

# SPITE, FEAR AND INTERGROUP CONFLICT

## Experimental Evidence from Nigeria

Miguel Ortiz\*

UC Berkeley

*(Job Market Paper)*

September 14, 2023

[Click here for latest version](#)

### Abstract

Understanding the psychological drivers of conflict is crucial to knowing which policies can be effective at reducing it. In this paper, I study to what extent intergroup conflict is driven by spite vs. fear, and how commonly used policies affect these drivers to increase cooperation. To this end, I develop a theory-driven experimental protocol that allows me to empirically disentangle the motivations behind conflict and determine their relative importance. In Nigeria, I deploy this protocol as a lab-in-the-field experiment to study the conflict between Christians and Muslims. I then run an RCT, in which I give participants access to a radio drama that seeks to promote intergroup cooperation, and use my experimental protocol again to investigate its effects. I find that non-cooperation is driven 24% out of spite and 76% out of fear, and that fear is mostly unwarranted—non-cooperators grossly exaggerate the likelihood that members of the other group will not cooperate. I then estimate a structural model to determine what a policy needs to effectively increase cooperation. I find that correcting unwarranted fears is highly effective, while reducing spite is not because spiteful people also have high levels of fear. Finally, I find that listening to the radio drama decreases spite but not fear, and that (in line with the findings of the model) this does not translate into increased cooperation.

---

\*m.ortiz@berkeley.edu. I am especially grateful to my main advisors, Ernesto Dal Bó and Francesco Trebbi, for their generous support and guidance throughout these years. I am also grateful to Ned Augenblick, Matilde Bombardini, Fred Finan, Edward Miguel, Ryan Oprea, Ricardo Pérez-Truglia and Guo Xu for their comments and support. Eunice Boba Atajiri did an outstanding job as the field manager of the experiments. I gratefully acknowledge the funding provided by the Association for Comparative Economic Studies, the Center for Effective Global Action, the Center for Equity, Gender & Leadership, and the Institute for Business Innovation. The pre-analysis plan of this project is available at [socialscienceregistry.org/trials/10605](https://socialscienceregistry.org/trials/10605).

# 1. Introduction

Despite the high societal costs of intergroup conflict, we still lack a proper understanding of the psychological motivations that drive it. In many expressions of conflict it is difficult to determine what the motivation behind them is. For example, when two ethnic groups arm themselves, it is unclear if they do so because they want to attack the other group, or instead because they seek to protect themselves from a potential attack, preempting it. Similarly, when two religious groups segregate each other, it is unclear whether they do so because they dislike living with the other group, or because they are afraid of having the other group living with them and its deleterious consequences.

Understanding the motives of conflict behavior matters because different motivations call for different policy solutions. To see this, suppose that in the first example one of the two ethnic groups is in power and controls the official army, while the other group harbors an irregular militia—a situation that resembles the conflict that is taking place in Ethiopia. If conflict is driven by mutual fears, disarming the militia would address these fears and lead to peace. If, instead, conflict is driven by a desire to harm the other, disarming the militia could break the balance of power and lead to genocide. The second example can represent processes of religious segregation that have recently taken place in countries like Nigeria and India. In these cases, if segregation is driven by mutual fears, a policy of integration could create trust (as suggested by contact theory). If, instead, segregation is driven by spite, integration could backfire and lead to violence. It is, therefore, crucial to identify the underlying motives driving conflict in order to determine what is the best policy to resolve it.

It is hard, however, to make progress on this matter. There are many potential drivers of conflict behavior, many without a clear definition that distinctly separates them from the others, and many more without an empirical measure. In this study I set the following three-part goal: (i) Reduce the dimensionality of motives into essential drivers that can be operationalized together in a simple framework. (ii) Based on this framework, design a methodology that can empirically disentangle the drivers and assess their relative importance. (iii) Demonstrate that, despite the simplifying assumptions, this study of motives has important explanatory power on conflict behavior and policy efficacy. My approach uses preferences and beliefs as a natural separation of motives for actions.

With this in mind, I seek to answer two questions. (1) To what extent is intergroup conflict driven by spite vs. fear? I define spite as a *preference* for harming the outgroup, and fear as a *belief* about the outgroup's spite towards the ingroup. Here, I further ask: If conflict is driven by fear, are beliefs accurate or misperceptions? After understanding what drives conflict, I turn to policy and ask: (2) Are local policies currently trusted to diminish conflict the right policies for the task? By which I mean, are these policies able to address what I identified to be the key driver of conflict? Here, I focus on the creation of media content to promote intergroup cooperation—in particular, radio drama series. Importantly, in this paper I conceptualize conflict as a manifestation of non-cooperation, in the tradition of Fearon and Laitin (1996).

The analysis takes place in Jos, a state capital situated in the region of Nigeria where the Muslim North and Christian South meet. Historically, the city has been inhabited by both religious groups. In the 2000s, the city experienced multiple outbreaks of religious violence perpetrated by ordinary citizens. These outbreaks created mistrust and animosity between religious groups and set in motion a process of segregation in all aspects of life. Today, there is little interaction between groups, religion is the key political cleavage, and politicians fuel negative narratives about the outgroup for political gain. This context closely resembles the theoretical characterization of “the politics of fear” (Padró i Miquel, 2007).

To answer question (1), I develop a theory-driven experimental protocol to empirically disentangle the motives driving intergroup conflict and assess their relative importance. Then, I deploy this protocol as a lab-in-the-field experiment, in Nigeria, to study the conflict between Christians and Muslims. I start by writing down a model of conflict, with spite and fear as primitives. I model conflict between groups as a coordination game, where cooperation is an equilibrium and offers the highest possible payoff for each player. In this game, players may prefer to not cooperate if they feel enough spite for the outgroup—that is, if a player prefers to sacrifice part of her payoff in order to reduce in a greater amount the payoff of the outgroup player. Alternatively, non-cooperation may stem also from fear. A player who is not spiteful but fears the outgroup player is spiteful (and therefore non-cooperative) will also want to not cooperate.

In the field, I measure cooperation between Christians and Muslims by having them play coordination games. To understand what drives non-cooperation, I elicit participants’ social preferences for the outgroup (which can be altruistic or spiteful), and their beliefs about the outgroup’s social preferences towards them. To recover the social preferences, I have participants take multiple money allocation decisions where they reveal their willingness to pay to decrease or increase the payoff of an outgroup member. To elicit beliefs about the social preferences of others, I ask participants to guess the outgroup’s willingness to pay to decrease or increase the payoff of an ingroup member. I then use this information to estimate a structural model to properly recover social preferences at the individual level. Using the estimated parameters and elicited beliefs, I determine the extent to which non-cooperation is driven by spite vs. fear. I then conduct counterfactual analysis to study how pacification policies that shift the drivers of conflict would affect cooperation. In particular, the paper shows how cooperation would change if a policy were to (i) solve unwarranted fears by correcting misperceptions about the outgroup, and (ii) completely eradicate intergroup spite.

To answer question (2), and study if commonly used policies are well suited to diminish conflict, I conduct a randomized control trial (RCT) where I randomly give participants access to a radio drama series that promotes intergroup cooperation. After the intervention, I deploy my experimental protocol again to investigate if this policy is able to increase cooperation by addressing what I identified to be the key driver of conflict. Radio dramas are both a popular form of entertainment in Sub-Saharan Africa and a common treatment instrument used by NGOs in the region. They have received increased

attention as a policy for conflict due to their perceived advantages: fictional stories make it easier to address sensitive topics of conflict (Slater and Rouner, 2002); stories increase attention and retention of the message (Kromka and Goodboy, 2019); and media interventions can be implemented in a wide range of contexts where alternative policies for conflict are unfeasible. To study this policy, I partnered with the radio production company hired by the largest NGOs in Nigeria, and produced a new radio show following the exact same steps NGOs take to produce their shows. This new radio drama speaks directly to the mechanisms of interest: the story is about two communities that, driven by spite and unfounded fears, miss out on mutually beneficial interactions. The treatment consisted of 24 episodes, each lasting between 10-15 minutes. Participants received the episodes four times a week via WhatsApp over a six-week period. To promote and monitor engagement, participants were incentivized to answer weekly quizzes on the show's content. The control group listened to a placebo radio drama with a message on health. At endline, participants went through the lab-in-the-field experiment again, which allowed me to examine if spite and/or fear were impacted and how this affected cooperation.

The first result is that in a game where cooperating is an equilibrium and the maximum payoff (which represents half a day of salary), people fail to cooperate in 31% of the interactions between groups—compared to only 6% of interactions within groups. This leads to a loss of 9.6% of attainable wealth in intergroup interactions. I estimate the model and find that it performs well in terms of sample fit: the spite and fear elicited from participants explain over 90% of the decisions made in the game. The estimated model leads to three main findings. First, 24% of non-cooperation decisions were motivated by spite, while 76% were motivated by fear. Second, fear is mostly unwarranted, as non-cooperators grossly exaggerate the percentage of spiteful people in the outgroup. Third, spiteful individuals tend to also be very fearful of the outgroup, while altruistic people can be either fearful or trusting. The counterfactual analysis reveals that if a policy solved unwarranted fears by correcting inaccurate beliefs about the outgroup, the number of people not cooperating would drop by 73%. This result underlines how misperceptions leading to unwarranted fears are the most important barrier to intergroup cooperation. In contrast, if a policy completely eradicated intergroup spite, the number of people not cooperating would drop by only 5%. The effect on cooperation is in this case small because spiteful people are also very fearful and therefore, even without spite, most of them will still want to not cooperate out of fear.

Furthermore, I find important differences in the way Christians and Muslims behave. Specifically, 84% of the decisions to not cooperate come from Christians and 16% come from Muslims. Additionally, Christians have more negative social preferences towards Muslims and more biased beliefs about them than the other way around. These results are in line with what was predicted in the pre-analysis plan, as I expected heterogeneity in this direction based on extensive fieldwork. The difference in behavior between religious groups is most likely driven by the salience of the armed group Boko-Haram in this area, which generates negative feelings towards only one of the groups.

Regarding the radio show, I find that the treatment is effective at reducing spite (by 0.45 SD), but ineffective at reducing unwarranted fears. Furthermore, the treatment proves ineffective at increasing

cooperation. The model allows to rationalize what could otherwise be a puzzling result. The radio show is an effective policy because it reduces spite, but it is the wrong policy for this context because it does not affect the key motive of non-cooperation, which is fear. This ultimately renders the policy ineffective at achieving its main goal, increasing cooperation. Beyond this, I find that the effect on preferences is strongest in the most spiteful subsample—a result that is not obvious *a priori*, as the most spiteful people could have more rigid preferences. I also find evidence that the radio show did reduce the fear of those who had the most biased beliefs. Importantly, I show the results are not likely driven by social desirability bias, following the methodology in Dhar et al. (2022).

Taken together, the results of this paper illustrate the value of the model-based protocol to explain conflict behavior and policy efficacy, and how these two explanations can be connected to gain deeper insights. First, my model-protocol allows me to diagnose what motives drive conflict in a particular place. In turn, this diagnosis informs what type of policy is needed in this particular place. In the case of Jos, I find that the main driver of conflict is unfounded fears, and therefore, the best policies to increase cooperation there are those that are effective at correcting intergroup misperceptions. Furthermore, I find that policies that are effective at reducing spite would not only be inefficient, because spite is a less pervasive problem than fear, but also ineffective, because changes in spiteful preferences would not translate into increased cooperation. Second, my protocol allows me to evaluate which drivers of conflict a particular policy is effective at shifting. This, in turn, informs which types of places would benefit the most from this particular policy. In the case of the radio show, I find that it is effective at reducing spite, but not at reducing unwarranted fears. This means that places where reducing spite is the most effective channel to increase cooperation are the places that would benefit the most from a radio show like this.

Because I use the same theory and protocol to study both context and policy, I can directly connect the two. By observing which motive drives conflict in a certain place, and which motive a certain policy shifts, I can determine if a particular place-policy combination is likely to increase cooperation. For the case of Jos and the radio show, my analysis allows me to conclude that the two are misaligned: a radio show that decreases spite will not increase cooperation in Jos, where the main driver of conflict is fear. Indeed, the null treatment effect I find on cooperation is evidence of this mismatch. Importantly, this protocol is portable and can be deployed elsewhere. Using my protocol for other contexts and other policies can advance the understanding of intergroup conflict and its solutions.

This paper makes three main contributions. First, it presents a novel theory-driven experimental protocol that proves effective at empirically disentangling spite and fear in a way that is useful to explain conflict behavior and policy efficacy. The literature has provided evidence of how group membership affects preferences and beliefs (for reviews see Shayo (2020) and Charness and Chen (2020)). However, these effects have been studied separately. Concerning preferences, group membership has been shown to affect social preferences positively and negatively (Choi and Bowles, 2007; Chen and Li, 2009; Kranton et al., 2020; Enke et al., 2023; Bauer et al., 2014). Concerning beliefs, social groups have been shown to affect trust, stereotypes and prejudice (Falk and Zehnder, 2013; Bénabou and Tirole, 2011;

Bonomi et al., 2021).

Second, this paper provides empirical evidence on how unwarranted fears can be an essential driver of conflict and how policies may struggle to solve them. This directly connects to the theoretical description of mutual fears leading to unnecessary social failure (Chassang and Padró i Miquel, 2007). In addition, these findings show that in a context that can be theoretically described by “the politics of fear” (Padró i Miquel, 2007), fear indeed becomes a core problem of intergroup relations, and what is more, one that proves hard to fix with policy. These findings speak, more generally, to the literature studying the reasons and dynamics of conflict and intergroup cooperation (Fearon and Laitin, 1996; Blattman and Miguel, 2010; Bauer et al., 2016; Trebbi and Weese, 2019). In addition, the findings help explain the underlying motives behind the market frictions that appear in all sorts of intergroup interactions: for example, in labor selection (Oh, 2023; Giuliano et al., 2009), performance (Hjort, 2014; Ghosh, 2022; Marx et al., 2021; Alesina and Ferrara, 2005), trade (Korovkin and Makarin, 2023; Anderson, 2011; Michelitch, 2015; Jha, 2013), public spending (Luttmer, 2001; Franck and Rainer, 2012; Hodler and Raschky, 2014; Francois et al., 2015; Kramon and Posner, 2016), political accountability (Casey, 2015).

Third, this paper provides evidence on the effectiveness of using media content to reduce intergroup conflict—in particular, radio dramas. This evidence contributes to the literature on policies to improve cooperation between groups in conflict (for a review see Paluck et al. (2021)). Some of the interventions that have attracted academic attention lately are intergroup contact (Lowe, 2021; Rao, 2019; Mousa, 2020; Scacco and Warren, 2018; Paluck et al., 2019; Enos, 2014; Bursztyn et al., 2021; Fouka and Tabellini, 2022), perspective taking (Alan et al., 2021; Adida et al., 2018) and the use of narratives (Broockman and Kalla, 2016). In addition, this finding contributes to the research on media and its effects on social and economic outcomes (for reviews see DellaVigna and La Ferrara (2015) and La Ferrara (2016)). More specifically, it contributes to the work studying how radio influences attitudes in conflict (Yanagizawa-Drott, 2014; Adena et al., 2015; DellaVigna et al., 2014; Paluck, 2009).

The rest of the paper proceeds as follows. Section 2 lays out the background of Jos, Nigeria. Section 3 presents the theory and experimental protocol. Section 4 describes the empirical model and estimation strategy. Section 5 describes the RCT on the radio drama and the data collection. Section 6 reports the results. Section 7 concludes.

## 2. Background

Nigeria is divided between a Muslim-dominated North and a Christian-dominated South. The Middle Belt is the region where these two religious communities intersect. Plateau is one of the states located in the Middle Belt and stands out as the most ethnically diverse state in the country. The experiments of this project took place in Jos, the capital of Plateau.

The city of Jos has historically had a balanced population of both Christians and Muslims. Through-

out much of Jos history, the coexistence between these two groups was characterized by peaceful and harmonious interactions. Less than 25 years ago both religious groups use to celebrate together Sallah and Christmas. However, with the onset of democratization in the 1990s, political groups with religious banners began competing for power, leading to heightened tensions and a looming threat of violence. In 2001 occurred what came to be known as the ‘first crisis’. This was a spontaneous outbreak of violence perpetrated by ordinary citizens, that spread throughout the city. The crisis lasted for several days and resulted in over a thousand fatalities.

After the first crisis, the relationship between the religious groups became even more tense and unstable. This led to similar spontaneous outbreaks of inter-religious violence in 2004, 2008 and 2011, each resulting in hundreds of fatalities. The crises deeply scarred the city and broke what was left of the once-harmonious relationship between Christians and Muslims. Since the first crisis, and reinforced by the ones that followed, a process of religious segregation was set in motion, permeating all aspects of life, such as residential areas, schools, jobs, local markets, hospitals and politics.

Today, due to the segregation that took place, there is minimal interaction between the religious groups in the city. Although the city has regained some stability and safety over the past decade, the traumatic experiences of the past have made people reluctant to venture outside their religious neighborhoods. This lack of contact has fostered high levels of mistrust and animosity between the religious groups. Additionally, religion has become the key political cleavage, with political parties using religious banners to mobilize voters in the quest for control over the city. Both sides fear that if the other religious group gains too much power, they may force them out of the city or block their growth in it. Many politicians exploit these fears for political gains, further exacerbating tensions. This context closely resembles the theoretical characterization of “the politics of fear”, described by Padró i Miquel (2007).

Currently, power over the city is relatively balanced between the two religious groups, which may explain the fragile peace the city has experienced in recent years. However, this equilibrium is fragile and constantly under threat.

### 3. Framework and Experimental Protocol

This section presents the theory that guides the design of the experimental protocol and then proceeds to describe the lab-in-the-field experiment.

#### 3.1. Framework

Consider a society with two groups,  $A$  and  $B$ . Let  $i$  be a member of group  $A$ , and  $j$  a member of group  $B$ . When interacting with  $j$ ,  $i$  has the the following utility function:

$$u_i = x_i + \beta_i(z_i) \cdot x_j$$

Where  $x_i$  is  $i$ 's payoff,  $x_j$  is  $j$ 's payoff, and  $\beta_i \in [-1, 1]$  is  $i$ 's parameter of social preferences towards members of group  $B$ . If  $\beta_i < 0$ ,  $i$  is spiteful towards members of group  $B$ ; if  $\beta_i > 0$ ,  $i$  is altruistic towards members of group  $B$ ; if  $\beta_i = 0$ ,  $i$  is selfish when interacting with members of group  $B$ . The bounds assumed on  $\beta_i$  signify that  $i$  can not care about  $j$  more than she cares about herself.  $z_i$  can be past experiences, education, etc. In what follows, I take  $z_i$  as given, but an alternative model can be found in the Appendix, where  $\beta_i$  is endogenous and depends on the beliefs about  $\beta_j$ .

Members of the different groups face each other in a coordination game<sup>1</sup> (or stag-hunt game), where there are two strategies: *Cooperate* (C) or *Not Cooperate* (N). I illustrate the theory with the following coordination game, which is the one subjects face in the experiment.

	C	N
C	1000 , 1000	500 , 900
N	900 , 500	750 , 750

In the case where there are no social preferences,  $\beta_i = 0$ , the game has two equilibria:  $(C, C)$  and  $(N, N)$ . The equilibrium  $(C, C)$  gives each player the highest possible payoff in the game, but carries some risk: if  $j$  decides to play  $N$ , then  $i$  would get the lowest possible payoff in the game.

There are two reasons why a player would choose  $N$  as her strategy. Before delving into them, notice that all players, regardless of their social preferences, prefer to not cooperate when the other player does not cooperate. That is, for all  $\beta_i$ ,  $u_i(N, N) > u_i(C, N)$ .<sup>2</sup> Intuitively, even if  $i$  is fully altruistic and has  $\beta_i = 1$ , she would still prefer to play  $N$  if  $j$  plays  $N$  because doing so increases her payoff more than it reduces  $j$ 's payoff (i.e., she increases the sum of both payoffs).

A first reason to choose to not cooperate is because a person has particularly strong spiteful preferences. To see this, we need to understand when a person will want to not cooperate even if the other player is going to cooperate. In other words, we analyze the conditions under which  $u_i(N, C) \geq u_i(C, C)$ .

$$\begin{aligned}
u_i(N, C) &\geq u_i(C, C) \\
900 + \beta_i 500 &\geq 1000 + \beta_i 1000 \\
\beta_i &\leq -0.2
\end{aligned}$$

Define the threshold  $T = -0.2$ . If  $i$  is spiteful beyond the threshold she will prefer to not cooperate regardless of what  $j$  is going to do. That is, if  $\beta_i < T$ ,  $N$  is a dominant strategy for  $i$ . In this case,  $i$  is

<sup>1</sup>Coordination games can allow for a richer study of the reasons behind non-cooperation, compared to other games like the prisoner's dilemma. In a prisoner's dilemma, non-cooperation is driven by selfish preferences ( $\beta_i = 0$ ). Instead, in a coordination game, selfish individuals could rationally want to cooperate. Therefore, non-cooperation in coordination games is driven by reasons beyond selfishness.

<sup>2</sup>Proof:  $u_i(N, N) \geq u_i(C, N) \Rightarrow 750 + \beta_i 750 \geq 500 + \beta_i 900 \Rightarrow \beta_i \geq 5/3$ . Because  $\beta_i \in [-1, 1]$ , it is always the case that  $\beta_i \leq 5/3$ .



spiteful enough to prefer to lose 100 and reduce  $j$ 's payoff in 500. When  $\beta_i < T$ , we say that  $i$  chooses to not cooperate out of spite.

A second reason to not cooperate is because a person fears that the other player may have particularly strong spiteful preferences. If  $i$  believes that  $j$  is spiteful beyond the threshold ( $\beta_j < T$ ), then  $i$  believes that  $j$  will not cooperate. And if  $j$  will not cooperate, then  $i$  will prefer to not cooperate as well, regardless of how altruistic she might be (i.e.,  $\forall \beta_i$ , as shown above). When  $i$  believes that  $\beta_j < T$ , we say that  $i$  chooses to not cooperate out of fear.

Of course, a person might not be sure if the other player will cooperate or not. Instead, she might believe that there is a certain probability that the other player will not cooperate, given that  $j$  is a member of  $B$ . Let  $s_i$  be  $i$ 's strategy, and  $\tilde{P}_i(s_j=N)$  be  $i$ 's belief about  $P(s_j=N)$ , the probability that  $j$  will not cooperate. Then  $i$ 's expected utility of choosing  $s_i$  is:

$$W_i(s_i) = \tilde{P}_i(s_j=N) \cdot u_i(s_i, N) + \tilde{P}_i(s_j=C) \cdot u_i(s_i, C)$$

Given this,  $i$  chooses to not cooperate if  $W_i(N) \geq W_i(C)$ . Solving for  $\tilde{P}_i(s_j=N)$  yields the following.

$$\tilde{P}_i(s_j=N) \geq \frac{2}{7} \left( \frac{1 + 5\beta_i}{1 + \beta_i} \right)$$

The condition above determines how fearful a person must be in order to not cooperate. Importantly, this depends on  $i$ 's social preferences. The condition shows that the less altruistic a person is, the less fearful she needs be to want to not cooperate out of fear.

Lastly, before moving forward it is important to make one clarification. A third reason to not cooperate could stem from higher-order beliefs. That is, a person could want to not cooperate because she believes that the other player believes that she will not cooperate. For empirical purposes, in this paper I assume that people playing the game do not have higher-order beliefs. This is an assumption that was validated in the field during the pilot of this project, and that is in line with the findings of the experimental literature that state that the median person does not have higher-order beliefs (Rubinstein, 1989).

This assumption has an important implication. Recall that there are two reasons why  $j$  would not cooperate: because he is spiteful enough, or because he is fearful enough. Notice that  $i$ 's belief about  $j$ 's fear is in fact a higher-order belief. Because individuals do not have higher-order beliefs, the only thing  $i$  considers when trying to guess if  $j$  will not cooperate is the likelihood that  $j$  is spiteful beyond the threshold. In other words,  $i$ 's belief on the probability of  $j$  not cooperating is equivalent to  $i$ 's belief on the probability of  $j$  being spiteful beyond the threshold:

$$\tilde{P}_i(s_j=N) = \tilde{P}_i(\beta_j < T)$$

### 3.2. Lab experiment design

This section presents the experimental protocol that disentangles the motives of conflict. Following the theory, three key pieces of information are needed from each person in the experiment: (i) the decision to cooperate; (ii) the social preferences towards the outgroup; (iii) the beliefs on probability that an outgroup member has social preferences beyond the threshold.

To make this inference, I use the following insight to design the experimental protocol. For each coordination game that participants in the experiment could play, it is possible to design a money allocation decision that mirrors the structure of the coordination game but removes the uncertainty, so that beliefs do not enter the empirical problem. In this way, the money allocation decision isolates preferences that play a role in the decision in the coordination game. To clarify the intuition for identification, consider the following example.

	C	N			You	Other
C	1000 , 1000	500 , 900	↔	Opt 1	1000	1000
N	900 , 500	750 , 750		Opt 2	900	500
<i>Coordination Game</i>				<i>Money Allocation Decision</i>		

In the money allocation decisions, one participant is the decision maker and gets to pick between the payoffs in Option 1 or Option 2, while her match is a receiver. Without imposing any structure on the utility function, we can characterize people into two groups by looking, side by side, at their decisions in the two situations above. If the participant prefers Option 2 to Option 1, she reveals that her preferences are such that she will prefer to not cooperate in the game, even if she thinks that her match will cooperate. We can infer this because the money allocation decision is presenting precisely that scenario. If the participant prefers Option 1 to Option 2 in the money allocation decision, she reveals that she would prefer to cooperate in the game if she thinks her match will cooperate. Therefore, if a participant prefers Option 1 to Option 2, but in the coordination game decides to not cooperate, beliefs must be playing a role.<sup>3</sup> As I will explain below, in the experiment participants will face multiple money allocation decisions to capture with more precision their level of social preferences. I will also use the money allocation decisions to elicit beliefs about others' social preferences.

#### Experiment set up

With the insight from above in mind, the lab experiment proceeds in the following way. First, participants are told there are two groups, the Blue Group and the Green Group, and that they will be randomly matched and play with one person from each group (although they do not know whom from each group). Then, participants are shown the list of names of the people that belong to one of the groups

<sup>3</sup>An underlying assumption in this analysis is that if a person believes the other player will not cooperate, they will prefer to not cooperate too.

(the group they face first is picked at random). Each group consists of ten people. Crucially, the names in the Blue Group are all Christian names, while the names in the Green Group are all Muslim names. In Nigeria, names are a clear signal of religious affiliation, so participants are able to easily identify that the common characteristic of the members of each group was religion. After playing some games with the group that was first revealed, participants are then shown the list of names of the second group and proceed to play with that group. By the end of the experiment, participants have gone through the same activities with each group.

This design has two advantages. First, using names to signal religions allows me to not mention religion explicitly, which helps to reduce experimenter demand bias. Furthermore, because participants do not see the list of names of each group at the same time, it is less explicit in the first stage of the experiment that groups are divided along religious lines, which further lowers demand bias. Second, by not knowing exactly who their match is within the group, participants are forced to think about the average behavior of the members of the group, of which the only discernible shared characteristic is religion. In this way, one can control for any change in the participant's behavior due to interacting with a male or female name, or names that are probably from older or younger cohorts. Importantly, there was no deception in this experiment—the names in the Blue and Green groups belonged to real people, who received money depending on the outcomes of the games.

### Eliciting social preferences

After participants are matched with an unknown person from their first group, they start the activities of the experiment with a series of money allocation decisions. There were 20 money allocation decisions a participant could potentially face. I designed an algorithm to elicit social preferences with the lowest number of questions possible. In the end, participants face 7 or 8 money allocation decisions with each match. In half of the 20 money allocation decisions a participant could face, Option 2 represents the spiteful option of reducing the match's payoff in 500 naira, for a price. In the other half, Option 2 represents the altruistic option increasing the match's payoff in 500 naira, for a price. Within each half, each possible decision varies the amount of money a person has to give up to pick Option 2 (i.e., each decision presents a different price for Option 2). In the end, this series of decisions allows to elicit the participant's willingness to pay to either increase or decrease in a fixed amount the payoff of their match. Full details on this design can be found in the Appendix.

The choices in the money allocation decisions sort participants into one of 21 types. Assuming the utility functional form presented in 3.1, I can assign to each type a calibrated social preference parameter using the following result: a money allocation decision where a participant picked Option 2 reveals that  $\beta_i \geq (x_{i1} - x_{i2}) / (x_{j2} - x_{j1})$ —where  $x_{i1}$  is the payoff for participant  $i$  if she picked Option 1.<sup>4</sup> Using this

---

<sup>4</sup>This expression results from the following process. Let the utility of picking Option 1 be  $u_i(Opt1) = x_{i1} + \beta_i x_{j1}$ . The utility of picking Option 2 is analogous. Then, choosing Option 2 means that  $u_i(Opt2) \geq u_i(Opt1)$ . That is,  $x_{i2} + \beta_i x_{j2} \geq x_{i1} + \beta_i x_{j1}$ . Solving for  $\beta_i$  we get  $\beta_i \geq (x_{i1} - x_{i2}) / (x_{j2} - x_{j1})$ .

calibration method, I can place each participant's social preference parameter in one of the following preference intervals:  $\hat{\beta}_i \in \{(-1, -0.9), \dots, (0.9, 1)\}$ . This calibration approach has the advantage of being simple and transparent. However, the downside is that it ignores sampling variability, as individual-level parameters are assumed to be precisely estimated with only 7 or 8 decisions. Section 4 presents an alternative approach that addresses this problem at the cost of being less direct.

### Eliciting beliefs about others' social preferences

After the money allocation decisions phase, the next module elicits beliefs on the probability of  $j$  not cooperating. Recall from 3.1 that  $\tilde{P}_i(s_j=N) = \tilde{P}_i(\beta_j < T)$ . Therefore, it is enough to elicit  $i$ 's belief on the probability of  $j$  being spiteful beyond the threshold to know  $i$ 's belief on the probability of  $j$  not cooperating.

With this in mind, I ask participants to guess the choices that other participants made in the money allocation decisions. If they guessed correctly, participants get extra payment. Notice, first, that beliefs on  $P(\beta_j < T)$  are determined by the beliefs on the distribution of social preferences of the group  $j$  belongs to. Put differently,  $\tilde{P}_i(\beta_j < T)$  is determined by  $i$ 's beliefs on the distribution of  $\beta_j | j \in G$  (with  $G = \text{Green, Blue}$ ). I elicit beliefs on the mean of this distribution and the mass of the tail at key points that are directly connected to coordination games that participants will play afterward.

First, participants go through the series of money allocation decisions again, trying to guess what their match from the Green/Blue group picked. With this, I elicit the beliefs on the mean,  $\tilde{E}_i[\beta_j]$ . Then, I ask participants to guess how many people (out of the 10) from the Green/Blue group picked Option 2 in some particular money allocation decisions. Each of these scenarios represents a threshold where someone would reveal to be spiteful enough to want to not cooperate out of spite in an upcoming coordination game. Ultimately, this question elicits the beliefs on the percentage of Muslims/Christians that are spiteful enough to want to not cooperate in an upcoming coordination game—that is, it elicits  $\tilde{P}_i(\beta_j < T)$  given that  $j \in G$ , which in the end elicits  $\tilde{P}_i(s_j=N)$ .

### Measuring cooperation

Lastly, participants play coordination games with their anonymous matches from the Green and Blue groups. With each match, they play two coordination games, where each game has a slight variation in payoffs that changes the threshold of how spiteful a person needs to be to want to not cooperate out of spite. After these, the activities of the lab conclude. Further details on the protocol can be found in the Appendix, where the reader can also find details of the coordination games and money allocation decisions, and screenshots of how these were presented to participants.

## 4. Empirical Model and Estimation

While simple and transparent, the approach presented in Section 3.2 to estimate the social preferences of participants has some drawbacks. First, individual-level parameters are being calibrated using 7 or 8 decisions per person. This procedure ignores sampling variability and does not allow estimating standard errors of the estimated preference parameters. Additionally, the first approach does not allow to test alternative models that introduce other parameters that could explain the decisions in the game. This section presents an estimation procedure that overcomes these problems and still allows to recover parameters at the individual level to determine the extent to which non-cooperation is driven by spite vs. fear.

In what follows I introduce an empirical model with random coefficients to recover  $\beta_i \forall i$ . [In it, uses everyone's full set of decisions to estimate the distribution of parameters, and then uses an individual's decisions to determine where in the estimated distribution the individual's parameter is likely to be] To simplify the explanation in this section, I will focus only on the case where a participant  $i \in A$  is matched with a participant  $j$  who belongs to the outgroup,  $j \in B$ . But notice that the same parameters can be calculated for the case where  $j$  belongs to the ingroup,  $j \in A$ .

In the experiment, participants are matched with an unspecified  $j \in B$ . They make  $M_i$  money allocation decisions, where  $M_i$  can be 7 or 8 depending on the participant's decisions. Their beliefs on the money allocation decisions  $j$  made are elicited. And they play  $G$  coordination games, with  $G=2$ .

In each money allocation decision  $m$ , participant  $i$  makes decision  $d_{im}$  between two options with sure payoffs for herself and her match  $j$ . Participant  $i$ 's utility of picking  $d_{im} \in \{Opt1, Opt2\}$  is her base utility function (as defined in 3.1), plus an error,  $\varepsilon_{id}$ , that has an extreme value distribution with mean zero. This error can be thought of as the result of limited attention in the experiment. The utility function is:

$$u(d_{im}) = x_{im}^{d_{im}} + \beta_i \cdot x_{jm}^{d_{im}} + \varepsilon_{id}$$

The data consists of  $d_{im}$  and the payoffs for  $i$  and  $j$  in each option of each money allocation decision. The unknown parameter is  $\beta_i$ . Because  $\varepsilon_{id}$  is distributed extreme value, the probability of participant  $i$ 's sequence of choices  $d_i = \langle d_{i1}, \dots, d_{iM_i} \rangle$  is:

$$\Lambda_{im} = \frac{\exp(u(Opt2) - u(Opt1))}{1 + \exp(u(Opt2) - u(Opt1))}$$

$$P(d_i | \beta_i) = \prod_{m=1}^{M_i} \Lambda_{im}^{\mathbb{1}(d_{im}=Opt2)} (1 - \Lambda_{im})^{\mathbb{1}(d_{im}=Opt1)}$$

Participants also play  $G$  coordination games. In each game  $g$ , participant  $i$  picks strategy  $s_{ig} \in \{C, N\}$ . Participant  $i$  has risk-neutral preferences and her expected utility function includes an error,  $\varepsilon_{is}$ , that has

an extreme value distribution with mean zero, and that is independent from  $\varepsilon_{id}$ . The expected utility function is:

$$W(s_i) = \tilde{P}_i(s_j=C) \cdot u(s_i, s_j=C) + \tilde{P}_i(s_j=N) [u(s_i, s_j=N) - \psi_i \cdot \mathbf{1}(s_i=C)] + \varepsilon_{is}$$

$$u(s_i, s_j) = x_i^{s_i, s_j} + \beta_i x_j^{s_i, s_j}$$

Where  $\tilde{P}_i(s_j=s)$  is  $i$ 's subjective beliefs on  $P(s_j=s)$ , given that  $j \in B$ . Recall that  $\tilde{P}_i(s_j=N) = \tilde{P}_i(\beta_j < T)$ , and  $\tilde{P}_i(\beta_j < T)$  is elicited directly in the experiment (see section 3.2 for details).

$\psi_i$  is a parameter that accounts for the psychological cost that  $i$  pays when she cooperates and  $j$  does not. Ultimately,  $\psi_i$  is a type of loss aversion that can be interpreted as a distaste for getting what is usually described as the “sucker’s payoff”. Importantly, notice that  $\psi_i$  is multiplied by  $\tilde{P}_i(s_j=N)$ , so not cooperating because of  $\psi_i$  also means not cooperating out of fear that  $j$  will not cooperate.<sup>5</sup>  $\psi_i$  and  $\beta_i$  are independent.

The data consists of  $s_{ig}$ ,  $\tilde{P}_i(s_{jg}=N)$ , and the payoffs for  $i$  and  $j$  in all four scenarios of each game. The unknown parameters are  $\beta_i$  and  $\psi_i$ . Because  $\varepsilon_{is}$  is distributed extreme value, the probability of participant  $i$ 's sequence of choices  $s_i = \langle s_{i1}, s_{i2} \rangle$  is:

$$\Lambda_{ig} = \frac{\exp(W(N) - W(C))}{1 + \exp(W(N) - W(C))}$$

$$P(s_i | \beta_i, \psi_i) = \prod_{g=1}^G \Lambda_{ig}^{\mathbf{1}(s_{ig}=N)} (1 - \Lambda_{ig})^{\mathbf{1}(s_{ig}=C)}$$

Combining both probabilities, I can define the probability of  $i$ 's sequence of choices in all the lab games,  $y_i = \langle d_{i1}, \dots, d_{iM_i}, s_{i1}, s_{i2} \rangle$ :

$$P(y_i | \beta_i, \psi_i) = P(d_i | \beta_i) \times P(s_i | \beta_i, \psi_i)$$

Let  $\theta_i \equiv (\beta_i, \psi_i)$ , a vector of our parameters of interest. I assume that  $\theta_i \sim \mathcal{N}(\mu, \Sigma)$  and has a probability density function  $f(\cdot)$ . So the probability of  $i$ 's sequence of choices  $y_i$  is:

$$P(y_i | \mu, \Sigma) = \int P(y_i | \theta_i) \cdot f(\theta_i | \mu, \Sigma) d\theta$$

A mixed logit likelihood function represents the probability of observing all the decisions of all individuals:

$$L = \prod_{i=1}^N P(y_i | \mu, \Sigma)$$

---

<sup>5</sup>Notice a simpler alternative model would have  $\psi_i=0$ . I will test for this in the results section, show that in fact  $\psi_i>0$ , and discuss why this is the case.

Because the integrals in the likelihood function are hard to calculate, they are approximated through numerical simulations. The parameters  $\mu$  and  $\Sigma$  are estimated through simulated maximum likelihood, following Train (2009).

After estimating  $\mu$  and  $\Sigma$ , I can use them to subsequently estimate  $\theta_i \forall i$ . Using Bayes' rule, I can derive a distribution of  $\theta_i$  conditional on  $i$ 's sequence of choices  $y_i$ :

$$g(\theta_i|y_i, \mu, \Sigma) = \frac{P(y_i|\theta_i) \cdot f(\theta_i|\mu, \Sigma)}{P(y_i|\mu, \Sigma)}$$

Using  $g(\cdot)$ , I can calculate the mean of the distribution conditional on the choice sequence  $y_i$ , and use it as an estimator of  $\theta_i$ :

$$\bar{\theta}_i = \int \theta_i \cdot g(\theta_i|y_i, \mu, \Sigma)$$

Once again, this integral is approximated through simulations, following Train (2009).

It is worth noticing that this estimation procedure manages to use all the information in one single stage while keeping the essence of the identification strategy of the experimental design, which is to estimate social preferences separately from the coordination games. In this estimation, 80% of the observations used to estimate  $\mu_\beta$  come from the money allocation decisions. Intuitively, what the estimation will tend to do is to pick a  $\mu_\beta$  to fit the money allocation decisions, and pick a  $\mu_\psi$  to fit the coordination game decisions that remain unexplained, given the individual beliefs imputed,  $\tilde{P}_i(s_j=N)$ .

## 5. Policy analysis: RCT of a radio drama

After understanding the motives behind social failure, the next step is to study whether local policies are increasing cooperation and why. The policy I analyze is the production of media content to promote intergroup cooperation. In particular, I focus on radio dramas, a policy that has been widely popular in Sub-Saharan Africa. In Nigeria, NGOs are constantly creating new radio dramas to promote messages on different topics, such as women's empowerment and Covid-19. Moreover, radio dramas have been used to promote messages on conflict-related issues. For instance, radio dramas have tackled topics such as how fake news fuels conflict and the reintegration of former Boko Haram members into society.

In Nigeria, policymakers view radio dramas as a highly valuable strategy for addressing conflict, citing three primary reasons. First, fictional stories make it easier to discuss sensitive topics (Slater and Rouner, 2002). Delving into historical and contemporary conflict tends to evoke strong emotions in the listeners, which can make them less receptive to the intended message. A fictional story circumvents this challenge. Second, dramatized narratives help to increase attention and retention of the intended message (Kromka and Goodboy, 2019). In an environment saturated with numerous NGOs constantly employing different sensitization campaigns to promote cooperation, novel initiatives struggle to capture people's attention. Radio dramas stand out due to their engaging nature, and using narratives has been shown to increase message retention. Third, when compared to alternative policies on conflict, radio

dramas can be easily implemented in a wide range of contexts. Lately, intergroup contact interventions have received considerable attention. But these policies can only be carried out in very particular contexts, where the two communities in conflict live together and tensions are not so high that such intervention could lead to violence. Instead, radio dramas can be implemented in places where only one of the two groups in conflict live.

These three reasons help explain why radio dramas have become a popular policy in Africa and make them an interesting policy to study. In addition, research in social psychology on whether they can improve relationships between groups after conflict has found mixed results (Paluck, 2007), indicating the need for further investigation. My goal is to evaluate this policy using the experimental protocol in order to understand its effects on cooperation, spite and fear.

### 5.1. The radio drama

I partnered with a radio drama production company from Nigeria to create a *new* radio drama. This company has been hired to create the radio shows of some of the most important NGOs in Nigeria, like Search for Common Ground and UN Women. Creating a new radio show has important advantages. First, it ensures that the participants of the experiment have not previously heard the treatment radio show. Using an existing radio show would pose a problem because these are widely broadcasted, which means that the subjects of the experiment could have already been treated. Alternatively, one could use a radio show that was broadcasted in an area that does not cover Jos. However, this strategy would not be as effective since the messages of this type of radio drama are tailored to the specific situation of the place in which they are broadcasted. Importing a radio drama would not have the same effect. Moreover, creating a new radio show allowed us to have a story that directly addressed the motives I explore in this paper—that is, a story that spoke about spite and fear between communities in conflict.

A possible concern about creating a new radio show is that one might not be evaluating the exact same policy implemented by policymakers. On this, it is important to note that even though the NGOs pay for the shows, the creative process relies on the production company. To emulate the policy creation process as closely as possible, I follow the exact same steps that Nigerian NGOs take to create their radio shows. These steps are straightforward. (i) The NGO hires the production company to create a new radio show. (ii) The NGO provides one page of pointers stating the main message they want the show to convey. (iii) The production company gets back to the NGO with an outline of the story and how it conveys the message, and the NGO approves or makes comments. (iv) The production company writes the scripts for the episodes and sends them to the NGO for approval. (v) The production company records the show and delivers the final product to the NGO.

The radio drama we created is called *The Convergence*. It tells the story of two fictional communities that are in conflict, and where spite and unwarranted fears lead to both communities to miss out on mutually beneficial interactions. The resolution of the story conveys a message on how reevaluating fear



and letting go of spite can lead to both communities being better off. The show consisted of 24 episodes lasting between 10 to 15 minutes, and was available in both English and Hausa and participants could listen to whichever they preferred.

## **5.2. RCT design**

The RCT for the radio show was conducted in the following way. Initially, subjects were recruited to participate in two surveys, two months apart. To reduce demand effects, only at the end of the baseline survey enumerators asked participants if they were interested in participating in “a different project the surveying company was carrying out.” They told participants this project consisted of listening to a new radio drama a production company was releasing and providing feedback. Participants were also informed that they could listen to it sometime before the second survey they had agreed to take. Those who were interested were invited to sign up immediately and were told that they would receive more details in the following days through WhatsApp (using the contact number collected for the second survey.)

Individuals were randomly assigned to either the treatment or control groups. Participants received new episodes through WhatsApp four times a week (on Mondays, Wednesdays, Fridays and Saturdays) over a six-week period. To promote and monitor engagement, participants received every Saturday a quiz on the content of that week’s episodes. Answering correctly the quiz put people in a weekly lottery for two prizes of 2,000 naira, and gave them one entry to the two grand prizes of 50,000 naira, which were awarded at the end of the sixth week. The quizzes also asked for participants’ opinions on the radio show. The control group was sent a placebo radio show with a message on health. They also received weekly quizzes with the same scheme of prizes.

A week after the radio show ended, the endline survey started. Participants in treatment and control groups went again through the lab-in-the-field experiment, which allowed me to measure again their preferences, beliefs and cooperation to estimate the effects of the radio drama on each margin.

The treatment and control groups were balanced on baseline levels of cooperation, social preferences, beliefs, religion, sex, age and other characteristics. A balance table can be found in the Appendix. The attrition rate at endline was 5%.

## **5.3. Data collection**

The fieldwork of this project took place between December 2022 and February 2023. Data collection for each survey (baseline and endline) lasted for two weeks. The treatment took place for six weeks in between surveys. At baseline, the team in the field surveyed 997 people from 41 Jos communities. The sample consisted of 499 Christians and 498 Muslims, both men and women aged between 18 and 60 years. Participants were required to have access to a phone with WhatsApp and be available to participate in a second survey two months later.

The recruiting process was the following. Every morning a pair of enumerators of the same religion visited a community of their religion. When in the community, enumerators picked a random starting point (like a school or water source) and started walking in opposite directions. To select a house to survey, they followed a 3/4 pattern, knocking on the 3rd house away from the starting point, then the 4th house from there, then the 3rd house from there, and so on. If someone answered the door, enumerators would briefly explain the survey and ask if someone in the household was interested in participating. If someone accepted, the lab-in-the-field experiment was carried out immediately at the person's home. Enumerators were instructed to maintain a balanced sample in age and gender. On average, the survey took around 45 minutes to complete.

At the end of the survey, enumerators asked participants if they wanted to participate in the radio show project. Because this implied no extra effort, everyone agreed to be contacted for this. Some days after the baseline was completed, we created two WhatsApp groups, one for the treatment group and another one for the control group. In them, we welcomed everyone to the radio show project and explained the logistics of it. Through the WhatsApp groups we sent the episodes of the radio drama and the link to the weekly quizzes, and announced the winners of the prizes. Only administrators could speak in these groups.

After the radio show ended, enumerators visited the communities again. Using the registered phones, enumerators contacted the participants and schedule appointments to carry out the endline survey. 947 participants from baseline participated in the endline survey—an attrition rate of around 5%.

For each survey, participants got compensation between 700 and 1,700 naira, depending on the results of the different lab games. In Jos, 1000 naira is approximately the payment for four hours of work. These payments were made in cash immediately after the survey ended. The payments of the quiz lotteries were made directly to winners' accounts via phone transfer as soon as the winners were announced.

## 6. Results

### 6.1. Descriptive evidence

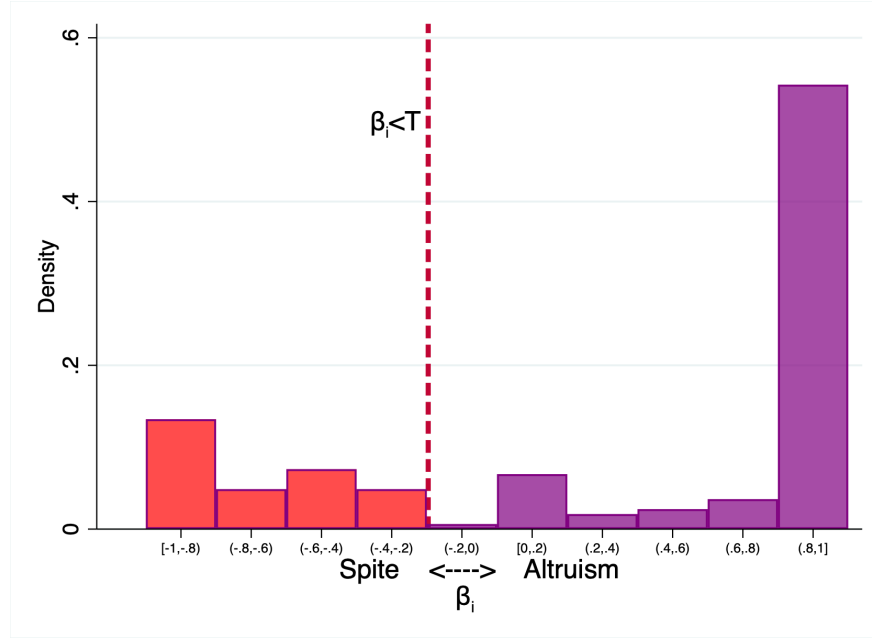
The first result is that in a game where cooperating is an equilibrium and the maximum payoff (which represents half a day of salary), people fail to cooperate in 31% of the interactions between groups—compared to only 6% of interactions within groups. This results in a 9.6% loss of total wealth in inter-group interactions.

Figures 1 and 2 displays three main diagnostic facts drawn from the baseline lab-in-the-field experiment. The social preferences parameters shown in these figures were estimated following the approach described in Section 3.2.

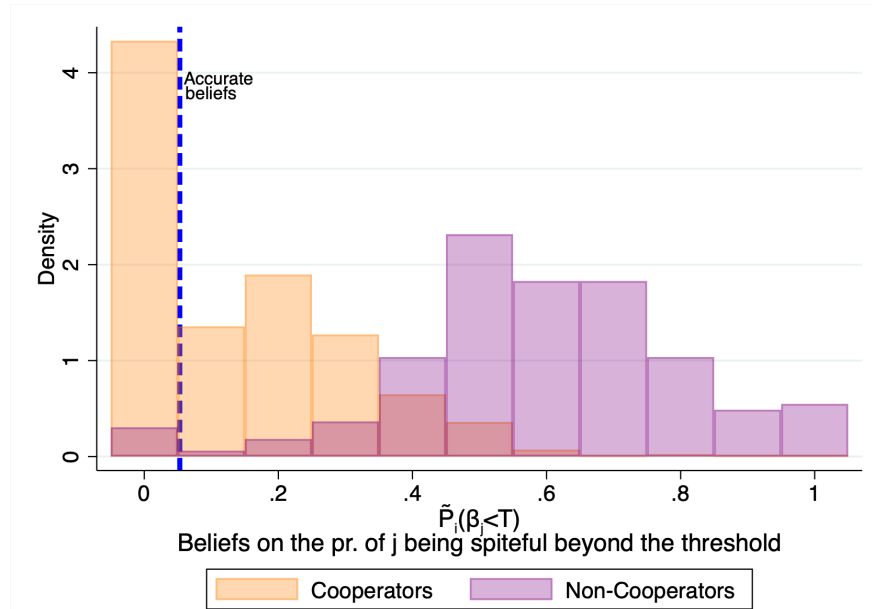
Figure 1A presents a histogram of the social preferences of the participants who decided to not

Figure 1

## A. Social Preferences (for Outgroup) of Non-Cooperators



## B. Beliefs about the Outgroup, by Game Strategy



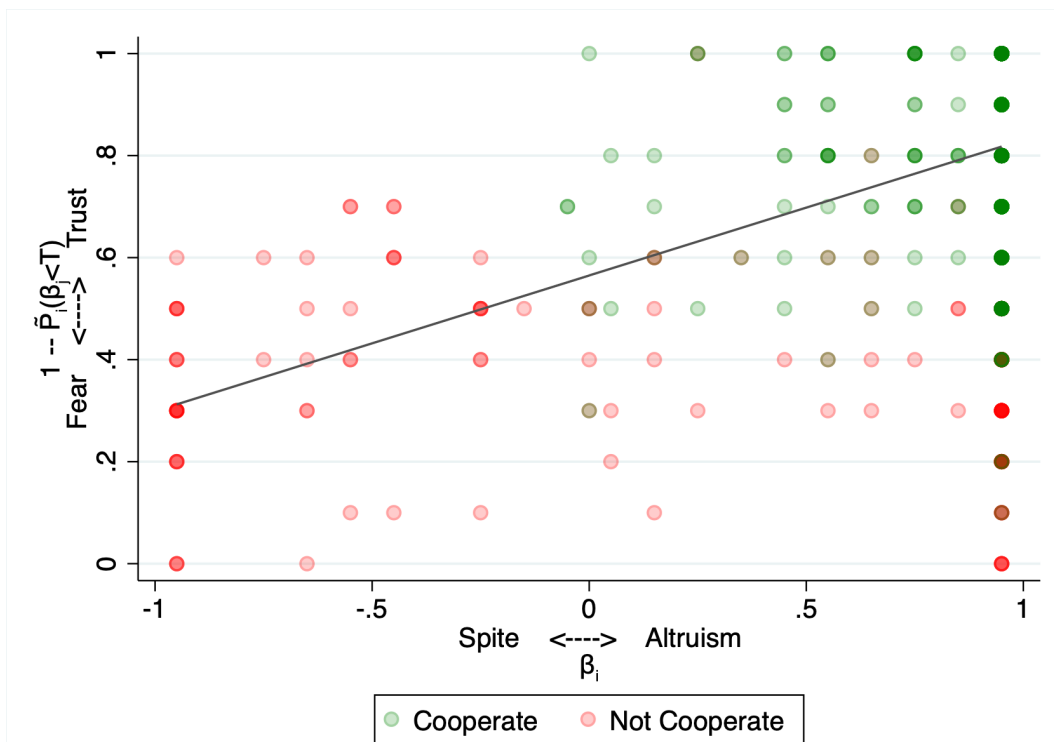
Notes: Figure 1A presents the social preferences ( $\beta_i$ ) of non-cooperators, estimated following the approach described in Section 3.2. The red line represents the threshold point where a person becomes spiteful enough to want to not cooperate out of spite. Figure 1B displays the beliefs about probability that an outgroup member has a level of spite beyond the threshold,  $P(\beta_j < T)$ , and therefore want to not cooperate out of spite. The blue line represents the actual probability of this event happening.

cooperate in the main coordination game of the experiment. The x-axis shows social preferences ranging from fully spiteful,  $\beta_i = -1$ , to fully altruistic,  $\beta_i = 1$ . The red line represents the threshold point where a person becomes spiteful enough to want to not cooperate out of spite. Figure 1A provides an initial classification of motives for non-cooperation. The 30% of non-cooperators who fall to the left of the red line chose to not cooperate out of spite. On the other hand, the 70% that fall to the right did not have a

spiteful motive to not cooperate, but did so out of fear. It is also worth noting the rightmost bar, which indicates that 52% of non-cooperators are in fact fully altruistic.

Figure 1B displays a histogram of the beliefs about the outgroup, for cooperators and non-cooperators. Specifically, it shows what people believe is the likelihood that an outgroup member wants to not cooperate out of spite. That is, the probability that an outgroup member has a level of spite beyond the threshold,  $P(\beta_j < T)$ . The blue line shows the actual probability of this event happening, which is 5%. On average, cooperators believe that 14% of the outgroup will not cooperate out of spite, while non-cooperators believe 59% will do so. This means that non-cooperators exaggerate the number of spiteful people in the outgroup by around 12 times. This fact is evidence of how unfounded fears play a central role in cooperation failure.

Figure 2: Preferences, Beliefs and Cooperation



*Notes:* This figure shows the relationship between preferences, beliefs and cooperation for the entire sample. The x-axis shows participants' social preferences towards the outgroup, and the y-axis shows participants' beliefs about the outgroup's social preferences towards the ingroup (more specifically, beliefs on the proportion of outgroup members that are not spiteful beyond the threshold). Each participant is represented by a dot. The dot is green if the participant cooperated and red if they did not. The dots overlap and are translucent. This creates varying opacities, with darker dots indicating a higher density of people at that preference-belief level. In addition, the greener a dot is, the more cooperation there is at that preference-belief level, and the redder it is, the more non-cooperation there is. The black line represents the fitted values. Social preference showcased here were estimated following the approach described in Section 3.2.

Figure 2 shows the relationship between preferences, beliefs and cooperation for the entire sample. The x-axis shows participants' social preferences towards the outgroup, and the y-axis shows participants' beliefs about the outgroup's social preferences towards the ingroup (more specifically, beliefs on the proportion of outgroup members that are not spiteful beyond the threshold). Each participant is represented by a dot. The dot is green if the participant cooperated and red if they did not. The dots

overlap and are translucent. This creates varying opacities, with darker dots indicating a higher density of people at that preference-belief level. In addition, the greener a dot is, the more cooperation there is at that preference-belief level, and the redder it is, the more non-cooperation there is. The black line represents the fitted values.

Figure 2 has a few facts worth noticing. First, there is a positive correlation between spite and fear, on average. This may be due to projection bias, whereby individuals assume that others behave similarly to themselves. Second, the correlation between spite and fear is stronger on the negative end of the social preferences spectrum. Spiteful people are very likely to be fearful too, whereas altruistic people show a wider range of beliefs, from trusting or fearful. Third, in line with the theory, everyone with social preferences below  $-.2$  does not cooperate, while those with social preferences higher than  $-.2$  are more likely to not cooperate as the level of fear increases.

### Heterogeneity by religion

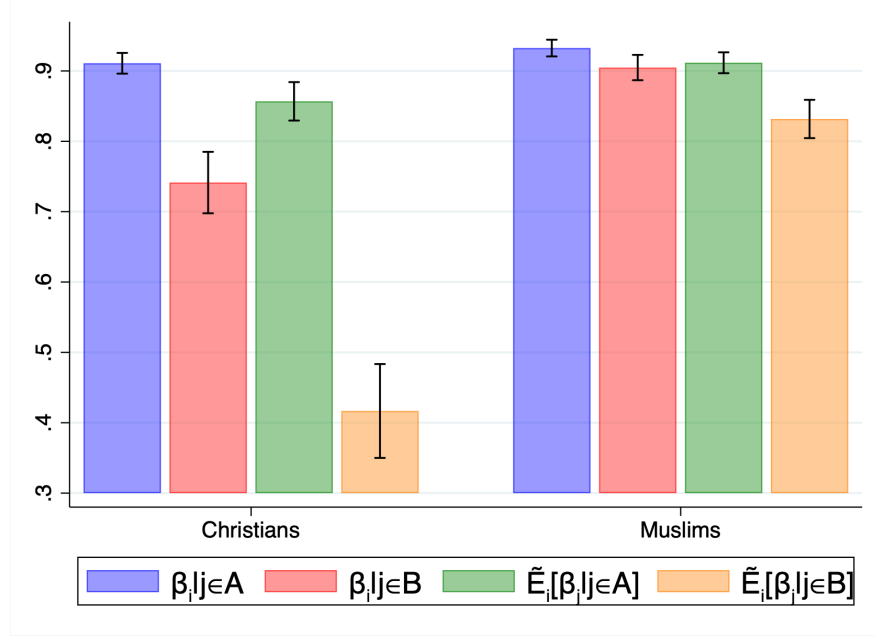
Do the two religious groups under study act in a similar way? Or if there is heterogeneity in behavior, in which direction does it go? In terms of cooperation, the difference is stark. Out of all the people who decided to not cooperate with the outgroup, 84% were Christians, while only 16% were Muslims. Figure 3 reports heterogeneity in social preferences and beliefs.

Figure 3A reports the following averages, from left to right: social preferences for the ingroup ( $\beta_i | j \in A$ ), social preferences for the outgroup ( $\beta_i | j \in B$ ), beliefs on the mean social preferences ingroup members have for the ingroup ( $\tilde{E}_i[\beta_j | j \in A]$ ), beliefs on the mean social preferences outgroup members have for the ingroup ( $\tilde{E}_i[\beta_j | j \in B]$ )—this for Christians and Muslims. When comparing the blue and the red bars, the figure shows that both Christians and Muslims have more positive social preferences with the ingroup than the outgroup. However, the gap between average social preferences for the ingroup vs. the outgroup is much greater in Christians than in Muslims. In addition, comparing both red bars evidence how Christians have worse social preferences toward Muslims than Muslims toward Christians. In terms of beliefs about the ingroup, comparing the green and the blue bars shows that both groups have somewhat accurate (although a little pessimistic) beliefs on how ingroup members treat other ingroup members. Finally, regarding the beliefs about the outgroup, comparing the yellow bar of one group with the red bar of the other group reveals an important difference between religious groups: Christians have very inaccurate and pessimistic beliefs about how Muslims treat them. Instead, if anything, Muslims have somewhat optimistic beliefs on how Christians treat them.

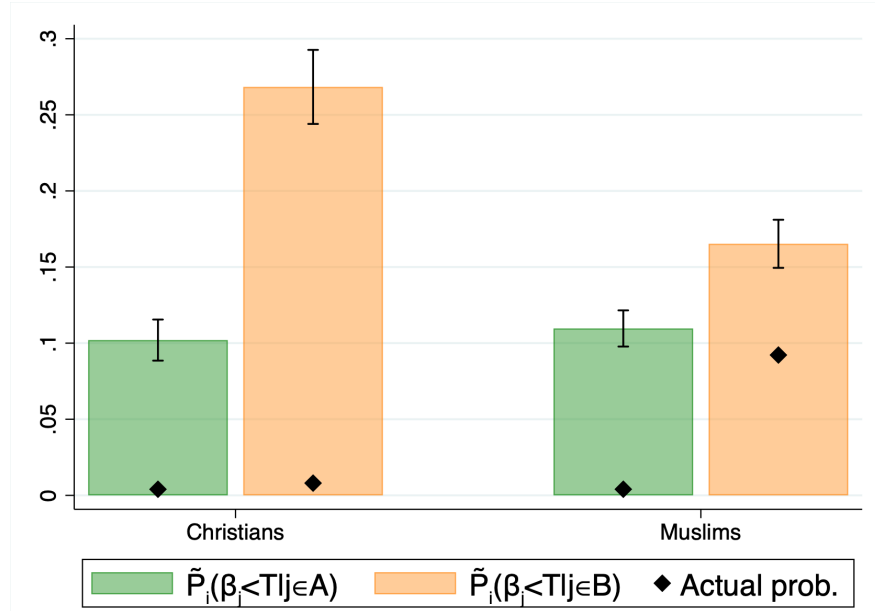
Figure 3B presents more evidence of the heterogeneity in misperceptions between groups by looking at beliefs about the tail of the distribution of social preferences—that is, beliefs about the percentage of people that are spiteful beyond the threshold,  $\tilde{P}_i(\beta_j < T)$ . Blue bars report beliefs about the ingroup and red bars beliefs about the outgroup. The first thing to note is that both groups exaggerate the percentage of spiteful people there is in both the ingroup and the outgroup. In terms of perceptions

Figure 3: Heterogeneity by Religion

*A. Preferences and Beliefs about the Mean*



*B. Beliefs about the Tail*



*Notes:* In both figures,  $j \in A$  means that  $j$  belongs to the ingroup, and  $j \in B$  means that he belongs to the outgroup. Figure 3A reports the following averages, from left to right: social preferences for the ingroup ( $\beta_i | j \in A$ ), social preferences for the outgroup ( $\beta_i | j \in B$ ), beliefs on the mean social preferences ingroup members have for the ingroup ( $\tilde{E}_i[\beta_j | j \in A]$ ), beliefs on the mean social preferences outgroup members have for the ingroup ( $\tilde{E}_i[\beta_j | j \in B]$ )—this for Christians and Muslims. Figure 3B presents beliefs about the tail of the distribution of social preferences, for Christians and Muslims.  $\tilde{P}_i(\beta_j < T | j \in A)$  is the beliefs on the percentage of ingroup members that are spiteful beyond the threshold.  $\tilde{P}_i(\beta_j < T | j \in B)$  is the beliefs on the percentage outgroup members that are spiteful beyond the threshold. The diamonds represent the actual percentage of people that are spiteful beyond the threshold for each case. In both figures, the black lines represent standard errors.

about the ingroup, the bias is very similar between the groups. However, respecting perceptions about the outgroup, the bias differs considerably. Christians believe that 26% of Muslims are spiteful beyond

the threshold, while Muslims beliefs 16% of Christians are. In addition, because there are fewer spiteful Muslims than Christians, Christians exaggerate the amount of spiteful people in the outgroup by 24 percentage points, while Muslims exaggerate this amount by 7 percentage points.

In sum, in this context, Christians are less cooperative than Muslims, and also have worse social preferences towards Muslims and more biased beliefs about them than the other way around. Importantly, in the pre-analysis plan I pre-registered that heterogeneity in these three outcomes would go in this direction, based on the focus groups done in the exploratory phase of this project. The main reason why this is the case is probably the salience of Boko Haram, the major armed group in the country, which distorts the beliefs of only one group and generates negative feelings towards only one group. Another feature in this context that probably plays a role is the location of Jos. Although the city has a very similar number of Christians and Muslims, most of the nearby cities outside of Plateau State are part of the Muslim north. This creates the feeling in some that the Christians in Jos are surrounded by Muslim communities and that therefore their presence in the area is threatened.

## 6.2. Estimated model and conterfactual analysis

Table 1 reports the results of the simulated maximum likelihood estimation of the random coefficients model presented in Section 4. The parameters of interest are  $\beta_i$ , the social preferences for the outgroup, and  $\psi_i$ , the loss aversion, both for all  $i$ . I estimate the mean and variance of the distribution where these parameters are drawn from.

Table 1: Random Coefficients Estimation

	Coefficient	Stand. Err.	
$\mu_\beta$	0.922	0.072	***
$\sigma_\beta$	0.420	0.059	***
$\mu_\psi$	532.7	108.6	***
$\sigma_\psi$	469.2	163.7	***
Observations		9,006	
Clusters		997	

*Notes:* This table reports the results of the simulated maximum likelihood estimation of the random coefficients model presented in Section 4. Each observation is one decision of one participant in either a money allocation decision or a coordination game.  $\mu_\beta$  and  $\sigma_\beta$  are the mean and variance of the distribution of the parameters of social preferences.  $\mu_\psi$  and  $\sigma_\psi$  are the mean and variance of the distribution of the parameters of loss aversion. Standard errors are clustered at the individual level.

The first thing to note is that all parameters are precisely estimated—all four parameters in Table 1 are significant at the 1% level. The estimated  $\mu_\beta$  shows that, on average, people are highly altruistic towards the outgroup. In addition,  $\sigma_\beta$  indicates the level of dispersion of social preferences around the mean. The estimated  $\mu_\psi$  shows that the penalty on getting the “sucker payoff” is not only significant but considerable, being half the amount of the maximum payoff in the game. Nevertheless, the size of  $\sigma_\psi$  highlights how this penalty varies considerably in the population.

The fact that  $\psi_i > 0$  warrants some discussion. The reason why this is the case is that half of the people who did not cooperate are fully altruistic, as shown in Figure 1. If  $\psi_i=0$ , a fully altruistic person would want to not cooperate only if she believes that  $P(s_j=N)>0.86$ . However, fully altruistic non-cooperators believe, on average, that  $P(s_j=N)=0.6$ . This implies that the potential costs of cooperating go beyond the monetary one. In other words, the risk of cooperating entails also paying a psychological cost when the other person does not cooperate.<sup>6</sup>

Using the estimated distributions, I estimate individual-level parameters to assign a  $\beta_i$  and  $\psi_i$  to each participant, following the procedure explained in Section 4. I use these parameters and the elicited beliefs in the analysis that follows. First and importantly, the model performs well in terms of sample fit: using the estimated individual-level parameters and the elicited beliefs, I correctly predict 94% of the decisions in the coordination game. This is indication that the core drivers of non-cooperation are captured by my model and measurements.

To determine to what extent non-cooperation is driven by spite vs. fear, I shut down the fear motive in participants' expected utility and observe how many non-cooperators would still prefer to not cooperate. By doing this I determine what percentage of people do not cooperate purely out of spite and what percentage require fear to decide to not cooperate. More specifically, what I do to shut down the fear channel is to set to zero participants' beliefs on the probability that  $j$  will not cooperate—that is, I set  $\tilde{P}_i(s_j=N)=0$  for all  $i$ . Doing so reduces the expected utility to  $W(s_i)=u(s_i, C)$ . Because I have estimated  $\beta_i$  for all  $i$ , I can calculate  $u(s_i, C)$  and determine for each  $i$  if  $u(N, C)>u(C, C)$ . As shown before,  $u(N, C)>u(C, C)$  means that  $\beta_i<T$ , which allows me to conclude that  $i$  chooses to not cooperate out of spite (and regardless of beliefs). I can also conclude that non-cooperators for whom  $u(N, C)<u(C, C)$  needed fear to choose to not cooperate—that is, they chose to not cooperate out of fear.

I find that 24% of the people who do not cooperate do so out of spite, while 76% do so out of fear. It is reassuring to see that the proportions of people not cooperating out of spite and fear estimated by this approach are remarkably similar to the ones that resulted from the simpler approach presented in the previous section. Notice it is not clear *a priori* this was going to be the case, as one approach calibrates individual-level parameters out of 8 money allocation decisions, while the other uses everyone's full set of decisions to estimate the distribution of parameters, and then uses an individual's decisions to determine where in the estimated distribution the individual's parameter is likely to be. The fact that the resulting numbers from both approaches are so similar suggests that they are not the artifact of a particular specification or estimation method.

I then turn to counterfactual analysis to study how policies that shift the drivers of conflict would af-

---

<sup>6</sup>An alternative approach to modeling this situation would be to have an expected utility function with risk aversion. I discard this approach because at this level of prices individuals should not exhibit risk aversion. Indeed, this is what I find in the survey. Using a canonical survey module to measure risk aversion, I find that over 90% of individuals are risk-neutral. The behavioral literature suggests that at low prices, behavior is better explained by loss aversion than risk aversion (Rabin, 2000; DellaVigna, 2018).



fect cooperation. First, I investigate how would cooperation change if a policy solved unwarranted fears by correcting misperceptions about the outgroup. In other words, I investigate how would cooperation change if people had accurate beliefs about the percentage of people in the outgroup that is spiteful. To do this, I replace everyone's subjective beliefs on the probability that an outgroup member does not cooperate out of spite with the empirical probability of this event happening—that is, I replace  $\tilde{P}_i(\beta_j < T)$  with  $P(\beta_j < T)$  for all  $i$ . Notice that I can calculate  $P(\beta_j < T)$  because I have estimated  $\beta_i$  for all  $i$ . Then, I calculate the expected utilities with the newly imputed beliefs and observe in this scenario how many people prefer to not cooperate,  $W(N) > W(C)$ . I find that if a policy solved unwarranted fears by correcting inaccurate beliefs about the outgroup, the number of people not cooperating would drop by 73%. This means that 96% of the people who do not cooperate out of fear do so due to misperceptions (recall that 76% of people do not cooperate out of fear, so  $73\%/76\%=96\%$ ). This result underlines how misperceptions leading to unwarranted fears are the single most important barrier to intergroup cooperation, and how policies should focus on tackling misperceptions to maximize their effectiveness.

I then investigate how would cooperation change if a policy were to reduce spite. I first simulate a policy that completely eradicates intergroup spite, such that nobody wants to not cooperate out of spite. To do this, I replace the social preferences of all spiteful people (those with  $\beta_i < 0$ ) with selfish social preferences ( $\beta_i = 0$ ). Then, I calculate the expected utilities with the newly imputed preferences and observe in this scenario how many people prefer to not cooperate,  $W(N) > W(C)$ . I find that if a policy completely eradicated intergroup spite, the number of people not cooperating would drop by only 5%. This means such a policy would only manage to convince 21% of the people not cooperating out of spite to switch to cooperation (recall that 24% of people do not cooperate out of spite, so  $5\%/24\%=21\%$ ). The reason why the effect is so small is that spiteful individuals tend to also be very fearful, so even without spite, most of them will still want to not cooperate out of fear. Interestingly, when I simulate a policy that increases altruism by half a standard deviation (0.22) on altruistic people (those with  $\beta_i > 0$ ), the number of people not cooperating drops by 7%. This drop happens because for a higher  $\beta_i$  the level of fear needed to justify not cooperating is higher too (as shown in section 3.1). This means that policies that can increase social preferences would be more effective in increasing cooperation if they focused on increasing altruism on altruistic people with fear, than on reducing spite on spiteful people. These results highlight how, in this context, policies that are effective at changing social preferences would be ineffective in increasing cooperation, especially if they only targeted spiteful people.

The main policy recommendation that can be drawn from these results is that policies that reduce fear would be significantly more effective in increasing cooperation than policies that reduce spite. This is not only because policies for spite affect a smaller percentage of non-cooperators (24% vs 76%), but also because they manage to switch to cooperation a smaller percentage of the population being they target (20% vs 96%).

## Data supporting the counterfactual analysis

I now use the endline data to look for further evidence supporting the lessons from the counterfactual analysis. I test for the following three lessons. First, to increase cooperation, changing beliefs should be more important than changing preferences. Second, if only preferences are changed, the effects on cooperation should be very small. Third, if only beliefs change, the effects on cooperation should be considerable. To test these, I run the regression below to study how changes in preferences and beliefs between baseline and endline affected changes in cooperation decisions.

$$\Delta Cooperate_i = \phi_0 + \phi_1 \Delta Preferences_i^{std} + \phi_2 \Delta Beliefs_i^{std} + \varepsilon_i$$

Here,  $\Delta Cooperate_i$  is an indicator variable equal to 1 if  $i$  switched to cooperate by endline, 0 if there was no change, and -1 if  $i$  switched to not cooperate.  $\Delta Preferences_i^{std}$  is a standardized variable of the change in social preferences  $i$  had between the baseline and endline surveys.  $\Delta Beliefs_i^{std}$  is a standardized variable of the change between surveys  $i$  had in her beliefs on the probability that the  $j$  will not cooperate out of spite. Importantly, because the two regressors are standardized, their coefficients can be compared to determine their relative importance. Note, however, that no exogenous variation is being considered here, so the results are just correlational. Table 2 reports the results.

Table 2: Testing Lessons from the Counterfactual Analysis

	$\Delta Cooperation$		
	(1)	(2)	(3)
$\Delta Preferences$	0.105*** (0.013)	-0.004 (0.003)	
$\Delta Beliefs$	-0.191*** (0.013)		-0.184*** (0.016)
Only if $\Delta Pref.=0$	N	N	Y
Only if $\Delta Beliefs=0$	N	Y	N
Mean Dep.Var.	.113	.113	.113
Observations	947	316	787

*Notes:* This table reports correlational effects of the change in preferences and beliefs between the baseline and the endline survey on the change in cooperation.  $\Delta Cooperate$  is an indicator variable equal to 1 if  $i$  switched to cooperate by endline, 0 if there was no change, and -1 if  $i$  switched to not cooperate.  $\Delta Preferences$  is a standardized variable of the change in social preferences  $i$  had between the baseline and endline surveys.  $\Delta Beliefs$  is a standardized variable of the change between surveys  $i$  had in her beliefs on the probability that the  $j$  will not cooperate out of spite. Column 1 includes all subjects. Column 2 restricts the sample to only those subject who showed no change in beliefs between surveys. Column 3 restricts the sample to only those subject who showed no change in social preferences between surveys. Standard errors are clustered at the individual level.

Column 1 reports that changes in both preferences and beliefs mattered in the decision to switch to cooperation by endline. Crucially, however, the coefficients highlight how changing beliefs was twice as important as changing preferences for a switch to cooperation. An increase of social preferences by one standard deviation increases the probability of switching to cooperation by around 10%, while a decrease

in one standard deviation on negative beliefs increases in around 20% the probability of switching to cooperation.

I then look at the cases where only preferences changed or where only beliefs changed. Column 2 shows that for the cases where only preferences changed but beliefs remained the same, an increase in social preferences did not translate into an increase in cooperation. On the other hand, Column 3 shows that for the cases where only beliefs changed but preferences did not, a decrease in negative beliefs did translate into an increase in cooperation. Furthermore, the effect in Column 3 is very similar in magnitude to the one found in Column 1. Hence, the results reported in Table 2 bring further evidence to support the lessons drawn from the counterfactual analysis.

### 6.3. Effects of the radio show

The main specification to study the effects of the radio show is the following.

$$Y_i = \gamma_0 + \gamma_1 Treated_i + \gamma_2 X_i + \varepsilon_i$$

Where  $Y_i$  is an outcome variable;  $Treated_i$  is a dummy variable equal to 1 if  $i$  belonged to the treatment group; and  $X_i$  is a vector of controls that includes the outcome variable at baseline, plus other characteristics like religion, sex and age.

Table 3 reports the main effects of the radio show on spite, fear and cooperation. Columns 1 and 2 report the effects on spite. The outcome variable in these columns is the negative of the social preferences for the outgroup,  $-\beta_i$ , such that a negative coefficient represents a reduction in spite. The preferences parameters used were estimated following Section 3.2. Columns 3 and 4 report the effects on fear, or the beliefs about the percentage of the outgroup that would want to not cooperate out of spite,  $\tilde{P}_i(\beta_j < T)$ . Columns 5 and 6 report the effect on the decision to not cooperate in the coordination game.

Table 3A reports the results for the full sample. Columns 1 and 2 show that the radio show reduced spite, although the effect is small in magnitude. Columns 3 and 4 indicate that the radio show had no effect on fear. However, it is worth noting that the point estimates go in the right direction, towards reducing negative beliefs. Columns 5 and 6 show that the show had no effects on cooperation either, although the point estimate has the expected sign.

It is important to note that this first specification might be underestimating the effects of the radio show because it estimates the effects over the full sample, where there are many subjects who are mechanically unresponsive to the treatment because their outcome variable cannot improve from baseline. In other words, many subjects were fully altruistic ( $\beta_i = 1$ ) or had fully optimistic beliefs ( $\tilde{P}_i(\beta_j < T) = 0$ ) at baseline, and therefore they would always show an effect equal to zero, at best. These zeros are not informative of the effectiveness of the policy. Because of this, I run the same regressions restricting the sample to individuals who could improve in the outcome variable of the respective column. Results are

Table 3: Main Effects

<i>A. Full sample</i>						
	Spite $-\beta_i$		Fear $\tilde{P}_i(\beta_j < T)$		Non-Cooperation $s_i = N$	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.026** (0.013)	-0.026** (0.012)	-0.012 (0.011)	-0.012 (0.010)	-0.012 (0.015)	-0.014 (0.014)
Controls	N	Y	N	Y	N	Y
Mean Dep.Var.	-.823	-.823	.218	.218	.169	.169
Observations	947	947	947	947	947	947
<i>B. Removing subjects that are mechanically unresponsive</i>						
	Spite $-\beta_i$		Fear $\tilde{P}_i(\beta_j < T)$		Non-Cooperation $s_i = N$	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.172** (0.067)	-0.190*** (0.065)	-0.021 (0.015)	-0.021 (0.014)	-0.086 (0.067)	-0.073 (0.068)
Controls	N	Y	N	Y	N	Y
Mean Dep.Var.	-.079	-.079	.343	.343	1	1
Observations	138	138	600	600	160	160

*Notes:* This table reports the treatment effect of the radio show.  $-\beta_i$  is negative of the social preferences for the outgroup estimated following the approach presented in Section 3.2.  $\tilde{P}_i(\beta_j < T)$  is the beliefs on the percentage of the outgroup that will not cooperate out of spite.  $s_i = N$  is the decision to not cooperate in the coordination game. The controls are the outcome variable at baseline, religion, sex and age. Table 1A report results for the full sample. Table 2A restricts the sample to individuals who were not mechanically unresponsive in the outcome variable of the respective column. Standard errors are clustered at the individual level.

reported in Table 3B. Columns 1 and 2, show there was a reduction in spite that is considerably greater than the one previously estimated. In particular, Column 2 indicates that listening to the radio show reduced spite by 0.19 units for this groups, which is 0.45 of a standard deviation. Columns 3 and 4 still show there were no effects on beliefs, although the point estimates doubles with respect to the previous estimation. And Columns 5 and 6 show that there is still no effect on cooperation, although the point estimates increases notoriously.

Finding that the radio show reduces spite but does not increase cooperation could have been a puzzling result that would make it difficult to conclude if the policy was ultimately effective or not. However, this result becomes easy to understand when viewed through the lens of the analysis done in Section 6.2. The radio show is an effective policy because it reduces spite, but it is the wrong policy for this context because it does not affect the key motive for conflict, which is fear. This ultimately renders the policy ineffective at achieving its main goal, increasing cooperation.

Figure 2 can also help illustrate this result. Consider the case of individuals with preferences  $\beta_i = -.25$ ,

who have spiteful preferences just above the threshold. And notice that these participants believe that at least 40% of the outgroup is spiteful. The radio show moved the dots of these individuals to the right, leaving them close to  $\beta_i = -.05$ , and effectively removing their spiteful motive to not cooperate. However, the radio show does not affect beliefs, so these dots do not move vertically. Importantly, the theory presented in Section 3 states that with  $\beta_i = -.05$ , it is enough to believe that 23% of the outgroup is spiteful to want to not cooperate. Therefore, these participants will still want to not cooperate out of fear, based on this simple graphical counterfactual.

This result underscores the importance of understanding the underlying drivers of an issue in order to accurately assess the effectiveness of a policy. Without understanding the relative importance of these drivers, it is difficult to determine whether a seemingly ineffective policy is ineffective in general or only in the specific context where it was evaluated because it was not aligned with the needs of that context. Furthermore, even if the policy proves effective, without understanding which drivers it shifted it will be hard to recommend its implementation in other contexts as it will be hard to assess if it correctly aligns with the needs of that other context.

Table 4: Heterogeneous effects

	Spite $-\beta_i$	Fear $\tilde{P}_i(\beta_j < T)$	Non-Cooperation $s_i = N$	
	(1)	(2)	(3)	(4)
Treated x Spite <sub>t=0</sub>	0.093*** (0.033)		0.003 (0.029)	
Treated x Fear <sub>t=0</sub>		-0.024* (0.013)		-0.018 (0.022)
Treated	-0.026** (0.012)	-0.012 (0.010)	-0.012 (0.014)	-0.012 (0.014)
Controls	Y	Y	Y	Y
Mean Dep.Var.	-.823	.218	.169	.169
Observations	947	947	947	947

*Notes:* This table reports the heterogenous treatment effect of the radio show.  $-\beta_i$  is negative of the social preferences for the outgroup estimated following the approach presented in Section 3.2.  $\tilde{P}_i(\beta_j < T)$  is the beliefs on the percentage of the outgroup that will not cooperate out of spite.  $s_i = N$  is the decision to not cooperate in the coordination game. Spite<sub>t=0</sub> and Fear<sub>t=0</sub> refer to the outcome variables of Column 1 and 2 at baseline. The controls are the outcome variable at baseline, religion, sex and age. Standard errors are clustered at the individual level.

I now look at how treatment effects varied depending on how spiteful or fearful participants were at baseline. To do this, I run the same specification as before and add as a regressor the interaction between the treatment variable and preferences or beliefs at baseline. Results are reported in Table 4. With respect to social preferences, Column 1 shows that the effects were strongest for the most spiteful people. It is not obvious *a priori* that this would be the case, as it was reasonable to think that the most spiteful individuals would have more rigid preferences. However, I find that social preferences

increase by around 0.12 units in fully spiteful people and that this effect decreases progressively until there is none in people with  $\beta_i=0.3$ . It is worth recalling that, according to the counterfactual analysis of Section 7.2, the radio show would be more effective in increasing cooperation if it increased the social preferences of altruistic people than of spiteful people. Therefore, because the effect of the radio show is concentrated on the most spiteful people, it was even less likely that this effect would translate into increased cooperation. With respect to beliefs, Column 2 shows some evidence that the radio show did reduce fear, and that this effect was the strongest in the people with the greatest misperceptions. Taken at face value, the coefficient says that people who believed that 100% of the outgroup was spiteful adjusted their beliefs to 76%. Finally, Columns 3 and 4 look at how the heterogeneous effects on spite and fear could have differentially affected cooperation. However, I find no evidence that the effects identified in Columns 1 and 2 translated into increased cooperation.

One potential threat to the results in this section is that they are driven by social desirability bias. Despite the choices made on the experimental design to reduce demand effects, one might be concerned that the treatment group could express more social desirability bias than the control group. Participants who listened to a radio show that aimed to promote intergroup cooperation might disingenuously express more positive attitudes towards the outgroup to present themselves in a good light to the surveyors. I now show evidence that this was not the case. Following Dhar, Jain & Jayachandran (2022), I include in the baseline survey a module that measures a person's propensity to give socially desirable answers. The module asks respondents if they have several too-good-to-be-true traits such as never being jealous, lazy or resentful. Those who report more of these traits are scored as having a higher propensity to give socially desirable answers. I use these individual-level scores to see if subjects with a higher propensity to have social desirability bias seem to be more positively affected by the radio show (which could drive the results). To test for this I run the following regression.

$$Y_i = \eta_0 + \eta_1 Treated_i + \eta_2 SDS_i + \eta_3 Treated_i \times SDS_i + \eta_4 X_i + \varepsilon_i$$

Where  $Y_i$  is an outcome variable;  $Treated_i$  is a dummy variable equal to 1 if  $i$  belongs to the treatment group;  $SDS_i$  is a variable from 1 to 10 indicating how many socially desirable answers  $i$  gave in that survey module; and  $X_i$  is a vector of controls that includes the outcome variable at baseline, plus other characteristics like religion, sex and age. Table 5 reports the results.

First, Row 3 indicates that participants with a higher tendency to give socially desirable answers indeed expressed less spite and less fear towards the outgroup in the survey. This result is also a validation of the measurement of social desirability provided by the survey module. Crucially, Row 1 shows that the tendency to give more socially desirable answers was not higher in the treatment group vs. the control group, which indicates that listening to the radio show did not increase demand effects. In addition, Column 1, Row 2, shows that the treatment effect on spite survives controlling for social desirability, and what is more, this effect is of the exact same magnitude as that reported in the main specification

Table 5: Social Desirability Bias

	Spite $-\beta_i$	Fear $\tilde{P}_i(\beta_j < T)$	Non-Coo. $s_i = N$
	(1)	(2)	(3)
Treated x SDS	0.007 (0.007)	0.007 (0.005)	0.002 (0.008)
Treated	-0.026** (0.012)	-0.012 (0.010)	-0.013 (0.014)
Soc.Des.Score	-0.007* (0.004)	-0.007* (0.004)	-0.009 (0.006)
Controls	Y	Y	Y
Mean Dep.Var.	-.823	.218	.169
Observations	947	947	947

*Notes:* This table reports the treatment effect of the radio show controlling for social desirability bias.  $-\beta_i$  is negative of the social preferences for the outgroup estimated following the approach presented in Section 3.2.  $\tilde{P}_i(\beta_j < T)$  is the beliefs on the percentage of the outgroup that will not cooperate out of spite.  $s_i = N$  is the decision to not cooperate in the coordination game. *Soc.Des.Score* and *SDS* refer to the individual-level social desirability bias score. The controls are the outcome variable at baseline, religion, sex and age. Standard errors are clustered at the individual level.

(Table 2A). Taken together, these results provide evidence that the effects of the radio show appear robust to issues of social desirability bias.

Finally, I address the issue that only 30% of the participants answered at least one of the quizzes about the radio show. It is hard to know to what extent this percentage reflects the number of people who actually listened to the radio show. However, it suggests that it might have been the case that a majority of people did not listen to the radio show. If this is the case, one would like to estimate the treatment effect on the treatment. Under the assumption that everyone who listened to the radio show answered at least one quiz and everyone who did not listened answered no quiz, I can use the quizzes answers as an IV to estimate the ATT. The results of this exercise are reported in the Appendix. I find the ATT on spite is -0.08 units, significant at the 5% level. I find no effects on fear or cooperation, consistent with the above findings.

## 7. Conclusion

This paper studies the extent to which intergroup conflict is driven by spite vs. fear, and how local policies affect these channels to increase cooperation. I find that non-cooperation is driven 24% out of spite and 76% out of fear, and that fear is mostly unjustified—non-cooperators grossly exaggerate the likelihood that members of the other group will not cooperate. Using a structural model, I find that correcting unwarranted fears is very effective while reducing spite is not because spiteful people also have high levels of fear. Finally, I find that the radio drama decreases spite but not fear, and that (in line

with the findings of the model) this does not translate into increased cooperation.

Taken together, the results of this paper illustrate the value of my model-protocol to explain conflict behavior and policy efficacy, and how these two explanations can be connected to gain deeper insights. First, my model-protocol allows me to diagnose what motives drive conflict in a particular place. In turn, this diagnosis informs what type of policy is needed in this particular place. In the case of Jos, I find that the main driver of conflict is unfounded fears, and therefore, the best policies to increase cooperation there will be those that are effective at correcting intergroup misperceptions. Furthermore, I find that policies that are effective at reducing spite would not only be inefficient, because spite is a smaller problem than fear, but also ineffective, because changes in spiteful preferences would struggle to translate into increased cooperation. Second, my model-protocol allows me to evaluate which drivers of conflict a particular policy is effective at shifting. This, in turn, informs which types of places would benefit the most from this particular policy. In the case of the radio show, I find that it is effective at reducing spite, but not at reducing unwarranted fears. This means that places where reducing spite is the most effective channel to increase cooperation are the places that would benefit the most from a radio show like this.

Because I use the same theory and protocol to study both context and policy, I can directly connect these two. By observing which motive drives conflict in a certain place, and which motive a certain policy shifts, I can determine if a particular place-policy combination is likely to increase cooperation. For the case of Jos and the radio show, my analysis allows me to conclude that the two are misaligned: a radio show that decreases spite will not increase cooperation in Jos, where the main driver of conflict is fear. Indeed, the null treatment effect I find on cooperation is evidence of this mismatch. Importantly, this protocol is portable and can be deployed elsewhere. Using my protocol for other contexts and other policies can advance the understanding of intergroup conflict and its solutions.



## References

- Adena, M., R. Enikolopov, M. Petrova, V. Santarosa, and E. Zhuravskaya (2015, November). Radio and the Rise of The Nazis in Prewar Germany. *The Quarterly Journal of Economics* 130(4), 1885–1939.
- Adida, C. L., A. Lo, and M. R. Platas (2018, September). Perspective taking can promote short-term inclusionary behavior toward Syrian refugees. *Proceedings of the National Academy of Sciences* 115(38), 9521–9526. Publisher: Proceedings of the National Academy of Sciences.
- Alan, S., C. Baysan, M. Gumren, and E. Kubilay (2021, November). Building Social Cohesion in Ethnically Mixed Schools: An Intervention on Perspective Taking\*. *The Quarterly Journal of Economics* 136(4), 2147–2194.
- Alesina, A. and E. L. Ferrara (2005, September). Ethnic Diversity and Economic Performance. *Journal of Economic Literature* 43(3), 762–800.
- Anderson, S. (2011, January). Caste as an Impediment to Trade. *American Economic Journal: Applied Economics* 3(1), 239–263.
- Bauer, M., C. Blattman, J. Chytilová, J. Henrich, E. Miguel, and T. Mitts (2016, September). Can War Foster Cooperation? *Journal of Economic Perspectives* 30(3), 249–274.
- Bauer, M., A. Cassar, J. Chytilová, and J. Henrich (2014, January). War’s Enduring Effects on the Development of Egalitarian Motivations and In-Group Biases. *Psychological Science* 25(1), 47–57. Publisher: SAGE Publications Inc.
- Blattman, C. and E. Miguel (2010, March). Civil War. *Journal of Economic Literature* 48(1), 3–57.
- Bonomi, G., N. Gennaioli, and G. Tabellini (2021, November). Identity, Beliefs, and Political Conflict. *The Quarterly Journal of Economics* 136(4), 2371–2411.
- Broockman, D. and J. Kalla (2016, April). Durably reducing transphobia: A field experiment on door-to-door canvassing. *Science* 352(6282), 220–224. Publisher: American Association for the Advancement of Science.
- Bursztyn, L., T. Chaney, T. A. Hassan, and A. Rao (2021, February). The Immigrant Next Door: Long-Term Contact, Generosity, and Prejudice.
- Bénabou, R. and J. Tirole (2011, May). Identity, Morals, and Taboos: Beliefs as Assets. *The Quarterly Journal of Economics* 126(2), 805–855.
- Casey, K. (2015, August). Crossing Party Lines: The Effects of Information on Redistributive Politics. *American Economic Review* 105(8), 2410–2448.

- Charness, G. and Y. Chen (2020). Social Identity, Group Behavior, and Teams. *Annual Review of Economics* 12(1), 691–713. \_eprint: <https://doi.org/10.1146/annurev-economics-091619-032800>.
- Chassang, S. and G. Padró i Miquel (2007). Mutual Fear and Civil War.
- Chen, Y. and S. X. Li (2009, March). Group Identity and Social Preferences. *American Economic Review* 99(1), 431–457.
- Choi, J.-K. and S. Bowles (2007, October). The Coevolution of Parochial Altruism and War. *Science* 318(5850), 636–640. Publisher: American Association for the Advancement of Science.
- DellaVigna, S. (2018, January). Structural Behavioral Economics. In B. D. Bernheim, S. DellaVigna, and D. Laibson (Eds.), *Handbook of Behavioral Economics: Applications and Foundations 1*, Volume 1 of *Handbook of Behavioral Economics - Foundations and Applications 1*, pp. 613–723. North-Holland.
- DellaVigna, S., R. Enikolopov, V. Mironova, M. Petrova, and E. Zhuravskaya (2014, July). Cross-Border Media and Nationalism: Evidence from Serbian Radio in Croatia. *American Economic Journal: Applied Economics* 6(3), 103–132.
- DellaVigna, S. and E. La Ferrara (2015, January). Economic and Social Impacts of the Media. In S. P. Anderson, J. Waldfogel, and D. Strömberg (Eds.), *Handbook of Media Economics*, Volume 1 of *Handbook of Media Economics*, pp. 723–768. North-Holland.
- Dhar, D., T. Jain, and S. Jayachandran (2022, March). Reshaping Adolescents’ Gender Attitudes: Evidence from a School-Based Experiment in India. *American Economic Review* 112(3), 899–927.
- Enke, B., R. Rodríguez-Padilla, and F. Zimmermann (2023, July). Moral Universalism and the Structure of Ideology. *The Review of Economic Studies* 90(4), 1934–1962.
- Enos, R. D. (2014, March). Causal effect of intergroup contact on exclusionary attitudes. *Proceedings of the National Academy of Sciences* 111(10), 3699–3704. Publisher: Proceedings of the National Academy of Sciences.
- Falk, A. and C. Zehnder (2013, April). A city-wide experiment on trust discrimination. *Journal of Public Economics* 100, 15–27.
- Fearon, J. D. and D. D. Laitin (1996, December). Explaining Interethnic Cooperation. *American Political Science Review* 90(4), 715–735. Publisher: Cambridge University Press.
- Fouka, V. and M. Tabellini (2022, August). Changing In-Group Boundaries: The Effect of Immigration on Race Relations in the United States. *American Political Science Review* 116(3), 968–984. Publisher: Cambridge University Press.

- Franck, R. and I. Rainer (2012, May). Does the Leader's Ethnicity Matter? Ethnic Favoritism, Education, and Health in Sub-Saharan Africa. *American Political Science Review* 106(2), 294–325. Publisher: Cambridge University Press.
- Francois, P., I. Rainer, and F. Trebbi (2015). How is power shared in africa? *Econometrica* 83(2), 465–503.
- Ghosh, A. (2022, August). Religious Divisions and Production Technology: Experimental Evidence from India.
- Giuliano, L., D. I. Levine, and J. Leonard (2009, October). Manager Race and the Race of New Hires. *Journal of Labor Economics* 27(4), 589–631. Publisher: The University of Chicago Press.
- Hjort, J. (2014, November). Ethnic Divisions and Production in Firms. *The Quarterly Journal of Economics* 129(4), 1899–1946.
- Hodler, R. and P. A. Raschky (2014, May). Regional Favoritism. *The Quarterly Journal of Economics* 129(2), 995–1033.
- Jha, S. (2013). Trade, institutions, and ethnic tolerance: Evidence from south asia. *American political Science review* 107(4), 806–832.
- Korovkin, V. and A. Makarin (2023, January). Conflict and Intergroup Trade: Evidence from the 2014 Russia-Ukraine Crisis. *American Economic Review* 113(1), 34–70.
- Kramon, E. and D. N. Posner (2016, April). Ethnic Favoritism in Education in Kenya. *Quarterly Journal of Political Science* 11(1), 1–58. Publisher: Now Publishers, Inc.
- Kranton, R., M. Pease, S. Sanders, and S. Huettel (2020, September). Deconstructing bias in social preferences reveals groupy and not-groupy behavior. *Proceedings of the National Academy of Sciences* 117(35), 21185–21193. Publisher: Proceedings of the National Academy of Sciences.
- Kromka, S. M. and A. K. Goodboy (2019, January). Classroom storytelling: using instructor narratives to increase student recall, affect, and attention. *Communication Education* 68(1), 20–43. Publisher: Routledge \_eprint: <https://doi.org/10.1080/03634523.2018.1529330>.
- La Ferrara, E. (2016). Mass media and social change: Can we use television to fight poverty? *Journal of the European Economic Association* 14(4), 791–827.
- Lowe, M. (2021, June). Types of Contact: A Field Experiment on Collaborative and Adversarial Caste Integration. *American Economic Review* 111(6), 1807–1844.
- Luttmer, E. F. P. (2001, June). Group Loyalty and the Taste for Redistribution. *Journal of Political Economy* 109(3), 500–528. Publisher: The University of Chicago Press.

- Marx, B., V. Pons, and T. Suri (2021, May). Diversity and team performance in a Kenyan organization. *Journal of Public Economics* 197, 104332.
- Michelitch, K. (2015, February). Does Electoral Competition Exacerbate Interethnic or Interpartisan Economic Discrimination? Evidence from a Field Experiment in Market Price Bargaining. *American Political Science Review* 109(1), 43–61. Publisher: Cambridge University Press.
- Mousa, S. (2020, August). Building social cohesion between Christians and Muslims through soccer in post-ISIS Iraq. *Science* 369(6505), 866–870. Publisher: American Association for the Advancement of Science.
- Oh, S. (2023, August). Does Identity Affect Labor Supply? *American Economic Review* 113(8), 2055–2083.
- Padró i Miquel, G. (2007, October). The Control of Politicians in Divided Societies: The Politics of Fear. *The Review of Economic Studies* 74(4), 1259–1274.
- Paluck, E. L. (2009, March). Reducing intergroup prejudice and conflict using the media: a field experiment in Rwanda. *Journal of Personality and Social Psychology* 96(3), 574–587.
- Paluck, E. L., S. A. Green, and D. P. Green (2019, November). The contact hypothesis re-evaluated. *Behavioural Public Policy* 3(2), 129–158. Publisher: Cambridge University Press.
- Paluck, E. L., R. Porat, C. S. Clark, and D. P. Green (2021). Prejudice Reduction: Progress and Challenges. *Annual Review of Psychology* 72(1), 533–560. \_eprint: <https://doi.org/10.1146/annurev-psych-071620-030619>.
- Rabin, M. (2000). Risk Aversion and Expected-Utility Theory: A Calibration Theorem. *Econometrica* 68(5), 1281–1292. Publisher: [Wiley, Econometric Society].
- Rao, G. (2019, March). Familiarity Does Not Breed Contempt: Generosity, Discrimination, and Diversity in Delhi Schools. *American Economic Review* 109(3), 774–809.
- Rubinstein, A. (1989). The Electronic Mail Game: Strategic Behavior Under "Almost Common Knowledge". *The American Economic Review* 79(3), 385–391. Publisher: American Economic Association.
- Scacco, A. and S. S. Warren (2018, August). Can Social Contact Reduce Prejudice and Discrimination? Evidence from a Field Experiment in Nigeria. *American Political Science Review* 112(3), 654–677. Publisher: Cambridge University Press.
- Shayo, M. (2020). Social Identity and Economic Policy. *Annual Review of Economics* 12(1), 355–389. \_eprint: <https://doi.org/10.1146/annurev-economics-082019-110313>.

- Slater, M. D. and D. Rouner (2002, May). Entertainment-Education and Elaboration Likelihood: Understanding the Processing of Narrative Persuasion. *Communication Theory* 12(2), 173–191.
- Trebbi, F. and E. Weese (2019). Insurgency and small wars: Estimation of unobserved coalition structures. *Econometrica* 87(2), 463–496.
- Yanagizawa-Drott, D. (2014, November). Propaganda and Conflict: Evidence from the Rwandan Genocide. *The Quarterly Journal of Economics* 129(4), 1947–1994.

## Appendix

You can find the Appendix [here](#).