

Radio Messaging and Community Acceptance of Ex-Combatants: Experimental Evidence from the Democratic Republic of the Congo*

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Abstract

Existing evidence highlights the potential for radio messaging—which remains the most common form of mass media across the African continent—to influence community attitudes towards peace. Relatively little evidence, however, examines what kinds of radio messages are most effective in promoting pro-peace attitudes. This paper uses an in-person factorial experiment of 1,675 respondents across 119 communities in the conflict-affected Ituri and Nord-Kivu provinces of the eastern Democratic Republic of the Congo. We randomly varied both the narrative framing (strictly informative versus emphasizing shared suffering and reconciliation) and the speaker who delivered the message (a government official, a religious leader, or an NGO representative). We find, on average, no differences between these treatments across a variety of attitudinal outcomes or responses to specific scenarios involving ex-combatants, though younger respondents react more positively to the reconciliation treatment. These findings underscore the importance of targeting pro-reintegration interventions to local context.

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I Introduction

Radio remains one of the most accessible forms of mass media across Africa. In the ninth round of the Afrobarometer survey, 41 percent of respondents said they got their news from the radio ‘every day’ and 24 percent said ‘a few times a week.’ Only 21 percent of respondents said they never got news from the radio. The high penetration of radio messaging means that radio can be an important driver of political behavior. In Rwanda, for example, anti-Tutsi propaganda broadcast by Radio Télévision Libre des Mille Collines impelled violence against the Tutsi minority in the lead-up to the 1994 genocide (Yanagizawa-Drott 2014). On the other hand, evidence from Nigeria suggests that radio messages can also be used to build peace and foster cooperation between different ethnic groups (Blair et al. 2021). While previous research demonstrates the capacity for radio messages to drive pro-peace attitudes, little is known about the specific types of messages which are likely to be most effective.

Existing research often uses religious speakers to deliver pro-peace messages, but the results are mixed: in Burkina Faso, pro-peace messages from a prominent Imam led to some backlash by priming respondents on ethnic identity (Grossman, Nomikos, and Siddiqui 2023). In Colombia, video apologies from former ex-combatants were more effective in improving attitudes towards ex-combatants when accompanied by a religious endorsement, but the overall effect depended on respondents’ perceptions of a peace deal. Likewise, messages that emphasize reconciliation or remorse have increased support for re-integration in a variety of contexts, relative to other types of messages (Godefroidt and Langer 2023; Agneman, Strömbom, and Rettberg 2025). Our factorial design allows us to expand from previous research which documents an effect of radio messaging to show that different types of radio messaging produce equivalent effects.

To test the effectiveness of these varying messaging strategies, we administered an in-person survey experiment to 1,675 respondents across 119 communities in the Ituri and Nord-Kivu provinces of the eastern Democratic Republic of the Congo (DRC).¹ Respondents listened to one of six pre-recorded audio messages, simulating a radio broadcast. Using a factorial design, the messages varied across two dimensions: (1) a neutral informational message or a reconciliation-focused message emphasizing shared suffering; and (2) whether the speaker was a government official, a religious leader, or the representative of an NGO. We then asked respondents about their attitudes towards ex-combatants in general as well as towards more specific scenarios involving

1. The pre-registration document is available at https://osf.io/3szkd/overview?view_only7f63fca834784d37beod9a7cdf729c8e. See Appendix D for information about the correspondence between the manuscript and the pre-registration document.

ex-combatants.

Contrary to our expectations, there were essentially no differences between these different treatment arms. Respondents who listened to the audio message emphasizing shared suffering and reconciliation were no more likely to support reintegration than respondents who listened to a purely informational message. Likewise, respondents whose messages were delivered by religious leaders or representatives of an NGO were no more supportive of reintegration than those whose audio messages were delivered by a government official. However, we document pre-registered subgroup effects: the reconciliation messaging is most effective among younger respondents. We also show slight evidence that the reconciliation message is more effective among respondents who report greater exposure to conflict.

This study extends the growing body of work testing radio-based interventions for ex-combatant reintegration and peacebuilding (Agneman, Strömbom, and Rettberg 2025; Blair et al. 2021; Grossman, Nomikos, and Siddiqui 2023). It complements a long standing body of evidence showing that mass media can shape political and economic behavior, for good or ill (Yanagizawa-Drott 2014; Paluck and Green 2009; Keefer and Khemani 2014). This literature shows the potential for radio messaging to build peace; we advance the literature by testing *what kinds* of radio messages are likely to be most effective, specifically whether the framing of a reintegration message and the identity of the messenger shape community acceptance of ex-combatants. Our null average treatment effects speak to a growing literature that places scope conditions on the effectiveness of religious-based peacebuilding interventions (Sperber, McClendon, and Kaaba 2025; Agneman, Strömbom, and Rettberg 2025). We find no advantage of religious messengers over government officials, suggesting that previous studies which rely on religious messengers may not generalize to contexts where religion plays less of a role in public life (Boyer et al. 2022; Blair et al. 2021).

More broadly, these null average treatment effects underscore the necessity of tailoring such interventions to unique local contexts: strategies that increase support for reintegration in one setting may have no effect, or even backfire, in another (Autesserre 2014). Our limited heterogeneous treatment effects point to conflict exposure and generational experiences of conflict as factors future programs should take into account. In fragile and conflict-affected areas like eastern DRC, community acceptance of ex-combatants remains uneven. Some individuals are welcomed back; others face hostility, social exclusion, or even retaliation. Understanding what drives these differences—and what interventions can improve reintegration—is critical for building peace.

2 Theory and Hypotheses

Extensive literature documents the effect of radio messages on a variety of political behavior. Radio messages in neighboring Rwanda, for example, helped escalate the country’s incipient genocide; Yanagizawa-Drott (2014) use high-resolution spatial variation in the reception of Radio Télévision Libre des Mille Collines to link approximately ten percent of the genocide’s violence to radio broadcasts. Also in Rwanda, Paluck and Greene (2009) document how extended exposure to a radio program increased respondents’ willingness to express dissent. In Tanzania, respondents who benefited from improved radio signal had higher levels of political interest and political knowledge (Green et al. 2024).

Other studies document the capacity of radio messaging to drive pro-peace attitudes, such as support for re-integrating ex-combatants. Radio messages also have the capacity to affect support for re-integrating ex-combatants. In Nigeria, Blair et al. (2021) test whether radio messages from trusted authorities—in their case, from religious leaders—are more effective in encouraging the reintegration of former members of Boko Haram. Relative to messages by a state authority, messages from a religious leader both increased support for re-integration and increased respondents’ perception that their community was supportive of reintegration. However, the details clearly matter: the messengers and messages which are most effective in promoting reintegration are likely to vary both across and within contexts. Peacebuilding, as always, is an intensely local affair (Autesserre 2014). One intervention in Burkina Faso illustrates this point: when a prominent imam delivered a pro-peace radio message it actually primed ethnic identity, leading to a reduction in out-group trust among some respondents (Grossman, Nomikos, and Siddiqui 2023). Consequently, this article explores which specific messages are likely to be most effective in promoting support for reintegrating ex-combatants.

Broadly, the literature suggests that messages which focus on reconciliation are likely to be more effective. In Nigeria, citizens are more willing to reintegrate former fighters who expressed remorse or who had undertaken conciliatory acts after having joined the insurgency (Godefroidt and Langer 2023). Outside of Africa, video apologies of former insurgents in Colombia improved attitudes towards ex-combatants, albeit largely among a subset of respondents who supported a recent peace deal with the relevant insurgent group (Agneman, Strömbom, and Rettberg 2025). Consequently, we hypothesize that:

H1 Exposure to narratives emphasizing trauma and reconciliation will increase support for reintegration relative to neutral messaging.

Public service announcements, such as pro-reintegration broadcasts, often come from representatives of the state. However, in areas of weak state presence such as the eastern DRC, state messengers may not be the most effective vehicles through which to deliver these messages. Religion can be a powerful motivator of political behavior across Africa (McClendon and Riedl 2015). In Nigeria, Blair et al. (Blair et al. 2021) use a radio message from a religious leader to increase support for reintegrating ex-combatants. Likewise, a religious counseling program in Uganda reduced intimate partner violence and shifted intra-household bargaining power more towards women (Boyer et al. 2022). We therefore hypothesize that:

H2 Reintegration messages delivered by trusted religious leaders will be more effective in increasing acceptance and reintegration of ex-combatants than messages from government officials.

Nevertheless, the evidence surrounding religious messengers for political messages is surprisingly mixed. In Burkina Faso, an anti-violent extremism message from a prominent imam actually led to some backlash because the religious contents of the message primed the respondents on ethnic lines (Grossman, Nomikos, and Siddiqui 2023). A Whatsapp campaign with religious messages in Zambia was no more effective than its secular counterpart in promoting civic engagement (Sperber, McClendon, and Kaaba 2025). Consequently, we also want to test an alternative respected speaker: representatives of an NGO. NGO representatives are often trusted actors in conflict zones who may also be seen as more impartial than actors that have a material stake in conflict outcomes (Bakaki and Dorussen 2023). Consequently, we also hypothesize that:

H3 Reintegration messages delivered by representatives of an NGO will be more effective in increasing acceptance and reintegration of ex-combatants than messages from government officials.

Not all messages will be equally effective among all populations: civilians with greater exposure to conflict violence often have stronger attitudes towards ex-combatants (Welsh and Khadka 2026; Lyall, Blair, and Imai 2013). Messages emphasizing reconciliation may therefore be less effective among populations who are more exposed to conflict or who may have been victims of the conflict (Blair et al. 2021). In Iraq, for example, civilians interviewed after the re-capture of Mosul by government forces preferred harsher punishment for Islamic State collaborators (Kao and Revkin 2023).

Exposure to violence is also a gendered experience in the Eastern DRC where our study takes place (Lindsey 2022; Welsh and Khadka 2026). Women are particularly vulnerable to violence.

Women and youth may therefore perceive the risks associated with accepting ex-combatants into one’s community differently. In Somalia, for example, Gelot and Khadka (2025) show that the extent to which civilians perceive ex-combatants as a threat shapes their willingness to support reintegration. Age also likely structures respondent’s experiences of the war. Young people may be concerned about being coerced into joining an armed group; older people may have greater generational exposure to conflict, particularly areas like the eastern DRC which have been conflict affected for extended periods of time. Consequently, we will look for heterogeneous treatment effects by both respondent’s prior exposure to violence and their demographic characteristics.

3 Research design

We administered an in-person survey to 1,675 respondents across 119 locations in the Ituri and Nord-Kivu provinces of the DRC. The survey was implemented by Mercy Corps’ DRC country office between February and March of 2026. Our sampling frame consists of RESET implementing locations—selected by the Mercy Corps country office based on current feasibility of access and other logistical constraints—and an additional set of nearby villages which could serve as ‘control’ villages.² Within these locations, we sampled across three strata: participants of RESET, randomly drawn from beneficiary rolls ($n = 619$), drawn from a random walk within RESET areas ($n = 581$), and drawn from a random sample in these non-RESET areas ($n = 475$).

During the survey, respondents listened to a short (approximately 90 second) pre-recorded audio message on the enumerator’s tablets, designed to simulate a radio broadcast. Respondents were randomly assigned to one of six versions of this audio message, which varied along two independently randomized dimensions in a 2×3 factorial design. This factorial design yields six treatment arms, as shown in Table 1. Respondents had the choice to listen to these radio programs in eight local languages: Alur, Ndruna, French, Hema, Kinande, Lingala, and Ituri or Nord-Kivu dialects of Swahili.

After the experiment, we asked two questions specifically to identify whether respondents listened to the radio programs: (1) Which of the following subjects was mentioned in the radio program? and (2) Who spoke in the radio message we played for you? Of 1,675 respondents, 1661 (99.1 percent) correctly identified the radio message as being about ex-combatants; 1550 (92.5 percent) correctly identified the speaker. We do not condition on these attention checks to avoid

2. We do not expect our results to be systematically different among villages which were selected for RESET and those which were not, but we control for sampling strata in all of the covariate-adjusted specifications. Appendix A describes our sampling strategy in greater detail.

Table 1. Reintegration Messaging Scripts

Script A: informational message	Script B: Reconciliation message
Delivered by a government official Good day. This is Commissioner XX from the local government office. I'm here to inform you that individuals who were formerly associated with armed groups are beginning to return home. They have undergone demobilization and are now eligible for reintegration support. The government is working closely with local leaders to ensure peaceful coexistence. Your cooperation is important during this time. Thank you. Delivered by a religious leader Peace be upon you all. This is Pastor XX speaking to the community today. As you may know, some of our sons and daughters who were involved with armed groups have started returning to their home villages. This process is part of a larger effort to stabilize the region. They have been disarmed, and many are receiving training or support to reintegrate peacefully. We encourage everyone to stay informed, remain calm, and follow guidance from the local authorities and community leaders. Thank you. Delivered by NGO representative Hello, I'm XX from the Community Peace and Development Association, an NGO in the area. We're seeing the return of people who were previously involved with armed groups. They are receiving support and guidance as they settle back into community life. Reintegration is never easy, but with mutual understanding, it can work. Please stay engaged with local initiatives and keep listening to this station for updates.	Delivered by a government official Greetings, this is Commissioner XX. The conflict has brought pain to every corner of our province. Families were broken, communities divided. Today, we are entering a new chapter. Those who were part of the violence are returning. They have renounced arms. Many are seeking to make amends. The government calls on every citizen to show restraint and humanity. Healing is not forgetting, but rebuilding. Together, we can move forward. Delivered by a religious leader My brothers and sisters, this is Pastor XX. We have all suffered during this long period of conflict. Many among us lost family, homes, or peace of mind. Now, some of our sons and daughters who went astray are coming home. They too carry wounds — some physical, some in the heart. The Lord teaches us about forgiveness and healing. Let us remember: those who return can become part of the rebuilding, if we welcome them with wisdom and boundaries. Let us pray for peace, and act with compassion. Delivered by NGO representative Hello, I'm XX from the Community Peace and Development Association, an NGO in the area. We know reintegration is difficult. The young men and women returning have seen terrible things. Some were forced. Others made choices they regret. But now they want to come back — to contribute, to rebuild, to live differently. Our work is to support this process, to make sure it is safe for everyone. Healing takes time, and it takes all of us. Let's listen, support, and stand for peace.

introducing post-treatment bias.

We then presented respondents with a series of follow up questions: (1) To what extent do you think ex-combatants are accepted by the majority of members of your community? (2) In your opinion, does the presence of former combatants pose a security threat in your community?; (3) Would you be willing to have a working relationship with an ex-combatant for economic reasons? (4) Would you be willing to have your daughter marry an ex-combatant? and (5) Would you be willing to have an ex-combatant as a neighbor? In addition to these attitudinal outcomes, we presented respondents with two specific narratives of ex-combatants who have returned to their communities (appendix A describes these scenarios, as well as the follow-up questions, in full

detail). We then asked a series of questions about whether the person should be allowed to stay, and how much the respondent would be willing to engage with the person. For each story, we combine these questions into an index by taking the average across questions.³

In our pre-registered power calculations, we show minimum detectable effects (MDEs) of approximately 0.14 for the message treatment and 0.18 for the speaker treatment. Our confidence intervals allow us to show that our results are inconsistent with the sizable effect sizes documented by Blair et al. (Blair et al. 2021). Looking specifically at the index of responses to the specific scenarios, our confidence interval for the covariate-adjusted regression is $[-0.17, 0.06]$, which excludes the 0.20 standard deviation effect of religious messengers reported by Blair et al. (4). We take these results as evidence that any effect in this context is more likely to be negligible than to resemble the sizeable effects—in either direction—documented in comparable settings.

One empirical concern is that our consent statement, by introducing enumerators as working for Mercy Corps, induces demand effects: responses may be driven by what the respondent thinks the enumerator wants to hear. However, this would be constant across the different treatment arms, meaning that it would not obfuscate comparisons between the different treatments.

3.1 Estimation strategy

To identify the effects of the different radio treatments on these outcomes, we estimate ordinary least squares (OLS) models that include indicator variables for each randomized treatment factor, omitting the informational messaging and the government official speaker as reference levels. Specifically, we estimate equations of the form:

$$Y_i = \beta_0 + \beta_1 \text{Script} + \beta_2 \text{Speaker} + \beta_3 \text{Script} \cdot \text{Speaker} + \psi_v + \varepsilon_i \quad (1)$$

Where (Script) indicates whether the respondent heard the acceptance and forgiveness radio message, (Speaker) indicates which of the three speakers voiced the radio message, and ψ_v is a vector of strata fixed effects. Observations are indexed by i for respondent and v for village. Because we randomized assignment to the six treatment arms, the inclusion of covariates is not required for unbiasedness; however, we include specifications with such controls to improve statistical precision and increase power by reducing residual outcome variance. All covariates included in these models are measured prior to treatment assignment. Our control variables will include respondent's age group, sex, education, profession, french language proficiency, and period of

3. In our pre-analysis plan we specified that we would include both the mean of questions and the first principal component of responses to these questions as our indices.

time within the village, as well as the strata from which the respondent was sampled. We also include specifications with both enumerator fixed effects and demographic controls, although we note that enumerator fixed effects do not explain a significant fraction of variation. Because treatment assignment occurs at the respondent level, we calculate robust standard errors, but do not otherwise cluster our results (Abadie et al. 2023).⁴ In our figures, we display the marginal effect of the treatment variables, rather than regression coefficients, to ease interpretations.

4 Results

Across our different outcomes, figure 1 shows no statistically significant difference between the different messages.⁵ Neither receiving the message which emphasized reconciliation and forgiveness, nor having the message delivered by a religious leader or NGO representative, increased support for reconciliation with former combatants. These null effects are consistent: respondents were no more likely to think their community accepted ex-combatants, no more likely to consider ex combatants a security threat, no more willing to have an economic relationship with an ex combatant, no more willing to have an ex-combatant as a neighbor, and were no more accepting of the two ex-combatants described in the narrative questions.⁶

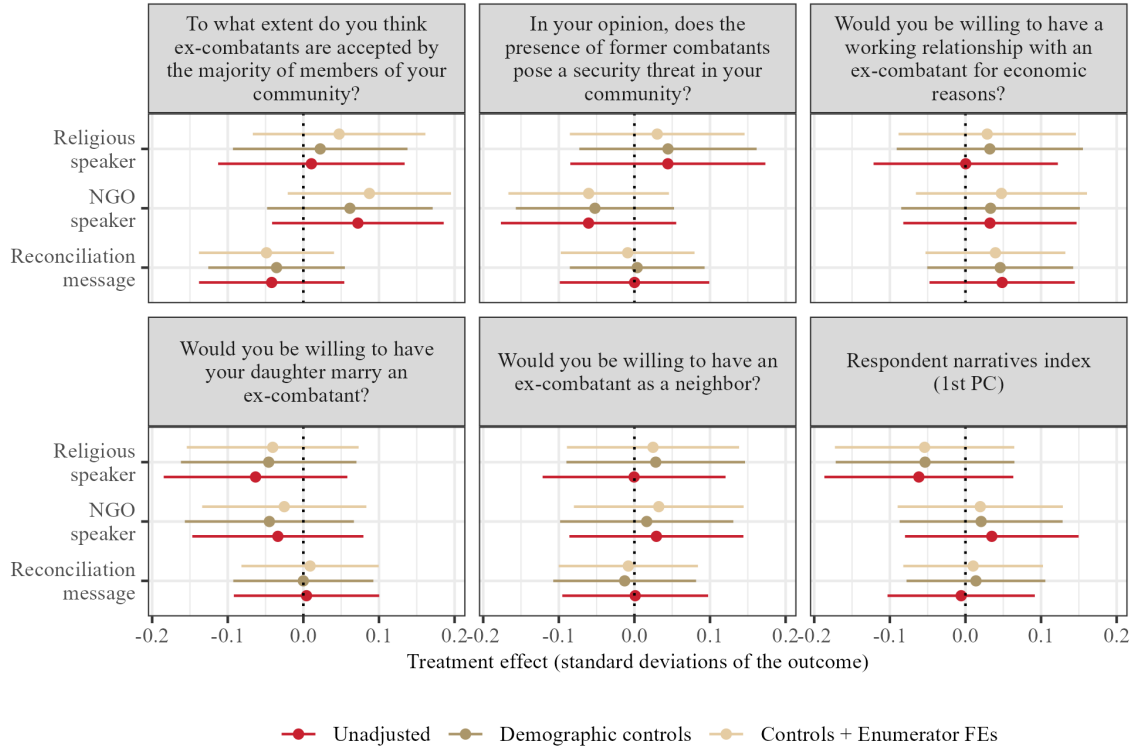
In addition to these direct outcomes, we are also interested in how demographic information and respondents' political beliefs shapes their uptakes of pro-reintegration messaging. First, figure 2 shows some heterogeneous treatment effects of the reconciliation message relative to the purely informative message. Among respondents at the 25th percentile of age (29 years old), receiving the reconciliation treatment was associated with a 0.13 standard deviation decrease in the belief that the presence of former combatants poses a security threat in the respondents' community. The reconciliation treatment produced no statistically significant effect among respondents at the 75th percentile of age (46 years old), but increases the perceived threats among respondents aged 51 or older (80th percentile of age). Likewise, receiving the reconciliation treatment was associated with more positive responses to the ex-combatant narratives among respondents aged 27 or younger (17th percentile) and more negative responses to the ex-combatant narratives among older respondents aged 58 or older (87th percentile). We see no heterogeneous treatment effects on the other outcomes.

4. Because there are two indices of responses to the narratives of returning combatants per respondent, we cluster standard errors at the respondent level for these outcomes.

5. All analyses presented in this section were enumerated in our pre-analysis plan.

6. The marginal effects of the speaker treatment by the script treatment are also statistically insignificant: there is no interaction effect between the two treatments.

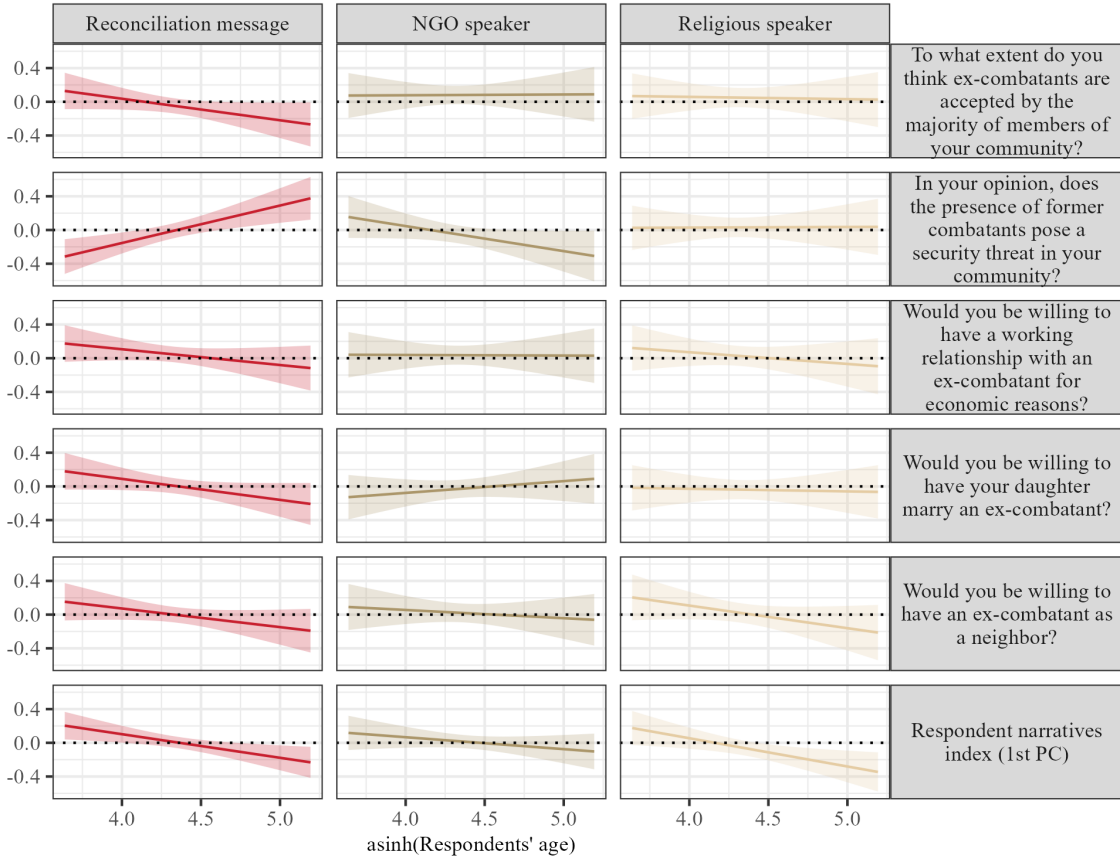
Figure 1. Marginal effects of message type and speaker



This figure shows the marginal effect of the different treatment conditions on all six outcomes. Error bars indicated 95 percent confidence intervals, calculating using robust (HCr) standard errors. Coefficients are standardized using standard deviations of the specific outcome. For the message, the baseline level is the purely informative script (script A); for the speaker, the baseline level is the government official. Appendix tables C1 and C2 show the raw means of these outcomes.

The message being delivered by a representative of an NGO produced no statistically significant treatment effects regardless of the respondents' age. The message being delivered by a religious leader was less effective among older respondents, relative to the message being delivered by a government leader—but only for the replies to the ex-combatant narratives. At the 25th percentile of age, the message being delivered by a religious speaker had no statistically significant effect on the acceptance of the ex-combatant; at the 75th percentile of age, the message being delivered by a religious speaker led to a 0.12 standard deviation *decrease* in acceptance of the ex-combatants ($p = 0.014$). However, there are no heterogeneous treatment effects by age for the other outcomes.

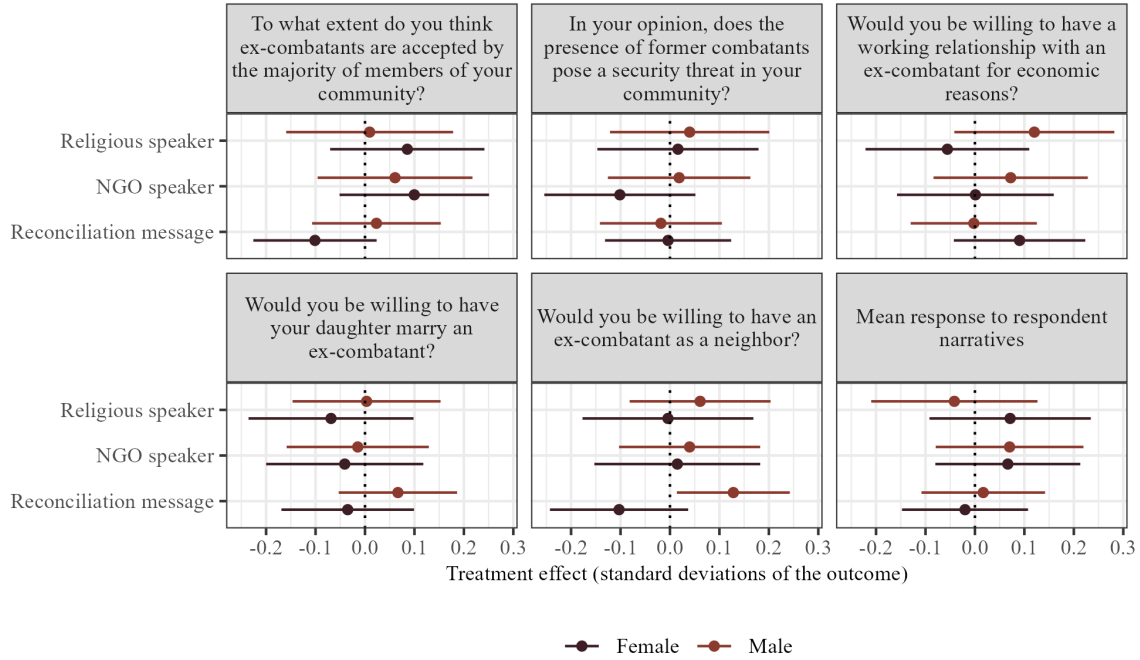
Figure 2. Marginal effects of treatment by respondent age



This figure shows the marginal effect of the different treatment conditions on all six outcomes, broken out by the respondents' age. We normalize the age variable using an inverse hyperbolic sine transformation. Error bars indicated 95 percent confidence intervals, calculating using robust (HCr) standard errors. Coefficients are standardized using standard deviations of the specific outcome. For the message, the baseline level is the purely informative script (script A); for the speaker, the baseline level is the government official.

Next, figure 3 shows that treatment effects were relatively homogeneous across male and female. Neither the reconciliation script nor the message speaker affected respondents' attitudes towards ex combatants differently across men and women. The sole exception is the reconciliation treatment, which produced a statistically significant 0.13 standard deviation increase in willingness to have an ex-combatant as a neighbor among men ($p = 0.027$) and no effect among women. However, given a single heterogeneous effect among a multiplicity of tests, we consider these results essentially insignificant. However, it is worth noting that we do in fact see strong

Figure 3. Marginal effects of treatment across men and women

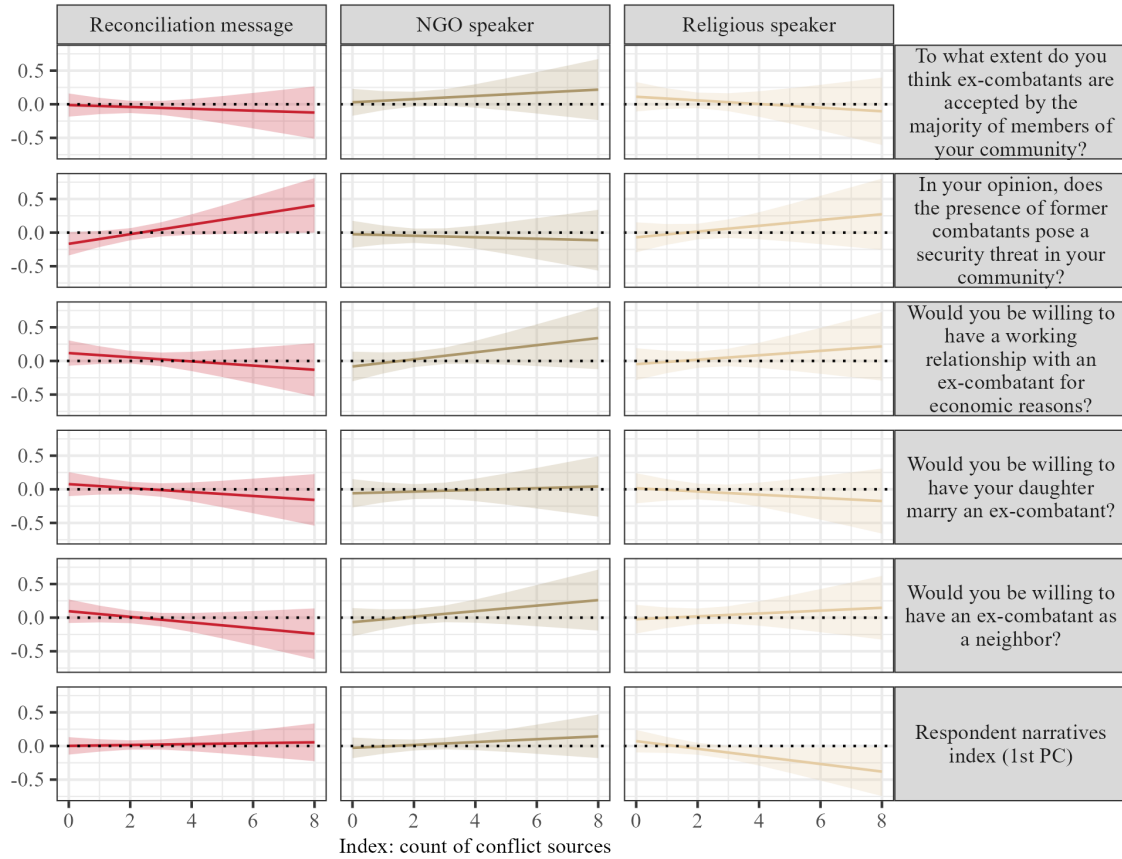


This figure shows the marginal effect of the different treatment conditions on all six outcomes, broken out by the respondents' sex. Error bars indicated 95 percent confidence intervals, calculating using robust (HC1) standard errors. Coefficients are standardized using standard deviations of the specific outcome. For the message, the baseline level is the purely informative script (script A); for the speaker, the baseline level is the government official.

baseline effects among women, who are 0.12 to 0.31 standard deviations less accepting towards ex-combatants than men independent of treatment.

Figure 4 shows heterogeneous treatment effects by this self-reported exposure to conflict measures. Similar to the above figures, figure 4 shows mostly null effects, with the exception of responses to the narrative questions. Lastly, we pre-registered a hypothesis that respondents with greater levels of distrust of the state would be more responsive to radio messages delivered by either representatives of an NGO or by religious leaders. However, we find no evidence of heterogeneous effects by respondents' trust in the government.

Figure 4. Marginal effects of treatment by self-reported exposure to conflict over the last twelve months



This figure shows the marginal effect of the different treatment conditions on all six outcomes, broken out by the count of conflicts respondents said “caused problems, conflicts, disrupted peace or made it harder for people in your community to live peacefully together” over the past 12 months. Error bars indicated 95 percent confidence intervals, calculating using robust (HCl) standard errors. Coefficients are standardized using standard deviations of the specific outcome. For the message, the baseline level is the purely informative script (script A); for the speaker, the baseline level is the government official.

5 Conclusion

This paper uses an in-person survey experiment administered to 1,675 respondents in the Nord-Kivu and Ituri provinces of the eastern DRC to test what types of radio messages are more effective in improving attitudes towards ex-combatants. In a two by three factorial design, we test

messages emphasizing reconciliation and shared suffering versus a purely informational message, as well as messages delivered by religious leaders and NGO representatives, relative to those delivered by government officials. Contrary to our expectations, we find no difference among any of our treatment arms, though we find some evidence that the reconciliation message is more effective within certain sub-populations. These precisely estimated null results are inconsistent with any non-negligible difference between our treatment arms (Rainey 2014).

These results caution a growing literature which explores when and how pro-peace messages broadcast over the radio can shift attitudes. Previous studies have largely tested one radio message against a pure control group; Blair et al. (2021), for example, use a message similar to our reconciliation script, delivered by a religious authority. One interpretation of this paper's results is that the presence of a pro-peace message matters more than the details of the script—or who speaks it. However, given the situational complexities of peacebuilding in any given context, it seems more likely that this research further evidences the necessity to tailor specific methods to specific audiences.

Nevertheless, the absence of a pure control group which did not receive a pro-reintegration message is a limitation of this research. We test which messages are effective *relative to each other*, but do not include a reference group to test whether these messages are effective overall. Though previous research documents the effectiveness of such methods (Blair et al. 2021; Grossman, Nomikos, and Siddiqui 2023; Agneman, Strömbom, and Rettberg 2025), future research could combine the flexibility of our factorial approach with a more clear-cut comparison group.

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Online Appendix:
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A Survey and measurement details

This research took place within the context of the Responsive Stabilisation Through Transition project (RESET) programme, funded by the British Foreign and Commonwealth Development Office and implemented by Mercy Corps. RESET aims to promote peace and stability in the North Kivu and Ituri provinces of the eastern Democratic Republic of Congo (DRC). Among other interventions, RESET supported the Collectif des Radios et Télévisions Communautaires du Nord-Kivu (CORACON) to develop a radio campaign that could promote non-violence, tolerance, and reintegration of ex-combatants. To ensure maximum effectiveness of this programming, we partnered with Mercy Corps to embed a series of radio messaging into RESET's previously planned midterm survey to better understand the types of radio messages which would be most effective in promoting peace within this context. This paper analyzes the results of these tests.

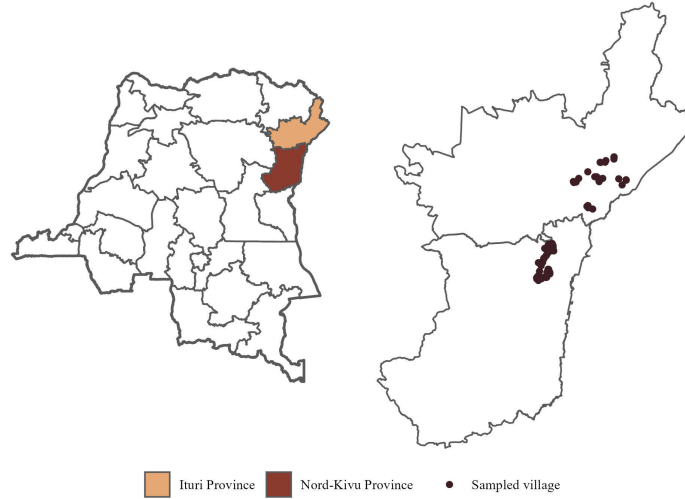
A.1 Sampling

We surveyed 1,675 respondents through a community-based household sampling in RESET implementation areas in eastern DRC and matched comparison communities in eastern DRC. The matched comparison communities were selected by Mercy Corps' local country office, which implemented the survey. The comparison groups were selected for geographic proximity. We do not rely on this comparison at any point in the analyses, so we do not overview its process here.

Interviews took place across three strata: RESET participants in RESET treatment areas, a sample drawn from a random walk in RESET treatment areas, and a sample drawn from a random walk in the comparison areas. In the Ituri province, all interviews took place in the Irumu territory ($n = 870$); in the Nord-Kivu province, interviews were split between the city of Butembo ($n = 131$), the city of Beni ($n = 352$), and the rural territory surrounding Beni ($n = 322$). All three strata were present in each of these areas, with the exception of Butembo, which did not receive any RESET interventions, so all respondents were part of the sample drawn from the random walk in non-RESET areas. Figure A1 shows these areas.

Trained enumerators coordinated with local leaders only to identify community boundaries and ensure safe access. Enumerators then approached randomly selected individuals directly to invite participation. Recruitment and interview occurred in person; the survey then took place at or near participants' homes (or another private location agreed with the participant). Participation was voluntary, with informed consent obtained before any questions are asked. Inclusion criteria were: (1) adults 18 years of age or older; (2) residents of the selected community; (3) able

Figure A1. Map of sampled provinces and villages



The left panel of this figure shows the locations of the Nord-Kivu and Ituri provinces in the eastern DRC; the right panel shows the sample locations within these provinces.

and willing to provide informed consent; (4) comfortable completing the survey in one of the study languages. After respondents have consented to participate, the survey lasted approximately one hour. We implemented the survey using tablets and SurveyCTO. We directly programmed all randomization into SurveyCTO. The audio messages through which we administered our treatments were also directly embedded into SurveyCTO.

A.2 Ethics

Political scientists have become increasingly concerned in recent years with the ethics of field research in Africa (Davis 2020). To avoid unethical interventions—and to ensure that the research is of use to African organizations and institutions rather than being purely extractive—we worked hand-in-hand with Mercy Corps’ Bunia office. We consulted with local experts in survey design and in survey implementation to make sure that (1) all questions were sensitive to local circumstances; and (2) to ensure that the research answered useful policy questions. We received IRB approval both from the Washington University in St. Louis IRB (protocol number 202602052) as well as the IRB at the Shalom University of Bunia (protocol number 2026_CEE_FAGE01FEV), in the DRC. Before the survey began, enumerators read the following consent statement:

Hello. My name is [your name] and I work with Mercy Corps. Mercy Corps is an

international non-governmental organization that works for economic development and social cohesion in your province. We are conducting a survey of households in this area and would like to speak with you.

The purpose of this survey is to learn more about your community and how Mercy Corps' programs are working here. The questions on this survey are about you, your experiences with ex-combatants in the community, and how you view your local government. Some questions may feel sensitive. You do not have to answer any question you do not want to answer.

Participation in this survey is completely voluntary. You can stop at any time, or take a break, and there is no penalty. Your decision will not affect your or your any member of your household's access to Mercy Corps programs or any services now or in the future. We cannot promise any direct benefit to you from participating, but your answers may help improve programs and better understand community needs.

We will keep your responses confidential. We will not share your individual answers with local leaders, the government, or anyone outside the study team. When we analyze or publish results, we will combine answers from many people, and we will not include information that identifies you.

In this survey, we are also working with a researcher from Washington University in St. Louis in the United States of America. Your survey responses may be used for future research studies or shared with other researchers only after removing any information that could identify you.

This survey usually takes about an hour to complete. If you agree, we may also ask separately whether you want to provide contact information to be reached for future surveys. Providing contact information is optional, and you can still do the survey even if you do not provide it.

We then gave respondents a chance to ask enumerators any questions they may have had before asking whether they consented to participate. We also provided respondent's with contact details for Mercy Corps' Bunia office in case of later questions or complaints.

One ethical concern is that this research demanded time and attention from particularly vulnerable respondents, given the economic situation in the eastern DRC. However, we note that the RESET program itself required a mid-term evaluation to satisfy donor requirements. Consequently, respondents would have participated in some manner of survey regardless—the addition

of the research component only added approximately ten minutes to this survey. We judged this cost to be proportionate to the potential benefit of allowing RESET to better target its peace-building radio program.

A.3 Questionnaire details

After presenting respondents with one of the six potential treatments, we asked the following question:

1. To what extent do you think ex-combatants are accepted by the majority of members of your community? Answer choices: (1) not accepted at all; (2) not very well accepted; (3) well accepted; and (4) very well accepted).
2. In your opinion, does the presence of former combatants pose a security threat in your community? Answer choices: (1) not a threat at all; (2) a low threat; (3) a moderate threat; and (4) a high threat
3. Would you be willing to have a working relationship with an ex-combatant for economic reasons? Answer choices included (1) no, not at all; (2) no, somewhat; (3) yes, somewhat; and (4), yes absolutely.
4. Would you be willing to have your daughter marry an ex-combatant? Answer choices included (1) no, not at all; (2) no, somewhat; (3) yes, somewhat; and (4), yes absolutely.
5. Would you be willing to have an ex-combatant as a neighbor? Answer choices included (1) no, not at all; (2) no, somewhat; (3) yes, somewhat; and (4), yes absolutely.

However, Westwood et al. (2022) show that presenting respondents with abstract questions overstated support for political violence. We believe that a similar mechanism may be at work here: respondents may express an unwillingness to consider reconciliation in the abstract, but may be more accepting when presented with individual narratives or scenarios. To that end, we presented respondents with the following two scenarios:

1. I want to tell you the story of a 22 year old lady from your community. She used to live here, working as a small trader, and helping around the home. A few years ago, she joined an armed group and spent over a year with them. Like many others, she was drawn in under difficult conditions, believing it might offer protection, purpose, or economic opportunity. While with the group, she received training, took on several roles and was involved in its

activities. Eventually, she left and returned to civilian life. Since then, she has completed a local reintegration program and has expressed remorse for the past, stating a desire to contribute positively to the community.

2. I want to tell you the story of a 23 year old man from your community. He used to live here, working as a small trader, and helping around the home. A few years ago, he joined an armed group and spent over a year with them. Like many others, he was drawn in under difficult conditions, believing it might offer protection, purpose, or economic opportunity. While with the group, he received training, took on several roles and was involved in its activities. Eventually, he left and returned to civilian life. Since then, he has completed a local reintegration program and has expressed remorse for the past, stating a desire to contribute positively to the community.

After each scenario, we will ask a series of yes/no questions:

1. This man wants to return to the community and stay. Will you agree for this man to stay in your community?
2. Would you trade with this man
3. Do you think this man should be allowed to attend community meetings?
4. Would you vote for this man if he contests for a leadership position against another man?
5. Now I want to know what is really in your own mind. If the decision was yours alone, do you think this man should be allowed to stay in the community?
6. Would you be angry with this man for what he did in the past?
7. Would you be afraid of this man for what he might do in the future?
8. Do you feel bitterness toward this man ?
9. Can you forgive this man ?
10. Can you forget what this man did?

We use the first principal component of these responses as an overall index of acceptance for these scenarios. The coefficient of correlation between the first principal component and the simple average is 0.49.

B Balance

This appendix shows that the control variables we include in the above regressions are balanced across all of the control variables. Table B1 shows balance for the script treatment, whether the treatment was the standard message (Script A) or the reconciliation message (script B); table B2 shows balance for the speaker treatments.

Table B1. All three speaker treatment groups are balanced across control variables

	Standard message		Reconciliation message		Difference
	Mean	Std.Err.	Mean	Std.Err.	T-score
Age	39.75	13.39	39.30	13.67	0.67
Female	0.53	0.50	0.56	0.50	-0.95
Native of village	0.57	0.50	0.54	0.50	1.30
French proficiency	0.65	1.18	0.71	1.20	-0.95
Education					
No education	0.11	0.31	0.09	0.29	1.40
Primary	0.38	0.49	0.36	0.48	0.74
Secondary	0.44	0.50	0.48	0.50	-1.81
Higher	0.08	0.26	0.07	0.25	0.49
Employment					
Agriculture	0.56	0.50	0.55	0.50	0.42
Commerce	0.15	0.36	0.13	0.34	1.01
Craft	0.06	0.23	0.07	0.25	-0.95
Wage labor	0.08	0.27	0.09	0.28	-0.55
Household labor	0.03	0.18	0.03	0.18	0.09
Other	0.07	0.26	0.08	0.27	-0.64
Unemployed	0.05	0.22	0.05	0.23	-0.12

Note: This table shows the balance of control variables across the standard messaging and the reconciliatory messaging. T-scores are calculated using the standard messaging as a baseline.

Table B2 shows imbalance on two variables. Respondents who received the religious treatment were more likely to have no education and respondents who received the NGO speaker treatment were more likely to be engaged in wage labor. However, two statistically significant variables within 30 t-tests is roughly in line with a successful randomization.

Table B2. All three speaker treatment groups are balanced across control variables

	Official speaker		Religious speaker			NGO speaker		
	Mean	Std.Err.	Mean	Std.Err.	T-score	Mean	Std.Err.	T-score
Age	40.00	13.40	39.37	14.35	-0.73	39.24	12.96	-0.98
Female	0.54	0.50	0.56	0.50	0.42	0.54	0.50	-0.08
Native of village	0.57	0.50	0.54	0.50	-0.86	0.55	0.50	-0.57
French proficiency	0.64	1.15	0.68	1.20	0.63	0.71	1.21	1.08
Education								
No education	0.09	0.28	0.12	0.33	1.99	0.09	0.29	0.19
Primary	0.40	0.49	0.35	0.48	-1.70	0.37	0.48	-1.04
Secondary	0.45	0.50	0.45	0.50	0.20	0.47	0.50	0.72
Higher	0.07	0.25	0.08	0.26	0.40	0.07	0.26	0.35
Employment								
Agriculture	0.56	0.50	0.55	0.50	-0.30	0.54	0.50	-0.70
Commerce	0.14	0.35	0.16	0.37	0.87	0.12	0.33	-0.96
Craft	0.06	0.24	0.05	0.22	-0.86	0.08	0.27	1.06
Wage labor	0.07	0.25	0.08	0.27	0.88	0.10	0.30	2.07
Household labor	0.03	0.17	0.03	0.18	0.26	0.04	0.19	0.44
Other	0.09	0.28	0.06	0.24	-1.35	0.08	0.26	-0.59
Unemployed	0.05	0.22	0.06	0.24	0.48	0.05	0.21	-0.33

Note: This table shows the balance of control variables across the three speaker treatment groups. T-scores are calculated using the official speaker group as a baseline.

C Additional Analyses

Table C1. Raw means for the narrative framing treatment

	Standard message		Reconciliation message		Difference
	Mean	Std.Err.	Mean	Std.Err.	T-score
To what extent do you think ex-combatants are accepted by the majority of members of your community?	3.19	0.84	3.15	0.90	0.85
In your opinion, does the presence of former combatants pose a security threat in your community?	1.75	0.88	1.75	0.90	-0.01
Would you be willing to have a working relationship with an ex-combatant for economic reasons?	3.17	1.00	3.22	1.00	-0.99
Would you be willing to have your daughter marry an ex-combatant?	3.16	1.05	3.17	1.08	-0.05
Would you be willing to have an ex-combatant as a neighbor?	3.46	0.81	3.46	0.87	-0.02
Respondent narratives index (1st PC)	0.00	0.70	0.00	0.72	0.21

Note: This table shows the raw means of our outcome variables across the standard messaging and the reconciliatory messaging. T-scores are calculated using the standard messaging as a baseline.

Table C2. Raw means for the speaker treatment

	Official speaker		Religious speaker			NGO speaker		
	Mean	Std.Err.	Mean	Std.Err.	T-score	Mean	Std.Err.	T-score
To what extent do you think ex-combatants are accepted by the majority of members of your community?	3.15	0.93	3.15	0.95	0.11	3.21	0.92	1.22
In your opinion, does the presence of former combatants pose a security threat in your community?	1.76	0.95	1.80	0.97	0.67	1.70	0.92	-1.04
Would you be willing to have a working relationship with an ex-combatant for economic reasons?	3.18	1.00	3.18	1.00	0.02	3.21	1.00	0.57
Would you be willing to have your daughter marry an ex-combatant?	3.20	1.01	3.13	1.05	-1.04	3.16	1.03	-0.59
Would you be willing to have an ex-combatant as a neighbor?	3.45	0.91	3.45	0.92	0.00	3.47	0.92	0.50
Respondent narratives index (1st PC)	0.00	0.70	-0.04	0.73	-1.43	0.03	0.69	0.85

Note: This table shows the raw means of the outcome variables across the three speaker treatment groups. T-scores are calculated using the official speaker group as a baseline.

D Deviations from pre-analysis plan

[URL to public anonymized registration](#)

	Specified in pre- registration?	Reported in manuscript?	Deviations (with justification and location of discussion)
Sampling	Yes	Yes, Re- search Design section	The PAP specified a target sample of approximately 1,500 respondents across 48 locations (28 RESET villages, 20 nearby control villages), with three sampling strata: (1) 500 respondents via random walk in RESET communities, (2) 500 respondents via random walk in control communities, and (3) 500 respondents drawn from RESET beneficiary rolls. The realized sample comprised 1,675 respondents across 119 locations. The larger number of locations and respondents reflects logistical adjustments by the Mercy Corps DRC country office during field-work implementation.
Sample Exclusions	Yes	Yes	None
Experimental Conditions	Yes	Yes	None. The 2×3 factorial design (two narrative framings $\times$ three messenger identities = six treatment arms) was implemented exactly as pre-registered.
Observed Measure i: Community acceptance of ex-combatants	Yes	Yes	None.

	Specified in pre- registration?	Reported in manuscript?	Deviations (with justification and location of discussion)
Observed Measure 2: Perceived se- curity threat	Yes	Yes	None.
Observed Measure 3: Willing- ness to have economic rela- tionship with ex-combatant	Yes	Yes	None.
Observed Measure 4: Willingness to have daugh- ter marry ex-combatant	Yes	Yes	None.
Observed Measure 5: Willingness to have ex- combatant as neighbor	Yes	Yes	None.

	Specified in pre- registration?	Reported in manuscript?	Deviations (with justification and location of discussion)
Observed Measure 6: Narrative vignette in- dices (female and male ex-combatant)	Yes	Yes	The PAP specified that the ten yes/no follow-up questions for each vignette scenario would be combined into an index using principal component analysis (PCA), yielding the first principal component as the outcome variable. It also stated that both the mean of the items and the PCA-based index would be reported. For brevity, we report only the PCA-based index rather than the mean. The coefficient of correlation between these two indices is 0.49, and using the mean generates substantively identical conclusions.
Hypothesis 1: Reconciliation framing increases support for reintegration	Yes	Yes	The PAP stated: “Exposure to civic media narratives emphasizing trauma, remorse, reconciliation, and shared suffering increases support for reintegration relative to neutral or security-heavy messaging.” The manuscript simplifies this to: “Exposure to narratives emphasizing trauma and reconciliation will increase support for reintegration relative to neutral messaging.” This is a presentational rather than substantive change.

	Specified in pre- registration?	Reported in manuscript?	Deviations (with justification and location of discussion)
Hypothesis 2 (PAP H2 / Article H2): Religious leader mes- senger more effective than government official	Yes	Yes	None. Wording is identical to the PAP.
Hypothesis 3: NGO rep- resentative messenger more effec- tive than government official	No	Yes	The PAP included the NGO representative treatment in the experimental design, but mistakenly omitted it from the list of hypotheses. Results are null.
Hypothesis 4: Prior exposure to violence moderates messaging effects	Yes	Yes	This hypothesis (PAP H3) is not numbered or explicitly stated in the manuscript's Theory and Hypotheses section; rather, it is discussed in the introduction to the heterogeneous treatment effects analysis in the Results section. The substantive content of the hypothesis—that trauma-informed reconciliation narratives should be more effective among respondents with greater exposure to violence—is retained and tested. The PAP did not specify a precise operationalization of violence exposure.

	Specified in pre- registration?	Reported in manuscript?	Deviations (with justification and location of discussion)
Hypothesis 5: Institutional trust attenuates threat effects	Yes	Yes	The PAP stated: “Perceived threat reduces support for reintegration and acceptance, but this effect is attenuated among individuals with higher trust in local/state institutions.” The manuscript tests a related but narrower version of this claim: it examines whether respondents with lower trust in the government are more responsive to messages delivered by NGO representatives or religious leaders, as opposed to government officials.
Hypothesis 6: Treatment effects vary by gender and age	Yes	Yes	This hypothesis (PAP H ₅) is not numbered in the manuscript’s Theory and Hypotheses section but is described in the final paragraph of the Theory section and tested in the Results. No deviation in substance.
Empirical test of H₁ (Reconciliation framing)	Yes	Yes	Two deviations from the PAP’s empirical strategy apply to all hypothesis tests and are documented here: (1) The PAP stated that results would be shown “with and without village fixed effects.” The manuscript uses strata fixed effects (ψ_v) rather than village-level fixed effects. This reflects the sampling design: the relevant grouping for fixed effects is the three sampling strata (RESET random walk, control random walk, RESET beneficiaries), not the village. Village-level fixed effects would absorb a large share of the identifying variation since many villages contain only a small number of respondents.

	Specified in pre- registration?	Reported in manuscript?	Deviations (with justification and location of discussion)
Empirical test of H₂ (Reli- gious leader messenger)	Yes	Yes	Same deviations as empirical test of H ₁ above.
Empirical test of H₃ (Article only: NGO representative messenger)	No	Yes	See Hypothesis 3 (Article only) row above.
Empirical test of H₄ (PAP H₃: Violence exposure mod- erates effects)	Yes	Yes	Same deviations as empirical test of H ₁ above. The specific operationalization of violence expo- sure was not specified in the PAP.
Empirical test of H₅	Yes	Yes	Same strategy deviations as empirical test of H ₁ .
Empirical test of H₆	Yes	Yes	Same strategy deviations as empirical test of H ₁ . For the age interaction, the manuscript uses an inverse hyperbolic sine transformation of respon- dents' age, which was not specified in the PAP. This transformation reduces the influence of the upper tail of the age distribution while preserv- ing meaningful variation across the distribution.

	Specified in pre-registration?	Reported in manuscript?	Deviations (with justification and location of discussion)
Were there studies included in pre-registration that are not reported in the manuscript?			We do not test PAP hypothesis 4 (“Perceived threat reduces support for reintegration and acceptance, but this effect is attenuated among individuals with higher trust in local/state institutions (e.g., police, local authorities).” This hypothesis uses purely descriptive data, not the experimental results. It was included in the PAP as part of the broader impact evaluation report which was shared directly with the Mercy Corps program office. No experimental hypotheses were excluded.
Additional comments			None.