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Nationality, Tribe and Trade: An Experiment on Intergroup Discrimination at the Sierra Leone–Guinea Border

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Abstract
Farmer–herder conflicts across West Africa are conventionally attributed to competition over land and water. Yet, identity – ethnic difference and outsider status – features prominently in qualitative accounts and in the peacebuilding programming that targets these conflicts. Because occupation and identity overlap closely in the field, observational data cannot easily distinguish these accounts. We test this experimentally, using a pre-registered vignette experiment among 1,765 farming households located at the Sierra Leone–Guinea border. We randomize the counterpart's identity while holding their occupation and the economic terms of an everyday exchange fixed. We find clear evidence of group-based discrimination: over a third of respondents always buy from their economic ingroup, even when doing so would mean paying more in the purchase scenario. However, this discrimination is not widened or heightened by the ethnic or national identity of the counterpart. Neither nationality nor tribe cues move purchase behavior or stated social acceptance: estimates are close to zero, and equivalence tests rule out effects at the lower bound of those documented in the intergroup-bias literature. The results suggest that the economic identity divide between farmers and herders is more salient than ethnic or national identity, with implications for where social-cohesion programming is best directed.

JEL Classifications
D74, J15, C93, D91, Z13

Keywords
vignette experiment; taste-based discrimination; ethnic identity; farmer–herder conflict; intergroup bias; Sierra Leone

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1 Introduction

Farmer–herder conflict is among the most widespread and deadly forms of communal violence in West Africa (Brottem, 2020; McGuirk & Nunn, 2025). Climate pressure, expanding cultivation, and shifting transhumance routes bring the two groups into increasingly frequent contact, raising competition over land, water, and the costs of crop damage (Hussein, Sumberg, & Seddon, 1999; McGuirk & Nunn, 2025). Political-ecology accounts frame this as fundamentally distributional: conflict is driven by resource scarcity and tenure insecurity, and group boundaries mainly mark who competes with whom (Benjaminsen, Alinon, Buhaug, & Buseth, 2012; Brottem, 2020; Turner, 2004). The toll can be severe: in Nigeria, farmer–herder violence was estimated to have killed more people in 2018 than the Boko Haram insurgency (International Crisis Group, 2018).

A second line of thought argues that identity, as well as or even separately from resource competition, drives intergroup tensions between farmers and herders. Herders across West Africa, predominantly Fullah/Fulani in origin, are frequently perceived as ethnic outsiders whose presence challenges farmers' claims to land and belonging (Olaniyan, Francis, & Okeke-Uzodike, 2015; Tonah, 2003). Qualitative work argues that the identity of the herder shapes conflict beyond what resource competition alone explains: it can intensify disputes, harden group boundaries, and sustain conflict in ways that resource accounts alone cannot explain (Adibe, 2020; Seter, Theisen, & Schilling, 2018). Both arguments can be simultaneously true, but they generate different predictions about what sustains everyday intergroup hostility and different prescriptions for addressing it.

Distinguishing between these mechanisms matters both theoretically and for policy, because the two arguments predict different sources of everyday tension and call for different responses. On the policy side: peacebuilding responses to farmer–herder conflict devote substantial resources to social-cohesion programming, including intergroup contact, dialogue, and peace messaging (Dawop, Grady, Inks, & Wolfe, 2019; Paluck, 2009). These work on the assumption that identity-based bias is present between the groups and operates independently of the economic and social tensions between them. Yet, the debate is hard to settle with observational data, because occupation and identity overlap closely. The herder a farmer encounters is historically almost always an ethnic and/or national outsider as well, such that identifying the identity channel requires holding the economic situation fixed while varying group membership.

We test the presence of group-based biases experimentally. In a pre-registered vignette experiment embedded in a survey of 1,765 farming households in the Sierra Leone–Guinea borderlands, respondents considered an everyday market exchange with a herder. We randomize his nationality (Sierra Leonean vs. Guinean) and tribe (Koranko, Limba, Yalunka, or Fullah), holding his occupation and the economic terms of the exchange fixed. We measure willingness to trade at a price premium and a set of stated social-acceptance outcomes.

We find clear evidence of group-based bias: over a third of respondents always buy from their economic ingroup, even when doing so would mean paying more in the purchase scenario. What the randomized cues show is that this bias is not amplified by the ethnic or national identity of the counterpart. Neither nationality nor tribe moves purchase behavior or stated social acceptance: effects are close to zero, and equivalence tests rule out effects at the lower bound of those documented in the intergroup-bias literature (Balliet, Wu, & De Dreu, 2014; Goette, Huffman, & Meier, 2012). These results join a small but growing body of field-experimental evidence showing that ethnicity often fails to produce preference-based discrimination on top of existing group boundaries (Barriga, Ferguson, Fiala, & Leroch, 2023; Berge et al., 2020; Habyarimana, Humphreys, Posner, & Weinstein, 2007). For the farmer–herder debate, they suggest that the economic identity divide is more salient than ethnic or national identity, with implications for where social-cohesion programming is best directed.

2 Setting

The study took place in Falaba District in northern Sierra Leone, along the country's border with Guinea. The 25 study communities are located in Mongo and Sulima chiefdoms, in a sparsely populated rural area where household livelihoods depend heavily on agriculture. Cross-border transhumance, whereby herders seasonally cross borders with their livestock in search of water and pasture, brings mobile cattle owners and their herds into farming areas, creating regular contact between livelihood groups that use the same land in different ways (International Organization for Migration, 2024). Livestock can damage cultivated fields, while cattle owners depend on access to grazing land, water, and movement corridors. Similar patterns occur across West Africa, where rainfall shocks and seasonal herd movements bring pastoralists into cultivated areas and where crop

damage and access to land and water are recurrent sources of disagreement (Hussein et al., 1999; McGuirk & Nunn, 2025). Figure 1 situates Falaba District within Sierra Leone and maps the study communities along the international border.

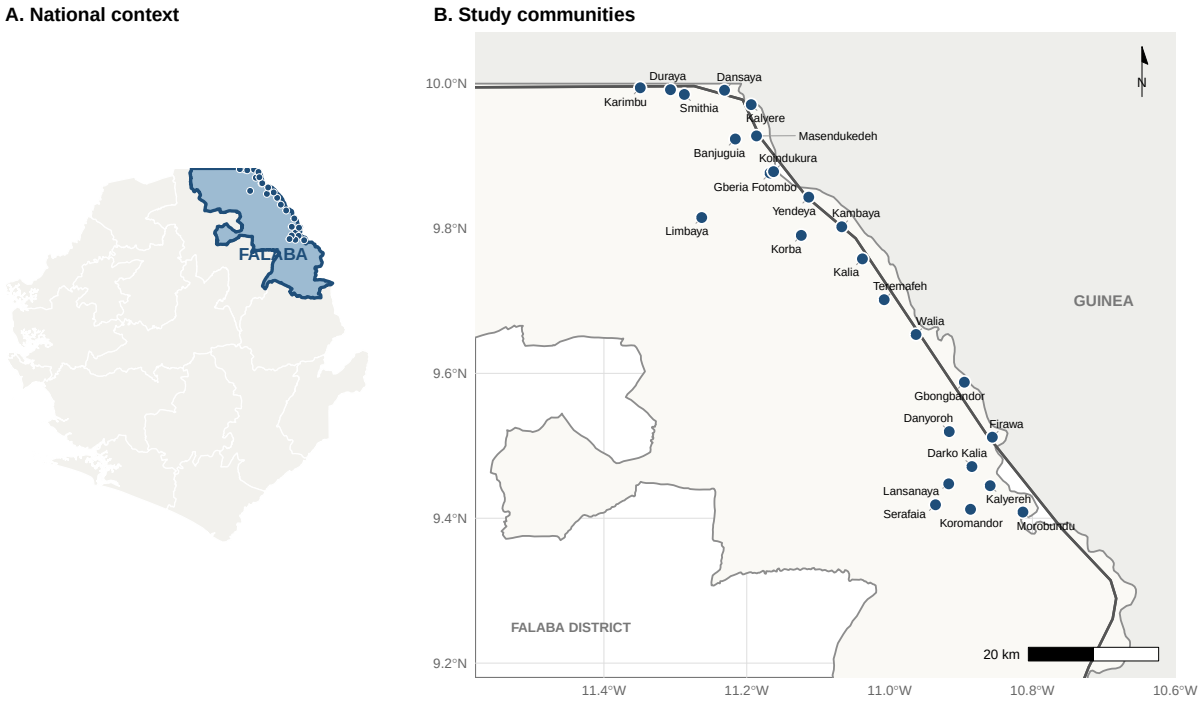

Figure 1: Study communities in Falaba District, Sierra Leone

Notes: Panel A shows Sierra Leone's district boundaries, with Falaba District highlighted. Panel B maps the 25 surveyed communities in Mongo and Sulima chiefdoms along the Sierra Leone–Guinea border. Community locations are the median household GPS coordinates recorded during fieldwork in May–June 2024. Sierra Leone's district boundaries are from the Government of Sierra Leone's Common Operational Dataset for Administrative Boundaries, distributed by OCHA through the Humanitarian Data Exchange (COD-AB v02, valid 15 December 2023; Government of Sierra Leone, 2015). Country outlines and international borders are from Natural Earth's 1:50m Admin 0–Countries data (Natural Earth, 2026).

The study communities lie on the Sierra Leonean side of the border, but the border is regularly crossed as part of seasonal livestock movement. Participatory mapping by IOM identifies routes connecting Falaba District with Faranah Prefecture in Guinea that cattle herders use to enter Sierra Leone with their herds (International Organization for Migration, 2022). Cattle owners encountered in the area may therefore be Sierra Leonean or Guinean. Nationality, tribe, and livelihood overlap without mapping perfectly onto one another. Across West Africa, pastoral livelihood, Fulbe/Fullah identity, and outsider status are often associated, although their relationship and political meaning vary across settings (Olaniyan et al., 2015; Seter et al., 2018; Tonah, 2003). In the study sample, 70% of respondents are Yalunka, 21% Koranko, and 5% Fullah; 99% are Muslim (Appendix B).

The presence of Fullah farmers in the sample illustrates that tribe and livelihood are not equivalent in this population.

Farmer–herder relations in the study area include both routine interaction and disputes. Just over one third of respondents from farming households (hereafter, "farmer respondents"; 36%) reported having personally experienced a dispute with a herder (Appendix B). Available assessments characterize conflict in the area as recurring local disputes rather than organized large-scale farmer–herder violence (United Nations Peacebuilding Fund, 2024; United Nations Sierra Leone, 2022).

3 Experimental Design and Data

Sample and survey administration

The vignette experiment was embedded in a household survey fielded face-to-face by trained enumerators, principally in Krio, using SurveyCTO in May–June 2024.[1] Rather than sample households within villages, the survey sought to enumerate every household across the 25 study communities. Within each household, the survey software randomly targeted a male or female adult representative; the vignette module was administered in households reporting engagement in farming. Ten communities had previously participated in a two-and-a-half-year UN Peacebuilding Fund–supported project, while 15 comparison communities were selected from the same two chiefdoms based on remoteness, border proximity, exposure to transhumance and farmer–herder disputes, village size, radio access, and the presence of a primary school. The vignette itself was separately pre-registered and randomized at the individual level.[2]

The resulting analysis sample comprises all $N = 1{,}765$ respondents from farming households who received the module. Respondents were, on average, 40 years old and 45% were women. Appendix B reports the complete sample descriptives, nonresponse, and balance across the experimental arms; the full English instrument and realized assignment counts appear in Appendix A.

[1] All respondents provided informed consent; ethical approval was granted by the Life Sciences Faculty at Humboldt University of Berlin (reference 2024-03-Guzman).

[2] A separate randomized radio-audio module preceded the vignette. Vignette assignment was independent of both audio assignment and prior community programme status; the estimates therefore average over these exposures.

Vignette and randomization

Respondents heard a standardized story about buying cake from two adjacent market stalls. The first stall is run by the wife of a farmer from the respondent's own tribe in Sierra Leone. The second is run by the wife of a *cattle owner*, whose nationality and tribe are independently randomized in a 2×4 factorial design. Nationality is described as Sierra Leonean ("from here in Sierra Leone") or Guinean ("from across the border in Guinea"). Tribe is described as Koranko, Limba, Yalunka, or Fullah and is signaled both explicitly and through the vendor's name.[3] Each respondent heard one of the eight resulting versions. Assignment occurred at the individual level through the survey software, with equal probability in each cell; realized cell sizes are approximately balanced.[4]

The second counterpart's occupation, the goods and prices under discussion, and all other story elements are held constant across the eight arms. Accordingly, the randomized contrasts identify the marginal effects of the counterpart's nationality and tribe conditional on the counterpart being a cattle owner. They do not identify the effect of cattle ownership itself, because the design contains no arm in which the second counterpart is a farmer. In 23% of assignments the counterpart's randomized tribe matches the respondent's own tribe; Appendix F reports additional analyses excluding these cases.

Figure 2 summarizes the study design, showing the factorial assignment, the elements held constant, and the two outcome families.

Outcomes and estimation

We measure two outcome families. First, a *purchase threshold*: told that the own-tribe stall is more expensive, respondents state when they would buy from the second stall using a 4-point scale ranging from "I would always buy from the person in my tribe" to "I would always buy the cheapest cake" (higher values = less discrimination). Motivated by the framework of Becker (1957), the measure captures respondents' stated willingness to pay more to avoid purchasing from the second stall.[5] Second, *social acceptance*: agreement with five statements about the cattle

[3] Enumerators read the description aloud while showing a tablet image of a cattle owner bearing the assigned country's flag. The aid helped sustain respondent engagement and reinforce the cues without relying on literacy; tribe was conveyed orally through the explicit label and name.

[4] Appendix A reports the full vignette wording and realized arm counts.

[5] The vignette task itself was not incentivized: respondents' purchase and acceptance answers carried no monetary consequence. Participants were compensated for the broader survey as a whole, which included incentivized behavioral

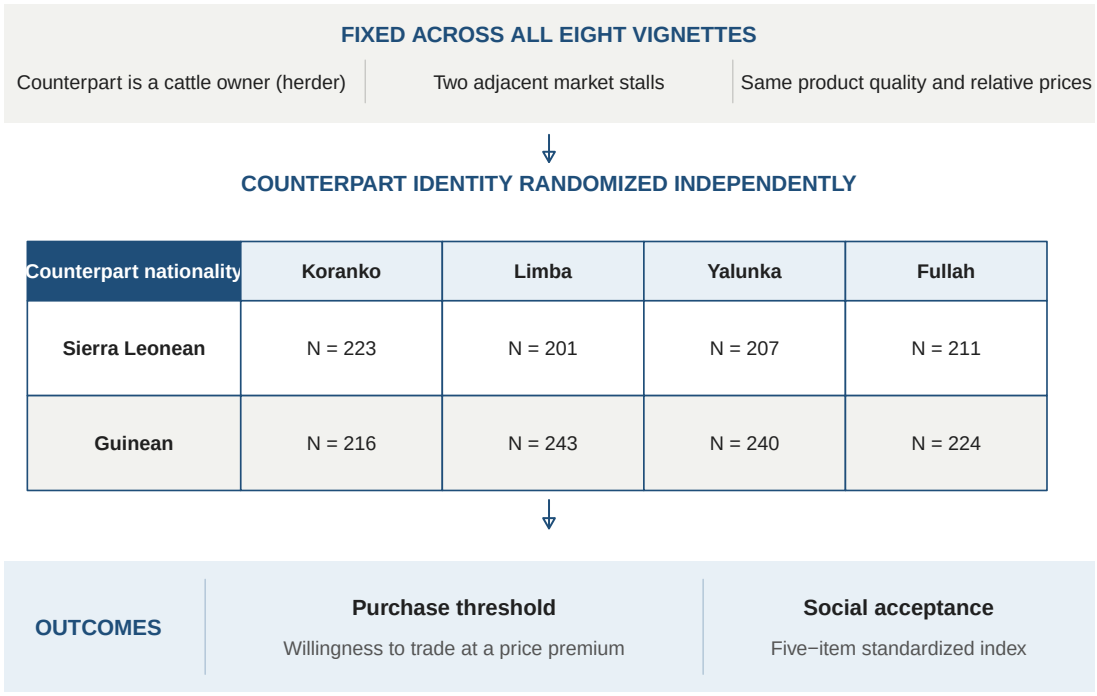

Figure 2: Experimental design and outcome families

Notes: The counterpart's nationality and tribe were independently randomized at the respondent level. Cell entries report realized assignment counts (N = 1,765). The counterpart's occupation and all other vignette elements were held constant across arms.

owner covering willingness to work with him, lend him money, have him as a neighbour, welcome him marrying one's child, and attend his social gathering (all on 4-point scales; higher = more accepting).[6] The items follow the Bogardus social distance scale (Bogardus, 1933; Mousa, 2020), adapted to the local context, and are summarized in a standardized index.[7]

We first report the descriptive distribution of choices in the two-stall scenario. These distributions are not experimental estimates of the effect of occupation. The causal quantities of interest are the intention-to-treat effects of the randomized nationality and tribe cues. We estimate them by OLS, regressing each outcome on a Guinean indicator and indicators for Limba, Yalunka, and Fullah counterparts, with Sierra Leonean and Koranko as the reference categories. Because the two factors were independently randomized, these coefficients estimate each cue's marginal effect averaged over the other factor; fully interacted estimates appear in Table 5. Adjusted models include the pre-specified covariates: age, gender, respondent tribe, religion, literacy, radio ownership, and prior farmer–herder dispute exposure.[8] Standard errors are clustered by community.[9]

4 Results

Do farmers discriminate at all?

Before turning to treatment effects, we assess whether our outcome families left room for identity cues to matter. Figure 3, Panel A, shows that purchase behavior is starkly bimodal: 59.8% of respondents report they would always buy from the cheaper stall regardless of the seller's identity, while 37.8% would always buy from their ingroup seller regardless of price; almost no one (fewer than 3%) occupies the intermediate categories, in which a price discount would induce switching. This pattern has two implications. First, a substantial minority of farmers do discriminate – categorically, even when doing so would mean paying more in the purchase scenario – demonstrating

games whose payouts varied with game outcomes; the average payout was 33.6 SLE, approximately US$5.55 (PPP-adjusted using the 2024 World Bank International Comparison Program conversion factor for Sierra Leone, 6.06 SLE = 1 international $; World Bank, 2024). Payment was unrelated to responses on the vignette module.

[6]The purchase decision is formally between stalls operated by the two men's wives, with the husbands' livelihood and identity describing the respective households; the acceptance items refer directly to the cattle owner.

[7]The index is the mean of the five items, each on the full 0–3 scale, standardized to mean zero and unit variance. Appendix A reports the full instrument wording; Appendix C details the construction of all outcomes and covariates.

[8]The pre-analysis plan was filed with the AEA RCT Registry prior to data collection (AEARCTR-0013396) and is available on request.

[9]There are 25 community clusters. Appendix F reports randomization-inference and wild-cluster-bootstrap p-values for the headline tests.

that group identity is relevant to respondents' stated choices in the purchase scenario. Second, because more than a third of respondents sit at the discriminatory pole, there was considerable scope for randomized identity cues to move behavior.

Social acceptance of the vignette counterpart is high, but far from saturated (Figure 3, Panel B). Agreement ranges from 68% for working together to 85% for attending a social gathering, with strong agreement between 47% and 64%. Disagreement of 15–32% across items likewise leaves room for identity cues to depress, or raise, stated acceptance. Notably, the most intimate item (a marriage tie) draws *more* agreement than the most quotidian one (working together), suggesting responses reflect genuine social judgments rather than uniform acquiescence.

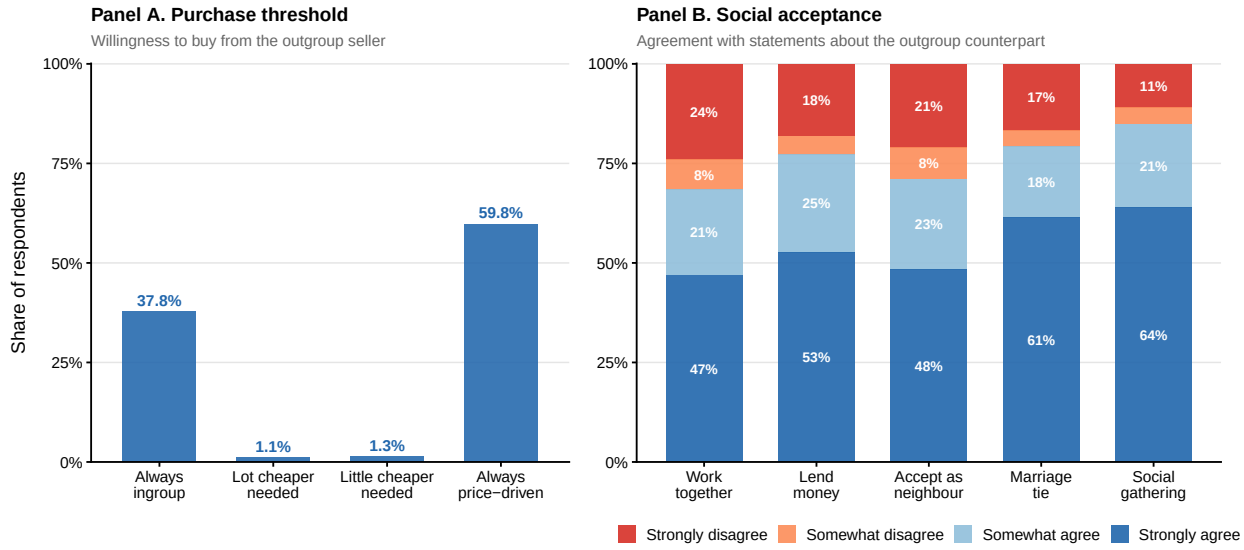

Figure 3: Distributions of the two outcome families

Notes: Panel A shows responses to a hypothetical purchase decision between an ingroup seller and the outgroup seller described in the vignette: "Always price-driven" indicates the respondent always buys from the cheaper stall regardless of the seller's identity; "Always ingroup" indicates the respondent always buys from the ingroup seller regardless of price. Panel B shows agreement with five statements expressing social acceptance of the vignette counterpart, measured on a four-point scale. Higher values indicate less discrimination (Panel A) and greater acceptance (Panel B) throughout. $N = 1{,}742$ farmer respondents (23 of 1,765 missing on the purchase outcome).

Identity cues do not move behavior or acceptance

Figure 4 plots the effects of the randomized nationality and tribe cues on the three headline outcomes, with and without pre-specified controls; Table 1 reports the same estimates in full, with standard errors, reference-arm means, sample sizes, and model fit. As shown, every coefficient is small and statistically indistinguishable from zero. Describing the counterpart as Guinean rather

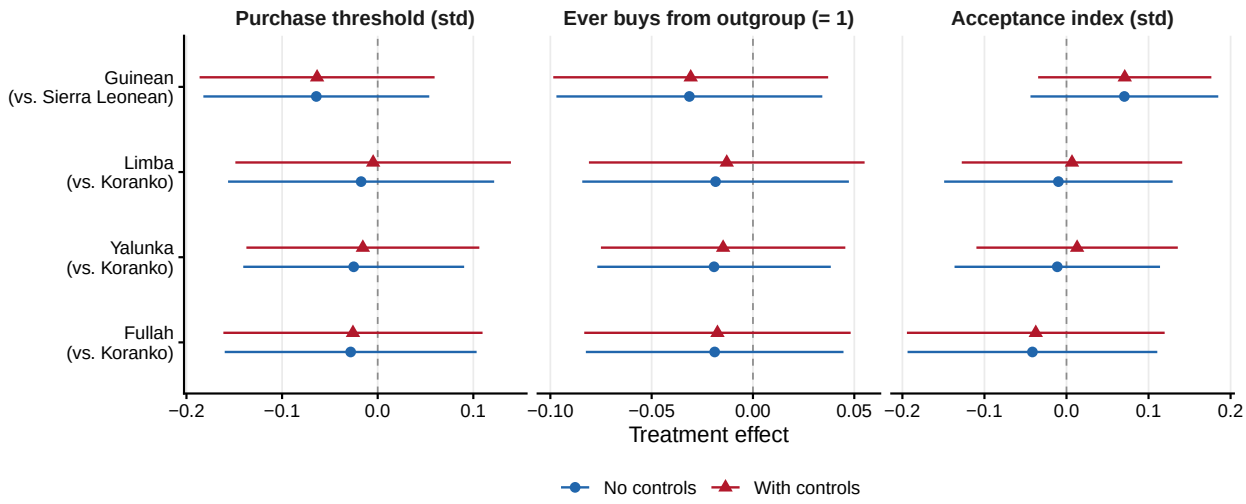

Figure 4: Effects of vignette nationality and tribe on the headline outcomes

Notes: OLS coefficients with 95% confidence intervals; standard errors clustered by community (N = 1,741–1,764, 25 clusters). Outcomes are coded so that positive coefficients indicate less discrimination or greater acceptance; the purchase threshold and acceptance index are standardized, and "ever buys from outgroup" is an indicator for not always buying from the ingroup stall. Tribe effects are relative to Koranko. Controls (pre-specified): age, gender, respondent tribe, religion, literacy, radio ownership, and farmer–herder dispute exposure. Full estimates in Table 1; Romano–Wolf adjusted p-values in Table 6.

than Sierra Leonean shifts the purchase threshold by −0.06 standard deviations (p = .30), the probability of ever buying from the outgroup by −3.1 percentage points (p = .36), and the acceptance index by +0.07 standard deviations (p = .18). Tribe effects are even smaller: no contrast with the Koranko reference exceeds 0.04 standard deviations in absolute value on any outcome (all $p \geq .59$). The nulls are visible in the raw data, with outcome distributions being nearly identical across counterpart-nationality arms (Appendix Figure 5).

The null results are robust to multiple-testing correction and interaction specifications. Under Romano–Wolf step-down corrections treating all twelve treatment–outcome tests as one family, adjusted p-values range from .49 to .99 (Table 6). Allowing nationality effects to vary by tribe yields no interpretable heterogeneity for the purchase outcomes: joint tests of the interactions are insignificant (p = .64 and .66). For the acceptance index, the joint test rejects the null that the interaction coefficients are jointly zero (p = .003, Table 5), driven by the Guinean × Limba contrast (+0.26 SD, p = .02), while the Guinean × Fullah contrast is negative (−0.14 SD, p = .44). The interactions point in opposite directions and do not align with a clear theoretical pattern: acceptance is higher for Guinean Limba counterparts but not for Guinean Fullah counterparts, the

Table 1: Main effects of vignette nationality and tribe

	Purchase threshold (std)		Ever buys outgroup		Acceptance index (std)	
	(1)	(2)	(3)	(4)	(5)	(6)
Guinean (vs. Sierra Leonean)	-0.064	-0.063	-0.031	-0.031	0.071	0.071
	(0.057)	(0.060)	(0.032)	(0.033)	(0.055)	(0.051)
Limba (vs. Koranko)	-0.017	-0.005	-0.018	-0.013	-0.010	0.007
	(0.067)	(0.070)	(0.032)	(0.033)	(0.067)	(0.065)
Yalunka (vs. Koranko)	-0.025	-0.016	-0.019	-0.015	-0.011	0.013
	(0.056)	(0.059)	(0.028)	(0.029)	(0.061)	(0.059)
Fullah (vs. Koranko)	-0.028	-0.026	-0.019	-0.018	-0.042	-0.037
	(0.064)	(0.066)	(0.031)	(0.032)	(0.074)	(0.076)
PAP controls	No	Yes	No	Yes	No	Yes
Reference-arm mean	0.092	0.092	0.673	0.673	0.036	0.036
Observations	1,742	1,741	1,742	1,741	1,765	1,764
R^2	0.001	0.013	0.001	0.012	0.001	0.087

Notes: OLS estimates; standard errors clustered by community in parentheses. Outcomes are coded so that higher values indicate less discrimination or greater acceptance; the purchase threshold and acceptance index are standardized. "Ever buys outgroup" is an indicator for not always buying from the ingroup stall. PAP controls: age, gender, respondent tribe, religion, literacy, radio ownership, and farmer–herder dispute exposure. Reference-arm mean is the outcome mean in the Sierra Leonean × Koranko cell. Tribe coefficients relative to Koranko. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$ (no estimate reaches any threshold).

group most often characterized as ethnic outsiders in this literature. We therefore treat this finding as exploratory.

How informative are the nulls?

These estimates are precise, suggesting that these nulls are informative and not a reflection of limited statistical power. Across the randomized contrasts, the 80% minimum detectable effects range from 0.17 to 0.20 standard deviations for the purchase threshold, from 8 to 9 percentage points for the binary purchase outcome, and from 0.14 to 0.21 standard deviations for the acceptance index. These ranges are below benchmark effects in the intergroup-bias literature ($d \approx 0.32$ in Balliet et al., 2014's meta-analysis of ingroup favoritism in cooperation; 15–20 percentage points for cooperation with ingroup members in Goette et al., 2012). Further, equivalence tests reject effects larger than ± 0.20 standard deviations (± 10 percentage points for the binary outcome) for every treatment–outcome pair (all $p < .05$; Table 7 and Appendix E). In short: if identity cues moved economic or social behavior in this setting at the magnitudes the literature reports, we would have detected it. Results are unchanged when excluding respondents whose own tribe matches the

counterpart's, they hold item-by-item across all five acceptance statements, and are insensitive to the inclusion of community fixed effects (Appendix F).

5 Discussion

In a setting where over a third of respondents discriminate categorically against a herder counterpart, randomized nationality and tribe cues move neither purchase behavior nor social acceptance. The precision of these nulls rules out effects at the lower bound of the intergroup-bias literature. Discrimination is real but undifferentiated: respondents who favor the ingroup stall do so whoever the outgroup seller is. This is consistent with accounts in which everyday farmer–herder friction tracks the distributional relationship between the two groups rather than hostility toward specific groups (Moritz, 2010; Turner, 2004), and with field-experimental evidence that ethnic cues often do not produce taste-based discrimination in non-politicized settings (Berge et al., 2020; Habyarimana et al., 2007). This pattern is consistent with the economic boundary between farmer and herder being the salient one: it is along this divide that respondents discriminate, and layering ethnic or national difference on top of it does not change much. Respondents discriminate no less against a herder who shares their own tribe and nationality than against one who shares neither (Table 12).

We note three study limitations. First, the vignette measures stated intentions rather than realized transactions. This is a standard concern for survey-experimental designs, though it is partially addressed here by the internal consistency of the nulls. If hypothetical bias were inflating acceptance, we would expect weaker nulls on the more behaviorally proximate purchase outcome than on the social acceptance statements. The nulls are equally precise across both families. Second, the study sites had experienced farmer–herder tension but not active organized violence at the time of data collection. Ethnic identity cues may activate under conditions of recent violence or political mobilization of group boundaries (Barriga et al., 2023; Seter et al., 2018). Our results may not generalize to settings where the identity dimension of conflict has already been politicized. Third, the design holds occupation constant. This means we cannot identify whether the categorical ingroup favoritism we observe reflects farmer–herder hostility specifically, generic outgroup wariness, or the pull of own-community ties. Each of these has different implications for intervention.

The results have implications for how peacebuilding resources are allocated. Programmes that

target intergroup perceptions through contact, dialogue, and peace messaging (Dawop et al., 2019; Paluck, 2009) typically presume that ethnic or national bias sustains everyday friction. Our findings suggest the more salient cleavage is the economic one between farmers and herders. This does not imply that social-cohesion programming is misplaced, but suggests that it should attend to the divide around which everyday discrimination appears to be organized. Programming that engages with the economic relationship between farmers and herders – e.g., tenure security, crop-damage compensation, and access to transhumance corridors – may therefore be more effective than approaches focused primarily on reducing ethnic or national animosity. The identity dimensions relevant to a particular setting should be tested rather than assumed in programme design.

These findings point to a more conditional research agenda to understand the conditions under which latent group boundaries become behaviorally active. Ethnic and national identities may shape farmer–herder relations under conditions that activate those boundaries, including recent organized violence (Seter et al., 2018), electoral mobilization (Berge et al., 2020), and the institutional framing of conflict in identity terms (Rivera-Burgos, 2025). Future research should test whether these conditions, and the economic stakes of the interaction, alter the influence of identity cues. This would move the literature beyond asking whether resource competition or identity explains farmer–herder tensions toward identifying when each matters.

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A Vignette instrument and randomization

The vignette was administered face-to-face by enumerators in Krio as part of the broader household survey; Box A1 reports the English wording from the survey instrument. Each respondent heard the base story followed by exactly one of eight second-stall descriptions, assigned with equal probability by the survey software at the individual level. The two randomized elements are the counterpart's nationality and his tribe; tribe was signaled both explicitly and through a tribe-typical name for the vendor (Saio for Koranko, Yeanor for Limba, Jeneba for Yalunka, Neneh Binta for Fullah). The counterpart's occupation (cattle owner) and all other story elements are identical across arms. The purchase question followed immediately, then the five acceptance statements.

Box A1: Vignette script (English version)

Introduction. "I will now tell you a short story and ask you some questions about what you think."

Base story. "Suppose you are buying some cake from the market. There are two stalls in the market right next to each other. The FIRST stall is run by AMINATA, the wife of a farmer from your tribe here in Sierra Leone."

Randomized second stall. "The SECOND stall is run by [SAIO / YEANOR / JENEBA / NENEH BINTA], the wife of a [KORANKO / LIMBA / YALUNKA / FULLAH] cattle owner [from here in SIERRA LEONE / from across the border in GUINEA]."

Purchase outcome. "The cakes seem to be of the same quality, but Aminata's cake at the first stall is more expensive. At what point would you buy from the second stall instead?"

- I would always buy the cheapest cake.
- The cake would need to be a little bit cheaper.
- The cake would need to be a lot cheaper.
- I would always buy the cake from Stall 1 (the person in my tribe).

Acceptance outcomes. "Thinking about the cattle owner presented in this story, can you tell me the degree to which you agree or disagree with the following statements about this person?" (Completely agree / somewhat agree / somewhat disagree / completely disagree)

- I would feel comfortable working with him/her.
- I would lend her/him money.
- I would like having her/him as my neighbour.
- I would welcome him/her marrying my daughter/son.
- I would attend a gathering they organize with their friends if I was invited.

Table 2: Randomization arms and cell counts

Counterpart tribe	Sierra Leonean	Guinean	Total
Koranko	223	216	439
Limba	201	243	444
Yalunka	207	240	447
Fullah	211	224	435
Total	842	923	1,765

Notes: Number of respondents assigned to each of the eight vignette arms. Assignment probabilities were uniform across cells; realized counts are consistent with chance (χ^2 test against equal cell sizes: $\chi^2(7) = 7.15$, $p = .41$). The modest surplus of Guinean-arm assignments does not differ significantly from an even split ($p = .054$) and tribe assignment is independent of nationality assignment ($p = .36$).

B Sample and balance

Table 3 reports descriptive statistics for the analysis sample. Respondents are on average 40 years old, 45% are female, and nearly all are Muslim (99%); 32% are literate and 33% own a radio. The sample is predominantly Yalunka (70%), with Koranko (21%) and Fullah (5%) minorities; the remaining 4% belong to other tribes, for which the survey includes no separate indicator. Reflecting these shares, 23% of respondents were randomly assigned a vignette counterpart from their own tribe. Just over a third (36%) report having personally experienced a dispute with a herder. Panel A confirms the outcome codings used throughout: higher values indicate less discrimination (purchase outcomes) or greater acceptance (acceptance items), and the acceptance index is the standardized mean of the five items on the full 0–3 scale.

Table 3: Descriptive statistics

	Mean	SD	Min	Max	N
Panel A: Outcomes					
Purchase threshold (0–3)	1.83	1.45	0	3	1742
Always buys ingroup (= 1)	0.38	0.48	0	1	1742
Always buys cheapest (= 1)	0.60	0.49	0	1	1742
Accept: work together (0–3)	1.91	1.22	0	3	1754
Accept: lend money (0–3)	2.12	1.13	0	3	1749
Accept: neighbour (0–3)	1.98	1.18	0	3	1753
Accept: marriage tie (0–3)	2.24	1.12	0	3	1746
Accept: social gathering (0–3)	2.38	0.99	0	3	1746
Acceptance index (std)	-0.00	1.00	-2.36	0.75	1765
Panel B: Baseline covariates					
Age (years)	39.95	16.36	18	100	1765
Female (= 1)	0.45	0.50	0	1	1765
Tribe: Koranko (= 1)	0.21	0.41	0	1	1765
Tribe: Yalunka (= 1)	0.70	0.46	0	1	1765
Tribe: Fullah (= 1)	0.05	0.23	0	1	1765
Muslim (= 1)	0.99	0.11	0	1	1765
Literate (= 1)	0.32	0.46	0	1	1764
Owns radio (= 1)	0.33	0.47	0	1	1765
Farmer–herder dispute (= 1)	0.36	0.48	0	1	1765
Tribe matches vignette counterpart (= 1)	0.23	0.42	0	1	1765

Notes: Analysis sample. Outcomes are coded so that higher values indicate less discrimination (purchase outcomes) or greater acceptance (acceptance items). The acceptance index is the standardized mean of the five items on the full 0–3 scale. Baseline covariates are the pre-specified controls; tribe match indicates that the respondent's own tribe equals the vignette counterpart's.

Table 4 examines covariate balance across the eight vignette arms. For each pre-specified covariate, we report means by counterpart nationality and p-values from tests of (i) the nationality difference, (ii) joint equality across the four tribe arms, and (iii) joint equality across all eight cells,

each from a regression of the covariate on the full set of treatment indicators with standard errors clustered by community. No covariate differs significantly across arms at the 5% level (smallest $p = .077$, radio ownership). An omnibus likelihood-ratio test from a multinomial logit of treatment assignment on all nine covariates jointly does not reject the null of balance ($\chi^2(63) = 63.30$, $p = .47$).

Item nonresponse is negligible and unrelated to treatment. The purchase outcome is missing for 23 respondents (1.3%) and individual acceptance items for 11–19 respondents each (0.6–1.1%); 56 respondents (3.2%) are missing at least one acceptance item. Regressing nonresponse indicators on the treatment dummies yields joint $p = .99$ for the purchase outcome and $p = .10$ for missing any acceptance item.

Table 4: Covariate balance across vignette arms

				p-values		
	Overall	SL arm	Guinea arm	Nationality	Tribe	All arms
Age (years)	39.95	39.93	39.97	.965	.997	.999
Female (= 1)	0.45	0.46	0.45	.657	.321	.462
Tribe: Koranko (= 1)	0.21	0.22	0.20	.311	.423	.578
Tribe: Yalunka (= 1)	0.70	0.70	0.70	.952	.411	.462
Tribe: Fullah (= 1)	0.05	0.05	0.06	.464	.402	.557
Muslim (= 1)	0.99	0.98	0.99	.158	.333	.166
Literate (= 1)	0.32	0.33	0.30	.333	.178	.211
Owns radio (= 1)	0.33	0.34	0.32	.316	.146	.077
Farmer–herder dispute (= 1)	0.36	0.34	0.38	.120	.889	.381
Omnibus LR test (multinomial logit): $\chi^2(63) = 63.30$, $p = .466$						

Notes: Means by counterpart-nationality arm. p-values are from regressions of each covariate on the full set of treatment indicators with standard errors clustered by community: Nationality tests the Guinean indicator; Tribe jointly tests the three tribe indicators (Koranko reference); All arms jointly tests all four indicators. The omnibus test is a likelihood-ratio test from a multinomial logit of arm assignment on all nine covariates (complete-covariate sample, $N = 1{,}764$).

C Variable construction

All outcomes follow a single direction convention: *higher values indicate less discrimination or greater acceptance*. "Don't know" and "refuse to answer" responses are coded as missing throughout.

Purchase threshold. The purchase question (Box A1) offers four response options. We code them 0–3 such that 0 = "I would always buy the cake from Stall 1 (the person in my tribe)" (maximum discrimination), 1 = "the cake would need to be a lot cheaper," 2 = "the cake would need to be a little bit cheaper," and 3 = "I would always buy the cheapest cake" (no ingroup price premium). For estimation we standardize this variable to mean zero and unit variance over the analysis sample. From it we derive two indicators: *always buys ingroup* (= 1 if the threshold equals 0) and its complement *ever buys from outgroup* (= 1− always buys ingroup), used as the binary headline outcome; *always buys cheapest* (= 1 if the threshold equals 3) is reported descriptively.

Social acceptance. The five acceptance statements (Box A1) use a four-point agreement scale. We code each item 0–3 such that 0 = "disagree strongly" and 3 = "agree strongly." The *acceptance index* is the respondent-level mean of the five items on the full 0–3 scale, standardized to mean zero and unit variance over the analysis sample. This formulation preserves within-agreement variation (e.g. "agree somewhat" vs. "agree strongly") that a binary dummy collapses. For each item we also retain a binary agreement indicator (= 1 if the respondent agrees somewhat or strongly) for descriptive reporting. Item-level codings and distributions are reported in Table 3 and Figure 3.

Treatment indicators. *Guinean* equals one if the vignette counterpart was described as being from across the border in Guinea (zero if Sierra Leonean). Tribe indicators mark counterparts described as Limba, Yalunka, or Fullah, with Koranko as the omitted reference category.

Covariates. The pre-specified controls are: age in years; an indicator for female respondents; indicators for the respondent's own tribe (Koranko, Yalunka, Fullah; respondents from other tribes, about 4% of the sample, form the omitted group, and the survey includes no separate Limba indicator); and indicators for being Muslim, being literate, owning a radio, and having personally

experienced a farmer–herder dispute. The *tribe match* indicator equals one when the respondent's own tribe equals the vignette counterpart's tribe; because the survey lacks a Limba respondent indicator, matches are identified for Koranko, Yalunka, and Fullah counterparts only.

D Main regression tables

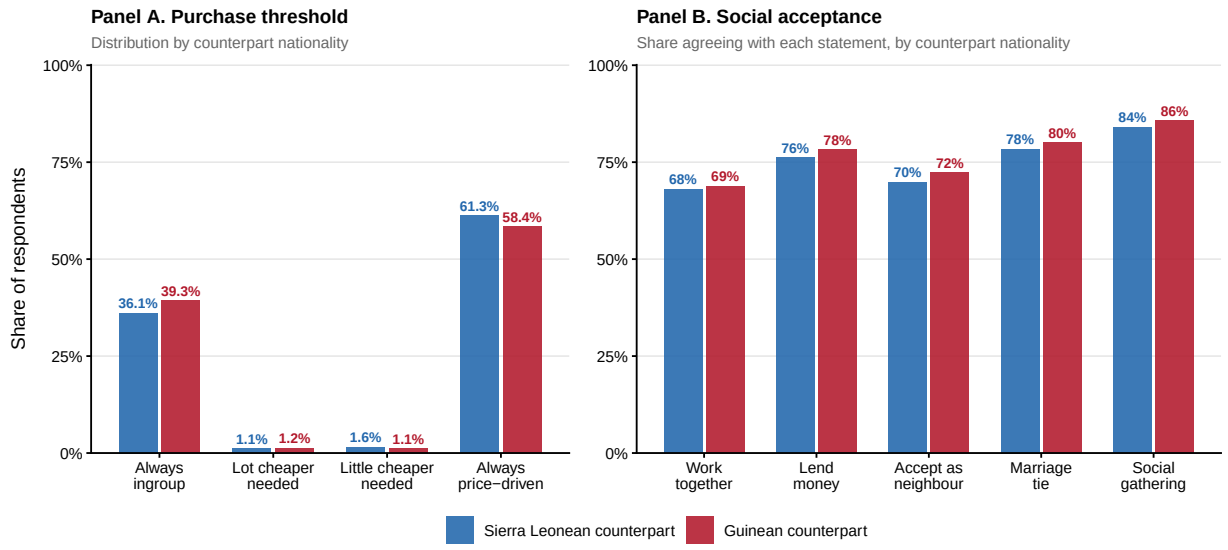

Figure 5: Outcome distributions by counterpart nationality

Notes: Panel A shows the distribution of the purchase-threshold outcome separately for respondents whose vignette counterpart was described as Sierra Leonean versus Guinean. Panel B shows the share of respondents who agree (somewhat or strongly) with each social-acceptance statement, by counterpart nationality. Differences between arms are small and statistically insignificant throughout (see Table 1). $N = 1{,}742$ (Panel A) and $N = 1{,}746$–$1{,}754$ per item (Panel B).

Table 5: Nationality × tribe interactions

	Purchase threshold (std) (1)	Ever buys outgroup (2)	Acceptance index (std) (3)
Guinean (vs. Sierra Leonean)	-0.130	-0.064	-0.008
	(0.084)	(0.044)	(0.107)
Limba (vs. Koranko)	-0.027	-0.026	-0.131
	(0.098)	(0.049)	(0.079)
Yalunka (vs. Koranko)	-0.091	-0.053	-0.088
	(0.088)	(0.045)	(0.080)
Fullah (vs. Koranko)	-0.062	-0.034	0.039
	(0.084)	(0.043)	(0.110)
Guinean × Limba	0.047	0.027	0.260**
	(0.135)	(0.065)	(0.108)
Guinean × Yalunka	0.146	0.073	0.196
	(0.116)	(0.060)	(0.129)
Guinean × Fullah	0.074	0.033	-0.143
	(0.113)	(0.058)	(0.185)
Joint p: interactions	.644	.663	.003
PAP controls	Yes	Yes	Yes
Observations	1,741	1,741	1,764
R^2	0.013	0.012	0.093

Notes: OLS with PAP controls; SEs clustered by community in parentheses. Main effects of tribe are for Sierra Leonean counterparts; the Guinean effect is for Koranko counterparts. "Joint p" tests the three interaction terms jointly. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

Table 6: Romano–Wolf multiple-hypothesis corrections

Outcome	Estimand	$\hat{\beta}$	Unadjusted p	Romano–Wolf p
Purchase threshold (std)	Guinean	-0.063	.298	.724
	Limba	-0.005	.946	.989
	Yalunka	-0.016	.795	.987
	Fullah	-0.026	.697	.987
Ever buys outgroup	Guinean	-0.031	.359	.816
	Limba	-0.013	.698	.987
	Yalunka	-0.015	.619	.976
	Fullah	-0.018	.587	.970
Acceptance index (std)	Guinean	0.071	.178	.492
	Limba	0.007	.919	.989
	Yalunka	0.013	.830	.987
	Fullah	-0.037	.627	.983

Notes: Estimates from the main specification (OLS with PAP controls, SEs clustered by community). Romano–Wolf step-down adjusted p-values treat all twelve treatment–outcome tests as one family; 1,000 cluster-bootstrap resamples (communities), seed 20260611.

E Statistical power and equivalence tests

Methods

We assess the informativeness of our null results in two ways. First, we compute minimum detectable effects (MDEs) from the realized cluster-robust standard errors of the main specifications: $\text{MDE}_{80} = (z_{1-\alpha/2} + z_{0.80}) \times \text{SE} = 2.80 \times \text{SE}$ for 80% power at $\alpha = .05$ (two-sided), and $\text{MDE}_{90} = 3.24 \times \text{SE}$ for 90% power. Because these MDEs are built from the standard errors actually obtained, they incorporate the realized sample sizes, the precision gains from covariate adjustment, and the clustering of outcomes within communities. We do not report "observed power," which is a deterministic transformation of the p-value and carries no additional information (Hoenig & Heisey, 2001).

Second, we conduct equivalence tests using the two-one-sided-tests (TOST) procedure (Hartman & Hidalgo, 2018). For each treatment–outcome pair we test the null hypothesis that the true effect is at least as large as an equivalence bound δ in either direction ($H_0\colon |\beta| \geq \delta$), rejecting at $\alpha = .05$ when the 90% confidence interval lies entirely within $(-\delta, +\delta)$. Rejection supports the conclusion that any true effect is smaller than δ.

Equivalence bounds

We derive the bounds from benchmark effect sizes in the intergroup-bias literature. For standardized outcomes we set $\delta = 0.20$ SD. This sits below the smallest meta-analytic anchor available – $d \approx 0.32$ for ingroup favoritism in cooperation (Balliet et al., 2014) – and well below effects reported in minimal-group studies: explicit evaluation $d = 0.60$ and identification $d = 0.93$ (implicit measures $d = 0.37$–0.44) in Schmidt and Drake (2023), and allocation biases of $d = 0.43$–0.85 in Gaertner and Insko (2000). For the binary purchase outcome we set $\delta = 0.10$ (10 percentage points), equal to the smallest benchmark in Goette et al. (2012), who find ingroup–outgroup cooperation gaps of 15 percentage points overall and 20 points for real social groups; their 10-point gap for minimally assigned groups is statistically insignificant. Rejecting effects beyond these bounds therefore rules out effects of the size this literature reports.

Results

Table 7 reports estimates, MDEs, 90% confidence intervals, and TOST p-values for all twelve treatment–outcome pairs. The design was powered (at 80%) to detect effects of 0.14–0.21 SD on the standardized outcomes and 8–9 percentage points on the binary outcome. The null of non-equivalence is rejected in every case (largest $p = .023$): all twelve 90% confidence intervals lie strictly within the equivalence bounds. The MDE for the Fullah contrast on the acceptance index (0.213 SD) marginally exceeds δ; its equivalence test nonetheless rejects ($p = .022$) because the point estimate is near zero. Design parameters: $N = 1{,}741$–$1{,}764$ across outcomes; 25 community clusters; intra-cluster correlations of 0.026 (purchase threshold), 0.019 (binary purchase), and 0.154 (acceptance index).

Table 7: Minimum detectable effects and equivalence tests

Outcome	Estimand	$\hat{\beta}$	SE	MDE_{80}	90% CI	p_{TOST}
Purchase threshold (std, $\delta = 0.20$)	Guinean	−0.063	0.060	0.167	[−0.165, 0.038]	.015
	Limba	−0.005	0.070	0.196	[−0.124, 0.115]	.005
	Yalunka	−0.016	0.059	0.165	[−0.116, 0.085]	.002
	Fullah	−0.026	0.066	0.184	[−0.138, 0.086]	.007
Ever buys from outgroup ($\delta = 0.10$)	Guinean	−0.031	0.033	0.092	[−0.087, 0.026]	.023
	Limba	−0.013	0.033	0.092	[−0.069, 0.043]	.007
	Yalunka	−0.015	0.029	0.082	[−0.065, 0.035]	.004
	Fullah	−0.018	0.032	0.089	[−0.072, 0.037]	.008
Acceptance index (std, $\delta = 0.20$)	Guinean	0.071	0.051	0.143	[−0.016, 0.159]	.009
	Limba	0.007	0.065	0.182	[−0.105, 0.118]	.003
	Yalunka	0.013	0.059	0.167	[−0.089, 0.115]	.002
	Fullah	−0.037	0.076	0.213	[−0.168, 0.093]	.022

Notes: Estimates from the main specification (OLS with PAP controls, SEs clustered by community; $N = 1{,}741$–$1{,}764$, 25 clusters). MDE_{80} is the minimum detectable effect at 80% power ($2.80 \times \text{SE}$). p_{TOST} is the larger of the two one-sided p-values testing $H_0\colon |\beta| \geq \delta$; equivalence bounds δ derive from literature benchmarks (see text). All tests reject at $\alpha = .05$. Tribe estimands relative to Koranko.

F Robustness

F1. Probit specification for binary purchase outcome

The headline binary outcome *ever buys from outgroup* is estimated by OLS (linear probability model) in the main analysis. Table 8 reports probit average marginal effects (AMEs) alongside OLS estimates and Romano–Wolf corrected p-values. AMEs are directly comparable in magnitude to OLS coefficients. As expected for a common binary outcome, the two estimators are nearly identical throughout, and all four Romano–Wolf p-values exceed .80.

Table 8: Binary purchase outcome: OLS vs. probit AME with Romano–Wolf correction

	OLS	Probit AME	Romano–Wolf p
	(1)	(2)	(3)
Guinean (vs. Sierra Leonean)	-0.031	-0.030	.816
	(0.033)	(0.033)	
Limba (vs. Koranko)	-0.013	-0.013	.987
	(0.033)	(0.033)	
Yalunka (vs. Koranko)	-0.015	-0.014	.976
	(0.029)	(0.029)	
Fullah (vs. Koranko)	-0.018	-0.018	.970
	(0.032)	(0.032)	
PAP controls	Yes	Yes	
Observations	1,741	1,741	

Notes: Column (1) replicates the OLS estimate from Table 1. Column (2) reports probit average marginal effects. Column (3) reports Romano–Wolf step-down corrected p-values from the main analysis (12 hypotheses: 3 outcomes $\times$ 4 estimands); these apply equally to Columns (1) and (2) given the near-identical estimates. Standard errors (in parentheses) clustered by community (25 clusters). PAP controls included throughout. Reference arm: Koranko $\times$ Sierra Leonean. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

F2. Alternative estimation of acceptance results

The main-text acceptance index aggregates five items, each estimated by OLS on a 0–3 ordinal scale. This subsection probes both choices in turn: it disaggregates the index to the level of the individual items, and it relaxes the equal-spacing assumption that OLS imposes on the response categories.

First, item-level effects. Figure 6 plots the nationality and tribe effects estimated separately on each raw item (0–3 scale, with PAP controls). No item shows a substantively large or statistically

significant effect of either cue, confirming that the index null is not an artifact of aggregation and does not mask offsetting effects across items.

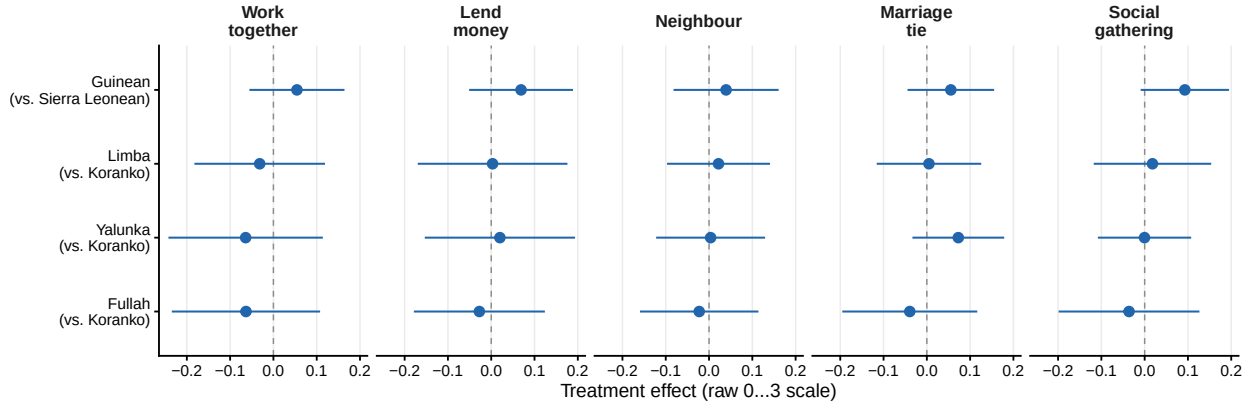

Figure 6: Treatment effects on individual acceptance items

Notes: OLS coefficients with 95% confidence intervals; standard errors clustered by community (N = 1,764, 25 clusters). Each panel shows effects of the nationality cue (Guinean vs. Sierra Leonean) and the three tribe contrasts (vs. Koranko) on a single acceptance item, coded 0–3 (higher = more accepting). PAP controls included throughout.

Second, ordered probit. Table 9 re-estimates the same item-level analysis using ordered probit instead of OLS, relaxing the assumption of equal spacing between the four response categories (0 = strongly disagree, 3 = strongly agree). Reported coefficients are effects on the latent propensity to endorse the item; signs and statistical significance are directly comparable to the OLS estimates. Results are consistent throughout: treatment effects are small and, with one exception (Guinean cue on the social-gathering item, $\hat{\beta} = 0.14$, $p < .05$), statistically indistinguishable from zero.

F3. Community fixed effects

The main specifications absorb community-level confounders through clustered standard errors. Table 10 compares the PAP-controls specification without and with community fixed effects. Because randomization was at the individual level within communities, identification is unaffected by the inclusion of FEs; they absorb any community-level differences in baseline acceptance or purchase behaviour. Odd-numbered columns replicate the main-text estimates; even-numbered columns add community FEs. Coefficients are nearly identical across both specifications and remain statistically indistinguishable from zero throughout.

Table 9: Item-level acceptance effects: ordered probit

	Work together (1)	Lend money (2)	Neighbour (3)	Marriage tie (4)	Social gathering (5)
Guinean (vs. Sierra Leonean)	0.062	0.081	0.031	0.066	0.135**
	(0.053)	(0.063)	(0.062)	(0.056)	(0.062)
Limba (vs. Koranko)	-0.045	-0.009	0.030	-0.000	0.038
	(0.078)	(0.090)	(0.062)	(0.072)	(0.095)
Yalunka (vs. Koranko)	-0.088	-0.005	0.005	0.058	-0.028
	(0.088)	(0.084)	(0.063)	(0.064)	(0.067)
Fullah (vs. Koranko)	-0.080	-0.046	-0.020	-0.051	-0.053
	(0.088)	(0.081)	(0.071)	(0.091)	(0.112)
PAP controls	Yes	Yes	Yes	Yes	Yes
Observations	1,753	1,748	1,752	1,745	1,745

Notes: Ordered probit coefficients (effects on latent scale) with robust standard errors (in parentheses) clustered by community (25 clusters). All five acceptance items are measured on a 0–3 scale (0 = strongly disagree, 3 = strongly agree). PAP controls included throughout. Reference arm: Koranko × Sierra Leonean. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

Table 10: Main effects without and with community fixed effects

	Purchase threshold (std)		Ever buys outgroup		Acceptance index (std)	
	(1)	(2)	(3)	(4)	(5)	(6)
Guinean (vs. Sierra Leonean)	-0.063	-0.075	-0.031	-0.036	0.071	0.062
	(0.060)	(0.059)	(0.033)	(0.033)	(0.051)	(0.051)
Limba (vs. Koranko)	-0.005	-0.002	-0.013	-0.011	0.007	0.045
	(0.070)	(0.071)	(0.033)	(0.033)	(0.065)	(0.062)
Yalunka (vs. Koranko)	-0.016	-0.017	-0.015	-0.015	0.013	0.072
	(0.059)	(0.061)	(0.029)	(0.030)	(0.059)	(0.057)
Fullah (vs. Koranko)	-0.026	-0.022	-0.018	-0.017	-0.037	-0.058
	(0.066)	(0.064)	(0.032)	(0.031)	(0.076)	(0.074)
Community FEs	No	Yes	No	Yes	No	Yes
PAP controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,741	1,741	1,741	1,741	1,764	1,764
R^2	0.013	0.049	0.012	0.042	0.087	0.204

Notes: OLS coefficients with standard errors (in parentheses) clustered by community (25 clusters). PAP controls included in all columns. Even-numbered columns add community fixed effects (*i*.community). Outcomes as in Table 1. Reference arm: Koranko × Sierra Leonean. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

F4. Tribe-match exclusion

About 23% of respondents share a tribe with the vignette counterpart (see Appendix C, paragraph on covariates). For these respondents the outgroup tribe cue is partly an ingroup signal, which could

mechanically attenuate effects toward zero. Table 11 replicates the main specifications on the subsample for whom the counterpart is unambiguously an outgroup member ($N \approx 1{,}352$). Coefficients are similar in magnitude to the full-sample estimates and remain statistically indistinguishable from zero in the pre-specified (with-controls) columns.

Table 11: Main effects excluding tribe-matched respondents

	Purchase threshold (std)		Ever buys outgroup		Acceptance index (std)	
	(1)	(2)	(3)	(4)	(5)	(6)
Guinean (vs. Sierra Leonean)	-0.083	-0.089	-0.040	-0.043	0.102*	0.092*
	(0.053)	(0.058)	(0.029)	(0.031)	(0.052)	(0.052)
Limba (vs. Koranko)	-0.043	0.015	-0.031	-0.006	-0.104	-0.010
	(0.079)	(0.079)	(0.036)	(0.037)	(0.073)	(0.062)
Yalunka (vs. Koranko)	-0.013	0.154*	-0.020	0.057	-0.303*	-0.013
	(0.089)	(0.086)	(0.040)	(0.039)	(0.168)	(0.095)
Fullah (vs. Koranko)	-0.071	-0.014	-0.039	-0.014	-0.146*	-0.037
	(0.074)	(0.078)	(0.035)	(0.038)	(0.079)	(0.057)
PAP controls	No	Yes	No	Yes	No	Yes
Reference-arm mean	0.110	0.110	0.679	0.679	0.066	0.066
Observations	1,337	1,337	1,337	1,337	1,352	1,352
R^2	0.003	0.017	0.003	0.015	0.009	0.093

Notes: OLS coefficients with standard errors (in parentheses) clustered by community. Sample restricted to respondents whose own tribe does not match the vignette counterpart's tribe ($N \approx 1{,}352$, 25 clusters). Outcomes and controls as in Table 1. Reference arm: Koranko × Sierra Leonean. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

F5. Identity overlap between respondent and vignette herder

The vignette always presents respondents with a choice between an ingroup farmer's stall and a herder's stall whose tribal and national identity are randomised. We classify each observation by the degree of identity overlap between the respondent and the vignette herder:

1. **Ingroup on both dimensions** (SL herder, same tribe as respondent; $n = 194$): the herder shares the respondent's nationality and tribe.

2. **Ingroup on nationality, outgroup on tribe** (SL herder, different tribe; $n = 648$): same nationality, different tribe.

3. **Outgroup on nationality, ingroup on tribe** (Guinean herder, same tribe; $n = 219$):

different nationality, same tribe.

4. **Outgroup on both dimensions** (Guinean herder, different tribe; $n = 704$): different nationality and different tribe.

Panel A of Table 12 reports unadjusted means for each group. Discrimination rates are strikingly similar: between 60.5% and 64.7% of respondents ever buy from the outgroup herder regardless of how much the herder's identity overlaps with their own. Panel B shows OLS coefficients relative to the fully-ingroup reference, controlling for PAP covariates; no coefficient attains conventional significance, and the joint F-test p-values range from 0.17 to 0.30 across outcomes. The pattern holds for the purchase threshold and social acceptance index as well. These results confirm that the discrimination documented in the main analysis is driven by the farmer–herder occupational divide rather than ethnic or national identity per se: even a herder who is fully ingroup faces the same level of resistance as one who is entirely outgroup.

Table 12: Discrimination by degree of identity overlap with the vignette herder

	Purchase threshold (std) (1)	Ever buys outgroup (2)	Acceptance index (std) (3)
Panel A: Unadjusted group means (SE in parentheses)			
SL herder, same tribe as respondent (ingroup reference, n = 194)	-0.024	0.613	0.008
	(0.073)	(0.035)	(0.066)
SL herder, different tribe from respondent (n = 648)	0.051	0.647	-0.050
	(0.039)	(0.019)	(0.041)
Guinean herder, same tribe as respondent (n = 219)	-0.022	0.612	0.007
	(0.069)	(0.033)	(0.068)
Guinean herder, different tribe from respondent (n = 704)	-0.034	0.605	0.042
	(0.038)	(0.019)	(0.037)
Panel B: OLS coefficients relative to ingroup reference (PAP controls included)			
SL herder, different tribe from respondent	0.088	0.040	-0.019
	(0.080)	(0.040)	(0.059)
Guinean herder, same tribe as respondent	0.006	0.001	0.016
	(0.105)	(0.057)	(0.077)
Guinean herder, different tribe from respondent	0.003	-0.000	0.070
	(0.115)	(0.060)	(0.073)
PAP controls	Yes	Yes	Yes
Joint F-test p-value	0.172	0.219	0.301
Observations	1,741	1,741	1,764
R^2	0.013	0.012	0.087

Notes: All respondents are Sierra Leonean farmers. Groups are defined by whether the vignette herder shares the respondent's nationality (Sierra Leonean vs. Guinean) and tribe. Tribe matching uses respondent indicators for Koranko, Yalunka, and Fullah; Limba respondents cannot be matched because no Limba indicator was collected and are classified as "different tribe" throughout. Panel A reports unadjusted means with simple standard errors (SD/ $\sqrt{n}$) in parentheses. Panel B reports OLS coefficients relative to the fully-ingroup reference (Group 1), with standard errors (in parentheses) clustered by community (25 clusters). PAP controls: age, female, respondent-tribe indicators, religion, literacy, radio ownership, and household dispute experience. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

F6. Alternative inference: randomization and wild-cluster bootstrap

Standard errors throughout are clustered on the 25 survey communities. With few clusters the conventional cluster-robust (CR1) variance estimator can be biased downward, so we verify the headline nulls against two inference methods that do not rely on a large number of clusters. Randomization-inference (RI) p-values permute the randomized cue labels under the sharp null of no effect; because assignment was at the individual level, this reproduces the experiment's assignment mechanism exactly and is valid in finite samples. The nationality and tribe factors were randomized independently and are permuted separately (10,000 permutations each). Wild-cluster-bootstrap (Wild-CB) p-values follow Cameron, Gelbach, and Miller (2008), imposing the null and resampling at the community level with Webb six-point weights (9,999 replications), the recommended small-cluster correction. Table 13 reports all three p-values side by side for the twelve headline tests. They agree closely, and every test remains far from conventional significance under all three methods.

Table 13: Analytic, randomization-inference, and wild-cluster-bootstrap p-values for the headline tests

			p-value		
Outcome	Cue	$\hat{\beta}$	Analytic	RI	Wild-CB
Purchase threshold (std)	Guinean	−0.063	.298	.189	.320
	Limba	−0.005	.946	.949	.947
	Yalunka	−0.016	.795	.820	.792
	Fullah	−0.026	.697	.705	.690
Ever buys outgroup	Guinean	−0.031	.359	.187	.383
	Limba	−0.013	.698	.697	.708
	Yalunka	−0.015	.619	.654	.624
	Fullah	−0.018	.587	.596	.582
Acceptance index (std)	Guinean	0.071	.178	.128	.189
	Limba	0.007	.919	.919	.917
	Yalunka	0.013	.830	.844	.835
	Fullah	−0.037	.627	.564	.665

Notes: Each row is one randomized identity cue in the PAP-controls specification (N = 1,741–1,764, 25 communities). $\hat{\beta}$ is the OLS coefficient (Koranko and Sierra Leonean reference categories). *Analytic* is the conventional cluster-robust p-value (standard errors clustered by community, referred to $t(24)$), as reported in the main tables. *RI* is a randomization-inference p-value from 10,000 permutations of the randomized cue labels under the sharp null, permuting the nationality and tribe factors separately. *Wild-CB* is a wild-cluster-bootstrap p-value (Cameron et al., 2008) with the null imposed, Webb weights, and 9,999 community-level resamples. Outcomes and controls as in Table 1.

G Heterogeneous effects

Tables 14 and 15 test whether nationality and tribe effects differ by respondent gender or age. Each model augments the main specification (PAP controls plus all four vignette-treatment indicators) with two interactions per treatment: treatment × Female and treatment × Age (centred at the sample mean of approximately 40 years). The main treatment coefficient is thus the effect for a male respondent of mean age. None of the twelve interaction terms in Table 14 or the eighteen in Table 15 attain conventional significance, and point estimates are small relative to their standard errors. There is no evidence that the identity-cue nulls are concentrated in particular demographic subgroups.

Table 14: Nationality effect heterogeneity by gender and age

	Purchase threshold (std) (1)	Ever buys outgroup (2)	Acceptance index (std) (3)
Guinean (vs. SL)	-0.097	-0.044	0.083
	(0.072)	(0.038)	(0.069)
× Female	0.074	0.029	-0.026
	(0.072)	(0.035)	(0.102)
× Age (centred)	-0.004	-0.001	0.001
	(0.003)	(0.001)	(0.004)
PAP controls	Yes	Yes	Yes
Tribe indicators	Yes	Yes	Yes
Observations	1,741	1,741	1,764
R^2	0.014	0.012	0.087

Notes: OLS coefficients with standard errors (in parentheses) clustered by community (25 clusters). "Guinean (vs. SL)" is the nationality effect for male respondents at the sample mean age. "× Female" is the interaction with a female indicator; "× Age (centred)" is the interaction with age in years, centred at the sample mean (≈ 40 years). Tribe-treatment indicators included but not reported. PAP controls: age, female, respondent-tribe indicators, religion, literacy, radio ownership, and household dispute experience. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

Table 15: Tribe effect heterogeneity by gender and age

	Purchase threshold (std) (1)	Ever buys outgroup (2)	Acceptance index (std) (3)
Limba (vs. Koranko)	0.044	0.005	0.038
	(0.111)	(0.054)	(0.137)
× Female	-0.102	-0.037	-0.069
	(0.163)	(0.075)	(0.185)
× Age (centred)	0.001	0.000	-0.001
	(0.005)	(0.002)	(0.002)
Yalunka (vs. Koranko)	-0.010	-0.014	0.060
	(0.090)	(0.045)	(0.094)
× Female	-0.011	-0.001	-0.109
	(0.128)	(0.061)	(0.153)
× Age (centred)	0.001	0.000	0.003
	(0.005)	(0.002)	(0.005)
Fullah (vs. Koranko)	-0.015	-0.016	-0.025
	(0.088)	(0.041)	(0.094)
× Female	-0.024	-0.003	-0.028
	(0.153)	(0.075)	(0.138)
× Age (centred)	0.002	0.001	-0.003
	(0.005)	(0.002)	(0.003)
PAP controls	Yes	Yes	Yes
Nationality indicator	Yes	Yes	Yes
Observations	1,741	1,741	1,764
R^2	0.013	0.012	0.088

Notes: OLS coefficients with standard errors (in parentheses) clustered by community (25 clusters). Each tribe main effect is estimated for male respondents at the sample mean age (reference tribe: Koranko). "× Female" and "× Age (centred)" are the interactions with a female indicator and with age centred at the sample mean (≈ 40 years), respectively. The nationality-treatment indicator (Guinean vs. Sierra Leonean) is included but not reported. PAP controls as in Table 14. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.