The Backstory Matters: Reducing Anti-Herder Bias Among Displaced Farmers in Benue State, Nigeria *

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Abstract

Do farmers displaced by farmer-herder conflicts in Nigeria update their views about herders when they are informed about the circumstances outside of the herders' control that forced them to encroach on the farmers' lands? We address this question by randomizing the exposure of nearly 2,000 displaced farmers to a video depicting the devastating impact of climate change on the herders' traditional grazing areas. Farmers exposed to the video are more compassionate about the herders' plight, report greater comfort with a range of hypothetical social interactions with herders, reduce their negative stereotypes about herders, become more trusting of herders, and voice greater support for policies that might address the structural sources of the conflict. Our findings underscore how victims' responses to harm can be shaped by a deeper appreciation of the circumstances that drove the behavior of the perpetrators.

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1 Introduction

Climate change—the warming of the planet and the shifting of rainfall patterns—is having devastating effects in northern Nigeria. Grazing lands are drying up, forcing herders to travel south to feed their animals. This causes friction and, in some cases, violence between herders and the farmers already living on those lands. Such conflicts have led to the displacement of millions of farmers and to profound distrust and animosity between farmers and herders. Reducing this distrust and animosity is crucial for restoring social cohesion and improving livelihoods in affected communities (Foa 2011; Ugokwe and Attamah 2019; Chatterjee, Gassier and Myint 2023). However, there is little consensus on how best to repair farmer-herder relations in this challenging setting. Despite efforts to reduce conflicts by strengthening traditional conflict mediating institutions (International Crisis Group 2017; Muhammad-Baba and Tukura 2026) or promoting intergroup contact (Grady et al. 2023; Dube, Henn and Robinson 2025), these efforts have had, at best, mixed success.

Our study investigates a new approach: informing displaced farmers about the circumstances outside of the herders' control—specifically, climate change—that caused the herders to encroach on the farmers' lands.[1] Rooted in the logic of perspective-getting (Kalla and Broockman 2023), this approach seeks to reduce anti-herder biases by providing the conflict victims with information about the circumstances that generated the behavior of the actors responsible for their suffering. Our hypothesis is that farmers armed with this information will be more understanding of the herders' motivations, and that this will lessen the blame the farmers attach to the herders for their displacement.

We investigate this hypothesis with a pre-registered randomized evaluation conducted among nearly 2,000 displaced farmers living in IDP camps in Benue State, Nigeria, an area deeply affected by farmer-herder violence (Alexander 2022; Okoye et al. 2024; Nwankwo 2025).[2] We randomize the exposure of the displaced farmers to a video depicting the devastating impact of climate change on the herders' traditional grazing lands (participants in the control group are shown a placebo video about meningitis). We compare the responses of the displaced farmers in the treated and control groups to a battery of questions aimed at measuring multiple dimensions of farmer-herder relations. We find strong effects of exposure to the video on all of these outcomes. Farmers exposed to the treatment video are more compassionate about the herders' plight and report greater comfort sharing water or space in their home with a herder, visiting a market if herders are present, having someone in their family marry a herder, or admitting a herder family to the camp. They hold fewer negative stereotypes about herders.

[1] An earlier, smaller-scale version of the present study is reported in Efobi, Adejumo and Kim (2025).

[2] A pre-analysis plan is available at `https://osf.io/syaed`. The handful of minor deviations from the pre-analysis plan are detailed in Appendix F.

They report greater trust that herders will honor an agreement and repay a small loan, and toward herders who access their water supply, graze their cattle on their stubble after harvest, or supply *wara* (a soft, unripened cheese made from cow's milk) for the camp. And they express greater support for policies that might improve herders' livelihoods and potentially address the structural roots of the conflict. Remarkably, these treatment effects are present even among farmers who were personally exposed to violence during the course of their displacement.

The estimated effect sizes on these outcomes are not substantively very large (ranging from about 0.1 to 0.25 SD of the control group mean), and the average sentiment toward herders is still negative on every measure, even among those exposed to the treatment video. We also find little evidence in a follow-up phone survey that these effects remained four months later—although we find suggestive evidence that the disappearance of treatment effects may be due as much to the spillover of impacts from treated to control participants as to the dissipation of treatment effects over time. Moreover, the fact that exposure to the video had such consistent social distance-narrowing effects, even if fleeting, is indicative of the power of information about the conditions that forced the herders onto the farmers' lands to blunt the negative sentiments caused by the farmers' experience of violence and displacement.

Our study makes several contributions. The first is to the literature on perspective-taking/perspective-getting to reduce negative attitudes toward outgroups (Bruneau and Saxe 2012; Broockman and Kalla 2016; Adida, Lo and Platas 2018; Audette, Horowitz and Michelitch 2020; Lowe 2021; Kalla and Broockman 2023; Adida et al. 2025; Kim 2025). Our study is noteworthy not just for the strong impacts we find but also for the source of the outgroup hostility that we aim to reduce: not stereotype or prejudice born from ignorance (as, for example, in studies that employ perspective-taking/-getting to combat racism, homophobia, or anti-immigrant bias), but direct experience with displacement and violence perpetrated by the outgroup. Anti-herder stereotype and prejudice are certainly present in our study setting (Muhammad-Baba and Tukura 2026), fortified by the politicization of farmer-herder disputes by politicians and the media (Achem and Aderinto 2025) and by the religious and ethnic overtones of the agriculturalist-pastoralist divide (HumAngle 2025; Tuki 2025). But these negative stereotypes and prejudices are reinforced by personal exposure to violence at the herders' hands, which makes the anti-herder bias much more difficult to overcome—and our findings all the more remarkable.

Our second contribution is to the literature on climate change and conflict (Homer-Dixon 1994; Burke, Hsiang and Miguel 2015; Brottem 2016; von Uexkull et al. 2016; Mach et al. 2019; McGuirk and Nunn 2024; Eberle, Rohner and Thoenig 2025; Koubi et al. 2018). Whereas most of this literature focuses the impact of climate change on violence, we focus on the downstream effects of violence on

intergroup relations. In this respect, our study joins the work of Prediger, Vollan and Herrmann (2014), De Juan and Hänze (2021), Almas et al. (2020), Chung and Rhee (2022), and Kim (2025), whose research explores the impact of climate change on trust, outgroup acceptance, altruism, public goods provision, and antisocial behavior.

In addition to being affected by exposure to conflict, the variables we focus on—compassion, social distance, negative stereotype, trust, and support for policies that might benefit the outgroup—also shape prospects for peaceful interactions between farmers and herders in the future, in part by potentially increasing receptivity to policies that hold promise of addressing the structural sources of conflict, such as initiatives that create ranches for migrating herders during the dry season. Our study therefore, also makes a third contribution to debates surrounding how to reduce violent conflicts in regions affected by climate change. This is a major policy issue throughout the Sahel (Benjaminsen et al. 2012), but especially in Nigeria (Amusan, Abegunde and Akinyemi 2017; Ojo 2023; International Crisis Group 2018, 2021), where recent government estimates put the number of lives lost due to farmer-herder conflicts at more than 60,000 (Muhammad-Baba and Tukura 2026). Insofar as our findings point to a promising approach for improving intergroup relations in the aftermath of climate change-induced conflicts, they offer a potentially useful tool for policymakers.

2 Climate change and farmer-herder conflict in Benue State

Transhumant pastoralism has defined the agro-ecological landscape of Sahelian West Africa for generations by facilitating the seasonal interactions between mobile herders and sedentary farmers. These interactions have historically been symbiotic, with herders grazing livestock on fallow farmland during the dry season and farmers benefiting from the natural fertilization provided by the cattle's manure and the clearance of residual crop material from their farmland (McGuirk and Nunn 2024). Such cooperation has enabled the efficient sharing of land and water resources while also fostering broader social cohesion between herders and farmers (Stenning 1959).

Persistent climate shocks in Northern Nigeria, and throughout the Sahel, have strained this long-standing coexistence.[3] The rainfall season in northern Nigeria has become shorter, with only four months of high variability rainfall (June - September), and decreases in overall annual rainfall totals over time (World Bank 2021). This changing climate pattern has led to diminished vegetation available for grazing and has compelled herders to adjust their traditional migration routes by moving south

[3]Northern Nigeria remains the world's most vulnerable in terms of the effects of climate change, with its temperature rising by at least 2°C in the short term (between 2021 and 2040), a rate that is 1.5 times higher than the global average (Trisos et al. 2022).

earlier in the season and traveling farther into agricultural zones in search of grazing lands (Ayantunde et al. 2014; McGuirk and Nunn 2024). These shifts in herder migration patterns have generated competition between farmers and herders, particularly in places like Benue State, where better climatic conditions and more reliable sources of fresh water have attracted growing numbers of migratory herders. The mean annual rainfall of Benue State is about 1,141 mm (Alexander 2022), more than double that of the herders' traditional grazing areas in the north. The contrast in grazing land availability between Benue State and the areas from which herders increasingly migrate is clearly visible in panel A of Figure 1, which plots average rainy season vegetative density in central and northeastern Nigeria between 2019 and 2024.[4]

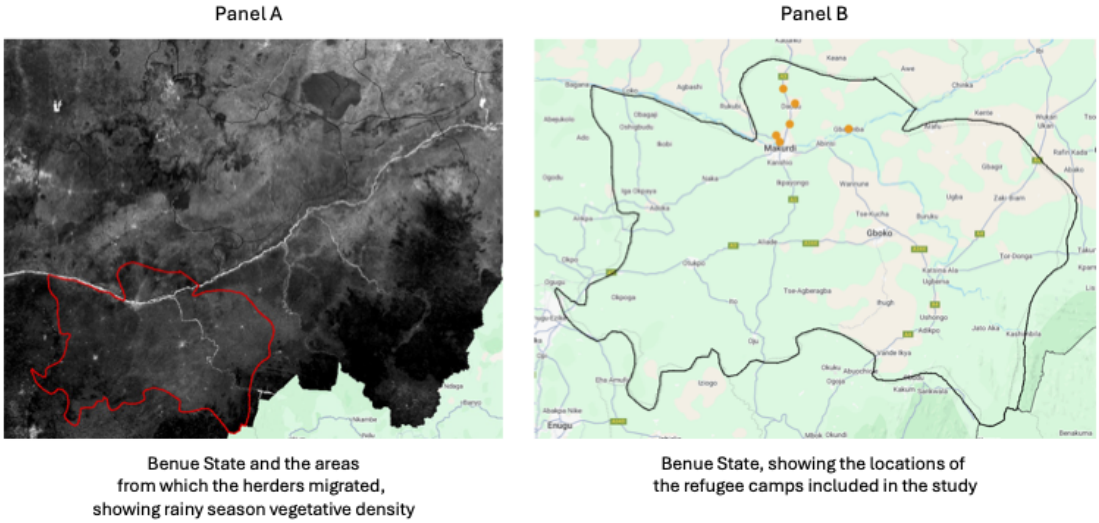


Figure 1

The farmers perceive the herders' arrival on their lands as an aggressive encroachment, which triggers retaliatory attacks and cycles of violence (Moritz 2010; Babatunde and Ibnouf 2024; HumAngle 2025). Between mid-2023 and mid-2025, clashes between farmers and herders generated more than 6,000 fatalities in Benue State alone, making it one of the most violent areas in Nigeria (Amnesty International 2025). For scarce land and water resources by designating areas for herders to graze their cattle during the dry season, along with basic amenities such as year between 2018 and 2025. As a

[4]The impact of climate change on the competition over land has been reinforced by government policies since the 1990s promoting agriculture as a means of reducing the country's dependence on oil. These policies have dramatically increased farming land in Benue State, leaving less land available for seasonally migrating pastoralists and contributing to the conflict between herders and farmers. According to figures cited in Akwagyiram and McNeill (2018), grazing land in Nigeria's Middle belt shrank from 61 percent to 38 percent between 1975 and 2013, while agricultural land grew from 14 percent to 42 percent. During this period, the Middle Belt lost about 84,000 square kilometers of land available to migrating herders. On the reduction of grasslands in Benue State's traditional cattle grazing corridors, see Odiji et al. (2024).

consequence of this violence, Benue State hosts one of the largest populations of internally displaced persons (IDPs) in Nigeria, estimated at more than 500,000 as of June 2024 (IOM 2024).

In 2019, the Nigerian government attempted to address the conflict between farmers and herders by launching a pair of initiatives: the Rural Grazing Areas (RUGA) settlement policy and the National Livestock Transformation Plan (NLTP). The initiatives sought to reduce competition over scarce land and water resources by setting aside designated areas for herders to graze their cattle during the dry season, along with basic amenities like schools, clinics, and veterinary services. The goal was to prevent encroachments on farmers' lands and crops by encouraging herders to shift from traditional nomadic grazing practices toward housing their animals in sedentary ranches. While offering a potential solution to the conflict between farmers and herders, the initiatives collapsed in the face of fierce opposition from southern and Middle Belt populations, who mistrusted the plans and viewed them as a threat to their sovereignty over ancestral lands (International Crisis Group 2021). The opposition to RUGA and NLTP was especially strong in Benue State (Daily Trust 2019; Ojo 2023)—hence the particular importance of testing an intervention that might heighten receptivity to similar initiatives in this setting.

3 Characteristics of our sample

We sampled 1,996 displaced farmers residing in eight IDP camps in the Makurdi local government area of Benue State (see panel B of Figure 1).[5] The average participant in our study was a 44 year old Catholic or Christian, had no primary education, and had lived in the camp for 6.5 years.[6] Ninety-nine percent of participants reported having experienced violence in their community prior to their displacement, with 76 percent reporting having experienced violence directed at them personally. On a scale from 1 to 10, with 10 indicating that the injuries they sustained were very severe and that they lost everything, the average displaced farmer in our study characterized their experience as a "9," but the modal response (given by 59 percent of participants) was a "10."

Given both this experience and the negative rhetoric about herders in the media and as voiced by local elites, it is understandable that the farmers in our study held negative views about herders.

[5] This is the sample employed in our analysis, after applying the exclusion criteria specified in our pre-analysis plan. The eight IDP camps are Abagena, Baka, Daudu 1, Daudu 2, Gbajimba 1, Gbajimba 2, NEPA Quarters, and Ortese. Because residents of IDP camps are a particularly vulnerable population, we adopted several protocols in our research to protect our study participants. These included hiring local enumerators who understand the cultural dynamics of the setting and the experiences our participants faced; training our field staff to recognize and cease interviews in the event of signs of re-traumatization; and reiterating that participants were free to withdraw from the study at any time without jeopardizing their (modest) compensation.

[6] Full descriptive statistics of the sample, including comparisons across participants assigned to treatment and control, are provided in Appendix A1.

At baseline, two-thirds said they would feel uncomfortable sharing food or water with a herder; 82 percent said they would feel uncomfortable sharing space in their home with a herder for a few nights; 83 percent said they would be uncomfortable with someone in their family marrying a herder someday; and 68 percent said they would feel uncomfortable visiting a market if several herders were present.[7] The farmers in our sample also held strongly negative stereotypes about herders. Ninety-three percent reported viewing herders as threatening; 88 percent saw them as selfish and arrogant, and 92 percent described them as militant.[8] In response to a series of questions about how much they would trust herders to perform various tasks, only 18 percent said they would trust herders to honor an agreement; just 14 percent said they would trust herders to repay a small loan; just 12 percent said they would trust herders to supply *wara* for the camp; and only 26 percent said they would trust herders to have their cattle access their water supply.[9] Eighty-seven percent of respondents describe relations between herders and farmers as "unfriendly" or "very unfriendly."

These strongly negative views toward herders are due, at least in part, to the farmers' beliefs about why the herders came onto their lands. As shown in Table 1, 73 percent of the farmers in our study said they thought the herders came to grab their lands, for religious reasons/jihad, or because they were foreign terrorists—characterizations reflecting the politicization of the herders' behavior by Southern politicians and in the Nigerian media.[10] Only 24 percent said that the herders came because their own lands could no longer support their cattle.

Table 1: Farmers' priors about why the herders came onto their lands (N=1,976)

	Percent
To grab our land	56.4
Because their lands could not support their cattle	23.7
Because they are foreign terrorists	11.4
For religious reasons/jihad	5.6
Not sure/don't know	2.9

We also asked study participants, pre-treatment, whether they thought the herders came due to forces outside of their control or whether the herders had a choice about coming onto the farmers' lands. The distribution of responses, shown in Figure 2, is strongly skewed toward the "completely

[7]The questions asked respondents to indicate their comfort level on a scale from 1 to 10, with 1 indicating that they would be very uncomfortable and 10 indicating that they would be very comfortable. The reported figures reflect the share of respondents who gave a response of 1, 2, or 3 on the 10-point scale. Figures here and throughout this paragraph are based on responses from the control group, as these questions were asked post-treatment.

[8]The question asked respondents how much they agreed with various statements about herders. The reported figures reflect the share of respondents who indicated that they "agree" or "strongly agree."

[9]The reported share is the percentage of respondents who said they trusted the herders "somewhat" or "very much."

[10]This question was asked pre-treatment. The response categories offered in the survey were chosen based on extensive focus groups with displaced farmers in which we asked open-ended questions about why they thought the herders had come onto their lands.

their choice" end of the spectrum.

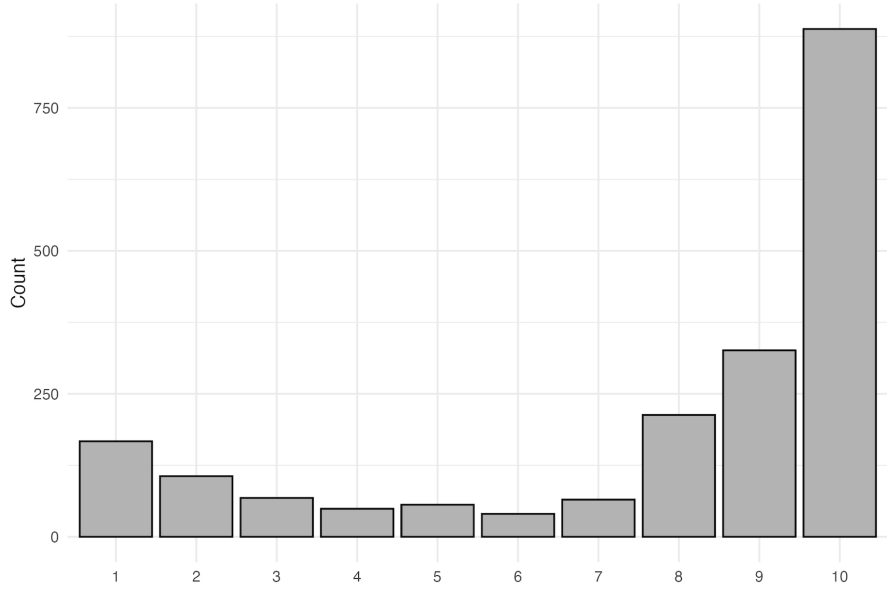


Figure 2: Pre-treatment priors: Did the herders come onto your lands due to forces outside of their control or did they have a choice? (1=no choice at all; 10=completely their choice)

The farmers' preconceptions about the circumstances surrounding the herders' arrival raise the question of whether the farmers' hostility toward the herders would be softened if they were more fully informed about the climate change-related circumstances that forced the herders to leave their ancestral grazing areas. Would such perspective-getting reduce the farmers' anti-herder bias? Would the farmers' responses to the harms they experience be altered by a deeper appreciation of the factors that motivated the behavior of the agents who harmed them?

4 Experimental Design

To answer this question, we randomized exposure of the displaced farmers to a treatment video that provided a backstory to the herders' arrival on their lands. The five minute video presents arresting, real footage of the devastating effects of climate change in Northern Nigeria and conveys the message that the herders had no choice but to leave their traditional grazing areas.[11] Images from the video, along with full text of the script, are provided in Appendix B.[12]

Trained enumerators invited randomly selected residents of the eight IDP camps, stratified by age

[11] We do not know precisely where the herders who displaced the farmers in our sample came from. However, we believe that the circumstances depicted in the treatment video provide a reasonable approximation of the conditions these herders faced.

[12] We are grateful to HumAngle Media for their permission to use footage from their documentary, "Climate Change Aggravating Farmer-Herder Crises in Nigeria," in creating our treatment video.

and gender, to participate in a survey about their displacement experiences. If they agreed, they were asked an initial module of questions about their individual characteristics, the events surrounding their displacement, and their beliefs about the circumstances that caused the herders to come onto their lands.[13] Participants randomly assigned to the treatment group were then shown the treatment video on a tablet; participants in the control group were shown a video of equal length about the diagnosis and treatment of meningitis.

After viewing the treatment or control videos, participants were asked a battery of questions designed to capture different dimensions of anti-herder bias: compassion, social distance, negative stereotypes, trust, and support for policies that might improve address the structural roots of the farmer-herder conflict. The specific questions we asked were informed by focus groups conducted in nearby IDP camps at an earlier phase of the project. As pre-registered, responses were combined in a series of averaged z-score indices, which serve as the main outcome measures in the analyses that follow. The full set of questions comprising each index, along with the mean responses across individuals in the treatment and control groups, is presented in Appendix A.

4.1 Exclusions

Our survey efforts yielded a total sample of 2,392 respondents. However, before analyzing our data, we implemented a series of pre-registered exclusions designed to insure data quality. Specifically, we dropped observations in which the interview duration was more than two standard deviations shorter than the mean interview length of 33 minutes, as we judged such interviews as too short for enumerators to have read and ensured the respondents' understanding of each question.[14] We also dropped observations where the enumerator reported distractions or interruptions during the interview or where the respondent was judged by the enumerator to be bored, impatient, suspicious, inattentive, or unwilling to freely share information. These exclusions reduced the sample to 2,178 participants. In addition, as pre-registered, we excluded participants who were inattentive to the treatment by dropping from our analysis any participants who provided two or more incorrect answers to a 4-question comprehension test administered immediately after viewing the treatment and control videos. This reduced our sample by an additional 182 respondents, leaving us with a sample size of 1,996.[15]

[13] Mean responses to these questions are presented in Appendix Table A1. Responses are well balanced across treatment and control.

[14] We further guard against such enumerator-related errors by including enumerator fixed effects in all estimates.

[15] As shown in Appendix Table A8, the results are unchanged if we include these inattentive participants, in keeping with an ITT approach.

4.2 Manipulation Check

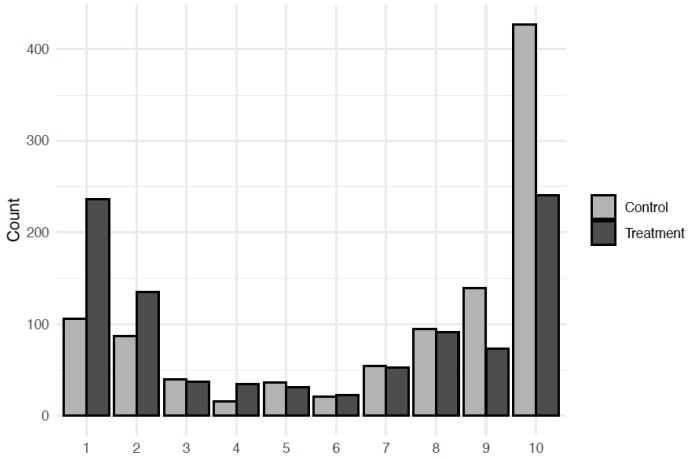
As a manipulation check, we compare responses collected pre- and post-treatment to a question about whether herders had a choice about coming onto the farmers' lands. Given that the core message of the treatment video was that the herders had been pushed out of their traditional grazing lands by forces outside of their control, we expected participants in the treatment group to update their views about the extent to which the herders' had a choice. The results presented in Table 2 confirm that they did. As shown in column 5, participants in the treatment group were significantly less likely than those in the control group to say, post-treatment, that the herders had a choice about coming onto the farmers' lands. In addition, the updating of participants' views on this issue was significantly greater in the treatment than the control group (column 6).

Table 2: Manipulation check: Did exposure to treatment cause farmers to update?

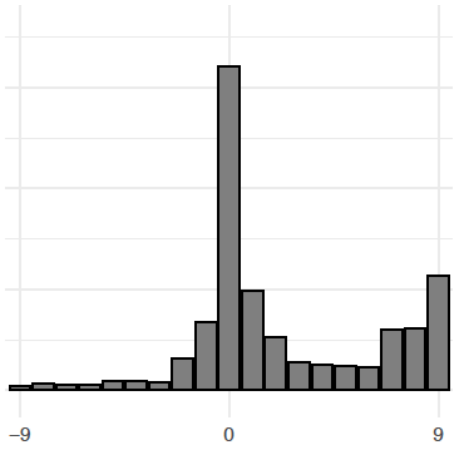
	Pre-treatment		Post-treatment		Difference	Difference (post-T−pre-T)−
	Treated	Control	Treated	Control	(post-T−post-C)	(post-C−pre-C)
	(1)	(2)	(3)	(4)	(5)	(6)
Did the herder choose to come onto your land (10) or did they have no choice (1)?	7.75	7.69	5.46	7.28	-1.81***	-1.87***

Question wording: "Some people say that the herders came onto your lands due to forces outside of their control. Others say that they had a choice and made the decision themselves to come onto your lands. On a scale from 1 to 10, with 10 being that it was completely their choice and 1 being that they had no choice at all, which do you think is closest to the truth?" *p<0.1; **p<0.05; ***p<0.01.

These shifts in perspective are clearly evident in Figure 3. The left panel compares the distribution of responses across treated and control participants to the question about whether the herders had a choice about entering the farmers' lands. The discernible leftward shift in responses among treatment group participants gives us confidence that the treatment was effective in updating the displaced farmers' beliefs about the circumstances that led to their displacement. The right panel plots the individual-level changes in the perceptions of treatment group participants, with positive numbers indicating a shift toward the view that the herders were forced onto the farmers' lands due to forces outside of their control and higher numbers indicating greater movement in this direction. The patterns indicate that exposure to the treatment video affected the vast majority of participants and moved them in the intended direction—in some cases very strongly. Taken together, these results give us confidence that our findings can be interpreted as causal effects of being informed about the backstory surrounding the herders' arrival on the farmers' lands.



(a) Distribution of post-treatment responses among treated and control participants (1=no choice at all; 10=completely their choice)



(b) Changes, pre- vs post-treatment (treatment group)

Figure 3: Post-treatment updating: Did the herders come onto your lands due to forces outside of their control or did they have a choice?

4.3 Estimating Equations

To evaluate the impact of exposure to the treatment video, we estimate the following pre-registered equation:

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \gamma_c + \phi_e + \epsilon_i \tag{1}$$

where Y_i is the outcome measure (in our main specifications, one of our five indices) of participant i. T_i is a binary variable indicating whether the participant was assigned to treatment and β_1 is the average treatment effect. X_i is a vector of pre-registered controls, γ_c are camp fixed effects, ϕ_e are enumerator fixed effects, and ϵ_i is the error term.

We estimate heterogeneous treatment effects similarly:

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 C_i + \beta_3 T_i * C_i + \beta_4 X_i + \gamma_c + \phi_e + \epsilon_i \tag{2}$$

where C_i is an indicator variable for the individual-level characteristic whose differential impact we are testing. β_3 is the marginal impact of the characteristic of interest on the treatment effect.

5 Treatment Impacts

5.1 Main Results

Our main results are presented in Figure 4, which reports the impact of exposure to the treatment video on all five of our outcome indices.[16] All effects are highly significant and in the expected direction, both in bivariate specifications and in specifications that control for individual respondent characteristics and camp and enumerator fixed effects, as in equation 1.[17] Our findings suggest that exposure to the backstory about the circumstances that led to the herders' arrival on the farmers' lands makes farmers more compassionate about the herders' plight, reduces social distance, makes farmers less likely to hold negative stereotypes about herders, more likely to trust herders in a variety of social and economic interactions, and more likely to support government policies that might have a positive impact on the herders' livelihoods and that might address the structural roots of the conflict. Although not substantively very large (the estimated treatment effects are associated with about a 0.6 point movement on the ten point social distance scale and about a 0.25 point movement on the four point trust scale), the impacts are highly statistically significant not just at the index level but, as shown in Appendix Tables A3-A7, for 23 of the 27 index components as well. While attitudes towards herder are still negative on the whole, they are less negative among farmers exposed to the treatment video. These results point to the power of perspective-getting to improve relations between members of otherwise hostile communities in the aftermath of conflict and displacement.

5.2 Heterogeneous effects

In addition to investigating the effects of exposure to the treatment video, we also pre-registered our intention to test for a set of conditional impacts. The first of these relates to the impact of prior exposure to violence. Our hypothesis was that farmers who were exposed to violence prior to their displacement might be hardened against the salutary effects of the treatment video.[18] The results presented in the top panel of Table 3 are consistent with this hypothesis: among farmers who were personally exposed to violence, the effect of exposure to the treatment video was weaker on all five of our outcome measures. In keeping with recent findings that exposure to violence in associated with prosociality (Bauer et al. 2016), we also find a direct effect of conflict exposure on three of our five outcome measures.

[16] Regression results are presented in Appendix Table A2.

[17] The results are present on almost every outcome in all eight camps, with results occasionally dropping below statistical significance in the camps with the smallest sample sizes due to limited power.

[18] For evidence that exposure to violence amplifies ingroup/outgroup divides, see Mironova and Whitt (2018).

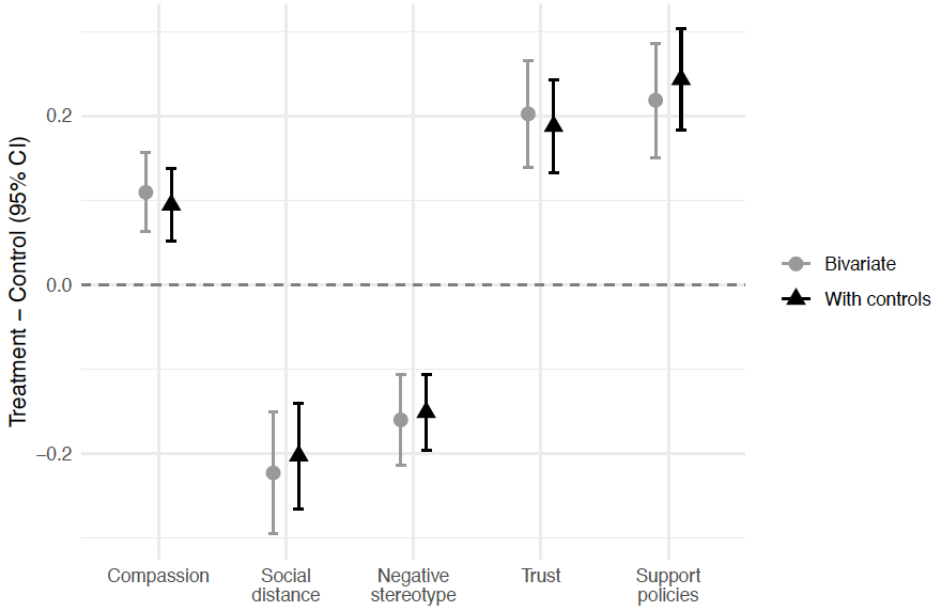


Figure 4: Impact of exposure to the treatment video

Controls include age, gender, religion, and education, as well as camp and enumerator fixed effects. Values are in standard deviations of the control group mean.

Table 3: Heterogeneous effects: Prior exposure to violence

	Compassion	Social distance	Negative stereotype	Trust	Support policies
As reported in survey					
Treatment	0.179***	-0.335***	-0.356***	0.362***	0.415***
	(0.045)	(0.065)	(0.046)	(0.057)	(0.062)
Prior exposure to violence[a]	0.052	-0.132**	-0.139***	0.131**	0.056
	(0.042)	(0.062)	(0.044)	(0.054)	(0.059)
Interaction term	-0.111**	0.175**	0.270***	-0.230***	-0.225***
	(0.051)	(0.075)	(0.053)	(0.066)	(0.071)
N	1993	1993	1993	1993	1992
ACLED data					
Treatment	0.110***	-0.188***	-0.139***	0.199***	0.247***
	(0.029)	(0.042)	(0.031)	(0.038)	(0.040)
Prior exposure to violence[b]	0.001	-0.004*	-0.003*	0.003	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Interaction term	0.000	0.000	-0.001	-0.001	0.002
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
N	1659	1659	1659	1659	1658

[a]Based on whether respondent indicated having experienced violence directed at them personally.

[b]Based on the number of violent events between farmers and herders in the farmer's LGA in the year prior to their displacement.

Note: All regressions include the full set of pre-registered controls, as well as camp and enumerator fixed effects. *p<0.1; **p<0.05; ***p<0.01.

While farmers personally exposed to violence were less swayed by the treatment video, it is important to note that they were still more positively disposed to herders than farmers in the control group. Becoming better informed about the backstory that explains the herders arrival on their lands makes farmers more sympathetic to the herders, *even when the herders' arrival resulted in violence directed at them personally.*

We find only weak conditional effects when exposure to violence is measured in terms of the number of violent events between farmers and herders taking place in the local government area (LGA) in the year prior to their displacement, as recorded in ACLED data (Table 3, bottom panel).[19] We suspect that the weakness of these results is because ACLED's LGA-level data is too coarse to capture village-level exposure and because 85 percent of our sample was displaced from a single LGA.

The second conditional impact we investigate is with respect to changes in the farmers' priors about why the herders came onto their lands. We hypothesized that farmers who updated their views more strongly (in the direction of the herders' movements being due to forces outside of their control) would show stronger treatment impacts. To undertake the analysis, we created a measure capturing the differences in the responses given pre- and post-treatment to the question analyzed in Table 2 about whether herders had a choice about coming onto the farmers lands, with larger values implying greater updating.[20] Although this analysis is not strictly speaking a heterogeneous treatment effect (as the measure includes a component that is post-treatment), we present it here because it sheds light on whether, as anticipated, treatment effects are stronger among study participants whose priors were updated more strongly. We can think of the analysis as an extensive margin complement to the main results presented in Figure 4. As shown in Table 4, we find statistically significant effects on all five interaction terms, all of which are in the hypothesized direction.

We also investigated whether particular individual characteristics of the farmers in our sample were associated with stronger or weaker treatment effects. The first of these is with respect to what a recent line of research refers to as "zero sum thinking": the idea that individuals vary in the extent to which they believe that gains for one individual or group come at the cost of others. Chinoy et al. (2026) provide evidence that zero-sum thinking is associated with a variety of attitudinal and behavioral outcomes in the United States, and suggest that it may be partly responsible for the country's polarization. We hypothesized that such a zero-sum mentality might also be related to the

[19] These null results are robust to measuring exposure to violence in terms of the number of fatalities in the LGA in the year prior to displacement rather than the number of violent events. See Appendix Table A9.

[20] These are the values plotted in Figure 3b. We investigated whether any individual participant characteristics (gender, age, education, years in camp) were associated with greater updating and found no associations. However, consistent with the results reported in Table 3 about the impact of prior exposure to violence on treatment impacts, we do find that having personally experienced violence is negatively associated with the extent of updating.

Table 4: Heterogeneous effects: Updated priors[a]

	Compassion	Social distance	Negative stereotype	Trust	Support policies
Treatment	0.046*	-0.116***	-0.086***	0.100***	0.142***
	(0.024)	(0.035)	(0.025)	(0.030)	(0.032)
Updated priors	0.008	-0.020**	-0.010*	0.022***	0.021***
	(0.005)	(0.007)	(0.005)	(0.006)	(0.007)
Interaction term	0.016**	-0.022**	-0.023***	0.023**	0.031***
	(0.007)	(0.010)	(0.007)	(0.008)	(0.009)
N	1967	1967	1967	1967	1966

[a]Based on whether respondent updated their view on whether the herders came onto their lands due to forces outside of their control.

Note: All regressions include the full set of pre-registered controls, as well as camp and enumerator fixed effects. *p<0.1; **p<0.05; ***p<0.01.

outcomes of interest in our study, with farmers ranking high on this measure potentially being less compassionate, exhibiting greater social distance, having more negative stereotypes, being less trusting, and offering lower support for policies that might help herders—and that all of these dispositions would be less affected by exposure to the treatment video.[21] While these expectations were well motivated, we find little evidence in Table 5 that zero-sum mentality is associated with any differential outcomes, apart from suggestive evidence that it may generate weaker support for policies that benefit herders but stronger positive responses to the treatment video.

Table 5: Heterogeneous effects: Zero sum mentality

	Compassion	Social distance	Negative stereotype	Trust	Support policies
Treatment	0.016	-0.165	-0.057	0.118	0.068
	(0.077)	(0.113)	(0.081)	(0.099)	(0.108)
Zero sum mentality	-0.037	0.015	0.019	-0.040	-0.081**
	(0.023)	(0.034)	(0.024)	(0.030)	(0.032)
Interaction term	0.030	-0.014	-0.037	0.027	0.068*
	(0.029)	(0.042)	(0.030)	(0.037)	(0.040)
N	1993	1993	1993	1993	1992

Note: All regressions include the full set of pre-registered controls, as well as camp and enumerator fixed effects. *p<0.1; **p<0.05; ***p<0.01.

We also tested whether farmers exhibiting more generalized trust had different orientations toward herders and/or responded differently to exposure to the treatment video.[22] While Table 6 provides only modest evidence for heterogeneous effects of exposure to the treatment video (suggesting that

[21]We measured zero-sum mentality with a short survey module modeled on the questions used in Chinoy et al. (2026), modified for our study context. A fuller description of the construction of this measure is provided in Appendix C.

[22]We measured generalized trust via a standard survey question, asked pre-treatment, about the participant's agreement/disagreement with the statement: "generally speaking, most people can be trusted."

generalized trust is associated with stronger impacts on trust and support for pro-herder policies), it does show that generalized trust is positively associated with compassion and negatively associated with social distance and holding negative stereotypes. Meanwhile, we find a (somewhat puzzling) negative association between generalized trust and support for pro-herder policies, although this association reverses itself among farmers exposed to the treatment video (as evidenced by the positive, large, statistically significant interaction term in the table's last column).

Table 6: Heterogeneous effects: Generalized trust

	Compassion	Social distance	Negative stereotype	Trust	Support policies
Treatment	0.108***	-0.179***	-0.105**	0.098*	0.029
	(0.039)	(0.057)	(0.041)	(0.051)	(0.055)
Generalized trust	0.036***	-0.042**	-0.032***	0.013	-0.030*
	(0.012)	(0.017)	(0.012)	(0.015)	(0.016)
Interaction term	-0.005	-0.015	-0.026	0.048**	0.111**
	(0.017)	(0.024)	(0.017)	(0.022)	(0.023)
N	1993	1993	1993	1993	1992

Note: All regressions include the full set of pre-registered controls, as well as camp and enumerator fixed effects. *p<0.1; **p<0.05; ***p<0.01.

Finally, we investigated two additional potential heterogeneous treatment effects that we had not pre-registered. The first was whether exposure to the video had different impacts along gender lines. Although men generally own the land in Benue State, women do most of the farming (Bryceson 2019). In addition, women tend to play stronger roles than men in policing cross-group boundaries (Yuval-Davis 1997). It is thus possible that women may respond differently to the arrival of herders on their lands, and (potentially) to being exposed to the backstory about why they came. As indicated in Table 7, women are indeed less comfortable interacting with herders, hold more negative stereotypes about them, and are less supportive of government policies that might help them. But they are not differently affected by exposure to the treatment video.

We also investigated whether length of residence in the camp affected attitudes toward herders and the impact of exposure to the treatment video. While the median participant in our study had been living in their IDP camp for seven years, the length of residence varied from a minimum of one year to a maximum of seventeen years. On the one hand, longer residence implies a longer interval since direct exposure to violence, which might be associated with a reduction in animosity. On the other hand, longer residence in the camp implies greater exposure to others who have been negatively affected by the herders' actions, which might be associated with an increase in mistrust and resentment. As shown in Table 8, we find that participants who had spent more time in their camp were more supportive of

Table 7: Heterogeneous effects: Gender

	Compassion	Social distance	Negative stereotype	Trust	Support policies
Treatment	0.096***	-0.186***	-0.129***	0.208***	0.212***
	(0.036)	(0.052)	(0.037)	(0.046)	(0.050)
Female	-0.005	0.181***	0.060*	-0.060	-0.097**
	(0.034)	(0.049)	(0.035)	(0.043)	(0.047)
Interaction term	-0.002	-0.026	-0.036	-0.032	0.051
	(0.045)	(0.066)	(0.047)	(0.058)	(0.063)
N	1993	1993	1993	1993	1992

Note: All regressions include the full set of pre-registered controls, as well as camp and enumerator fixed effects. *p<0.1; **p<0.05; ***p<0.01.

policies that might help them—consistent with the first hypothesis about diminishing animosity over time. However, we see no evidence that length of residence in the camp affects the impact of seeing the treatment video.[23]

Table 8: Heterogeneous effects: Length of residence in camp

	Compassion	Social distance	Negative stereotype	Trust	Support policies
Treatment	0.096***	-0.202***	-0.167***	0.176***	0.218***
	(0.033)	(0.048)	(0.034)	(0.042)	(0.046)
> median yrs in camp	0.029	0.005	-0.027	-0.009	0.097*
	(0.036)	(0.053)	(0.038)	(0.046)	(0.050)
Interaction term	-0.002	-0.002	0.028	0.022	-0.065
	(0.044)	(0.064)	(0.046)	(0.057)	(0.061)
N	1993	1993	1993	1993	1992

Note: The median length of residence is 7 years. All regressions include the full set of pre-registered controls, as well as camp and enumerator fixed effects. *p<0.1; **p<0.05; ***p<0.01.

5.3 Social desirability bias

In a project whose outcome measures are built from self-reported attitudes toward outgroup members and undertaken in a context where hospitality to outsiders is prized, it is important to assess whether the answers respondents provided may have been affected by social desirability concerns or experimenter demand effects. A first piece of evidence against this possibility comes from the strongly negative attitudes that respondents volunteered about herders in our direct survey questions.[24] Overwhelming majorities of farmers in our sample characterized herders as selfish or arrogant, said they would feel uncomfortable with someone in their family marrying a herder someday, and indicated that

[23] These null results are robust to measuring length of residence in terms of years rather than above or below the median.

[24] The examples that follow are based on control group means, as these questions were asked post-treatment.

they would not trust a herder to repay a small loan or honor an agreement. When asked about the compassion they have for herders given the challenges they have faced due to the loss of their traditional grazing lands, the average response was 3.6 on a scale from 1 (little feeling) to 10 (great compassion), but the modal response was a "1." These are not the responses of individuals who are holding back on expressing their disdain for outgroup members.

But even if farmers were willing to express negative attitudes about herders, it is still possible that these views were not as negative as they might have been had the farmers not been concerned about violating social norms against unkindness to outsiders or revealing their views to our survey enumerators. This would be a problem for our analysis if such biases were differentially present among participants in the treatment and control groups, which could occur if the treatment video made participants believe that the organizers of the intervention wanted them to be sympathetic to the herders, which then caused them to answer questions in light of this presumed preference.

To address this possibility, we take advantage of a pair of list experiments embedded in our post-treatment survey. List experiments are a widely used technique for eliciting truthful answers in contexts where shame, fear, social desirability concerns, or experimenter demand effects may bias how study participants respond to direct questions (Blair and Imai 2012; Glynn 2013). The list experiments allowed us to ask questions (about how comfortable respondents would be with sharing food or water with a herder and whether they would support arresting all herders and forcing them to go back to their home area) in a way that allowed respondents to hide their potentially socially undesirable responses. Respondents were randomized to one of two groups in each list experiment. Those randomized to the first group were read a list of four situations/actions and asked how many would make them feel uncomfortable/they would support.[25] Respondents randomized to the second group were read the same list plus a fifth situation/action, the situation/action of interest. The differences in the average number of uncomfortable situations reported/actions supported in groups 1 and 2 convey the share of the respondents in the study population that would find the fifth situation uncomfortable or would support the fifth action.

As reported in Appendix D, we find no statistically significant differences in the list experiments across participants exposed to the treatment and placebo videos, even as a slightly lower share of treatment group participants indicate that they would feel uncomfortable sharing food or water with a herder or would support having the federal or state government arrest all herders and force them to

[25] As is standard, the list of situations and actions ranged from ones that most people in the study setting would find only slightly uncomfortable or would likely support to ones that most people would find very uncomfortable or would be very unlikely to support. See Appendix D for further details.

go back to their home areas. Although the directions of these estimated impacts are in keeping with the results found in our other analyses, the absence of statistical significance is at odds with the highly consistent and strong results presented thus far. Given that these outcomes were measured in a way that was designed to mitigate social desirability bias and experimenter demand effects, these findings raise questions about whether such bias may have shaped our study's results.

Two considerations caution against putting too much weight on the list experiment results, however. First, as discussed in Appendix D, a comparison of results from the first list experiment question with the findings from a directly posed version of the same question asked elsewhere in the survey reveals that participants were more willing to express discomfort with sharing food or water with a herder in the directly-asked version of the question than in the version asked indirectly through the list experiment. This is not what we would expect to observe if social desirability concerns were biasing respondents against admitting dislike for herders.

Second, while it is reasonable to think that the treatment video might have generated expectations that we wanted study participants to feel compassion for the herders and perhaps support policies that might help them, it would be a stretch to think that it conveyed messages about whether herders should be trusted, how socially distant one should feel toward them, or whether one should hold negative stereotypes about them. The strong treatment impacts we find on these three other outcomes weigh against the concern that our findings are driven by experimenter demand effects.

5.4 Longer term impacts

The main findings presented in Figure 4 are based on survey responses collected immediately after exposure to the treatment video. Impressive as they may be, their substantive implications depend in part on whether they decay over time and, if so, how quickly. Such long term impacts are ignored in most studies of prejudice reduction, just 8 percent of which measure outcomes more than one day after treatment exposure (Paluck et al. 2021). To find out whether the effects of exposure to the treatment video persisted, we recontacted 895 study participants by phone approximately four months after our original data collection.[26]

To ensure the validity of our comparisons of short- and long-term impacts, we collected information in the follow-up phone survey on the respondent's age and the name of the village in which they were living before their displacement. Our intention was only to include respondents in the follow-up analysis for whom the age and village reported in the phone survey matched the age and village reported in the

[26] Details of our sampling procedures for the follow-up phone survey are provided in Appendix E.

survey conducted immediately post-treatment. Unfortunately, these matches were rarely exact—either because the person who answered our phone call was not the same person who provided responses in our survey four months prior (in which case excluding them was the right call) or because some people in our sample were unable to reliably report their exact age and/or referred to the place they came from using different labels (village, clan/community, local government area, etc).[27] These matching challenges were further complicated by spelling differences (for example, "Tse Kanye" vs. "Tse-kpaye"; "Ikyo" vs. "Ikoo"; "Mbakura" vs. "Bakuram"), as our data set contained place names recorded by enumerators (almost always different across the two surveys) based on what respondents told them orally. To deal with this issue, we drew on the expertise of our survey enumerators to scrutinize the village names provided by each respondent in the main and follow-up surveys and, drawing on their local knowledge of how villages map onto communities and local government areas, make judgments about which "villages" should be treated as matching.

A further complication was that the follow-up phone survey did not include the full set of questions that were asked in the main survey. This led to some necessary modifications in the calculation of the outcome indices used in the analysis in order to ensure comparability across the effects estimated immediately post-exposure and four months later.[28]

For these reasons, we present our findings in Table 9 using multiple samples. The "exact match" sample only includes study participants whose ages and village names were exact matches across the main and follow-up surveys. This sample is quite small (N=218), but it provides the greatest confidence that the people being interviewed immediately after treatment exposure and four months later are the same, and that the answers they provide in the follow-up survey can be interpreted as capturing change over time. A second sample, "imperfect match 1," relaxes the matching criteria to admit respondents whose village names match and whose reported ages are within three years of one another (N=328). "Imperfect match 2" relaxes the inclusion rule further by admitting respondents whose village names match *or* whose reported ages are $\leq$ 3 years apart. This last cut provides the largest sample size (N=745) but at the cost of likely including comparisons of responses given by different people, thus undermining our ability to interpret the results as meaningfully capturing decay over time in treatment impacts.

The results in Table 9 provide little evidence that the striking effects found immediately after exposure to the treatment video persisted four months later. Only the estimated treatment effect on

[27] On the inaccuracy of age reporting in surveys conducted in LMCs, see Ewbank (1981) and Rosenzweig (2021), among others.

[28] A listing of questions dropped from the follow-up survey is provided in Appendix Table E1.

Table 9: Long term impacts

	Compassion	Social distance	Negative stereotype	Trust	Support policies	N
Exact match						
Immediately post-exposure	0.673***	-0.208**	-0.136	0.278***	0.335***	218
	(0.136)	(0.105)	(0.094)	(0.091)	(0.104)	
4 months later	0.159	-0.120	-0.007	-0.013	-0.149	218
	(0.123)	(0.103)	(0.106)	(0.084)	(0.114)	
Imperfect match 1						
Immediately post-exposure	0.485***	-0.257***	-0.199**	0.246***	0.359***	328
	(0.100)	(0.080)	(0.079)	(0.073)	(0.081)	
4 months later	0.192**	-0.111	-0.097	0.082	0.014	328
	(0.091)	(0.079)	(0.083)	(0.064)	(0.086)	
Imperfect match 2						
Immediately post-exposure	0.577***	-0.279***	-0.185***	0.244***	0.290***	745
	(0.072)	(0.053)	(0.050)	(0.048)	(0.050)	
4 months later	0.118**	-0.017	0.001	0.000	-0.015	745
	(0.060)	(0.051)	(0.051)	(0.041)	(0.054)	

Note: Outcome indices for all estimates are calculated using only the components that were collected in both the main and follow-up surveys (see Appendix Table E1). The reported values are regression coefficients from models that include the same controls and fixed effects as in Figure 4. *p<0.1; **p<0.05; ***p<0.01.

the compassion index is significant in the follow-up survey, and only in two of the three specifications. While this is discouraging for those hoping for the intervention to have generated permanent reductions in farmer-herder tensions, we note that four months' duration is a hard test for the persistence of treatment impacts, especially from a light touch intervention like ours and in an environment where prominent voices in the media and in local politics propagate anti-herder messages. In addition, we cannot rule out that the farmers in our study may have still been more positively disposed toward herders had we measured their sentiments a week later, two weeks later, a month later, or even two months later. From a policy perspective, the key will be to undertake follow-up work that allows us to better characterize the decay rate of the treatment effects, and then to devise additional policy interventions that can take advantage of that window of opportunity to potentially generate more permanent improvements in intergroup relations.[29]

There is evidence, however, to suggest that the results in Table 9 may not in fact point to a dissipation of treatment effects over time. The outcomes estimated in the analysis reflect differences in responses across participants who saw the treatment and placebo videos. While it is reasonable to assume that a shift from significant differences found immediately after exposure to no differences four months later is due to a weakening of the treatment effect over time, it is also possible that the disappearance in the gap between treated and control participants is because the control participants came to adopt the views and attitudes of the treated participants. This might occur if participants in the treatment group discussed the contents of the treatment video with others in the IDP camp, causing its impact to spread beyond the people who viewed it firsthand as part of our project. Some

[29] For an example of such work, see Allcott and Rodgers (2014).

of the patterns in Figure 5 are consistent with this interpretation.[30]

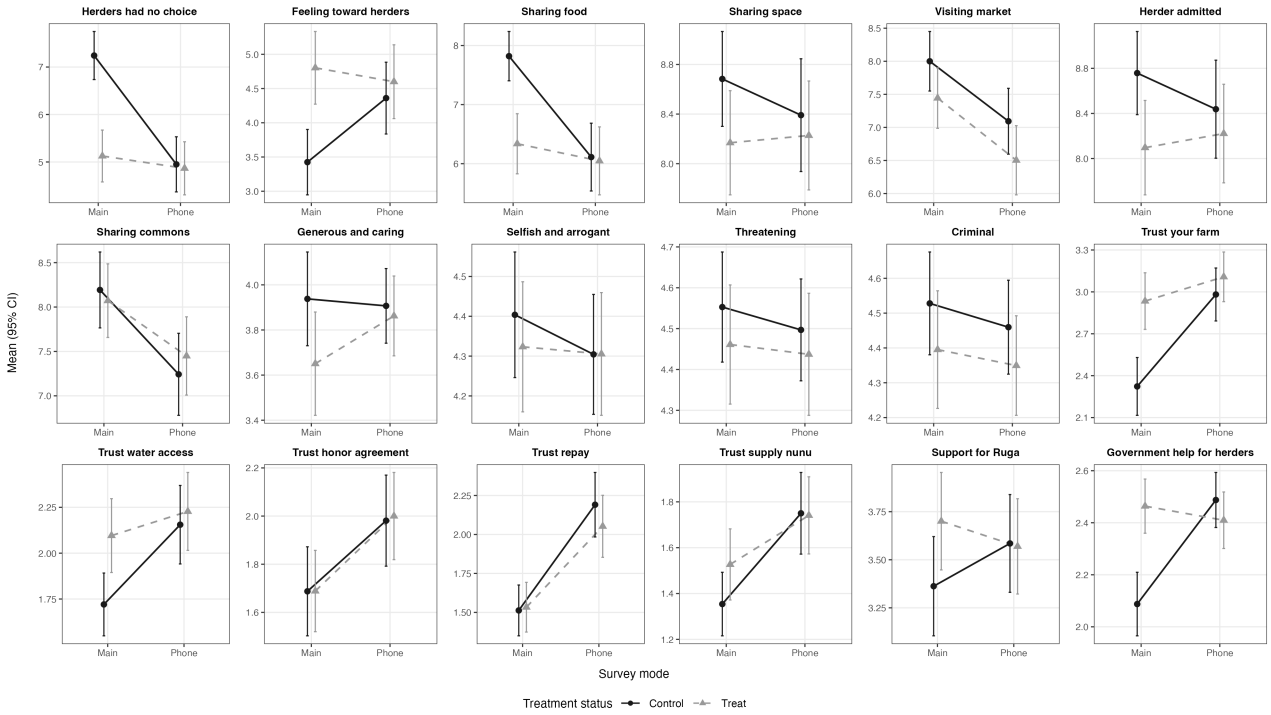


Figure 5: Outcomes in treated and control groups, measured immediately after exposure and four months later

Figure 5 plots the average responses among treated and control participants as recorded immediately after exposure to the treatment and placebo videos and in the follow-up phone survey four months later. Each panel provides the comparison for one of the 18 outcome measures that were collected in both the main and follow-up phone surveys. In 6 of the 17 panels—including the important "did the herders have a choice?" question (upper left)—we observe a shift from a statistically significant difference between responses in the treated and control groups in the main survey to an insignificant difference in the follow-up phone survey. In every case, the change is driven by a statistically significant shift in responses in the *control* group. Indeed, in all of the panels, the changes in responses are almost always greater for control than treatment group participants, and in every case move those control group participants in the direction of the treatment effect—even for outcomes where the differences across treated and control participants were not statistically significant in the post-treatment survey. These patterns are consistent with the spillover of treatment impacts from treated to control partici-

[30]It is also possible that the convergence in responses in the control group toward those in the treatment group is due to the mode of enumeration of the follow-up survey, as phone surveys have been shown to be more vulnerable to social desirability bias than face-to-face surveys (Holybrook, Green and Krosnick 2003; Lupu and Wolsky 2022; Anderson et al. 2024). While this explanation might account for the softening in the control group of expressed views toward herders, it would not account for why such softening was not also occurring in the treatment group, whose views toward herders were still negative on the whole.

pants rather than the dissipation of treatment impacts among treated participants over time. Viewing the results of the long-term analysis through this lens, the interpretation of our findings become rosier. They suggest that the impact of a perspective-getting intervention like ours can have impacts that are not only positive but that, over time, may spread beyond the those who are directly exposed to the treatment.

6 Discussion

It is worth reflecting on why we found such strong and robust impacts. One possible explanation is that the information provided in the treatment video was genuinely new to the farmers in our study.[31] Although our focus groups revealed that most people in the IDP camps were generally aware of climate change and its impacts, it is reasonable to believe that none of our study participants had experienced first hand the utter devastation depicted in the treatment video. Very few had ever traveled outside of Benue State, and the vegetative density that they were exposed to was very different from the conditions faced by the herders who came onto their lands (as shown in panel A of Figure 1). The treatment may have been as effective as it was because it opened the farmers' eyes to a set of circumstances that they had previously not been able to imagine.

The intervention's effectiveness may also have been rooted in the kind of perspective-getting encouraged by the treatment video. As Adida et al. (2025) note, most perspective-getting interventions are designed to generate an empathetic emotional response. Our intervention, by contrast, was at least as much an information or misperception-reducing treatment (seeking to educate farmers about the conditions that herders faced in their traditional grazing lands) as it was an intervention meant to generate reactive empathy—even as it undoubtedly had that effect as well. Insofar as perspective-getting exercises sometimes fail when people are asked to feel empathy for members of socially distant outgroups (Simas, Clifford and Kirkland 2020; Adida et al. 2025), our intervention may have been as successful as it was because it was not designed principally to trigger an emotional response. The fact that the treatment combined aspects of information provision with perspective-getting may also have bolstered its impact (Adida et al. 2025).

We suspect that these aspects of the intervention's design may have been reinforced by the medium through which the treatment was presented. Many perspective-taking or perspective-getting experiments introduce study participants to an outgroup member's point of view by having them think

[31] On the importance of information being novel for its provision to alter behavior, see Lieberman, Posner and Tsai (2014) and Dunning et al. (2019).

about an experience (Broockman and Kalla 2016), read a vignette or testimony (Adida, Lo and Platas 2018; Adida et al. 2025), reflect on one's family history (Dinas, Fouka and Schlapfer 2021; Williamson et al. 2021), or listen to an audio recording of a personal narrative (Audette, Horowitz and Michelitch 2020). These approaches, rooted in personal reflection or the processing of written words or audio, are likely less powerful than viewing a video that provides arresting visual images. A growing literature underscores the power of visual images to shape political attitudes (Brader 2006; Schill 2012; Harris and Lin-Greenberg 2025), and at least one recent study has found strong impacts of a video treatment aimed at reducing prejudice toward Venezuelan migrants in Colombia (Rodríguez Chatruc and Rozo 2024). To the extent that the medium through which our treatment was delivered contributed to its impact, our study holds important implications for future research and policymaking that employs treatments aimed at persuasion and generating attitudinal change.

It is also worth noting that the treatment video only addresses why the herders came onto the farmers lands, not how the herders conducted themselves once they arrived. One could imagine a study participant being convinced by the video that the herders had no choice but to leave their traditional grazing lands but still being enraged by the herders' violence and disregard for the farmers' crops and property.[32] Such a farmer would likely not increase their compassion, trust, etc. upon seeing the treatment video. This logic suggests a likely downward bias in our treatment effects, which makes the impact of exposure to the treatment video (which would explain the herders' arrival but not their violent behavior) all the more astounding.

7 Conclusion

The understandable animosity felt by displaced farmers toward the herders who caused their displacement is a major impediment to peace and well-being in wide swaths of the Sahel. In this study, we test the efficacy of a novel perspective-getting approach to addressing this problem: providing victims of violence and displacement with information about the circumstances that contributed to the behavior of the actors responsible for their suffering. We expose a large sample of displaced farmers living in IDP camps in Benue State, Nigeria to a video depicting the devastating impact of climate change on the herders traditional grazing lands, conveying the message that the herders had little choice but to search for new areas for their cattle. We show that exposure to the video caused farmers to update

[32]Evidence that the farmers do distinguish between the mere trespass of cattle on their lands and damage to crops or the commitment of acts of violence comes from answers to a question about the appropriate penalty in each of these situations. Whereas the modal response to a situation involving the trespass of cattle resulting in no damage was that the penalty should be simply a warning, the modal response for damage to crops was a 200,000 Naira fine, and the modal response for an act of violence against the farmer's family was life in prison. See Appendix Figure A1.

their views about why the herders came onto their lands and is associated with a significant softening of the farmers attitudes toward herders: they become more compassionate and trusting, harbor fewer negative stereotypes, become more comfortable with cross-group social interactions, and increase their support for government policies that might ease the herders' plight and potentially address the systemic roots of the conflict.

Like many studies in the information provision and prejudice reduction space, our estimated effects are limited in size (Paluck et al. 2021; Coppock 2022). However, unlike most studies, our sample is quite large, our findings are extremely robust across a broad range of attitudinal measures of cross-group relations, and the impacts we estimate appear to diffuse beyond those directly exposed to our intervention.[33] Our study also represents a "hard case" for prejudice reduction, given both the relatively light touch of our intervention and the life-changing physical harm experienced by our study participants at the hands of members of the outgroup. Our estimated effects might have been even larger and more long-lasting had we supplemented the viewing of the five minute video with group discussions about its contents (Grady et al. 2023), or if we had involved trusted authorities in the messaging (Blair et al. 2021; Grady et al. 2023). Our findings thus underscore the potential of perspective-getting—especially perspective-getting that provides the backstory explaining the behavior of the outgroup—in reducing intergroup animosity and fostering greater social cohesion.

In addition to offering lessons for social scientists interested in prejudice reduction, our study holds important implications for policymakers. Intergroup violence in contexts of resource scarcity is often a product of over-reactions to small harms born from assumptions, sometimes incorrect, about the perpetrators' motivations (Mernyk et al. 2022; Nwankwo 2025). Interventions like ours that address these assumptions and offer a backstory that might account for the perpetrator's actions may reduce conflict under such circumstances.

The deep distrust between farmers and herders is also a critical obstacle to the acceptance of government policies that might address the structural sources of the conflict. Climate change itself will not be stopped by behavioral modifications or policy interventions in places like Benue State. But the land competition and conflict that climate change generates can potentially be addressed by initiatives like RUGA, NLTP, and schemes like them that are designed to manage scarce resources and reduce incendiary contact between farmers and herders. Such initiatives are only likely to be embraced, however, if cross-group hostility and mistrust can be moderated. To the extent that interventions like

[33] Our finding of significant attitudinal changes stands in contrast to prominent studies in the prejudice reduction literature (Paluck 2009; Scacco and Warren 2018; Mousa 2020), which find impacts on behaviors but not attitudes. In this respect, our findings are closest to Grady et al. (2023), who find impacts on both attitudes and behaviors, albeit in a different type of intervention involving cross-group contact rather than perspective-getting.

the one studied here can successfully reduce animosity and build trust between farmers and herders—even if only fleetingly—they may create windows of opportunity for launching policies that hold promise of tackling the conflict's structural roots.

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Online Appendix

A Additional tables and figures

Table A1: Descriptive statistics and balance[a]

	Full sample	Treated	Control	Difference (T-C)
Age	43.6	43.9	43.2	0.7
Female	62.2	63.2	61.2	1.0
Education				
No primary	52.2	51.5	52.8	-1.3
Some/completed primary	17.0	18.5	15.5	3.0**
Some secondary	11.5	12.0	11.0	1.0
Completed secondary or more	19.4	17.9	20.8	- 2.9*
Religion				
Catholic	64.1	61.8	66.2	-4.4**
Pentecostal	21.7	22.1	21.3	0.8
Occupation prior to displacement				
Agriculture	95.1	95.2	95.1	0.1
Residence in camp (yrs)	6.5	6.4	6.6	-0.2*
Experience w violence prior to displacement				
Personally	76.0	77.3	74.6	2.6*
Immediate family	91.0	91.2	90.9	0.3
Village/community	98.9	99.2	98.6	0.6
Degree of violence experienced (1-10 scale)	9.0	9.0	9.0	0
Returning home				
Would like to return home someday	96.1	96.2	95.9	0.3
Very/somewhat likely by next year	86.5	86.2	86.9	-0.7
Very/somewhat likely after next year	88.0	87.5	88.4	-0.9
Why did herders came to your area? (pre-treatment)				
To grab our land	56.4	58.1	54.8	3.3*
Their lands could not support their cattle	23.7	23.4	24.0	-0.6
For religious reasons/jihad	5.6	5.9	5.4	0.5
Because they are foreign terrorists	11.4	9.9	12.7	-2.8*
Not sure/don't know	2.9	2.7	3.1	-0.4
Herders chose to come onto your lands (1-10)	7.71	7.73	7.69	0.04
Zero-sum mindset index	2.56	2.54	2.59	-0.05*
Most people can be trusted (agree/strongly agree)	20.1	19.1	21.1	-2.0

[a] Measured pre-treatment.

*p<0.1; **p<0.05; ***p<0.01.

Table A2: Impact of exposure to the treatment video

	Compassion		Social distance		Negative stereotype		Trust		Support policies	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	0.109***	0.095***	-0.223***	-0.203***	-0.160***	-0.151***	0.202***	0.188***	0.219***	0.243***
	(0.024)	(0.022)	(0.037)	(0.032)	(0.027)	(0.023)	(0.032)	(0.028)	(0.035)	(0.030)
Female		-0.006		0.169***		0.043		-0.076**		-0.073**
		(0.026)		(0.038)		(0.027)		(0.033)		(0.036)
Age		0.002**		0.001		0.001		-0.001		-0.003***
		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)
Catholic		0.005		-0.069		-0.035		0.208		0.293*
		(0.115)		(0.168)		(0.121)		(0.148)		(0.161)
Other Christian		0.076		-0.043		-0.020		0.165		0.217
		(0.119)		(0.174)		(0.124)		(0.153)		(0.166)
Pentecostal		0.007		-0.096		0.001		0.245		0.295*
		(0.117)		(0.171)		(0.122)		(0.151)		(0.163)
Traditionalist		-0.010		0.030		-0.105		0.303		0.375*
		(0.155)		(0.226)		(0.162)		(0.199)		(0.215)
Unaffiliated		0.272		0.046		-0.019		0.155		0.458*
		(0.174)		(0.253)		(0.182)		(0.223)		(0.242)
Education		0.015**		-0.024**		-0.013*		0.015		-0.004
		(0.008)		(0.011)		(0.008)		(0.010)		(0.011)
N	1996	1993	1996	1993	1996	1993	1996	1993	1995	1992

Note: This table provides the regression version of Figure 4. Multivariate models (even columns) include camp and enumerator fixed effects. Religion coefficients are relative to the omitted category (Anglican). *p<0.1; **p<0.05; ***p<0.01.

Table A3: Compassion for herders

	Treated	Control	Difference (T-C)
Compassion index[a]	0.11	0.00	0.11***
Compassion for herders[b]	4.95	3.61	1.35***
Appropriate penalty for herder who trespasses...[c]			
...without damage	6.74	6.67	0.07
...and damages crops	4.90	4.85	0.05
...and commits act of violence against family	2.40	2.30	0.10
Support for arresting all herders and forcing them to go back to their home area[d]	2.84	2.83	0.01

[a] An index taking the average of the normalized values of the components below.

[b] Question wording: "As you know, herders in the north have been facing challenges due to the loss of their traditional grazing lands. On a scale of 1 to 10, with 1 indicating that you have little feeling for them and 10 indicating that you feel great compassion for them, how much compassion do you feel for these herders from the north who are facing these challenges?"

[c] Question wording: "Imagine a situation where a herder's cattle trespass on a farmer's lands [but do not do any damage; damage the farmer's crops; damage the farmer's crops and the herder commits an act of violence against the farmer's family]. The herder is arrested. What do you think should be the appropriate penalty? Life imprisonment? Ten years in prison? One year in prison? One month in prison? A fine of 200,000 Naira? A fine of 20,000 Naira? A warning not to let it happen again?" (1=warning; 7=life imprisonment).

[d] Asked via a list experiment.

*p<0.1; **p<0.05; ***p<0.01.

Table A4: Social distance with herders

	Treated	Control	Difference (T-C)
Social distance index[a]	0.22	0.00	0.22***
How comfortable would you be...[b]			
...sharing food or water with a herder	4.44	3.33	1.12***
...sharing space in home with a herder for a few nights	2.92	2.27	0.66***
...if someone in family married a herder	2.60	2.31	0.29**
...visiting market if several herders were present	3.76	3.17	0.59***
...if a herder family were admitted into the camp	2.83	2.36	0.46***
How much shared in common with herders[c]	3.26	2.74	0.53***

[a] An index taking the average of the normalized values of the components below.
[b] Question wording: "Next, I would like you to tell me how comfortable you would be with certain situations. On a scale of 1 to 10, with 1 indicating that you would be very uncomfortable and 10 indicating that you would be very comfortable, how comfortable would you be..."
[c] Question wording: "On a scale from 1 to 10, with 1 being that you do not share anything in common and 10 being that you share very much in common, how much do you share in common with herders?"
*p<0.1; **p<0.05; ***p<0.01.

Table A5: Negative stereotypes about herders

	Treated	Control	Difference (T-C)
Negative stereotype index[a]	-0.16	0.00	-0.16***
Herders are...[b]			
...generous and caring	3.67	3.86	-0.19***
...selfish and arrogant	4.29	4.44	-0.15***
...threatening	4.39	4.61	-0.22***
...hard workers	2.35	2.47	-0.12*
...criminals	4.30	4.48	-0.18***
...intelligent	2.60	2.81	-0.21***
...militants	4.48	4.60	-0.13***
...all the same	3.68	3.91	-0.22***
Share of herders thought to be militant[c]	3.88	4.11	-0.23***

[a] An index taking the average of the normalized values of the components below, with reverse codings as appropriate.
[b] Question wording: "Now I want to read some stereotypes that people have about herders here in Nigeria. Please tell me whether you agree or disagree with each statement, and how strongly you agree or disagree" (1=strongly disagree; 5=strongly agree).
[c] Question wording: "Earlier, we asked you if you agreed with the statement "herders are militant." Of course, it is possible that some herders are militant and others are not. What share of herders do you think are militant?" Note that this was not a pre-registered outcome.
*p<0.1; **p<0.05; ***p<0.01.

Table A6: Trust toward herders

	Treated	Control	Difference (T-C)
Trust index[a]	0.20	0.00	0.2***
How much would you trust a herder to...[b]			
...graze their cattle on your stubble after harvest	2.71	2.44	0.27***
...have their cattle access your water supply	2.15	1.82	0.33***
...honor an agreement	1.72	1.54	0.19***
...repay a small loan	1.59	1.42	0.18***
...supply wara for the camp	1.57	1.40	0.17***

[a] An index taking the average of the normalized values of the components below.
[b] Question wording: "Now I want to ask you some questions about how much you would trust a herder to perform various tasks. For each task, I want you to tell me whether you would trust them very much, somewhat, just a little bit, or not at all" (1=not at all; 4=very much).
*p<0.1; **p<0.05; ***p<0.01.

Table A7: Support for policies that would help herders and potentially address the structural roots of the conflict

	Treated	Control	Difference (T-C)
Policy support index[a]	0.22	0.00	0.22***
Support for RUGA policy...[a]	3.88	3.63	0.25***
How much support should government provide?[b]	2.42	2.19	0.23***

[a] Question wording: "Several years ago, the Nigerian government introduced a new rural grazing area policy (RUGA) to provide land for herders to graze their cattle as well as access to water and other basic services. Do you support or oppose this policy, and how strongly do you support or oppose it?" (1=strongly oppose; 5=strongly support).
[b] Question wording: "Apart from the specific RUGA policy, do you think the Nigerian government should be trying to help herders who have been forced from their grazing lands because they have become too dry? If so, how much support should the government provide?" (1=no support; 3=a lot of support).
*p<0.1; **p<0.05; ***p<0.01.

Table A8: Impact of exposure to the treatment video

	Compassion		Social distance		Negative stereotype		Trust		Support policies	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	0.113***	0.103***	-0.218***	-0.197***	-0.158***	-0.155***	0.201***	0.189***	0.242***	0.241***
	(0.023)	(0.021)	(0.035)	(0.031)	(0.026)	(0.022)	(0.031)	(0.027)	(0.033)	(0.029)
Female		0.003		0.161***		0.042		-0.068**		-0.060*
		(0.025)		(0.036)		(0.026)		(0.032)		(0.034)
Age		0.002**		0.001		0.001		-0.001		-0.003***
		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)
Catholic		0.044		-0.040		-0.033		0.147		0.228
		(0.111)		(0.161)		(0.116)		(0.143)		(0.154)
Other Christian		0.121		-0.021		-0.018		0.117		0.159
		(0.114)		(0.167)		(0.120)		(0.148)		(0.160)
Pentecostal		0.049		-0.096		-0.017		0.199		0.226
		(0.112)		(0.163)		(0.117)		(0.145)		(0.157)
Traditionalist		0.018		0.047		-0.097		0.224		0.278
		(0.150)		(0.218)		(0.157)		(0.194)		(0.209)
Unaffiliated		0.309*		0.057		-0.020		0.102		0.404*
		(0.171)		(0.249)		(0.179)		(0.221)		(0.239)
Education		0.013*		-0.024**		-0.013*		0.015		-0.002
		(0.007)		(0.011)		(0.008)		(0.009)		(0.010)
N	2169	2167	2169	2167	2169	2167	2169	2167	2168	2166

Note: This table replicates the results reported in Figure 4 but includes inattentive participants who failed the comprehension test. Multivariate models (even columns) include camp and enumerator fixed effects. Religion coefficients are relative to the omitted category (Anglican). *p<0.1; **p<0.05; ***p<0.01.

Table A9: Heterogeneous effects: Prior exposure to violence

	Compassion	Social distance	Negative stereotype	Trust	Support policies
ACLED data					
Treatment	0.113***	-0.185***	-0.139***	0.197***	0.252***
	(0.029)	(0.042)	(0.031)	(0.038)	(0.040)
Prior exposure to violence[a]	0.000	-0.001	-0.001**	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Interaction term	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
N	1659	1659	1659	1659	1658

[a]Based on the number of fatalities in the farmer's LGA in the year prior to their displacement.

Note: All regressions include the full set of pre-registered controls, as well as camp and enumerator fixed effects. *p<0.1; **p<0.05; ***p<0.01.

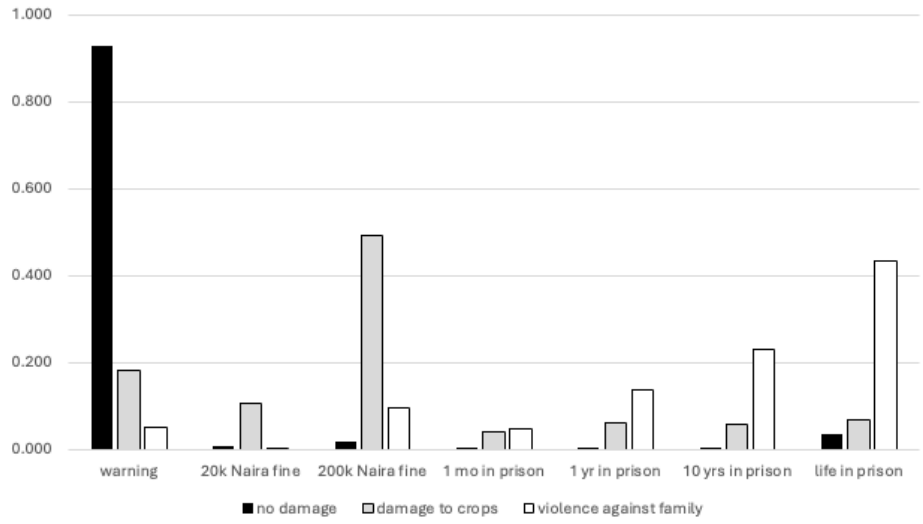

Figure A1: Appropriate punishment if a herder's cattle trespasses on a farmer's lands

Table A10: Heterogeneous effects: Prior exposure to violence, with covariates

	Compassion	Social distance	Negative stereotype	Trust	Support policies
As reported in survey					
Treatment	0.179***	-0.335***	-0.356***	0.362***	0.415***
	(0.045)	(0.065)	(0.046)	(0.057)	(0.062)
Prior exposure to violence[a]	0.052	-0.132**	-0.139***	0.131**	0.056
	(0.042)	(0.062)	(0.044)	(0.054)	(0.059)
Interaction term	-0.111**	0.175**	0.270***	-0.230***	-0.225***
	(0.051)	(0.075)	(0.053)	(0.066)	(0.071)
Age	0.002**	0.001	0.001*	-0.001	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.007	0.170***	0.046*	-0.078**	-0.077**
	(0.026)	(0.038)	(0.027)	(0.033)	(0.036)
Catholic	0.005	-0.065	-0.034	0.205	0.296*
	(0.115)	(0.168)	(0.120)	(0.148)	(0.160)
Other Christian	0.078	-0.045	-0.024	0.168	0.221
	(0.119)	(0.173)	(0.124)	(0.153)	(0.165)
Pentecostal	0.008	-0.094	0.000	0.245	0.299*
	(0.117)	(0.171)	(0.122)	(0.150)	(0.163)
Traditionalist	-0.001	0.022	-0.125	0.318	0.399*
	(0.155)	(0.226)	(0.161)	(0.199)	(0.215)
Unaffiliated	0.269	0.053	-0.011	0.148	0.454*
	(0.173)	(0.253)	(0.180)	(0.223)	(0.241)
Education	0.015**	-0.024**	-0.014*	0.015	-0.004
	(0.008)	(0.011)	(0.008)	(0.010)	(0.011)
N	1993	1993	1993	1993	1992
ACLED data					
Treatment	0.110***	-0.188***	-0.139***	0.199***	0.247***
	(0.029)	(0.042)	(0.031)	(0.038)	(0.040)
Prior exposure to violence[b]	0.001	-0.004*	-0.003*	0.003	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Interaction term	0.000	0.000	-0.001	-0.001	0.002
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Age	0.002**	0.001	0.001	-0.001	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.026	0.214***	0.077***	-0.110***	-0.121***
	(0.028)	(0.041)	(0.030)	(0.037)	(0.039)
Catholic	-0.065	-0.081	-0.182	0.374**	0.299
	(0.135)	(0.197)	(0.143)	(0.175)	(0.184)
Other Christian	-0.007	-0.052	-0.162	0.320*	0.208
	(0.138)	(0.202)	(0.146)	(0.179)	(0.189)
Pentecostal	-0.075	-0.083	-0.129	0.375**	0.262
	(0.137)	(0.200)	(0.144)	(0.177)	(0.187)
Traditionalist	-0.060	0.012	-0.277	0.585**	0.501**
	(0.178)	(0.260)	(0.188)	(0.231)	(0.244)
Unaffiliated	0.190	0.055	-0.166	0.318	0.476*
	(0.190)	(0.278)	(0.201)	(0.247)	(0.260)
Education	0.011	-0.019	-0.007	0.010	-0.012
	(0.008)	(0.012)	(0.009)	(0.011)	(0.011)
N	1659	1659	1659	1659	1658

[a]Based on whether respondent indicated having experienced violence directed at them personally.
[b]Based on the number of violent events between farmers and herders in the farmer's LGA in the year prior to their displacement.
Note: All regressions include camp and enumerator fixed effects. Religion coefficients are relative to the omitted category (Anglican). *p<0.1; **p<0.05; ***p<0.01.

Table A11: Heterogeneous effects: Updated priors, with covariates[a]

	Compassion	Social distance	Negative stereotype	Trust	Support policies
Treatment	0.046*	-0.116***	-0.086***	0.100***	0.142***
	(0.024)	(0.035)	(0.025)	(0.030)	(0.032)
Updated priors	0.008	-0.020***	-0.010*	0.022***	0.021***
	(0.005)	(0.007)	(0.005)	(0.006)	(0.007)
Interaction term	0.016**	-0.022**	-0.023***	0.023***	0.031***
	(0.007)	(0.010)	(0.007)	(0.008)	(0.009)
Age	0.002**	0.001	0.001	-0.001	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.005	0.163***	0.044	-0.076**	-0.066*
	(0.026)	(0.037)	(0.027)	(0.033)	(0.035)
Catholic	-0.004	-0.051	-0.023	0.188	0.278*
	(0.115)	(0.166)	(0.119)	(0.145)	(0.156)
Other Christian	0.071	-0.039	-0.022	0.155	0.206
	(0.118)	(0.171)	(0.123)	(0.150)	(0.160)
Pentecostal	0.002	-0.093	0.002	0.238	0.299*
	(0.116)	(0.169)	(0.121)	(0.148)	(0.158)
Traditionalist	-0.025	0.056	-0.087	0.273	0.347*
	(0.154)	(0.223)	(0.160)	(0.195)	(0.209)
Unaffiliated	0.268	0.047	0.020	0.158	0.436*
	(0.176)	(0.256)	(0.183)	(0.224)	(0.239)
Education	0.015**	-0.027**	-0.013*	0.017*	0.003
	(0.008)	(0.011)	(0.008)	(0.010)	(0.010)
N	1967	1967	1967	1967	1966

[a] Based on whether respondent updated their view on whether the herders came onto their lands due to forces outside of their control.

Note: All regressions include camp and enumerator fixed effects. Religion coefficients are relative to the omitted category (Anglican). *p<0.1; **p<0.05; ***p<0.01.

Table A12: Heterogeneous effects: Zero sum mentality, with covariates

	Compassion	Social distance	Negative stereotype	Trust	Support policies
Treatment	0.016	-0.165	-0.057	0.118	0.068
	(0.077)	(0.113)	(0.081)	(0.099)	(0.108)
Zero sum mentality	-0.037	0.015	0.019	-0.040	-0.081**
	(0.023)	(0.034)	(0.024)	(0.030)	(0.032)
Interaction term	0.030	-0.014	-0.037	0.027	0.068*
	(0.029)	(0.042)	(0.030)	(0.037)	(0.040)
Age	0.002**	0.001	0.001	-0.001	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.006	0.169***	0.042	-0.076**	-0.073**
	(0.026)	(0.038)	(0.027)	(0.033)	(0.036)
Catholic	0.011	-0.071	-0.035	0.214	0.304*
	(0.115)	(0.169)	(0.121)	(0.149)	(0.161)
Other Christian	0.080	-0.045	-0.020	0.170	0.225
	(0.119)	(0.174)	(0.124)	(0.153)	(0.166)
Pentecostal	0.012	-0.098	0.002	0.250*	0.304*
	(0.117)	(0.171)	(0.122)	(0.151)	(0.163)
Traditionalist	-0.005	0.028	-0.109	0.308	0.385*
	(0.155)	(0.226)	(0.162)	(0.199)	(0.215)
Unaffiliated	0.282	0.042	-0.024	0.165	0.479**
	(0.174)	(0.254)	(0.182)	(0.224)	(0.242)
Education	0.015**	-0.024**	-0.013*	0.015	-0.004
	(0.008)	(0.011)	(0.008)	(0.010)	(0.011)
N	1993	1993	1993	1993	1992

Note: All regressions include camp and enumerator fixed effects. Religion coefficients are relative to the omitted category (Anglican). *p<0.1; **p<0.05; ***p<0.01.

Table A13: Heterogeneous effects: Generalized trust, with covariates

	Compassion	Social distance	Negative stereotype	Trust	Support policies
Treatment	0.108***	-0.179***	-0.105**	0.098*	0.029
	(0.039)	(0.057)	(0.041)	(0.051)	(0.055)
Generalized trust	0.036***	-0.042**	-0.032***	0.013	-0.030*
	(0.012)	(0.017)	(0.012)	(0.015)	(0.016)
Interaction term	-0.005	-0.015	-0.026	0.048**	0.111***
	(0.017)	(0.024)	(0.017)	(0.022)	(0.023)
Age	0.001**	0.001	0.001*	-0.001	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.008	0.171***	0.045*	-0.077**	-0.073**
	(0.026)	(0.037)	(0.027)	(0.033)	(0.036)
Catholic	-0.002	-0.061	-0.029	0.205	0.300*
	(0.115)	(0.168)	(0.120)	(0.148)	(0.160)
Other Christian	0.069	-0.036	-0.015	0.165	0.226
	(0.119)	(0.173)	(0.124)	(0.153)	(0.165)
Pentecostal	-0.004	-0.085	0.010	0.243	0.309*
	(0.117)	(0.170)	(0.122)	(0.150)	(0.162)
Traditionalist	-0.028	0.052	-0.088	0.293	0.383*
	(0.154)	(0.225)	(0.161)	(0.199)	(0.214)
Unaffiliated	0.261	0.066	0.002	0.134	0.431*
	(0.173)	(0.253)	(0.181)	(0.223)	(0.240)
Education	0.016**	-0.024**	-0.014*	0.015	-0.004
	(0.008)	(0.011)	(0.008)	(0.010)	(0.011)
N	1993	1993	1993	1993	1992

Note: All regressions include camp and enumerator fixed effects. Religion coefficients are relative to the omitted category (Anglican). *p<0.1; **p<0.05; ***p<0.01.

Table A14: Heterogeneous effects: Gender, with covariates

	Compassion	Social distance	Negative stereotype	Trust	Support policies
Treatment	0.096***	-0.186***	-0.129***	0.208***	0.212***
	(0.036)	(0.052)	(0.037)	(0.046)	(0.050)
Female	-0.005	0.181***	0.060*	-0.060	-0.097**
	(0.034)	(0.049)	(0.035)	(0.043)	(0.047)
Interaction term	-0.002	-0.026	-0.036	-0.032	0.051
	(0.045)	(0.066)	(0.047)	(0.058)	(0.063)
Age	0.002**	0.001	0.001	-0.001	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.005	0.181***	0.060*	-0.060	-0.097**
	(0.034)	(0.049)	(0.035)	(0.043)	(0.047)
Catholic	0.005	-0.069	-0.036	0.207	0.294*
	(0.115)	(0.168)	(0.121)	(0.148)	(0.161)
Other Christian	0.076	-0.043	-0.020	0.165	0.217
	(0.119)	(0.174)	(0.124)	(0.153)	(0.166)
Pentecostal	0.007	-0.096	0.001	0.244	0.295*
	(0.117)	(0.171)	(0.122)	(0.151)	(0.163)
Traditionalist	-0.010	0.031	-0.104	0.304	0.373*
	(0.155)	(0.226)	(0.162)	(0.199)	(0.215)
Unaffiliated	0.272	0.047	-0.018	0.156	0.457*
	(0.174)	(0.253)	(0.182)	(0.224)	(0.242)
Education	0.015**	-0.024**	-0.013*	0.015	-0.004
	(0.008)	(0.011)	(0.008)	(0.010)	(0.011)
N	1993	1993	1993	1993	1992

Note: All regressions include camp and enumerator fixed effects. Religion coefficients are relative to the omitted category (Anglican). *p<0.1; **p<0.05; ***p<0.01.

Table A15: Heterogeneous effects: Length of residence in camp, with covariates

	Compassion	Social distance	Negative stereotype	Trust	Support policies
Treatment	0.096***	-0.202***	-0.167***	0.176***	0.281***
	(0.033)	(0.048)	(0.034)	(0.042)	(0.046)
> median yrs in camp	0.029	0.005	-0.027	-0.009	0.097*
	(0.036)	(0.053)	(0.038)	(0.046)	(0.050)
Interaction term	-0.002	-0.002	0.028	0.022	-0.065
	(0.044)	(0.064)	(0.046)	(0.057)	(0.061)
Age	0.002**	0.001	0.001	-0.001	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.006	0.169***	0.043	-0.076**	-0.073**
	(0.026)	(0.038)	(0.027)	(0.033)	(0.036)
Catholic	0.007	-0.069	-0.037	0.207	0.298*
	(0.115)	(0.168)	(0.121)	(0.149)	(0.161)
Other Christian	0.077	-0.043	-0.022	0.165	0.221
	(0.119)	(0.174)	(0.124)	(0.153)	(0.166)
Pentecostal	0.007	-0.096	0.000	0.244	0.296*
	(0.117)	(0.171)	(0.122)	(0.151)	(0.163)
Traditionalist	-0.007	0.030	-0.106	0.304	0.379*
	(0.155)	(0.226)	(0.162)	(0.199)	(0.215)
Unaffiliated	0.278	0.047	-0.021	0.156	0.469*
	(0.174)	(0.254)	(0.182)	(0.224)	(0.242)
Education	0.016**	-0.024**	-0.013*	0.015	-0.003
	(0.008)	(0.011)	(0.008)	(0.010)	(0.011)
N	1993	1993	1993	1993	1992

Note: The median length of residence is 7 years. All regressions include the full set of pre-registered controls, as well as camp and enumerator fixed effects. Religion coefficients are relative to the omitted category (Anglican). *p<0.1; **p<0.05; ***p<0.01.

B Treatment video

B.1 Video clips

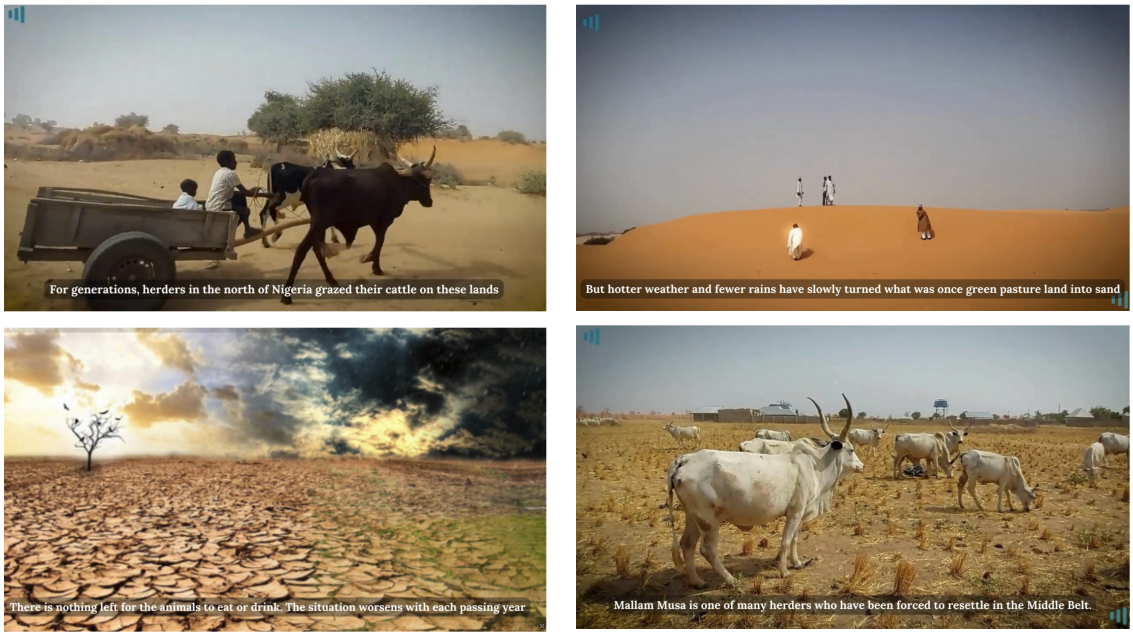

Figure B1: Clips from the treatment video

B.2 Video script

Narrator: For generations, herders in the north of Nigeria grazed their cattle on these lands. But hotter weather and fewer rains have slowly turned what was once green pasture land into sand. Water has dried up and the grasses are gone. There is nothing left for the animals to eat or drink. The situation worsens with each passing year. The problem is not going to stop anytime soon. According to one estimate, the sand dunes shift toward the south at a rate of 30 hectares each year, putting 11 states in northern Nigeria at risk of desertification. The families that raised their cattle here for generations would like to stay, but they have no choice. They must move south in search of water and pastureland for their cattle. The lands that once supported them will support them no longer. If they stay, they will not survive.

Local resident: The desert has already taken most of it and this is all just what is left. It has gone far. It has eaten so much. And we don't know what is going to follow after.

Narrator: Zakari lies on the fringes of the Sahara desert. About 50 years ago, it was like any other arid northeast Nigerian community with decent vegetation, a smooth road network, sunbaked mud buildings and it graced many people who had lived there for generations. Now it is just miles

and miles of sand dunes. Desert encroachment has pushed out the majority of the residents. Many of them are herders. Mallam Musa is one of many herders who have been forced to resettle in the Middle Belt.

Mallam Musa, local herder: Some people moved because of lack of security and many of us also came because of the water. Of course, if there's no water, you will move to a place that has water.

Dr Salisu Damagum, animal scientist: Around January/February, you hardly see anything. In the village that the animal can take as food and there's not even water to drink. So this is one of the factors that would make them come southwards for water and for dry grasses. Definitely the desert encroachment is one of the factors that is concentrating the animals in a place and making the clashes between farmers and herders more frequent. Had it been that in the areas encroached by desert there was grasses and water for them, they may not be there. But now it is necessary that they must come southwards as the dry season is approaching in search of little pasture and water.

Narrator: The situation faced by the herders is desperate. They do not want to move to the Middle Belt but they have been forced to by circumstances beyond their control.

C Measuring zero-sum mentality

We measured zero-sum mentality via an index built from the average, normalized responses to a five-question pre-treatment survey module. The questions were modeled on those used in Chinoy et al. (2026), modified to suit our study context. The questions were as follows:

Now I am going to read you a series of statements. I would like you to say whether you agree or disagree with each statement, and how strongly you agree or disagree.

- If one group/tribe/religion becomes richer, this comes at the expense of other groups/tribes/religions [*strongly agree; agree; neither agree nor disagree; disagree; strongly disagree*]
- When people buy and sell with one another, if one person makes more money, then the other person makes less money [*strongly agree; agree; neither agree nor disagree; disagree; strongly disagree*]
- If people in one community do better economically, then it is at the expense of other communities [*strongly agree; agree; neither agree nor disagree; disagree; strongly disagree*]
- Wealth can grow so there is enough for everyone [*strongly agree; agree; neither agree nor disagree; disagree; strongly disagree*]

- If one person is using a water source, then others cannot also use it [*strongly agree; agree; neither agree nor disagree; disagree; strongly disagree*]

Figure C1 shows the distribution of the zero-sum mentality index in our sample.

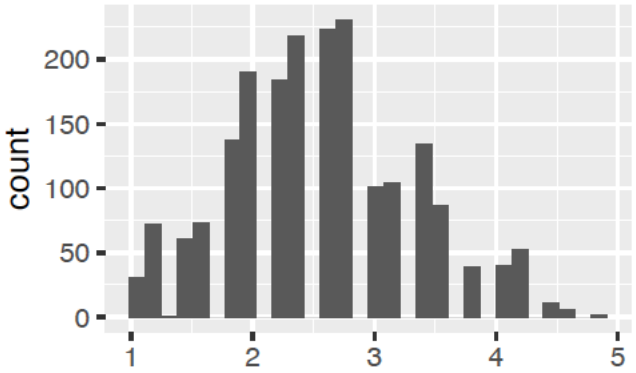

Figure C1: Zero-sum mentality (higher values imply more zero-sum thinking)

D List experiment

We randomized survey respondents to two groups in a pair of list experiments modeled on the version presented in Lépine, Treibich and D'Exelle (2020), developed for studying risky sexual behavior in Senegal. In the fist list experiment, respondents randomized to group 1 were presented with the following instructions: "Next, I will describe some situations that people sometimes find uncomfortable. I will then ask you how many of these situations you find uncomfortable yourself. You should not tell me which specific situations make you feel uncomfortable but the number of situations that make you feel uncomfortable. I will give you some small balls and ask you to hold them in your right hand. Keep both of your hands behind your back. For each of the situations I describe, if it makes you feel uncomfortable, please transfer one ball from your right hand to your left hand behind you. If it does not make you feel uncomfortable, please do not transfer a ball. I will not be aware, and please do not inform me. At the end, I would like to know the total number of situations that make you feel uncomfortable. This number should correspond to the number of balls you have in your left hand. I will now describe the situations:

- neighbors asking to borrow money
- speaking in front of a large number of people
- seeing someone urinate in public

- being asked to keep a secret from a close family member

How many of these situations do you find uncomfortable?"

Respondents randomized to group 2 were read the same instructions. The only difference was that the enumerator also read a fifth situation: being asked by a herder to share food or water. Study participants could hide the extent to which this specific situation made them uncomfortable because they were only asked to report the number of situations that made them feel that way, which should remove incentives to be untruthful so as not to violate a social norm. We can learn the true, unbiased proportion of respondents who feel uncomfortable sharing food or water with a herder by comparing the average number of balls reported by participants in the first and second groups (Blair and Imai 2012).

In the second list experiment, participants were provided with the same general instructions but this time they were read a list of actions that the federal and state governments might take against herders. Participants were asked to indicate how many of these actions they thought the federal and state governments should take. The list of actions was:

- register all herders and their animals and have their ID visible
- stop all herders from crossing state lines and prohibit their entry into neighboring states
- allow all herders to graze wherever they can find access to grasses and water for their animals
- disarm all herders if they carry weapons

Respondents randomized to group 1 were read just these four actions. Those randomized to group 2 were also read a fifth action: arrest all herders and force them to go back to their home area. As with the first list experiment, we can learn the true, unbiased proportion of respondents who support the fifth action (having the federal and state governments arrest all herders and force them to go back to their home area) by comparing the average number of policies supported by participants in the first and seconds.

List experiment questions are challenging to enumerate, especially when employing a technique like the one adopted from Lépine, Treibich and D'Exelle (2020) that requires a lengthy explanation about how to keep track of the number of situations/actions that participants are asked about at the end of the list. Based on the information that SurveyCTO collects about the time enumerators spend on each question, we determined that some respondents progressed more quickly through the list

experiment questions than would be required to ensure that the instructions were read and understood. We determined these cut-offs to be about 90 seconds for the first question and about 60 seconds for the second, since the second question duplicates the instructions from the first. We therefore drop from our analysis all participants whose times spent on the list experiment questions were below these thresholds.[34]

The results of the list experiments are reported in Tables D1 and D2. The second column in Table D1 indicates that an estimated 42% of control group study participants would feel uncomfortable sharing food or water with a herder. Because we asked nearly the same question directly elsewhere in the survey, we can get a handle on the extent of social desirability bias by comparing this result with responses to the directly asked question.[35] Unfortunately those responses are not directly comparable because the direct question asks respondents to rank their degree of comfort on a 1 to 10 scale, whereas the structure of the list experiment requires asking the question in a yes/no form. To provide greater comparability with the list experiment results, we can recalculate the direct version of the question in terms of the share of respondents who gave responses of 5 or lower, indicating that they were on the "uncomfortable" end of the comfort spectrum. Seventy-five percent of control group respondents gave responses in this range—a share significantly *higher* than the share that indicated lack of comfort sharing food or water in the indirectly asked version of the question.[36]

Table D1: Number of situations that make you feel uncomfortable

	Exposed to video treatment	
	Yes	No
List experiment group 1	2.05	2.01
List experiment group 2	2.40	2.43
Difference	0.35	0.42

Note: N=1,465. Participants who completed this list experiment question in less than 90 seconds are excluded. Statistical significance of the difference: *p<0.1; **p<0.05; ***p<0.01.

Turning now to the comparison between participants in the video treatment and control groups, we see that whereas an estimated 42% of participants in the control group say they would feel uncomfortable sharing food or water with a herder, an estimated 35% of participants who were exposed to

[34]For the first list experiment question, this resulted in dropping 531 participants. For the second, it resulted in dropping 134. The results reported in Tables D1 and D2 are unchanged when we extend or reduce the time cut-offs by 15 seconds.

[35]We did not ask the second list experiment question directly, so the discussion here is only possible with the first.

[36]We reach the same conclusion if we define "uncomfortable" more stringently as the share of respondents who gave responses of 3 or lower in the directly-asked version of the question. Defined this way, 66 percent say they would feel uncomfortable sharing food or water with a herder, still significantly higher than the share indicating in the list experiment that they would feel uncomfortable.

the treatment video indicate that they feel this way—a difference that is not statistically significant, even as it is directionally in keeping with the findings reported elsewhere in the paper.

In the second list experiment, we find that an estimated 63% of participants in the control group indicate that they think the government should arrest all herders and send them back to their home area, compared to 57% in the treatment group (see Table D2). This difference is, again, not statistically significant, although the direction of the difference is, again, in line with the findings from the directly-asked questions.

Table D2: Number of actions the federal and state governments should take against herders

	Exposed to video treatment	
	Yes	No
List experiment group 1	2.55	2.52
List experiment group 2	3.12	3.15
Difference	0.57	0.63

Note: N=1,862. Participants who completed this list experiment question in less than 60 seconds are excluded. Statistical significance of the difference: *p<0.1; **p<0.05; ***p<0.01.

E Follow-up phone survey

E.1 Sampling

To test for longer-term impacts of exposure to the treatment video, we recontacted a sample of study participants by phone roughly four months after the main study. We asked participants during the consent process for the study if they would be willing to be recontacted, and we collected the first names and phone numbers of those who agreed. We drew a random sample of 1,500 participants from this population, leaving aside cases where the respondent's interview length was less than one SD below the mean interview duration and where the respondent did not have what appeared to be a valid phone number.

In drawing the sample, we matched the respondents' language abilities with those of our five enumerators, yielding 900 Tiv-speakers and 600 English/Pidgin-speakers among the respondents to be recontacted. We then distributed the names and contact information from this list to our enumerators, who ultimately contacted and completed surveys for 1,051 study participants. After dropping observations with duplicated and invalid respondent IDs, the sample was reduced to 1,011. Applying the inclusion criteria used in the main analyses further narrowed the sample to 895 respondents.

E.2 Modification of the outcome indices

Table E1 provides an accounting of the outcome measures collected in the main post-treatment survey that were also asked in the follow-up survey.

Table E1: Outcome index components included in follow-up phone survey

	Included in follow-up phone survey?
Compassion measures	
Compassion for herders (1-10 scale)	Yes
Appropriate penalty for herder who trespasses...	
...without damage	No
...and damages crops	No
...and commits act of violence against family	No
Support for arresting all herders and forcing them to go back to their home area	No
Social distance measures	
How comfortable would you be...	
...sharing food or water with a herder	Yes
...sharing space in home with a herder for a few nights	Yes
...if someone in family married a herder	No
...visiting market if several herders were present	Yes
...if a herder family were admitted into the camp	Yes
How much shared in common with herders[c]	Yes
Negative stereotype measures	
Herders are...	
...generous and caring	Yes
...selfish and arrogant	Yes
...threatening	Yes
...hard workers	No
...criminals	Yes
...intelligent	No
...militants	No
...all the same	No
Share of herders thought to be militant	No
Trust measures	
How much would you trust a herder to...	
...graze their cattle on your stubble after harvest	Yes
...have their cattle access your water supply	Yes
...honor an agreement	Yes
...repay a small loan	Yes
...supply wara for the camp	Yes
Policy support measures	
Support for RUGA policy...	Yes
How much support should government provide?	Yes

E.3 Results including covariates

Table E2: Long term impacts, including covariates, exact match

	Compassion		Social distance		Negative stereotype		Trust		Support policies	
	post-exposure	follow-up	post-exposure	follow-up	post-exposure	follow-up	post-exposure	follow-up	post-exposure	follow-up
Treatment	0.673***	0.159	-0.208**	-0.120	-0.136	-0.007	0.278***	-0.013	0.335***	-0.149
	(0.136)	(0.123)	(0.105)	(0.103)	(0.094)	(0.106)	(0.091)	(0.084)	(0.104)	(0.114)
Female	-0.120	0.061	0.190	0.113	0.071	0.018	-0.102	-0.061	-0.077	-0.014
	(0.168)	(0.146)	(0.130)	(0.123)	(0.117)	(0.126)	(0.113)	(0.100)	(0.129)	(0.135)
Age	-0.004	-0.001	0.002	-0.001	0.004	-0.003	-0.004	-0.002	-0.002	-0.002
	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Catholic	-0.494	0.610	0.510	-0.452	-0.763	-1.085	0.807	0.778	0.063	1.603**
	(1.001)	(0.860)	(0.772)	(0.732)	(0.696)	(0.752)	(0.674)	(0.596)	(0.769)	(0.806)
Other Christian	-0.182	0.786	0.355	-0.614	-0.805	-1.224	0.837	0.893	0.120	1.801**
	(1.005)	(0.878)	(0.776)	(0.746)	(0.699)	(0.767)	(0.677)	(0.608)	(0.772)	(0.822)
Pentecostal	-0.531	0.810	0.546	-0.584	-0.765	-1.125	0.820	0.975	-0.017	1.497*
	(1.019)	(0.869)	(0.786)	(0.739)	(0.708)	(0.759)	(0.686)	(0.602)	(0.783)	(0.815)
Traditionalist	-0.769	0.415	1.097	0.008	-1.281	-0.633	0.643	0.798	0.222	1.496
	(1.157)	(0.999)	(0.893)	(0.849)	(0.805)	(0.873)	(0.779)	(0.692)	(0.889)	(0.936)
Education	-0.036	0.036	0.019	-0.030	0.009	-0.033	-0.009	-0.001	-0.057	0.028
	(0.046)	(0.045)	(0.035)	(0.038)	(0.032)	(0.039)	(0.031)	(0.031)	(0.035)	(0.042)
N	217	216	218	218	218	218	218	218	218	216

Note: Outcomes for all estimates are calculated using only the components that were collected in both the main and follow-up surveys. Estimates include camp and enumerator fixed effects. Religion coefficients are relative to the omitted category (Anglican). *p<0.1; **p<0.05; ***p<0.01.

Table E3: Long term impacts, including covariates, imperfect match 1

	Compassion		Social distance		Negative stereotype		Trust		Support policies	
	post-exposure	follow-up	post-exposure	follow-up	post-exposure	follow-up	post-exposure	follow-up	post-exposure	follow-up
Treatment	0.485***	0.192**	-0.257***	-0.111	-0.199**	-0.097	0.246***	0.082	0.359***	0.014
	(0.100)	(0.091)	(0.080)	(0.079)	(0.079)	(0.083)	(0.073)	(0.064)	(0.081)	(0.086)
Female	-0.145	0.067	0.059	0.120	0.093	0.084	0.012	-0.068	-0.035	0.056
	(0.123)	(0.109)	(0.099)	(0.094)	(0.097)	(0.099)	(0.089)	(0.077)	(0.099)	(0.103)
Age	-0.007	-0.001	0.003	0.002	0.004	0.000	-0.000	-0.004	-0.003	-0.003
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
Catholic	-0.269	0.705	0.465	-0.607	-0.798	-1.239*	0.654	0.851	0.277	1.654**
	(0.914)	(0.809)	(0.730)	(0.705)	(0.718)	(0.741)	(0.662)	(0.572)	(0.736)	(0.769)
Other Christian	-0.026	0.872	0.444	-0.750	-0.760	-1.423*	0.675	0.910	0.210	1.679**
	(0.919)	(0.821)	(0.734)	(0.716)	(0.722)	(0.752)	(0.666)	(0.580)	(0.739)	(0.781)
Pentecostal	-0.226	0.933	0.479	-0.813	-0.772	-1.266*	0.642	1.064*	0.273	1.648**
	(0.925)	(0.816)	(0.739)	(0.711)	(0.727)	(0.747)	(0.671)	(0.577)	(0.745)	(0.776)
Traditionalist	-0.464	0.470	0.873	-0.191	-1.237	-0.756	0.617	0.881	0.535	1.670*
	(1.059)	(0.939)	(0.846)	(0.818)	(0.832)	(0.860)	(0.767)	(0.663)	(0.852)	(0.893)
Education	-0.039	0.039	-0.017	-0.040	0.002	-0.036	0.022	0.002	-0.020	0.052*
	(0.034)	(0.033)	(0.027)	(0.029)	(0.027)	(0.030)	(0.025)	(0.023)	(0.028)	(0.031)
N	327	326	328	328	328	328	328	328	328	326

Note: Outcomes for all estimates are calculated using only the components that were collected in both the main and follow-up surveys. Estimates include camp and enumerator fixed effects. Religion coefficients are relative to the omitted category (Anglican). *p<0.1; **p<0.05; ***p<0.01.

Table E4: Long term impacts, including covariates, imperfect match 2

	Compassion		Social distance		Negative stereotype		Trust		Support policies	
	post-exposure	follow-up	post-exposure	follow-up	post-exposure	follow-up	post-exposure	follow-up	post-exposure	follow-up
Treatment	0.577***	0.118**	-0.279***	-0.017	-0.185***	0.001	0.244***	-0.000	0.290***	-0.015
	(0.072)	(0.060)	(0.053)	(0.051)	(0.050)	(0.051)	(0.048)	(0.041)	(0.050)	(0.054)
Female	-0.064	0.016	0.196***	0.142**	0.116*	0.113*	-0.128**	-0.054	-0.106*	-0.008
	(0.085)	(0.070)	(0.063)	(0.060)	(0.059)	(0.061)	(0.057)	(0.048)	(0.059)	(0.064)
Age	-0.005**	-0.003	0.004*	-0.000	0.003*	0.000	-0.003	-0.002	-0.005***	-0.003
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Catholic	-0.235	0.241	0.102	-0.488	-0.069	-0.107	0.118	0.050	-0.372	0.488
	(0.486)	(0.402)	(0.360)	(0.345)	(0.341)	(0.348)	(0.325)	(0.277)	(0.340)	(0.364)
Other Christian	-0.026	0.408	0.011	-0.569	-0.134	-0.185	0.133	0.039	-0.428	0.524
	(0.495)	(0.411)	(0.367)	(0.352)	(0.347)	(0.355)	(0.331)	(0.283)	(0.345)	(0.372)
Pentecostal	-0.240	0.441	0.040	-0.632*	-0.016	0.013	0.135	0.135	-0.411	0.429
	(0.492)	(0.406)	(0.365)	(0.348)	(0.345)	(0.351)	(0.329)	(0.279)	(0.343)	(0.368)
Traditionalist	-0.425	0.083	0.290	-0.049	-0.307	0.434	-0.078	0.166	-0.155	0.395
	(0.685)	(0.566)	(0.508)	(0.485)	(0.480)	(0.489)	(0.459)	(0.390)	(0.479)	(0.513)
Unaffiliated	0.483	0.806	0.259	-0.403	0.139	0.268	0.327	-0.244	-0.471	0.456
	(0.740)	(0.612)	(0.549)	(0.524)	(0.519)	(0.529)	(0.496)	(0.421)	(0.517)	(0.554)
Education	0.009	0.012	-0.028	0.003	0.009	-0.004	0.013	-0.008	-0.015	0.025
	(0.024)	(0.022)	(0.018)	(0.019)	(0.017)	(0.019)	(0.016)	(0.015)	(0.017)	(0.020)
N	740	742	745	745	745	745	745	745	745	741

Note: Outcomes for all estimates are calculated using only the components that were collected in both the main and follow-up surveys. Estimates include camp and enumerator fixed effects. Religion coefficients are relative to the omitted category (Anglican). *p<0.1; **p<0.05; ***p<0.01.

F Deviations from the pre-analysis plan

This appendix reports deviations from the pre-analysis plan.

- We pre-registered the intention to measure perceived social distance with herders via an index built from questions 7-11 of the post-treatment portion of the questionnaire and, separately, via a question about "how much do you share with herders?" We decided to include this last question as part of the index rather than treat it separately. Table F1 replicates the social distance index values from Appendix Table A2 (corresponding to Figure 4 in the main paper), dropping the "how much do you share with herders?" component, as originally pre-registered.

Table F1: Impact of modifying the perceived social distance index

	Results from Figure 4		As pre-registered	
	(1)	(2)	(3)	(4)
Treatment	-0.223***	-0.203***	-0.228***	-0.212***
	(0.037)	(0.032)	(0.038)	(0.034)
Controls	No	Yes	No	Yes
N	1996	1994	1996	1994

Note: Columns 2 and 4 also include camp and enumerator fixed effects. *p<0.1; **p<0.05; ***p<0.01.

- We pre-registered the intention to measure negative stereotypes toward herders via an index built from questions 13-20 of the post-treatment portion of the questionnaire. We later realized that an additional post-treatment question, about the share of herders that the respondent thinks

are militant, also captures negative stereotypes and decided to add this component to the index. Table F2 replicates the negative stereotype index values from Appendix Table A2, dropping this additional component.

Table F2: Impact of modifying the negative stereotype index

	Results from Figure 4		As pre-registered	
	(1)	(2)	(3)	(4)
Treatment	-0.160***	-0.151***	-0.156***	-0.149***
	(0.027)	(0.023)	(0.027)	(0.023)
Controls	No	Yes	No	Yes
N	1996	1994	1996	1994

Note: Columns 2 and 4 also include camp and enumerator fixed effects. *p<0.1; **p<0.05; ***p<0.01.

- In examining the conditional effect of prior exposure to violence on treatment impacts, we pre-registered the intention to measure such exposure "through the most proximate level of violence exposure (i.e., personally, by immediate family, by community) gleaned from responses to questions 17-19 in the pre-treatment portion of the questionnaire." In the event, there was very little variation in exposure to violence among our respondents: 98.9% reported having experienced violence directed at members of their community and 91% reported having experienced violence directed at their immediate family members. The only measure of violence exposure that provided any significant variation was violence directed at the respondent personally (which was claimed by 76% of respondents). We therefore limited our measure of exposure to violence to personal exposure for the analysis of heterogeneous treatment effects.

- The main manipulation check we report in the paper is based on a question asking whether farmers think the herders came onto their lands due to forces outside of their control. We also pre-specified a second cut at testing the impact of updating priors, based on a pre-treatment question about why the farmers thought the herders came to their area (response options were: to grab their land, because their grazing lands could no longer support their cattle, for religious reasons/jihad, or because they were foreign terrorists). We hypothesized that treatment impacts might be greater among those who, prior to exposure to the video, provided an answer other than "because their grazing lands could no longer support their cattle," as it was possible for such respondents to update their views after exposure to the video. However, upon further reflection we realized that farmers who volunteered other reasons for why the herders came onto their lands (to grab their land, for jihad, etc) were probably predisposed *not* to be affected by exposure to the treatment video, which would work against our hypothesized conditional effect. Also, unlike the

question about whether the herders were affected by forces outside of their control, we did not ask this question post-treatment, which makes it challenging to use it to learn about updated priors. For both of these reasons we elected not to report the results of this pre-registered manipulation check. In the event, we find no conditional effects based on responses to this pre-treatment question.

- We pre-registered the intention to cluster standard errors in all of our analyses at the level of the IDP camp. Upon further reflection, we determined that this was not the correct approach, as the unit of randomization is the individual rather than the camp. We therefore departed from our pre-registered specification by dropping the clustering of standard errors by camp. The findings are unchanged.