



## Ethnic cooperation and conflict in Kenya

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## ABSTRACT

There is growing evidence that ethnic divisions and conflict experiences affect social capital and economic interactions in both positive and negative ways. We conduct a set of experiments measuring social capital in Kenya between the two largest ethnic groups, the Luo and Kikuyu, who experienced violence in the 2007 and 2008 post-electoral riots. Our findings indicate trust, coordination, altruism, and cooperation between these groups are not affected by priming people on the ethnic identity of their partners or on the salience of election conflict. Our results suggest electoral violence does not necessarily lead to changes in economic behavior between ethnic groups and that cooperative failure across groups may be easily overstated or might have other mechanisms. These findings are consistent with recent evidence suggesting that experience of electoral violence in Kenya does not correlate with laboratory behavior between the Luo and Kikuyu.

## 1. Introduction

E<sup>1</sup>thnic conflict is a large social pressure point in many developing countries as it leads to economic inefficiencies and reduced social welfare (Chen et al., 2008; De Luca and Verpoorten 2015). Group-based conflicts remain a major impediment for entrepreneurship (Naudé et al., 2011), human capital accumulation (Leon, 2012; Justino, 2011), social capital (Coletta and Cullen, 2000), health (Ghobarah et al., 2004), and economic growth and development in general (World Bank, 2011). Trust and cooperative behavior are crucial for economic development, as they can determine access to jobs, credit and participation in informal saving and insurance arrangements. This is particularly relevant in societies where economic interactions are not generally governed by formal contracts (Heikkilä et al., 2016; Kimuyu

and Omiti, 2000). Evidence suggests ethnic divisions can negatively affect economic performance (Easterly and Levine, 1997; Cederman and Luc, (2007); 2011; Alesina and Spolaore, 1997; La Ferrara, 2003a; Miguel, 2014) and damage productivity, particularly at the firm level (Hjort, 2014).

Despite how often signs of ethnic bias arise in observational data (Weidmann et al., 2016; Ilorah, 2009), efforts at establishing a behavioral microfoundation that explains these real-world findings have proven less apt at capturing biases across ethnicities in the developing world. Which intergroup behaviors are affected and how are key questions for understanding how such biases might transmit to the wider economy, and where logical sources of intervention might be. Work to date has tended to show no traces of ethnic biases in general decision-making. Ferguson et al. (2022) show no biases in

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choices between host and refugee communities in Jordan and Lebanon; Berge et al. (2019) none between members of different ethnic groups in Kenya.<sup>2</sup> When biases have been found, they tend to arise in the presence of other factors, such as conflict (Whitt et al., 2021; Mironova and Whitt, 2014), which is theoretically tractable. Economic cooperation may become more difficult after violence. It may lead to economic and social isolation, influencing individuals' decision-making and inter-group interactions that are not present when the salience of conflict is low. Consequently, in this article, we are interested both in whether we can capture traces of behavioral impacts of group identity across individuals of different ethnicities in Kenya, and to explore the role ethnicity plays in individuals' behavior in the aftermath of interethnic violence. Specifically, we are interested in whether the salience of group identity, the salience of memories of past violence or both changes individuals' economic behavior towards members of their out-group.

This builds on a growing literature that seeks to understand the impacts of past ethnic-violence episodes on behavior (see Bauer et al., 2016, for a review), though these papers often use vague measures, like "social cohesion", rather than indicators on economic decision-making (Voors et al., 2012; Gneezy and Fessler, 2012; Bauer, Fiala, & Levely, 2018; Bauer et al., 2014a).<sup>3</sup> Our approach utilizes a more recent approach that seeks to understand outcomes throughout economic behavior (Berge et al., 2019).

Coincidentally, we ran an overlapping, but not fully identical set of behavioral games, as Berge et al. (2019)<sup>4</sup>. This concurrence allows us to make three contributions to the literature. First, it allows us to partially replicate the results of Berge et al. (2019) in the range of overlapping games present in both studies – specifically, the dictator and public goods games – which is relevant, given that it is surprising to some extent that the study failed to capture behavioral traces of observed ethnic biases on those ethnic groups. Second, by playing additional games – specifically the trust game and the stag hunt game – we are able to address a key conclusion from Berge et al. (2019) which states that it is important to extend the range of behaviors in order to determine the behavioral effects of known ethnic biases in these societies. We specifically selected a range of games that differ both in the nature of the task (e.g., division or cooperation games) and in the role of beliefs. This approach enables to cover the full dimensions of behavior in two key domains – the role of beliefs in equilibrium behavior and the level of conflict of interest of outcomes in the game. This allows us to examine the domains in which findings do, or do not, arise. Third, by priming a subsample of our respondents, we test whether or not interethnic

behaviors can be triggered by quasi-exogenous factors – specifically, the salience of past conflict. More generally, we contribute new understanding the behavioral microfoundations of ethnic biases and their economic effects.

Specifically, in this study, following a pre-analysis plan, we analyze the role of situations of inter-ethnic tension on four facets of interpersonal behavior: TACC (trust, altruism, cooperation, and coordination) captured via the dictator and public goods games, as well as the trust and stag hunt games. The latter measures the inclination to coordinate and, in our specific implementation of the game, asks subjects whether they prefer to open their own business or a joint business with a riskier, higher payoff. This ensures a structured approach to covering the strategic settings in which different economic behaviors sit, allowing us to test whether results sit in a particular part of a behavioral "matrix" pertaining to beliefs and competition.

This set of experiments were conducted in Kenya with the two largest ethnic groups, the Kikuyu and Luo, to explore how group-based behavioral differences are influenced by the salience of past election violence. In general, Kenya is a suitable case-study for work of this sort. Since it achieved independence, it has had a system of government based, at least partly, on ethnic patronage, which have given rise to intermittent electoral violence. After sporadic violence following elections in the 1980s and 1990s, large-scale violence broke out following a contested election in 2007. Although ostensibly political, the violence had an ethnic component, affording a unique opportunity to test the main hypotheses of the work.<sup>5</sup> We focus our data collection efforts in three areas within Nairobi that were particularly badly affected by the violence: Kibera, Kawangware, and Viwandani. These places were chosen to ensure sufficient variation in experience of violence across individuals. The high levels of violence in these neighborhoods ensured that enough of our sample would have experienced violence to ensure the salience of the violence-based prime. Noting that all three neighborhoods in Nairobi were multiethnic, we repeated the experiment in rural areas just outside of Kisumu, an area with a significant Luo majority. Participants are 654 men<sup>6</sup> from the four locations in Kenya.

Consistent with Berge et al., (2019), the effects from the present study are not significantly different from zero in the dictator and public goods games. Results show a modest interaction between reminded experiences of conflict and differences towards in-group or out-group, but overall estimates are not statistically significant. However, in the stag hunt game, we observe that the probability of coordination decreases significantly for those who were primed and played in-group. This finding is not fully unexpected. Theoretically it is plausible that being reminded of past violence encourages individuals engage in a "risk dominant" behavior rather than exhibiting group-based favoritism. Punishing in-group members for past violence episodes or distrust may explain the direction of this finding, as found in Mendoza et al. (2014) and Shinada et al. (2004). In line with these results, money allocation decreases in the trust game for the primed individuals playing in-group, with the coefficient marginally insignificant. Coefficients are robust to

<sup>2</sup> Intergroup relationships are studied more generally in the literature, e.g., Bauer et al., 2014a; 2014b) and Cecchi et al. (2015); However, these studies do not differentiate "in-group" directly in terms of ethnicity. Rather, in-groups (out-groups) are, respectively: people from the same village (distant village); classmates (members of other classes); players in the same sports team (opposing team).

<sup>3</sup> We note that the nature and intensity of violence – although acute – in Kenya in 2007 and 2008 differs significantly to that in (e.g.) Voors et al. (2012), which opens questions as to the relationship between the structure of violence and behavioral responses (see: Davenport et al., 2019). Although valid concerns, the Bauer et al. (2016) review suggests that behavioral responses emerge in a range of conflict types and structures. For example, Silva and Mace (2015) show behavioral responses in the aftermath of the long, low-intensity conflict in Northern Ireland. Hager et al. (2019) show responses after an acute but short round of ethnic violence in Kyrgyzstan. Ferguson and Leorch (2022) show behavioral responses following personal victimization by violence in the Kenyan context. We remain cognizant that the nature of violence in Kenya should, however, play a role in interpretation of our findings.

<sup>4</sup> The approach, setting and topic of Berge et al. (2019) are, coincidentally similar to this project. Both projects followed similar timelines, particularly in data collection. Data collection of the two projects took place in Kenya within a matter of weeks of each other in 2015.

<sup>5</sup> We note that both recent and past work differentiates electoral and ethnic violence (e.g. Bauer et al., 2016 deliberately omit electoral violence from their meta-analysis and interesting discussions about the particularities of electoral violence have taken place elsewhere (e.g. Birch et al., 2020). This work has shown electoral violence to have notable impacts on related indicators to those we collect, such as voting behavior (e.g. Gutiérrez-Romero and LeBas, 2020) and in more general outcomes in Kenya (e.g. Klaus, 2020). As the main faultline of the violence was ethnically based, we broadly treat it as such but note the potential that the electoral component could be important in contextualizing the findings we produce.

<sup>6</sup> Given findings in Berge et al. (2019) and Bauer et al. (2016), we are sufficiently confident that this decision should not unduly influence the key findings presented in this article but note that results should be viewed and understood in the context of this choice.

	Joint	Own
Joint	(450, 450)	(0, 250)
Own	(250, 0)	(250, 250)

Fig. 1. Stag hunt payoff matrix

the inclusion of controls and hold in general for location and ethnic groups, ruling out a standard inter-group contact theory explanation (Pettigrew and Tropp, 2006).

We contribute to the literature on conflict and ethnic salience by examining the long-term behavioral consequences on inter-group economic decision-making in Kenya. We find that co-ethnic biases or favoritism towards one's in-group are not as pronounced in economic behaviors as they are often hypothesized. Effects are small in the set of games subjects played and suggest, if anything, that memories of past violence induce punishments of in-groups, rather than of out-groups. Overall, this suggests that behavioral traces of ethnic biases in economic behavior are modest, if present at all, and not easily manipulated. These findings hold, broadly, over a range of economic behaviors that differ in their strategic setting. The remainder of this manuscript proceeds as follows. In section 2 we review some of the current research on ethnic interactions. In section 3 we present the experimental methods and the games defined in the pre-analysis plan. Section 4 shows results, Section 5 discusses findings, and Section 6 concludes.

## 2. Ethnic interactions: In-group and out-group behavior

The literature suggests that experiencing conflict influences the salience of social identity. A common argument is that people turn favorably towards their in-groups in times of conflict, as norms are enforced more rigidly in-group during these episodes (Gneezy and Fessler, 2011; Voors et al. 2012; Bauer et al., 2016; Bohm et al., 2018; Shayo 2020). For instance, violent conflict along ethnic lines can potentially increase ethnic salience relative to salience of common nationality (Sambanis and Shayo, 2013). A rich body of papers on in-group behavior and bargaining shows that resource allocations are larger towards in-groups (Fehr et al., 2013; Bernhard et al., 2006; Bowles and Gintis, 2004) and those individuals tend to punish more if a norm violator is out-group and the receiver is in-group (Schiller et al., 2014; Jordan et al., 2014). The bias is more likely to be higher if individuals have experienced previously some sort of conflict or if respondents self-reported feelings of closeness to their in-group (Jetten et al., 1996; Bernhard et al., 2006; Chen and Li, 2009; Leider et al., 2009; Fong and Luttmmer, 2009; McLeish and Oxoby, 2011; Iyengar and Westwood, 2015; Zhang et al., 2019). Furthermore, studies that examine the role of conflict on social identity in the domain of cooperation and trust find that, in general, cooperation is higher when playing in-group. In these papers, in-group bias tends to increase when out-group competition is higher (Bornstein and Ben-Yossef 1994; Glaeser et al., 2000; Bornstein G. 2003; Eckel and Grossman, 2005).

Another group of studies supports that exposure to violent conflict makes people less favorable towards out-group members (Schubert and Lambdsdorff, 2014; Georg et al. 2013). For example, Becchetti et al. (2014) found that victimized subjects exhibit reduced trustworthiness towards individuals from different ethnicities. However, ethnic biases driving out-group discrimination is not the consensus. For instance, the contact hypothesis (Allport, 1954) supported by several studies (Pettigrew & Tropp, 2016; Beaman, Chattopadhyay, Duflo, Pande, & Topolova, 2009; Mironova and Whitt, 2014; Whitt 2014) argue that

intergroup interactions typically reduce discrimination. Moreover, Berge et al. (2019) finds little evidence that ethnic biases lead to in-group and out-group preferences and suggest that the individuals' degree of altruism might dilute the possible biases.<sup>7</sup>

## 3. Experimental design and data

In our pre-analysis plan, we developed the experimental approach to test four facets of interpersonal behavior, trust, altruism, cooperation, and coordination (TACC). This format was chosen for two main reasons. We posit that TACC are important to study in the face of inter-ethnic conflict. Previous macro work on ethnic cooperation suggests that ethnic heterogeneity is detrimental for trust (Hakansson and Sjöholm 2005). But it remains unclear if this holds at the micro-level, with some recent work suggesting a less pessimistic view (Becchetti et al., 2014). Trust between groups is likely an important component of building functional local economies and facilitating development (Knack and Keefer, 1997). Moreover, trust and altruism have an important role for societies, growth, and economic progress (Luhman, 1979; Gambetta, 1990; Fukuyama, 1996; Knack and Keefer, 1997; Benabou and Tirole, 2006; Basu, 2006). Similarly, cooperation is likely to be one of the key drivers of economic development and, thus, any new information is likely to raise important policy questions in areas where different ethnic groups come together. Coordination, perhaps, goes further again and deals with the capacity of individuals to reach socially optimal behavior. Second, the games chosen are very common in the economics and political science literature, thus the interpretation is robust, and issues can be minimized.

Kenya was struck by massive outbreaks of ethnicity-based violence after elections in 2007 and 2008. To exploit the ethnic dimension of these riots, we only invited subjects of the two main groups who had clashed following the disputed election, the Kikuyu and the Luo, to be part of the experiment. Following our pre-analysis plan, only male subjects<sup>8</sup> were recruited from the four different Kenyan regions of interest. To elucidate choices in economic decision-making scenarios, 654 individuals played all four simple one-shot two-player behavioral games.

Busara Center for Behavioral Economics conducted the games with the protocols and game descriptions outlined in our pre-analysis plan (which are replicated, in full, in the Annex). Participants were recruited through the placement of advertisements within locations of interest. This included information about where and when the experiment would take place, the guaranteed and potential payouts and information on the Busara Center, itself. Individuals self-selected into participation. Potential participants were screened for eligibility. Specifically, we sought men over the age of 18 who had lived in the neighborhoods in question at the time of the violence; who were of Luo or Kikuyu ethnicity; and who had not participated in previous experiments involving the games we played. All eligible callers were invited to the sessions with almost all invitees showing up. Although we conducted the games around the same time as Berge et al. (2019) in the same facility, we requested all our participants had never, before, played incentivized behavioral experiments, ensuring no overlap between the samples in the two studies. All individuals played four one-shot games, the basic outline of which we describe below.

<sup>7</sup> The authors discuss other plausible mechanisms motivating the divergence in observed preferences by region, such as the way subjects assimilate norms in the context of migration jointly with urbanization and the role of democratization expansion.

<sup>8</sup> We invited male subjects only for two reasons: First, males were predominant in the riots. Second, inviting females alongside males might have introduced gender-biased behavior, which could have complicated the analysis (Solow and Kirkwood, 2002; Buchan et al., 2008; Gambarino and Slonim, 2009).

**Table 1**  
Balance checks for groups

Variable	(1)	(2)	(3)	(4)	t-test					
	Control Mean/SE	Prime	In-group	Prime x in-group	Difference (1)-(2)	(1)-(3)	(1)-(4)	(2)-(3)	(2)-(4)	(3)-(4)
Unemployed	0.281 (0.035)	0.241 (0.037)	0.215 (0.031)	0.277 (0.034)	0.041	0.067	0.005	0.026	-0.04	-0.06
Marital Status	0.03 (0.013)	0.015 (0.011)	0.023 (0.011)	0.011 (0.008)	0.015	0.007	0.019	-0.01	0.004	0.011
Age	31.317 (0.988)	31.934 (1.107)	32.447 (1.070)	33.087 (1.119)	-0.62	-1.13	-1.77	-0.51	-1.15	-0.64
N	167	133	177	177						

Notes: Column 1 presents means for the control group composed by subjects randomly assigned to playout-group. Columns 2-4 present means for the treatment groups. The variable unemployment is measured in percentages. The variable marital status measures the percentage of subjects who are married to more than 1 wife. All individuals from the sample are married.

The value displayed for t-tests are the differences in the means across the groups.

\*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

### 3.1. Games

#### 3.1.1. The dictator game

The dictator game is commonly considered to measure altruism. Individuals were asked to divide a fixed amount of 250KSH between themselves and a partner, about whom they were given a small amount of information, some salient and some innocuous. At 50KSH intervals, ranging from 0KSH to 250KSH, individuals were asked to select how much of this amount they would like to keep and how much they would give to the partner. They were informed that their payoff, should this game be selected to pay out, would be any amount they had chosen to keep plus any amount their partner chose to send. By the same token, the partner's payoff was any amount the partner chose to keep plus any amount that was sent by the active player. The full wording of the task can be seen in the Annex.

#### 3.1.2. The public goods game

The public goods game is commonly considered to measure the extent to which individuals can cooperate. In our setting, the game was played "simultaneously" by two players. Both players were given a 250KSH endowment and were assigned a partner about whom they were given a small amount of salient and innocuous information. At 50KSH intervals, ranging from 0KSH to 250KSH, individuals were asked how much of the endowment they would like to invest in a public good and how much they would like to keep. All investments in the public good, by either player, were multiplied by 1.5 and divided equally among the two players. Any amounts not invested in the public good were kept by the player. Payoffs were, thus, calculated as the total investment in the public good, multiplied by 1.5 and divided by two, plus any amounts not invested in the public good by that player.

#### 3.1.3. The trust game

The trust game is traditionally played in two components: the Player A role and the Player B role. In the Player A role, commonly considered to measure *trusting* behavior, each individual was given an endowment of 250KSH and were given a choice about sending some amount, again spaced at 50KSH intervals from 0KSH to 250KSH to a partner, about whom he'd been given some innocuous and salient information. Whatever the player did not send was his to keep. Whatever was sent to the partner was multiplied by three. Everything not sent was kept by the active player. In the Player B role, commonly considered to measure *trustworthy* behavior, the "receiver" (that is, the partner who did not make the distribution decision) must decide how much of the about he has received that he will keep and how much he will return. For ease of implementation in the field, we collected this information via the strategy method. That is, individuals were asked

how much they would send back if Player A had made an offer of each possible point on the scale (50KSH; 100KSH; 150 KSH; 200 KSH; or 250KSH). Return decisions were also spaced at 50KSH intervals to minimize noise. Player B was given some innocuous and salient information about his partner. Payoffs in the trust game were calculated as the sum of outcomes from both the Player A and Player B role. Payoffs from the Player A role were calculated as the sum of whatever Player A kept in round one and whatever was returned to him by Player B. Payoffs in the Player B role were based on whatever Player B did not return to Player A, matched to a real decision by another participant.

#### 3.1.4. The stag hunt game

The stag hunt game is commonly considered to capture the extent to which two individuals are willing or able to coordinate. In our case, individuals were offered an opportunity to invest in a business with a partner about whom they were given some innocuous and some salient information. Participants could choose to open a small business, giving a payoff of 250KSH, or a large business, with a potential payoff of either 0KSH or 450KSH, depending on the choice of the partner. If the partner also chose the large business, both partners received 450KSH. If only one partner chose the large business, he received 0KSH, while the partner received the guaranteed 250KSH from the small business.

All individuals played the trust game first<sup>9</sup>, while half of our subjects first took the role of the sender, the other half that of responder<sup>10</sup>. The order of the remaining games was randomized with the restriction that dictator games should not follow the Player B role in the trust game directly, as the decision is strategically identical. In our study, in the trust game Player A was given an endowment of KSH 250 with the option of sending any increment of KSH 50 to Player B. The amount transferred was tripled before reaching Player B, who was then asked which share of the tripled amount he would return to Player A. To increase the number of analyzable responses, we elicited responses using the strategy method, so that Player B had to respond to any possible amount sent by player A.

For each possible tripled amount sent by Player A, Player B was then provided with a set of increments of KSH 50 from which he had to select his returning choice. We define trustworthiness as a variable that captures the mean percentage of the tripled amounts that would have been sent back. In the stag hunt game, Player A and Player B could simultaneously choose between two options: either opening a joint business

<sup>9</sup> The reason for having the trust game first was to separate sending and responding behavior as far as possible.

<sup>10</sup> The results of a set of Mann-Whitney tests showed that we are not able to reject the null hypothesis that the mean sending or returning behavior differed between subjects playing first movers first and those playing second mover first.

with the matched partner or an individual business. Opening a business would assure a subject KSH 250, independent of the partner's choice. Opening a joint business introduced a possible yield of KSH 450 if the partner chose to open the joint business too, but this option came at the risk of earning nothing if the partner chose to open an individual business. This game has two equilibria in pure strategies: both players choose the large payoff, or both choose the smaller payoff. While the larger payoff is socially optimal, the smaller payoff is risk dominant. The game is commonly thought to measure individuals' abilities to coordinate on higher outcome equilibria. The corresponding payoff matrix in KSH is: (Fig. 1).

The third game is the dictator game. Player A was given an endowment of KSH 250 with the option of sending any increment of KSH 50 of it to player B.

Finally, in the two-person public goods game, both Player A and Player B were given endowments of KSH 250 each and they could simultaneously choose to contribute any increment of KSH 50 to a joint fund. The contributed amount is multiplied by 1.5 and divided equally among both players. Any monetary unit not contributed would be kept as additional personal income beyond returns from the joint fund. Hence, the total money each player receives is the money kept plus half of the final amount in the public good.

### 3.2. Treatments

From the 654 subjects who participated in the experiment, half were randomly selected to receive a priming treatment referring to their experiences during the 2007 and 2008 post-electoral riots to induce a mindset of exposure to conflict<sup>11</sup>. The prime consisted of asking questions related to those participants' experience of ethnic riots before they undertook the gaming tasks. These questions asked about personal exposure, exposure of relatives, or witnessing violence perpetrated against strangers.<sup>12</sup> This works on the underpinning principle that our work is, in part, focused on the victims of violence. The salience of any prime related to violence, therefore, is likely to vary with heterogeneous exposure to violence, regardless of the extent to which it is "common" across those exposure histories, or tailored to them. On this basis, any proper understanding would require the collection of these exposure histories. To minimize the burden on participants, we incorporated this fact into our prime. As the assignment to the violence prime is random, this should not affect the internal validity of our experiment – that is, through randomization, we should see similar distributions of exposure histories in both the primed and unprimed sub-samples. The other half of the subjects were asked an identical number of innocuous questions at the same point (e.g., the number of siblings, ever having traveled to a foreign country or time spent on the phone per week), in order to ensure maximum comparability of the experimental setup. Those asked the

<sup>11</sup> Noting both the general sensitivity of working with human subjects and the additional sensitivity of potentially retraumatizing victims of violence by asking them to recall their experiences, we sought and received IRB confirmation of the study. All materials were designed to minimize risk of traumatizing participants and all standard protections were put in place for participants. IRB was awarded by the University of Connecticut under protocol #H14-285.

<sup>12</sup> The specific questions used are: "Now we would like to ask you about your experiences during the elections in 2007. Some of these experiences might be upsetting to think or talk about. Please remember that your answers are confidential. We are going to mention several events. Please tell us if you experienced any: Were you physically injured? (1) No; (2) Minor (e.g. being pushed around); (3) Some (e.g. cuts or bruises); (4) Significant (e.g. heavy beating); (5) Severe (e.g. leading to permanent disabilities). Were one or more of your kin physically injured? (1) No; (2) Minor (e.g. assault, being pushed around); (3) Some (e.g. cuts or bruises); (4) Significant (e.g. heavy beating); (5) Severe (e.g. leading to permanent disabilities); (6) Killed. Did you witness mob violence or mob justice? (1) No; (2) Physical harming / assault of others; (3) Killing of others.

innocuous questions were asked about their exposure to violence after they had completed the tasks. The innocuous survey questions were also collected from the "treated" group after they had completed the tasks.<sup>13</sup>

Within both the priming or non-priming treatments, individuals were also randomly assigned to play either with a partner of their own ethnic group (in-group treatment) or the other group (out-group)<sup>14</sup>. In order to prime ethnicity, we included the mother tongue a partner spoke alongside other innocuous information (age, marital status and employment status). Individuals were randomly selected to play with a partner whose mother tongue identified him as a member of the same ethnic group as the participant, or a member of the other ethnic group. Data were collected in sessions containing only individuals of one ethnicity. For ease of implementation, we randomly selected each session to be, either, an "in-group" or an "out-group" session.<sup>15</sup>

Balance checks are presented in Table 1.

Subjects did not switch treatments during the experiments, and partners stayed matched throughout the play of all four games. The outcome of one game was randomly selected for payout to ensure participants behaved as if any game might pay out, rather than attempting to maximize outcome across all of them (i.e., someone who kept a larger proportion of their endowment in the dictator game might then for instance be more willing to choose the payoff, rather than the risk-dominant strategy in the stag hunt game). We present summary statistics for all games in Table 2.

<sup>13</sup> We are confident that this prime successfully induced a conflict-related mindset, which might overcome some of the potential concerns raised in previous literature about priming on past experiences of conflict (e.g., Callen et al., 2014). In a field test, participants were asked about how they perceived their safety in the presence of members of their out-group immediately following completion of the games. Non-primed subjects responded to this question before being asked about their experiences of conflict. Primed subjects reported feeling significantly less safe (two-sided Mann-Whitney test,  $p < 0.01$ ) than non-primed subjects. This suggests that we successfully and significantly elevated salience of conflict. Further, it suggests that the salience of the prime did not diminish significantly over the series of tasks individuals undertook. Despite this, the prime and non-primed groups do not report differences in experiences of conflict, suggesting a comparable level of average underlying salience of conflict across the two groups. Most participants experienced violence in some manner – directly, indirectly or as a witness – further reducing concerns about low marginal additional salience of the prime, or low marginal salience among particular groups, such as those who were exposed to violence. This approach was designed to minimize possible risks of experimenter demand problems, although we are aware that we cannot eradicate such risks fully but the basic structure of our data confirms that such risks are minimal. For example, in the same way as individuals who figure out the purpose of the experiment might change their behavioral responses in the main games, we might equally expect them to exaggerate their experience of that episode of violence. We see no difference in reported experiences between primed and unprimed, suggesting the risk of experimental demand biases with this prime are low.

<sup>14</sup> In Nairobi, we had a mix of Kikuyu and Luo respondents, and matching was done within the session. In Kisumu, we only collected data from Luo participants. Participants in Kisumu selected to play with an out-group partner were matched to Kikuyu players in Nairobi. Payoffs were designed

<sup>15</sup> A range of ethnicity primes were considered, and field tested, including name and hometown, as well as language. Of the primes field tested, language performed best. Our desirable criteria were a prime that was subtle enough not to give away the purpose of the game but strong enough to influence behavior. During the field testing, we conducted a qualitative assessment of these outcomes. When asked, players were able to tell us the tribal identity of their partners but were unable to guess that the purpose of our experiment pertained to interethnic attitudes using the language prime. This outperformed the other options in this regard. Again, this suggests that the risks of experimenter demand biases are quite low in this case.

**Table 2**  
Summary Statistics

Variable	Treatment	Amount sent (mean)	Std. Dev.	Min	Max
Trust A		94.72	59.21	0	250
	Prime	100.38	59.83		
	In-group	98.31	60.28		
	Prime x in-group	91.24	54.56		
Returned		0.421	0.230	0	0.898
	Prime	0.385	0.228		
	In-group	0.429	0.240		
	Prime x in-group	0.440	0.226		
Stag		0.64	0.48	0	1
	Prime	0.70	0.46		
	In-group	0.67	0.47		
	Prime x in-group	0.58	0.49		
Dictator		89.68	54.62	0	250
	Prime	84.21	52.72		
	In-group	95.20	58.55		
	Prime x in-group	87.29	48.79		
Public		133.26	74.24	0	250
	Prime	127.56	72.84		
	In-group	135.59	77.27		
	Prime x in-group	138.70	73.17		
Luo dummy		0.37	0.48		
N = 654					

Notes: The sample includes 654 subjects, 133 were randomly assigned to treatment prime, 177 to play in-group, and 177 to both treatments, prime x in-group. The remaining subjects are in the standard or control group or those who played the games out-group and were not reminded of violence experienced during the 2007 and 2008 elections.

### 3.3. Empirical methods

In the pre-analysis plan we set the goals and scope of the study. The main objective of the study is to better understand the relationship between ethnicity and TACC behavior and whether past conflict experiences or location have a potential effect on outcomes. Thus, in addition to looking at means by experimental group, we also run a set of regressions on the laboratory-generated data, following the equation:

$$Y_{ij} = \alpha + \beta_1 prime_i + \beta_2 ingroup_i + \beta_3 prime_i \times ingroup_i + \epsilon_{ij}$$

$Y_{ij}$  is the transfer by subject  $i$  to partner in game  $j$ ,  $prime_i$  is an indicator variable for whether the individual  $i$  was randomly reminded of violence experienced during 2007 and 2008 elections,  $ingroup_i$  is an indicator variable for being paired with a co-ethnic,  $prime_i \times ingroup_i$  corresponds to the interaction term of both treatments, and  $\epsilon_{ij}$  is the error term. We adjust p-values following the Family-Wise error-rate (FWER) corrections to adjust for testing multiple hypotheses. We also conduct robustness checks by running specifications that include a matrix of  $X$  controls, including age, location, employment, and marital status

### 4. Main results

In the pre-analysis plan, we did not form strong theoretical priors about the anticipated direction of effects, instead focusing on the complexity that had begun to arise from micro-level work at the time. In that sense, while our pre-analysis plan noted the significant body of work that tended to show preferences towards in-groups, the motivating principle for undertaking this work was a micro-foundation in this literature that offered less clear-cut predictions. Specifically, we noted, “Macro work on ethnic cooperation suggests that there are difficulties for individuals to cooperate. Recent micro work suggests this relationship might be more complicated at the individual level. For instance, it is not clear if ethnic groups really distrust each other.” In this regard, our generated hypotheses left open the potential for a set of predominantly null findings.

Results from the games for the different groups are presented in

Fig. 2. We also present the regression analysis of the games in Table 3. All treatments, prime, in-group, and the interaction term are in the analysis along with FWER p-values. Results for the trust game are shown in column 1. In general, we do not find statistically significant effects. Trust in in-groups is actually lower than the out-groups, yet the coefficient is almost zero and highly insignificant, indicating there is no ethnic preference in choices in this game. This reduction is larger if in-group players are also primed on their experience of violence.

We also present an analysis on trustworthiness in the Appendix (Fig. A1). The effects are positive but close to zero for the in-group players and players who were primed and interacted in-group. Consistent with the trust game, none of the coefficients are statistically significant (Table A1).

The stag hunt game outcomes are in column 2 of Table 3 and were coded (1) for individuals who chose payoff dominant cooperation versus (0) for individuals who chose the risk dominant individual action. Similar to the trust outcomes, the stag hunt results reveal no significant in-group effect. The probability of coordination increases in the treatment prime, but the effect is statistically insignificant. For those who played with their in-group, the effect is almost zero. However, among those who receive both an in-group and prime treatment, the probability of coordination goes down. In other words, individuals who are reminded of their experience of electoral violence are less willing to coordinate with members of their own group than with members of their outgroup. Primed individuals playing with in-group are 16.6% less likely to coordinate and open a joint business. A similar effect emerges in the trust A choices – those who receive the prime treatment give about 15KSH less, on average, to their in-group. The effect here is only marginally insignificant, however ( $p \sim 0.11$ ).

Columns 3 and 4 show the amounts sent in the dictator and public games which are negative if individuals were reminded of past violence and positive for the other two treatments. Notwithstanding, coefficients are highly insignificant, suggesting an absence of in-group preferences, as in the other games. Findings are robust to the addition of a set of controls (Table 4).

After estimating means and coefficients (Table 2 and Table 3), to better interpret effects of the treatment groups, prime, in-group, and especially the group composed by an interaction, prime x in-group, we

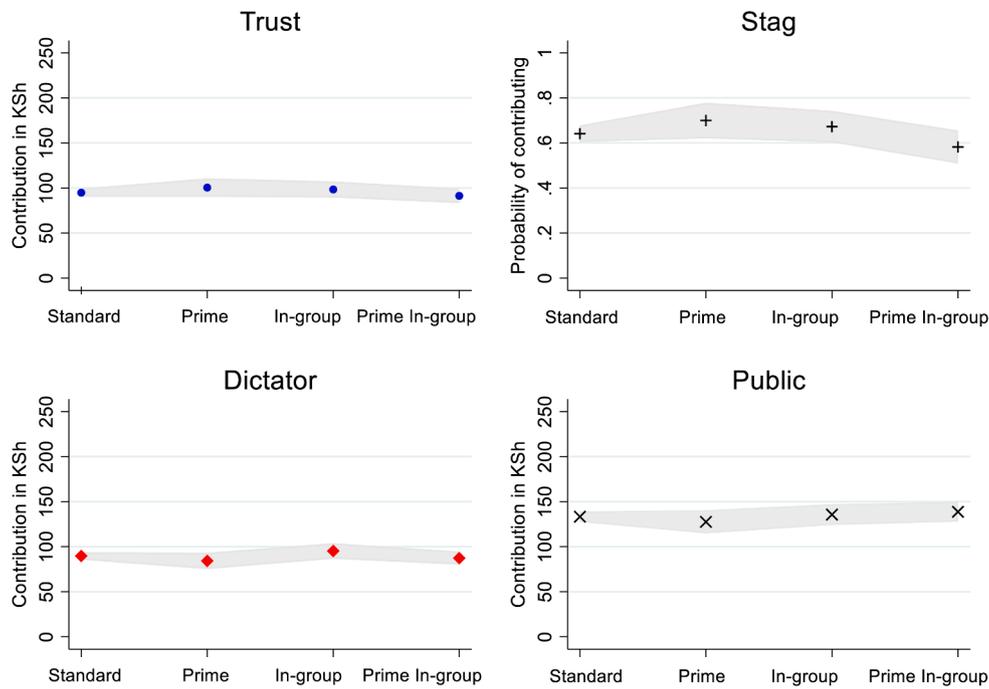


Fig. 2. Game results by treatment

Figure 2. Results from the four games: Trust game, stag hunt game, dictator game, and public goods game. Shaded areas represent 95% confidence intervals.

Table 3  
Main results

Variables	(1) Trust A	(2) Stag	(3) Dictator	(4) Public
Prime	7.921 (0.270) {0.295}	0.076 (0.190) {0.192}	-9.298 (0.154) {0.169}	-2.3 (0.801) {0.813}
In-group	-0.327 (0.971) {0.983}	0.008 (0.916) {0.921}	2.05 (0.799) {0.820}	5.221 (0.637) {0.649}
Prime x in-group	-15.060 (0.114) {0.118}	-0.166 (0.031**) {0.040**}	1.381 (0.873) {0.890}	5.258 (0.661) {0.683}
Observations	654			

Notes: The dependent variable in Columns 1, 3, and 4 are the amount sent in KSH in each game. The dependent variable in Column 2 is a dummy variable, 1 = opening a joint business with the matched partner or 0 = own a business. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level. Unadjusted p-value in (parentheses) and FWER-adjusted p-value in {curly brackets}.

Bootstraps = 100,000 were used to calculate FWER-adjusted p-values.

conduct a predictive margin analysis for each of the groups. Table 5, Fig. A2, Table A2, and Fig. A3 (Appendix A) present results of the games outcomes for the conditional effect of being primed on the status of playing in-group and vice-versa. Or in other words, Panel A indicates the average contribution of each of the treatment groups on the games, if everyone was primed we would expect they send \$x, or if everyone was in-group we would expect to send \$x, (i.e., treatment prime has a predicted contribution of 103.68 KHS on the Trust game). Panel B from Table 5 includes Wald tests to assess whether there is a significant difference in the game outcomes between in-group vs out-group and prime vs. non-prime. We find no evidence that predictive margins differ between either group.

Table 4  
Robustness checks

Variables	(1) Trust A	(2) Stag	(3) Dictator	(4) Public
Prime	6.153 (0.395) {0.414}	0.079 (0.179) {0.179}	-9.435 (0.154) {0.163}	-2.217 (0.810) {0.811}
In-group	-3.641 (0.684) {0.696}	0.020 (0.783) {0.790}	0.296 (0.971) {0.981}	5.749 (0.610) {0.619}
Prime x in-group	-13.42 (0.161) {0.161}	-0.170 (0.029**) {0.035**}	1.510 (0.863) {0.868}	3.837 (0.750) {0.749}
Observations	654			

Notes: The dependent variable in Columns 1, 3, and 4 are the amount sent in KSH in each game. The dependent variable in Column 2 is a dummy variable, 1 = opening a joint business with the matched partner or 0 = own a business. Control variables include age, location, employment, and marital status.

\*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level. Unadjusted p-value in (parentheses) and FWER-adjusted p-value in {curly brackets}.

Bootstraps = 100,000 were used to calculate FWER-adjusted p-values.

#### 4.1. Heterogeneity tests

In this section we seek to understand whether or not findings are common across locations and ethnic groups, or whether these overall findings are driven by individuals with particular endowments (Table 6). This allows us to consider whether or not the results in Table 3 are driven by similar underpinning processes, or if, in fact, what causes movement in different indicators varies. We conduct a set of regressions with the data pooled instead of subsample analysis to avoid losing power. We interact each of the three treatments (prime, in-group, prime-in-group) with participants' location (Nairobi or Kisumu) and ethnic groups (Kikuyu or Luo). Table 6 presents results of the joint effects of Nairobi-Kikuyu, Nairobi-Luo, and Kisumu-Luo.

While we note potential power issues in these analyses, they still offer important context in the interpretation of the main results in

**Table 5**  
Marginal analysis

	Trust A (1)	Stag (2)	Dictator (3)	Public (4)
Panel A	96.467***	0.647***	93.925***	131.823***
Baseline	(5.757)	(0.046)	(5.230)	(7.298)
Prime	103.676***	0.722***	83.590***	128.313***
	(6.100)	(0.049)	(5.542)	(7.745)
In-group	94.071***	0.652***	93.918***	134.127***
	(5.232)	(0.042)	(4.753)	(6.464)
Prime x In-group	87.009***	0.562***	86.009***	137.234***
	(5.232)	(0.043)	(4.753)	(6.645)
Panel B				
Test <sup>a</sup>				
Primed = Non-primed	0.914	0.702	0.307	0.981
In-group = Out-group	0.227	0.470	0.868	0.564

Notes: The dependent variable in Columns 1, 3, and 4 are the amount sent in KSH in each game. The dependent variable in Column 2 is a dummy variable, = opening a joint business with matched partner, or 0 = own a business. The Delta Method was employed to estimate the standard errors in parentheses.

<sup>a</sup> Wald tests were employed to assess differences across treatments, prime and in-group.  
\*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

**Table 6**  
Heterogeneity tests in Nairobi and Kisumu by ethnicity

	(1)	(2)	(3)	(4)
Panel A: Nairobi x Kikuyu x <sup>a</sup>	Trust A	Stag	Dictator	Public
Prime	-0.261 (0.975) {0.974}	-0.019 (0.781) {0.798}	-2.420 (0.754) {0.741}	-17.771* (0.076) {0.097}
In-group	-5.963 (0.574) {0.574}	-0.017 (0.839) {0.840}	11.773 (0.222) {0.171}	-5.769 (0.659) {0.654}
Prime x In-group	-2.098 (0.891) (0.891)	-0.206* (0.091) {0.100}	0.543 (0.969) {0.980}	-19.300 (0.309) {0.310}
Panel B: Nairobi x Luo x <sup>b</sup>				
Prime	17.179 (0.102) {0.148}	0.125 (0.149) {0.170}	-13.591 (0.154) {0.191}	3.437 (0.798) {0.799}
In-group	6.906 (0.426) {0.466}	0.115* (0.095) (0.100)	1.353 (0.864) {0.874}	7.688 (0.474) {0.474}
Prime x In-group	-26.199 (0.071) {0.110}	0.039 (0.570) {0.581}	1.711 (0.897) {0.950}	4.090 (0.823) {0.830}
Panel C: Kisumu x Luo x <sup>c</sup>				
Prime	3.053 (0.851) (0.851)	0.140 (0.289) {0.287}	-12.991 (0.383) {0.418}	19.987 (0.324) {0.401}
In-group	-6.989 (0.637) {0.670}	-0.062 (0.607) {0.634}	-5.458 (0.686) {0.705}	4.399 (0.810) {0.841}
Prime x In-group	-15.324 (0.426) {0.410}	-0.162 (0.299) {0.291}	-13.764 (0.313) {0.354}	1.811 (0.922) {0.934}
Obs.	654			

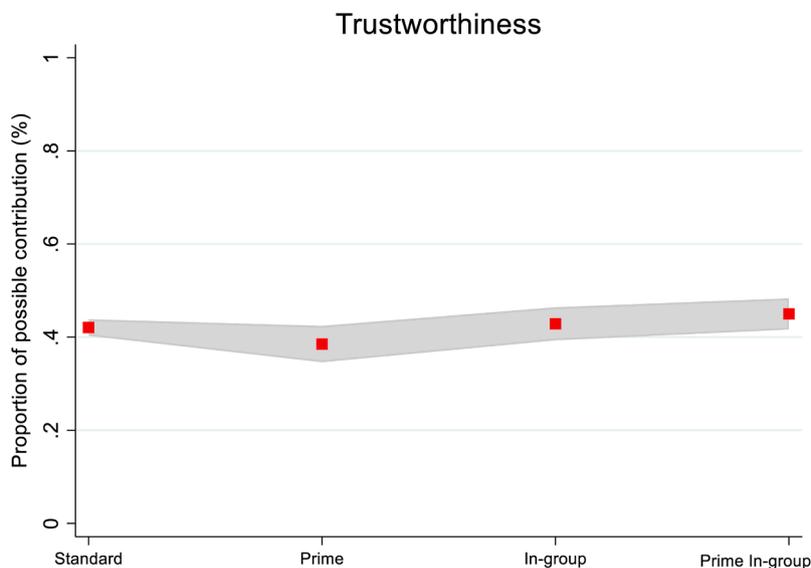
Notes: The dependent variable is the amount sent in KSH in the game. Three separate regressions were conducted with the data pooled. Ethnicity and location are dummy variables.

<sup>a</sup> Nairobi = 1, Kikuyu = 1.  
<sup>b</sup> Nairobi = 1, Luo = 1.  
<sup>c</sup> Kisumu = 1, Luo = 1.

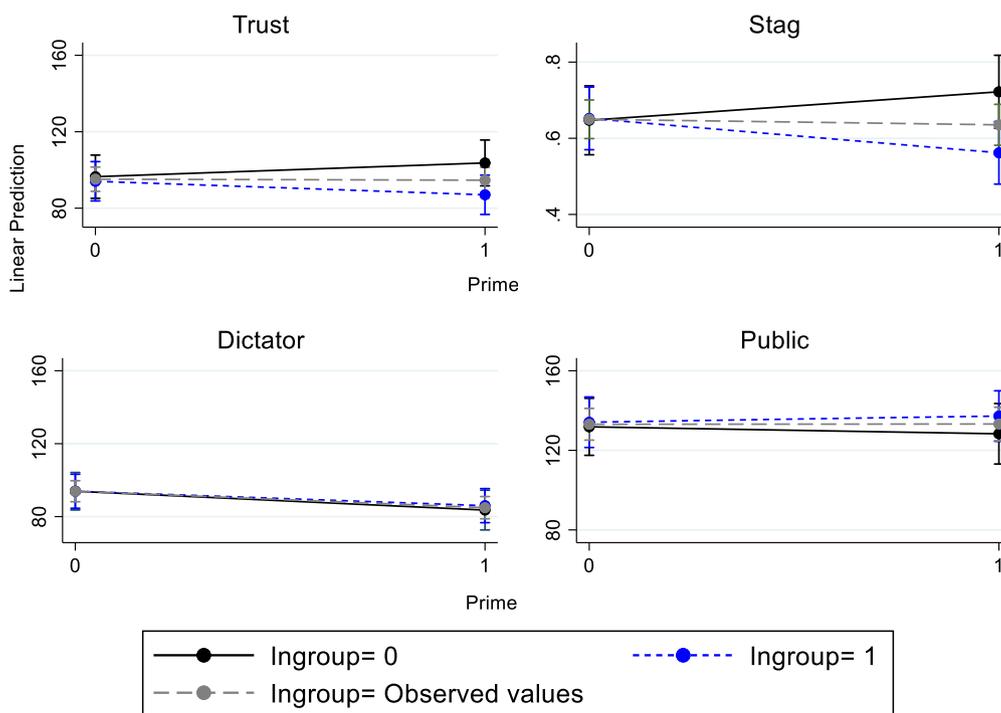
\*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level. Standard errors in parentheses.

**Table 3.** Results from this approach reinforces the main findings. First, while signs tend to go in opposite directions among ethnic groups for those individuals coming from Nairobi, most coefficients are small in size and not statistically different from zero for all the three regressions (Panel A and Panel B). This set of results show again no strong evidence

of coethnic bias in the games for either ethnicity. Second, for the stag hunt game, this analysis allows us to tease out the main results. The *prime x in-group* coefficient suggests that the likelihood of opening a joint business decrease by 20.6% and is statistically significant for the subjects who live in Nairobi and are Kikuyu (Panel A). For the subjects



**Fig. A1.** Trustworthiness results by treatment  
Figure A1. Results from the trustworthiness game. Shaded area represents 95% confidence interval.

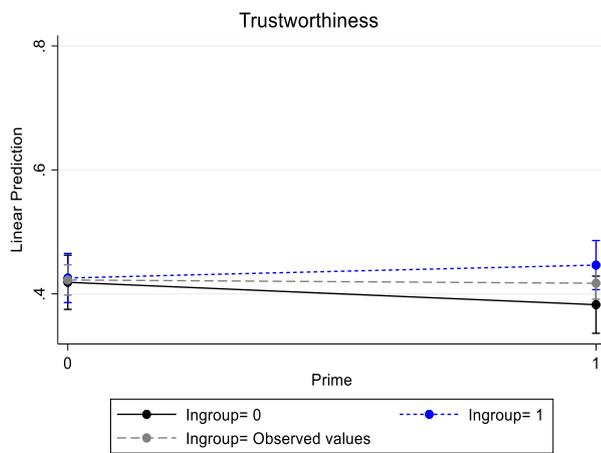


**Fig. A2.** Marginal effects for the games. Notes: The figure presents predicted changes in the coefficient of in-group conditional on whether individuals are primed.

coming from Nairobi and who are Luo (Panel B), the probability of coordination increases by 11.5% if subjects play in the treatment *prime*, coefficients are insignificant in the other two treatments. More generally, we see variation in this table across both locations and ethnicities, although it is unclear which – if either – of these effects are pushing the outcomes we see. For example, Table 6 shows, both, differences between Kikuyu and Luo responses but, also, differences between Luo in Nairobi and Kisumu. The extent to which either ethnicity or location is the salient driving force, therefore, is not clear from these analyses and other, omitted, factors – such as “victors” engaging in conciliatory behavior, could equally explain such a cluster of findings.

Overall, findings from the stag hunt game may suggest that punishing in-group for past violence episodes, the so-called “black sheep” effect<sup>16</sup> (Marques and Paez, 1994), and distrust may help explain the risk-dominant behavior observed in the game. These results also indicate the potential that this effect manifests itself in an impact on the beliefs held by our participants about their in-group partners. The role of beliefs is important in the equilibrium behavior in the stag hunt game and under some belief sets in the Player A role in the trust game. By contrast,

<sup>16</sup> The effect when in-group individuals perceived as norm deviant are derogated to a greater extent than out-group members who are similar.



**Fig. A3.** Marginal effects for trustworthiness. Notes: The figure presents predicted changes in the coefficient of in-group conditional on whether individuals are primed.

**Table A1**  
Trustworthiness results

Variable	Trustworthiness	
	(1)	(2)
Prime	-0.036 (0.193) {0.170}	- 0.037 (0.187) {0.199}
In-group	0.007 (0.842) {0.918}	-0.002 (0.939) {0.932}
Prime x In-group	0.057 (0.122) {0.180}	0.058 (0.119) {0.127}
Controls	No	Yes
Observations	654	

Notes: The dependent variable is the amount sent in KSH in the game. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level. Unadjusted p-value in parentheses and FWER-adjusted p-values in curly brackets. Bootstraps = 100,000 were used to calculate FWER-adjusted p-values.

**Table A2**  
Marginal analysis for trustworthiness

	Trustworthiness
Baseline	0.419*** (0.022)
Prime	0.383*** (0.023)
In-group	0.426*** (0.020)
Prime x In-group	0.446*** (0.021)
Test <sup>a</sup>	
Primed = Non-primed	0.742
In-group = Out-group	0.337

Notes: The dependent variable is the amount sent in KSH in the game.

<sup>a</sup> Wald tests to assess differences across treatments, prime and in-group. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level. Standard errors in parentheses.

they do not, at least in theory, influence equilibrium behavior in either the dictator or public goods games.

This combination of results mean that we replicate those of [Berge et al. \(2019\)](#) in both the dictator or public goods games. By focusing on an extended range of behavioral scenarios, however, we also offer new insight into the impact of the interaction of violence and ethnicity in

other important behavioral domains. Results from the heterogeneity tests, however, suggest that these findings do not arise among all strategically involved groups or in all locations.

#### 4.2. Discussion of results

Broadly speaking, this work shows two clusters of findings that, *a priori*, might be surprising from the bulk of in-group / out-group literature to date (e.g. [Brewer, 1979](#)). First, broadly speaking, we see no suggestion that individuals - generally - behave more generously or cooperatively with co-ethnics than those from other ethnic groups in settings in Kenya. Second, while we do stimulate an effect by reminding some of our sample of a past episode of violence, this effect shows people reducing so-called pro-social behaviors with members of their own group. At some level, this is surprising - both in the Kenyan context and beyond, observational data suggests that group-based ethnic biases exist. In this section, we seek to discuss some potential interpretations of these findings.

First is the fact that we seek to understand ethnicity-based group behaviors several years after a major episode of violence in Kenya. Our data were collected just over 7 years after this outbreak. Although this is not strictly problematic in itself (for example, in their review, [Bauer et al. \(2016\)](#) include multiple examples of conflict induced behavioral change remaining present in societies more than a decade later), group-based relationships can normalize or change over time. In the Kenyan case, this is particular noticeable. While the 2007 election was contested by parties aligned with the Kikuyu and Luo groups, Kikuyu and Luo parties had joined forces in subsequent elections in the period before our data were collected. Potentially, our results simply capture this aggregate-level normalization of relations at the individual level. In this sense, data collected from other ethnic groups in Kenya might show a different set of results than those presented here. Second, and related to this, is that the unusual intensity of the electoral violence in 2007 and 2008 saw significant external responses and internal changes in Kenya, including the signing of the National Accord and Reconciliation Act in 2008, the process of which could have substantively changed perceptions and attitudes among everyday Kenyans. Consequently, other post-violent situations might experience rather different behavioral trajectories than those observed here.

Third, our results - particularly in [Table 6](#) - show substantial variation across both ethnicities and locations, suggesting potential intra-group variations and rural / urban differentials that we cannot fully analyze using the data held. Again, this suggests the need to understand our results in the context of the data we do hold. Future work might like to consider the extent to which these differentials are important for driving the results we show. Similarly, it is possible that concepts like contact theory ([Allport, 1954](#)) differ across these divides, at least in some situations. In our case, for example, individuals in Nairobi are likely to engage, economically, with members of out-groups on a daily basis, which is less likely for those in monoethnic Kisumu. Again, our results should be understood in that context.

That said, it is also plausible - either through the passage of time or more generally, that our results capture a situation where individuals do not exhibit ethnic bias in their behaviors. This could be, simply, because such biases do not exist; or that they exist but not in the domains captured in our experiment. Interestingly, while some signs of ethnic bias emerge in experiments similar to ours in the literature (e.g. [Cecchi et al., 2015](#); [Bauer et al., 2014a](#); [Bauer et al., 2014b](#); [Mironova and Whitt, 2013](#)), they have not done so when identities have been primed directly but, rather, by a series of proxies, such as living in a different village; proximity to outgroups; or classmates. In settings where ethnic background has been primed more generally (e.g. [Berge et al., 2019](#); [Ferguson et al., 2022](#)), bias does not appear to emerge. This, again, could suggest that ethnic bias does not manifest itself in the specific economic behaviors analyzed (at least in fairly integrated, anonymous, societies), or, alternatively, could capture problems either with the primes used in

these analyses (e.g. they do not sufficiently raise the salience of a partner's ethnicity) or could even point to a more general concern with using behavioral games to analyze these kinds of outcomes.

Next, we note that the nature of violence in Kenya – both in the sense that it was, in part, electoral violence and, in part, ethnic violence might play a role in how these findings are interpreted. Similarly, that the violence was acute but short in duration and might not compare to the civil war settings present in much of the relevant literature might point to places of potential departure from the body of work so far and explain some components of our findings. Our findings, therefore, should be understood in the context of the nature of violence that took place.

Finally, while we have taken significant steps to minimize risks of experimenter bias in our experimental design, we note the general concern that such biases could be a literal explanation for our findings. Our findings, therefore, should be interpreted in that context.

## 5. Conclusion

It is a common understanding that people often prefer to interact with those who are similar to themselves. Furthermore, the literature shows strong evidence that cultural and ethnic boundaries can be foundations for nationalism, war, and genocide. Evidence from previous conflicts indicates there could be important tensions between ethnic groups during and after the 2007 and 2008 Kenyan elections. However, we find no general preferences towards one's in-group and neither treatment seems to be driving significant differences on how individuals allocate money in the games. In particular, results are negative and only significant for the stag hunt game in the *prime x in-group* treatment, while there are some hints of a similar effect in the Player A role in the trust game. If we view violence as the breakdown of cooperation (Fearon, 1995), it is perhaps expected that individuals being reminded of their experience of violence exhibit reduced willingness to engage in exercises that require coordination or rely on beliefs that one's partner will behave well. These findings may be associated with literature on the willingness to punish perpetrators or norm violators (Bernhard et al., 2006; Shinada et al., 2004), the “black sheep” effect (Marques and Paez, 1994), or to group members perceiving goals competitively (De Dreu, 2007). Moreover, our findings also reveal no significant differences in money allocation associated with where subjects live (Nairobi and Kisumu), overall, suggesting that the null findings on the role of ethnicity are not driven by regular social and economic interactions between groups in multi-ethnic Nairobi.

We conclude that something in elections may trigger civil unrest and agitation between ethnic groups, or that civil unrest and agitation are deliberately triggered by specific people, but this behavior is not present in daily interactions. Where then does this conflict come from? One possible answer is that conflict is simply a means that politicians use to contest elections. By stoking anger and fear during elections politicians may gain an upper hand over opponents, even if this anger and fear is not present during normal interactions. Future research could explore this question more, including by examining post-conflict effects on daily activities under different conditions and regions.

## Data availability

Data will be made available on request.

## Supplementary materials

Supplementary material associated with this article can be found, in the online version, at [doi:10.1016/j.socec.2023.102050](https://doi.org/10.1016/j.socec.2023.102050).

## Appendix

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