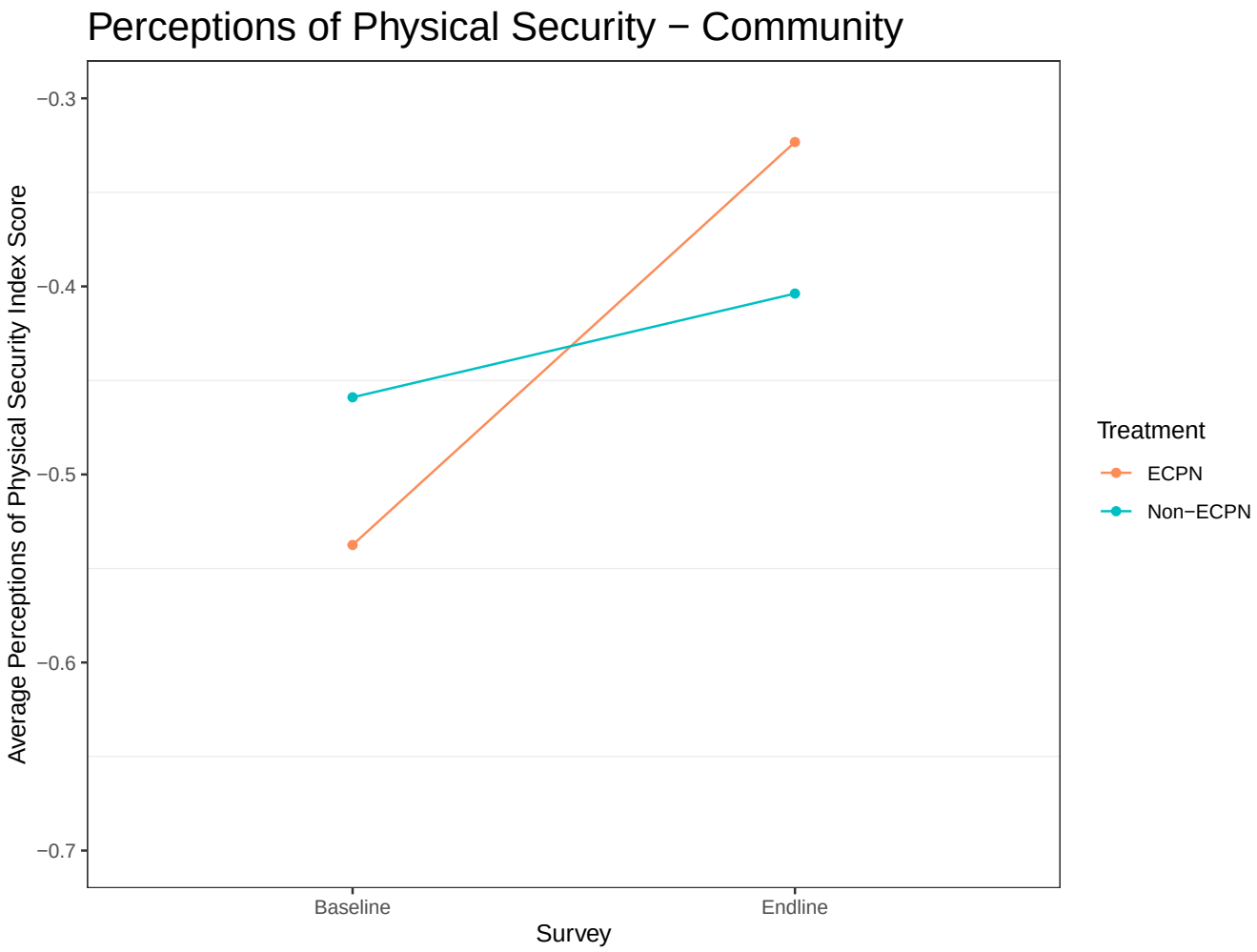

Fig. S7. Difference-in-differences plot comparing change in treatment to change in control.

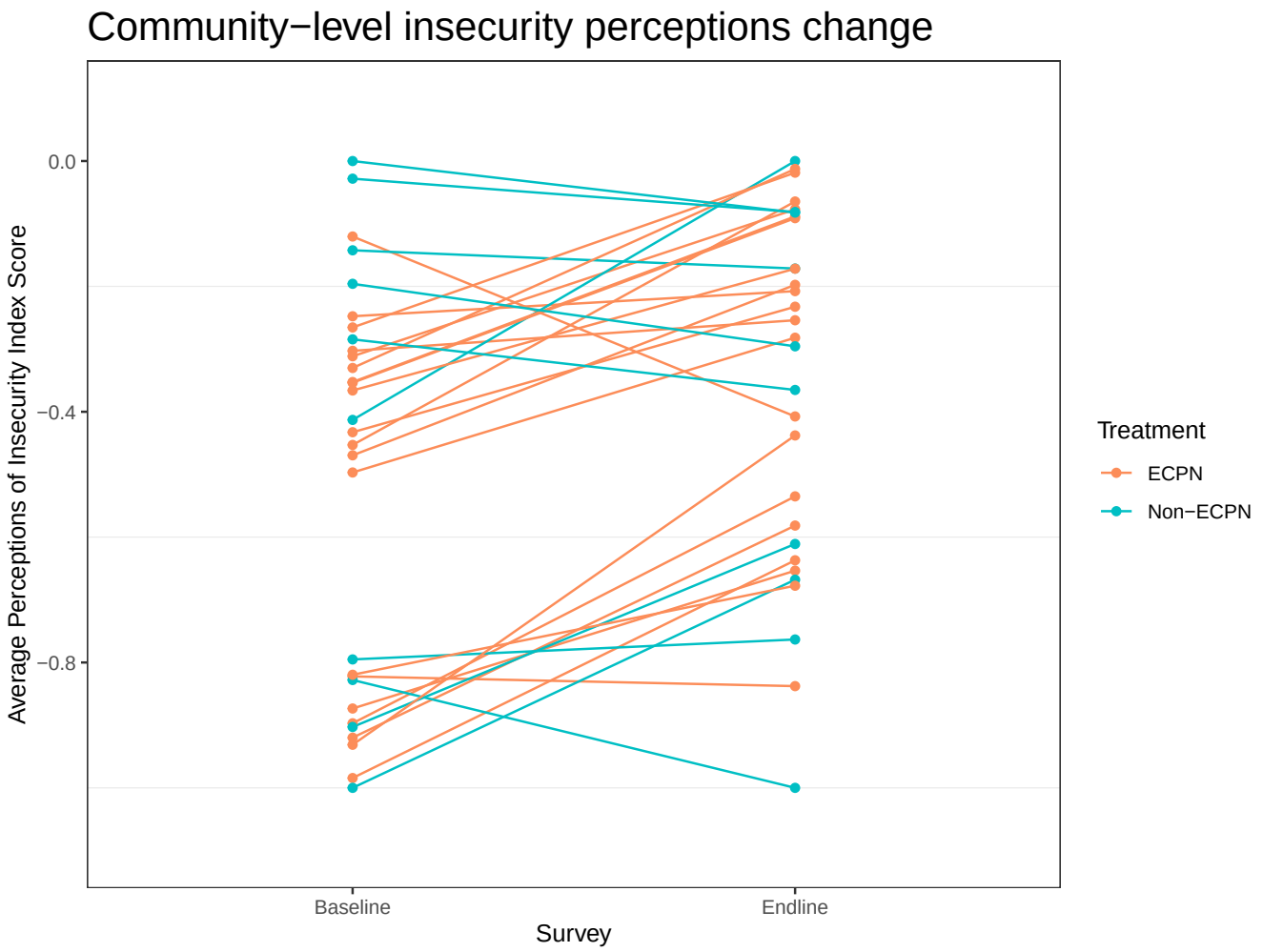

Fig. S8. Each point represents a community. This graph shows (1) that overall changes are not driven by a large change in a single community and (2) that the overall change does not reflect a ceiling or floor effect.

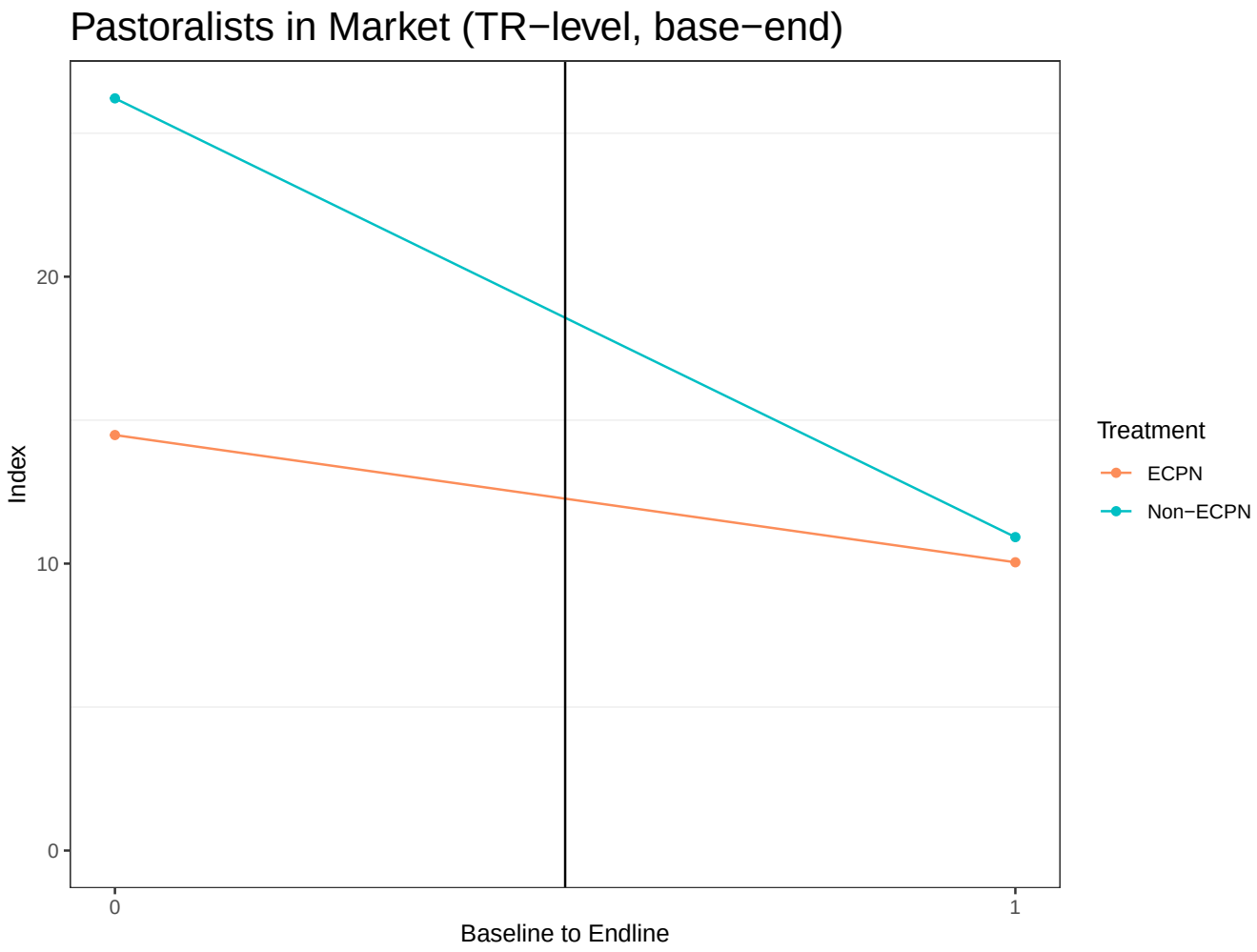

Fig. S9. Difference-in-differences plot comparing change in treatment to change in control.

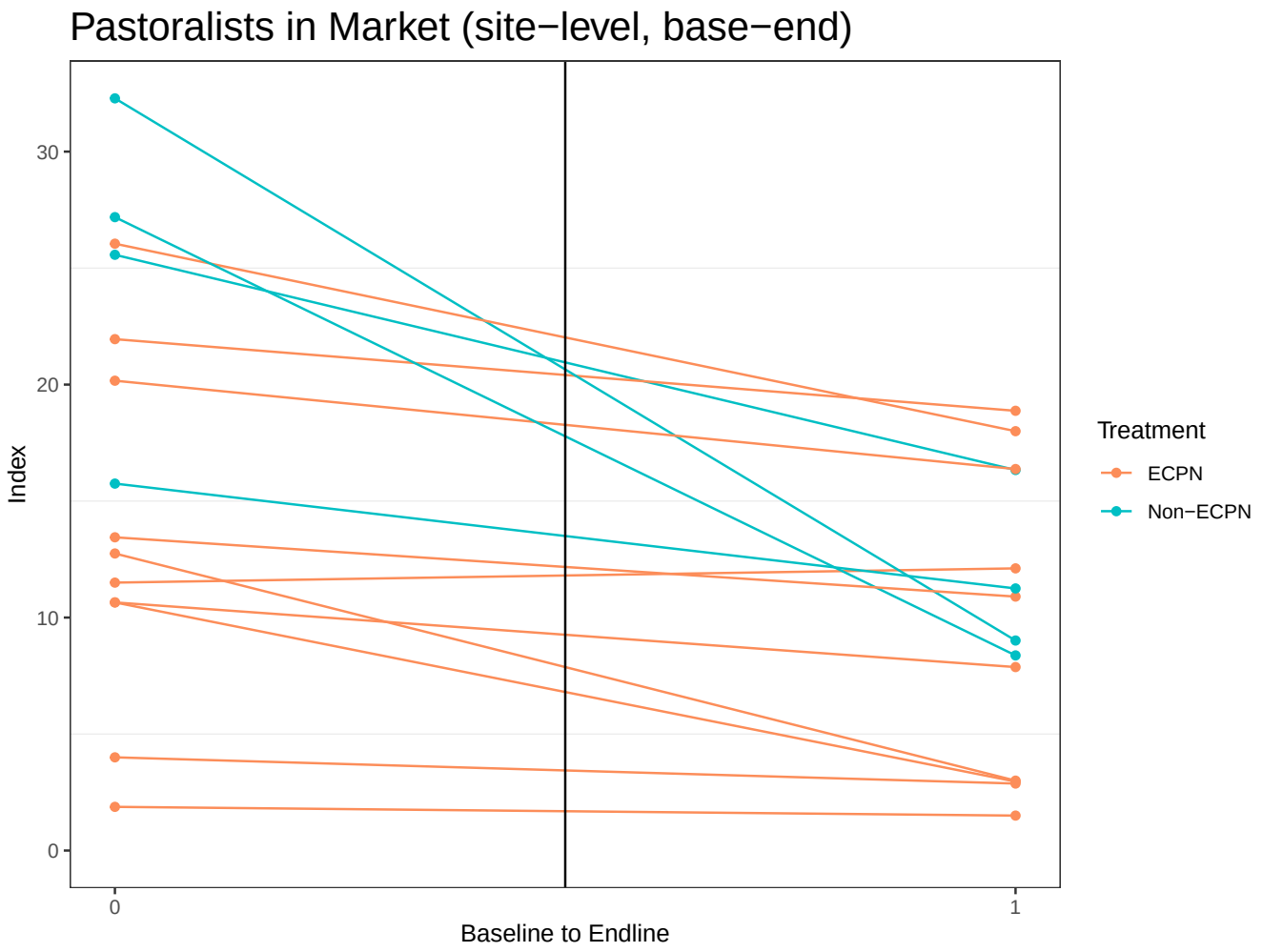

Fig. S10. Each point represents a site. This graph shows (1) that overall changes are not driven by a large change in a single community and (2) that the overall change does not reflect a ceiling or floor effect.

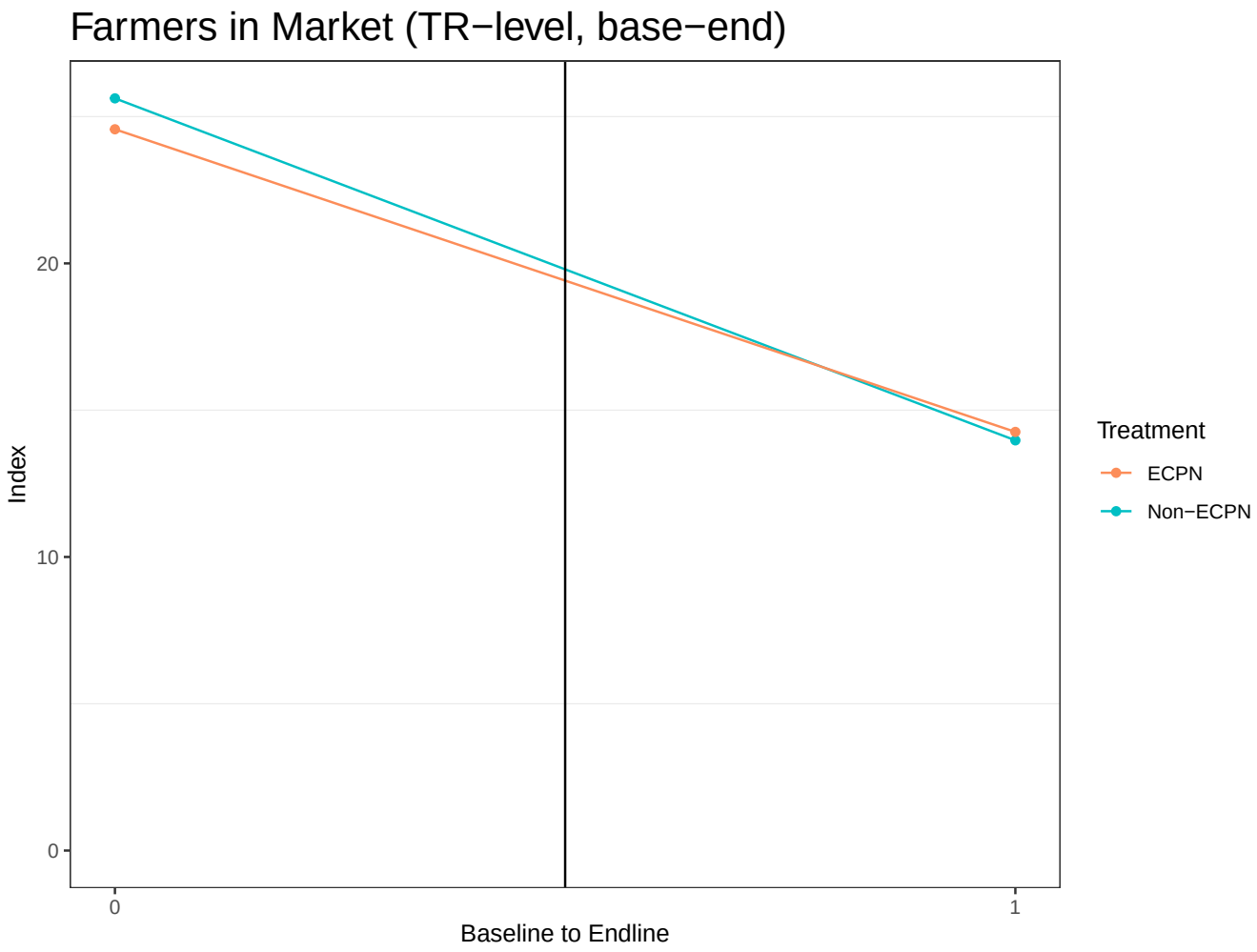

Fig. S11. Difference-in-differences plot comparing change in treatment to change in control.

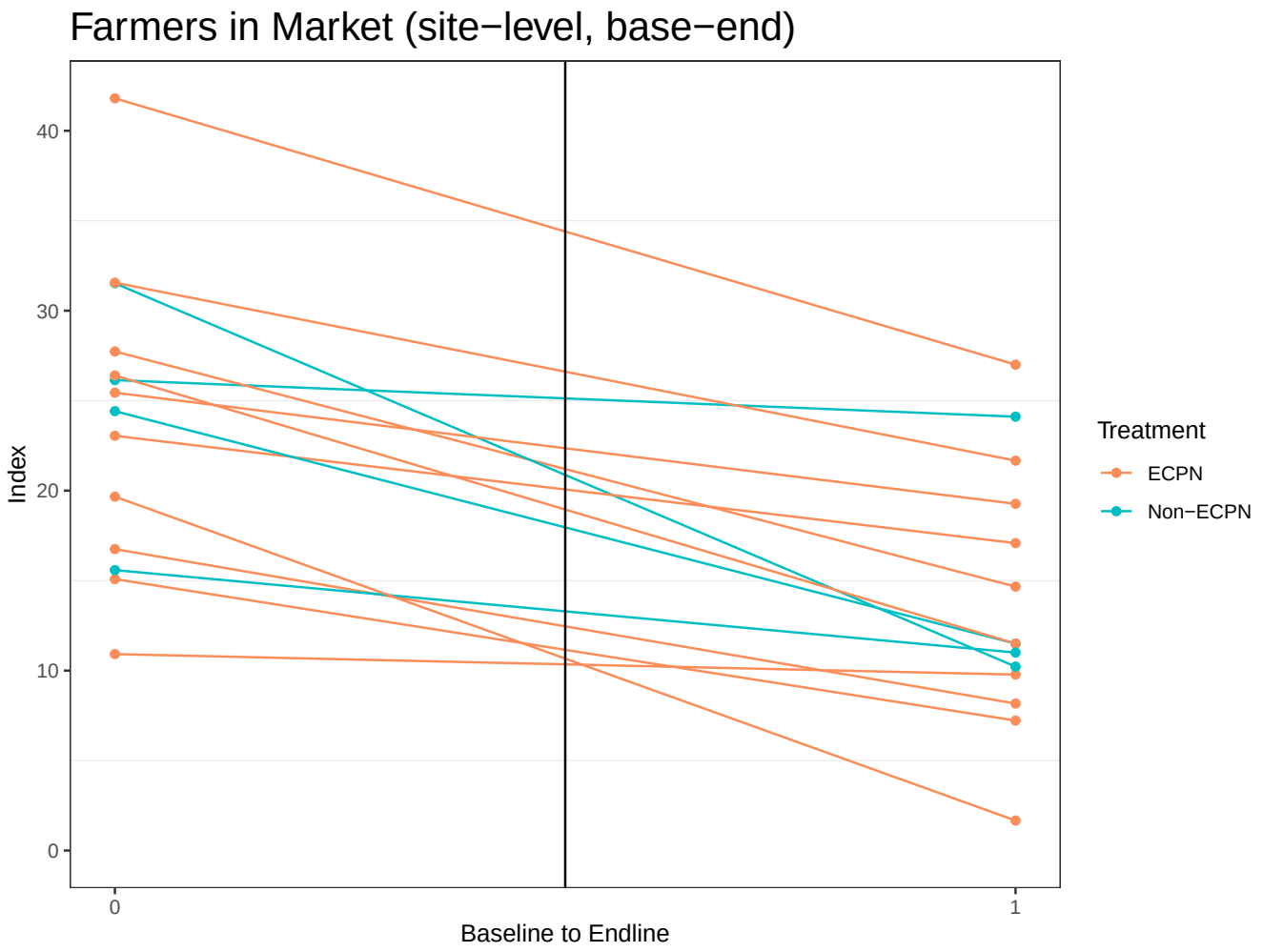

Fig. S12. Each point represents a site. This graph shows (1) that overall changes are not driven by a large change in a single community and (2) that the overall change does not reflect a ceiling or floor effect.

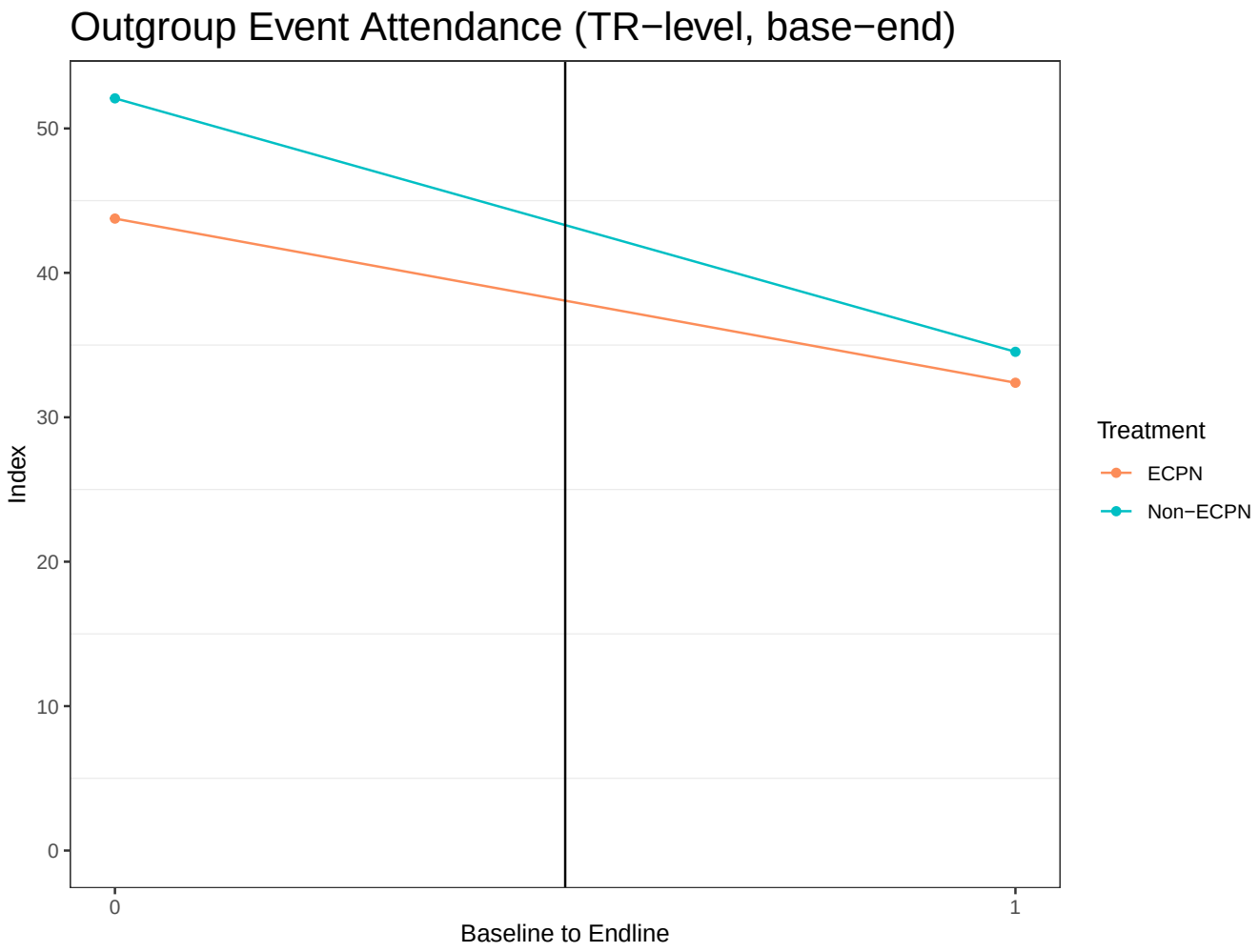

Fig. S13. Difference-in-differences plot comparing change in treatment to change in control.

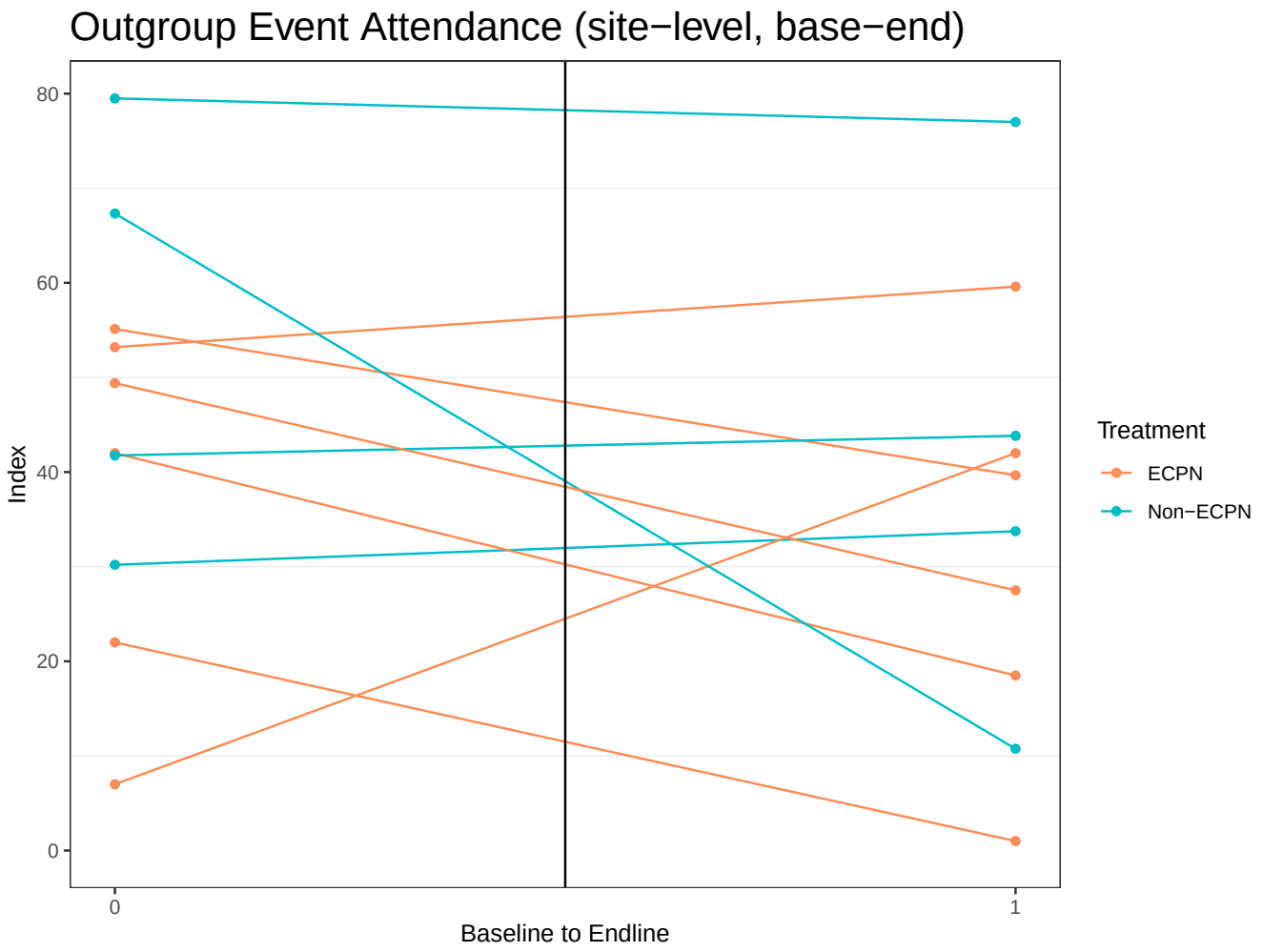

Fig. S14. Each point represents a site. This graph shows (1) that overall changes are not driven by a large change in a single community and (2) that the overall change does not reflect a ceiling or floor effect.

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Table S1. Community Placebo: Attitudes towards violence index. Effect of ECPN on placebo outcome using alternative methods of estimation and index construction. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Controlling-for & ICW	0.010	0.466	0
Controlling-for & Additive	0.004	0.441	0
Differencing & ICW	-0.067	0.687	1
Differencing & Additive	-0.027	0.679	0

Table S2. Community Placebo: Components of violence index. Effect of ECPN on components of placebo index (attitudes towards violence) using alternative methods of estimation and index construction. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Bring criminals to justice: Controlling-for	0.034	0.252	0
Bring criminals to justice: Differencing	-0.092	0.792	1
Defend ones group: Controlling-for	-0.026	0.659	1
Defend ones group: Differencing	-0.026	0.663	0
Defend ones religion: Controlling-for	-0.031	0.662	1
Defend ones religion: Differencing	-0.031	0.671	0
Force the government to change their policies: Controlling-for	0.004	0.436	1
Force the government to change their policies: Differencing	-0.012	0.640	0
Maintain culture and traditions: Controlling-for	-0.004	0.558	1
Maintain culture and traditions: Differencing	-0.011	0.585	0
Retaliate against violence: Controlling-for	-0.007	0.658	1
Retaliate against violence: Differencing	0.011	0.387	0

Table S3. Community Placebo: Trust towards religious outgroups. Effect of ECPN on placebo outcome using alternative methods of estimation and index construction. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Controlling-for	0.017	0.349	1
Differencing	-0.002	0.519	0

Table S4. Community Placebo: Radio listening frequency. Effect of ECPN on placebo outcome using alternative methods of estimation and index construction. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Controlling-for	0.021	0.430	1
Differencing	0.021	0.441	0

Table S5. Individual Placebo: Attitudes towards violence index. Effect of ECPN on placebo outcome using alternative methods of estimation and index construction. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Non: Controlling-for & ICW	-0.057	0.801	0
Part: Controlling-for & ICW	0.013	0.413	0
Non: Controlling-for & Additive	-0.163	0.770	0
Part: Controlling-for & Additive	0.022	0.441	0
Non: Differencing & ICW	-0.033	0.642	1
Part: Differencing & ICW	-0.016	0.549	1
Non: Differencing & Additive	-0.058	0.580	0
Part: Differencing & Additive	-0.023	0.508	0

Table S6. Individual Placebo: Components of violence index. Effect of ECPN on components of placebo index (attitudes towards violence) using alternative methods of estimation and index construction. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Non: Bring criminals to justice: Controlling-for	-0.154	0.680	0
Part: Bring criminals to justice: Controlling-for	-0.031	0.535	0
Non: Bring criminals to justice: Differencing	-0.292	0.813	1
Part: Bring criminals to justice: Differencing	-0.494	0.933	1
Non: Defend ones group: Controlling-for	-0.078	0.616	1
Part: Defend ones group: Controlling-for	0.039	0.440	1
Non: Defend ones group: Differencing	0.034	0.447	0
Part: Defend ones group: Differencing	-0.021	0.525	0
Non: Defend ones religion: Controlling-for	-0.250	0.892	1
Part: Defend ones religion: Controlling-for	0.141	0.276	1
Non: Defend ones religion: Differencing	-0.143	0.726	0
Part: Defend ones religion: Differencing	0.067	0.378	0
Non: Force the government to change their policies: Controlling-for	-0.227	0.781	1
Part: Force the government to change their policies: Controlling-for	0.031	0.457	1
Non: Force the government to change their policies: Differencing	-0.142	0.684	0
Part: Force the government to change their policies: Differencing	0.013	0.496	0
Non: Maintain culture and traditions: Controlling-for	-0.190	0.797	1
Part: Maintain culture and traditions: Controlling-for	0.024	0.443	1
Non: Maintain culture and traditions: Differencing	0.022	0.453	0
Part: Maintain culture and traditions: Differencing	0.161	0.261	0
Non: Retaliate against violence: Controlling-for	-0.091	0.639	1
Part: Retaliate against violence: Controlling-for	-0.080	0.601	1
Non: Retaliate against violence: Differencing	0.176	0.254	0
Part: Retaliate against violence: Differencing	0.156	0.286	0

Table S7. Individual Placebo: Trust towards religious outgroups. Effect of ECPN on placebo outcome using alternative methods of estimation and index construction. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Non: Controlling-for	0.178	0.208	1
Part: Controlling-for	-0.060	0.586	1
Non: Differencing	0.140	0.283	0
Part: Differencing	-0.079	0.616	0

Table S8. Individual Placebo: Radio listening frequency. Effect of ECPN on placebo outcome using alternative methods of estimation and index construction. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Non: Controlling-for	0.178	0.208	1
Part: Controlling-for	-0.060	0.586	1
Non: Differencing	0.140	0.283	0
Part: Differencing	-0.079	0.616	0

Table S9. Benefit variables affect on outcomes. Mean coefficient, p-value, and t-statistic for each outcome, across benefit variables. Benefit variables are not significantly related to any outcome.

	coefs	pvals	tstat
Attitudes	0.1936077	0.1836764	0.9056779
Security	0.2448961	0.2794338	1.2928731
Contact	0.3207580	0.2643255	1.3698100
Contact Willingness	0.3714763	0.0915421	1.9362679
Endorsement Exp	0.1398422	0.6779022	0.3172168
PGG Donation	-0.0306856	0.2763725	-1.3456686
PGG Amount	-35.9157762	0.4960119	-0.7851551
All	NA	0.3241806	0.5272889

Table S10. Benefit variables affect on outcomes. Mean coefficient, p-value, and t-statistic for each benefit variables, across outcomes. This tables removes Public Goods Game outcomes because (1) their coefficients are on a different scale than the survey outcomes and (2) the coefficients and t-statistics are negative, which could hide the effect of benefit variables on the other outcomes. However, this table shows that benefit variables are not significantly related to any survey outcome.

	coefs	pvals	tstat
Borehole use	0.3355362	0.2547888	1.5838113
Borehole aware	0.2377543	0.3878788	0.9939178
QIP benefit	0.0657282	0.3007221	-0.0346520
QIP aware	0.3774457	0.2541144	2.1143994
all	NA	0.2993760	1.1643691

Table S11. Benefit pastoralists awareness and benefit from development projects. Coefficients, t-statistics, and p-values comparing Benue pastoralists in the treatment group to the rest of the treatment group. Benue pastoralists were significantly less likely to be aware of the boreholes or use the boreholes; they were marginally less likely to be aware of the quick-impact projects.

	estimate	statistic	p.value
Borehole Awareness	-0.4309759	-4.3361033	0.0111897
Borehole Use	-0.3406948	-5.1907843	0.0058327
QIP Awareness	-0.2907236	-2.4706291	0.0662889
QIP Benefit	-0.0644359	-0.6369732	0.5574075

Table S12. Benefit outcomes for pastoralists in the treatment group. Coefficients, p-values, and t-statistics comparing Benue pastoralists in the treatment group to the rest of the treatment group. Treatment effects are not smaller among Benue pastoralists than the rest of the treatment group. Note: the mean coefficient excludes the Public Goods Game, which is on a different scale than the other variables; it is included in the p-value and t-statistic.

	coef	pvalue	tstat
Attitudes	-0.0317303	0.8260431	-0.2398319
Security	-0.1668610	0.6400484	-0.5187857
Contact	0.2343247	0.3188031	1.1945197
Contact Willingness	0.0301262	0.8672572	0.1820363
Endorsement Exp	-0.5074066	0.1968861	-1.6571511
PGG Donation	-0.0342750	0.5305509	-0.7077525
PGG Amount	49.9043759	0.6549041	0.4949424
All	-0.0883094	0.5763561	-0.1788604

Table S13. Mediation Exposure. Exposure to mediation in treatment and control sites in endline survey. Only 52 randomly selected respondents in treatment had any exposure to mediation. No control respondents had exposure to mediation.

	Control	Treatment
None	504	996
Exposure	0	52

Table S14. P-values controlling the family-wise error rate.

Outcome	None	Holm	Family
Pastoralists in Market	0.0014	0.0070	Contact
Self-reported contact	0.0595	0.2380	Contact
Contact willingness	0.2389	0.5568	Contact
Farmers in Market	0.2416	0.5568	Contact
Outgroup event attendance	0.1856	0.5568	Contact
Perceptions of security	0.0205	0.0205	Insecurity
Self-reported attitudes	0.0448	0.0896	Attitudes
Endorsement Experiment	0.2117	0.2117	Attitudes
Public Goods Donation	0.2938	0.5876	Cooperation
Public Goods Amount	0.8522	0.8522	Cooperation

Table S15. Robustness check for behavioral observations. This table shows the baseline-endline change in outcomes for the two senior enumerators who may have intuited the study's hypotheses. The first column shows coefficients from OLS regression, the second column shows p-values. The row names show outcomes (pastoralists in the market, farmers in the market, and outgroup attending events). The two enumerators reported decreases in market outcomes in treatment sites and no change to event outcomes in treatment sites, suggesting that they were not influenced by knowledge of the study's hypotheses.

	coefficient	p-value
market-pastoralists_check	-4.4079254	0.1744306
market-farmers_check	-10.8863636	0.0161117
events-outgroup_check	0.1363636	0.9929502

Table S16. Robustness check for behavioral observations. This table shows behavioral observation outcomes with two senior enumerators removed. The first column shows coefficients from OLS regression, the second column shows p-values. The row names show outcomes (pastoralists in the market, farmers in the market, and outgroup attending events) and subsets of the data (all data, data without two enumerators, and data from only those two enumerators).

	coefficient	p-value
market-pastoralists_all	12.532	0.001
market-pastoralists_without	10.285	0.103
market-farmers_all	3.339	0.242
market-farmers_without	1.286	0.456
events-outgroup_all	9.228	0.186
events-outgroup_without	6.333	0.136

Table S17. Community Attitudes. Effect of ECPN on self-reported attitudes using alternative methods of estimation and index construction. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Controlling-for & ICW	0.116	0.045	1
Controlling-for & Additive	0.093	0.038	0
Differencing & ICW	0.100	0.145	0
Differencing & Additive	0.073	0.116	0

Table S18. Community Perceptions of Security. Effect of ECPN on perceptions of security using alternative methods of estimation and index construction. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Differencing & ICW	0.159	0.020	1
Differencing & Additive	0.054	0.213	0

Table S19. Community Contact. Effect of ECPN on contact using alternative methods of estimation, index construction, and measuring count variables. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Differencing & ICW & Ranks	0.138	0.060	1
Differencing & Additive & Ranks	0.015	0.182	0
Differencing & ICW & Categories	0.120	0.064	0
Differencing & ICW & Raw	0.071	0.207	0

Table S20. Community Contact Willingness (Percent Experiment). Effect of ECPN on willingness to have contact with the outgroup using alternative methods of estimation. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Controlling-for	0.093	0.051	0
Differencing	0.062	0.239	1

Table S21. Community Endorsement Experiment. Effect of ECPN on endorsement experiment using alternative methods of estimation. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Controlling-for	0.103	0.158	0
Differencing	0.123	0.212	1

Table S22. Community Public Goods Game. Effect of ECPN on probability of donating and on donation amount. The first column shows coefficients from OLS regression and the second column shows p-values from randomization inference.

	coefficient	p-value
Donation (binary)	0.022	0.294
Donation amount	-35.124	0.852

Table S23. Individual-level Attitudes. Effect of ECPN on self-reported attitudes using alternative methods of estimation and index construction. Rows show participants and nonparticipants. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Non: Differencing & ICW	0.054	0.130	1
Part: Differencing & ICW	0.060	0.130	1
Non: Differencing & Additive	0.183	0.144	0
Part: Differencing & Additive	0.296	0.049	0

Table S24. Individual-level Perceptions of Security. Effect of ECPN on perceptions of security using alternative methods of estimation and index construction. Rows show participants and nonparticipants. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Non: Differencing & ICW	0.045	0.178	1
Part: Differencing & ICW	0.050	0.186	1
Non: Differencing & Additive	0.083	0.244	0
Part: Differencing & Additive	0.146	0.123	0

Table S25. Individual-level Contact. Effect of ECPN on contact using alternative methods of estimation, index construction, and measuring count variables. Rows show participants and nonparticipants. The first column shows coefficients from OLS regression, the second column shows p-values from randomization inference, and the third column shows which method was used in the paper.

	coefficient	p-value	base
Non: Differencing & ICW & Ranks	0.002	0.492	1
Part: Differencing & ICW & Ranks	0.098	0.018	1
Non: Differencing & Additive & Ranks	-0.005	0.580	0
Part: Differencing & Additive & Ranks	0.063	0.017	0
Non: Differencing & ICW & Categories	0.017	0.407	0
Part: Differencing & ICW & Categories	0.130	0.029	0
Non: Differencing & ICW & Raw	-0.002	0.531	0
Part: Differencing & ICW & Raw	0.066	0.025	0

Table S26. Individual Public Goods Game. Effect of ECPN on probability of donating and on donation amount. The first column shows coefficients from OLS regression and the second column shows p-values from randomization inference.

	coefficient	p-value
Non: Donation (binary)	0.050	0.081
Part: Donation (binary)	0.020	0.295
Non: Donation amount	-27.023	0.743
Part: Donation amount	-53.740	0.875

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Table S27. State-level heterogeneous effects in community-level analysis. There are not significant differences between the effect of the contact intervention by state. The first column shows coefficients from OLS regression, the second column shows p-values from OLS regression.

	coefficient	p-value
Attitudes	-0.176	0.315
Perceptions of security	0.133	0.383
Contact	-0.104	0.469
Percent Experiment	-0.251	0.158
Endorsement Experiment	0.191	0.488
PGG donation	-0.093	0.257
PGG amount	-84.807	0.442

Table S28. Farmer-pastoralist heterogeneous effects in community-level analysis. There are not significant differences between the effect of the contact intervention by farmer/pastoralist. The first column shows coefficients from OLS regression, the second column shows p-values from OLS regression.

	coefficient	p-value
Attitudes	-0.090	0.278
Perceptions of security	0.021	0.905
Contact	0.031	0.911
Percent Experiment	-0.014	0.913
Endorsement Experiment	-0.194	0.626
PGG donation	-0.064	0.386
PGG amount	23.284	0.784

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Table S29. Balance: Observational Data All Outcomes.

	Control.strat	Treatment.strat	adj.diff.strat	adj.diff.null.sd.strat	std.diff.strat	z.strat	p.strat
Pastoralists_index_rank_events	35.415	25.141	-10.275	12.377	-0.585	-0.830	0.406
Farmers_index_rank_events	32.978	36.182	3.204	7.104	0.303	0.451	0.652
Pastoralists_index_rank_markets	25.458	14.151	-11.308	6.576	-1.375	-1.720	0.086
Farmers_index_rank_markets	24.417	25.250	0.834	5.699	0.101	0.146	0.884

Table S30. Balance: Observational Data Omnibus P-value.

	chisquare	df	p.value
strat	6.494	4	0.165

Table S31. Balance: Survey Data All Outcomes.

	Control.strat	Treatment.strat	adj.diff.strat	adj.diff.null.sd.strat	std.diff.strat	z.strat	p.strat
Baseline_Attitudes	0.542	0.566	0.023	0.065	0.098	0.357	0.721
Baseline_Perceptions_of_Security	-0.459	-0.537	-0.079	0.071	-0.246	-1.113	0.266
Baseline_Contact	0.496	0.336	-0.159	0.104	-0.585	-1.528	0.127
Baseline_Percent_Experiment	0.443	0.474	0.031	0.056	0.206	0.543	0.587
Baseline_Endorsement_Experiment	-0.212	-0.250	-0.038	0.169	-0.067	-0.225	0.822

Table S32. Balance: Survey Data Omnibus P-value.

	chisquare	df	p.value
strat	6.302	5	0.278

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Table S33. Farmer and Pastoralist Demographics. This table shows demographic characteristics for farming groups and pastoralist groups

farm_pastor	age	female	income	fulani	christian	muslim	farming	pastoral	trading	radio
farmers	36.31	0.43	42235.09	0	0.64	0.35	0.80	0.00	0.12	2.32
pastoralists	34.66	0.46	30407.69	1	0.00	1.00	0.13	0.85	0.09	1.46

Table S34. Farmer and Pastoralist Demographics by State. This table shows demographic characteristics for farming groups and pastoralist groups, separating respondents from Nasarawa and Benue

farm_pastor	state	age	female	income	fulani	christian	muslim	farming	pastoral	trading	radio
farmers	ben	36.32	0.41	32521.10	0	0.98	0.00	0.97	0.00	0.02	2.50
farmers	nas	36.30	0.44	48373.50	0	0.42	0.57	0.70	0.00	0.19	2.20
pastoralists	ben	35.04	0.39	24947.04	1	0.00	1.00	0.04	0.88	0.10	1.55
pastoralists	nas	34.40	0.51	34050.07	1	0.00	1.00	0.18	0.83	0.08	1.40

Table S35. Regressions with community-average demographic variables. This table shows coefficients from each survey regression either (1) the version in the paper that does not control for demographic characteristics, or (2) a regression that aggregated demographic characteristics to the community-level and included them as control variables. The coefficients are similar – the sum of the coefficients when controlling for demographic characteristics is slightly larger.

	paper	check	difference
attitude	0.1160135	0.0770404	0.0389731
in	0.1590911	0.1681601	-0.0090690
contactOnly	0.1377790	0.1066938	0.0310852
rMean	0.0621330	-0.0080656	0.0701985
end_exp	0.1225162	0.2701144	-0.1475982
pgp_donate_end	0.0224200	0.0218400	0.0005800
pgp_amount_end	-35.1235007	-32.4998039	-2.6236968
total (excludes pgp_amount)	0.6199528	0.6357832	-0.0158305

Table S36. Regressions with individual demographic variables. This table shows coefficients from each survey regression either (1) the version in the paper that does not control for demographic characteristics, or (2) a regression that includes individual-level demographic characteristics as control variables. The coefficients are similar – the sum of the coefficients when controlling for demographic characteristics is slightly smaller.

	paper	check	difference
attitude	0.0602411	0.0515518	0.0086894
in	0.0495368	0.0461179	0.0034189
contactOnly	0.0975058	0.0451252	0.0523806
pgp_donate_end	0.0203803	0.0405599	-0.0201796
pgp_amount_end	-53.7397532	-38.3723911	-15.3673621
total (excludes pgp_amount)	0.2276640	0.1833548	0.0443092

Concept	Items
Attitudes	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]? From 1-5, how much do you trust people from [X GROUP] in your area? Now I'm going to ask you questions about your community here in Benue/Nassarawa, including [X GROUP]. Please tell me how strongly you agree/disagree with each of the following statements: People in this area can be trusted.
Contact	Now I'm going to ask you questions about your contact with [X GROUP] in your area: • 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? In the past month, have you: • 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?
Security	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?

Table S37. Survey outcomes and index construction for attitudes, contact, and perceptions of security. In this table we report on the question text used to measure the items in each self-report index.

Concept	Items
Endorsement Experiment	Imagine that there is a proposal by [**the 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?
Percent 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?

Table S38. Survey wording for Endorsement Experiment and Percent Experiment.

Concept	Items
Violence Placebo	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

Table S39. Survey outcomes and index construction for violence placebo. In this table we report on the question text used to measure the items in the violence placebo index.

Concept	Item
Public Goods Game	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. These are your individual donation envelopes. All the donations will be private – only you will know how much money you donated. It is 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. Remember, 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.

Table S40. Wording for Public Goods Game.

SI Dataset S1 (ag_df.csv)
Community-level dataset.

SI Dataset S2 (panel_df.csv)
Individual-level dataset.

SI Dataset S3 (markets.csv)
Dataset of behavioral observation in markets.

SI Dataset S4 (events.csv)
Dataset of behavioral observation at events.

References

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2. DeclareDesign, Use change scores or control for pre-treatment outcomes? depends on the true data generating process, (DeclareDesign Blog), Technical report (2019).