Can the Wounds of War be Healed? Experimental Evidence on Reconciliation in Sierra Leone*

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Abstract

Wars destroy not just physical capital but also social capital and psychological wellbeing. Post-conflict recovery seems contingent on healing individuals and restoring their social ties. Can social renewal only occur alongside psychological renewal? We experimentally evaluate community-level reconciliation in Sierra Leone. As a part of the intervention, victims detail war atrocities, and perpetrators confess to war crimes. We find that reconciliation led to greater forgiveness of former perpetrators. It also forged social capital: social networks were stronger and people displayed more community-oriented behavior including higher contributions to public goods. Yet, the process also worsened psychological health, increasing depression, anxiety and post-traumatic stress disorder. These impacts, positive and negative, persisted for nearly three years after the intervention. Our results suggest that individual healing is not a pre-condition for reconciliation to renew social ties: rather social capital grew at the expense of psychological wellbeing.

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1 Introduction

Wars destroy more than physical capital. Conflict severs social ties, especially when neighbors take up arms against each other. Post-conflict societies may be left with lower civic engagement, weaker provision of public goods, and reduced trust among community members.[1] Recovering from wars, therefore, also means repairing social capital.[2]

There are, broadly, two approaches to restoring social capital war-torn societies. Community Driven Development/Reconstruction (CDD/R) programs provide aid and require communities to develop participatory institutions for disbursing these resources. CDD/R programs are predicated on the idea that social cohesion emerges from people working together under these institutional arrangements. Truth and Reconciliation processes, on the other hand, focus on airing war-time grievances. These programs provide forums for victims to talk about war atrocities, and for perpetrators to confess their war crimes.

Reconciliation is not just a practically different way of building social capital — it conceptualizes rebuilding differently. It presumes that societal healing and individual healing are intertwined. Under this approach, confronting the past, and forgiveness itself, are held to be cathartic (Kritz 1999; Cobban 2002; Hamber 2003). As such, social cohesion is presumed to emerge concurrently with psychological healing (Biggar 2003; Lederach 1999; Asmal et al. 1996; Truth and Reconciliation Commission [TRC] 1998).

Both approaches are pervasive. Nearly every country coming out of internal conflict

[1] Rohner et al. (2013a) present a theoretical account of how conflict serves to erode trust, while Rohner et al. (2013b) provide empirical evidence of how civil conflict leads to a deterioration of social capital. Alesina and La Ferrara (2002) also show that exposure to traumatic experiences lower trust.

[2] Of course, in some cases war itself could increase social capital if it brings communities together to cope with an external threat (Gilligan et al. 2014) or if the war experience subsequently makes preferences more pro-social (Voors et al. 2012), motivating civic engagement (Bellows and Miguel 2009; Blattman 2009).

in the last few decades has implemented some type of reconciliation process.[3] And, the World Bank alone has disbursed over $1.3 billion per year for community driven development over the past decade (Mansuri and Rao 2012). Yet, while we have started accumulating evidence about CDD/R programs (Fearon et al. 2009; Casey et al. 2012; Beath et al. 2012 and 2013; Humphreys et al. 2013; Adveenko and Gilligan forthcoming; Fearon et al. forthcoming)[4] we know little about whether, and how, reconciliation works. As a result, we have a limited understanding of its conceptual underpinnings. Is promoting forgiveness a meaningful way of restoring social capital? Are psychological renewal and social renewal complementary?

Past work, while not answering these questions fully, provides some building blocks of knowledge. We have learned that attitudes toward other groups can improve in the aftermath of a nation-wide TRC (Gibson 2004)[5] and with exposure to therapeutic counseling (Staub et al. 2005). Other types of interventions targeted toward individuals have also provided insight into how we can improve day-to-day dispute resolution (Blattman et al. 2014) and how we can change prejudice (Paluck 2009). But what happens when we induce targeted, person-to-person forgiveness throughout a community? We lack systematic evidence on how these efforts influence the dual ends of individual and societal healing (Mendeloff 2004; Mendeloff 2009).

In this paper we make an initial contribution toward filling this gap. We experimentally evaluate a community-level reconciliation effort in Sierra Leone, which emerged from a brutal civil war in 2003. We evaluate local-level reconciliation forums that are held in sections of 10 villages, and implemented by a NGO called Fambul Tok (FT)—

[3] Some examples include: Chad, Colombia, Congo, El Salvador, Fiji, Ghana, Guatemala, Kenya, Liberia, Morocco, Nigeria, Peru, Rwanda, Sierra Leone, Solomon Islands, South Africa, South Korea, Sri Lanka, Sudan, East Timor, Uganda.

[4] A recent spate of impressive papers have studied Community Drive Development / Reconstruction (CDD/R) programs using careful empirical techniques such as well-implemented randomized control trials. Several find no institutional impact of these programs (Casey et al. 2012; Humphreys et al. 2013; Adveenko and Gilligan forthcoming) while others find notable impacts on community cooperation (Fearon et al. 2009; Fearon et al. forthcoming) and improved female participation (Beath et al. 2013). Understanding when and where these programs will work is an ongoing challenge.

[5] Drawing on rich survey data, Gibson finds that accepting truths about apartheid in the wake of South Africa's TRC led whites, but not blacks, to hold more positive attitudes toward other racial groups. Truth-telling seemed to exert larger effects on those with less direct knowledge of past violence.

which means "Family Talk" in Krio. Specifically, we use random assignment to determine the impact of these reconciliation processes, and conduct our evaluation starting 2012.

Each country's reconciliation effort has some distinct features. But the FT process shares several features common to TRCs around the world: victims describe the violence they experienced; perpetrators seek forgiveness for their crimes; and no one is prosecuted or punished for participating. In addition, no monetary resources are provided to the treatment communities.

Our study includes over 2200 individuals in 200 villages. In a sub-sample of villages, we were able to survey respondents up to 31 months after the intervention. This timeframe allows us to speak to longer run effects than is typically feasible in evaluations of this type. We also pre-registered our plan for analyzing all our outcomes to avoid the follies of 'fishing' out significant effects (Casey et al. 2012; Humphreys et al. 2013).

Our analysis shows that reconciliation served as a powerful force for societal healing. First and foremost, it led to greater forgiveness of perpetrators in treated communities. It also served as an engine for building social capital. Trust of ex-combatants increased, and social networks grew as people developed more friendship and sought more help from each other. Individuals residing in treated villages also became more community-oriented in their behavior: they joined more civil associations such as Parent Teacher Associations (PTAs) and religious groups, and contributed more to public goods.

Yet, our analysis also shows that confronting the past collectively proved psychologically difficult. In particular, individuals in treated communities scored worse on three dimensions of psychological wellbeing — anxiety, depression and post-traumatic stress disorder (PTSD). These results suggest that reliving war memories may re-open old wounds, and accord with psychology studies suggesting possibilities for re-traumatization from one-time debriefing (van Emmerik et al. 2002; Rose et al. 2002).

Our results are also consistent with past evidence that national level TRC participation doesn't necessarily improve psychological health (Kaminer et al. 2001) and may worsen it (Brounéus 2008; Brounéus 2010). And, they accord with findings that

participating in these entities may yield mixed perceptions of justice and satisfaction (Backer 2004, 2007). It is challenging to infer causality from these previous works since TRC participation is not randomized, and those who choose to participate may differ systematically from others. Nonetheless, our paper is part of a growing literature that questions the presumption that truth telling, in its basic form, confers unambiguous psychological benefits to participants.[6]

Taken together, our findings challenge the view that societal and individual healing occur concurrently in response to reconciliation. Ultimately, both findings point to the long and lasting impact of war. The positive impacts on social capital suggest the need for reconciliation persists for nearly a decade after the end of the conflict. At the same time, the negative psychological impacts show that war memories can be easily re-invoked and continue to exert negative impacts on individual's wellbeing.

The remainder of the paper is organized as follows. Section two provides an overview on the channels through which reconciliation can influence healing. Section three gives background on the institutional context. Section four discusses the intervention and evaluation design. Section five discusses the data, while section six presents the results. Lastly, section seven concludes.

2 Healing through Reconciliation

There are two views on how reconciliation influences individual and societal healing in post-conflict societies.

One view holds that reconciliation simultaneously promotes both forms of healing. Individual healing may emerge for one of two reasons. First, confronting and talking about the past may be cathartic and bring closure to individuals (Huyse, 2003; Cobban, 2002; Hamber 2003). Second, truth-telling may encourage individuals to let go of their anger and resentment toward perpetrators. Clinical studies have shown that

[6] Other studies have also examined the impact of TRCs on peace (Lie, Binningsbo, and Gates 2007; and democracy (Brahm 2005; Gibson 2006). For a comprehensive review the impact of TRCs and other transitional justice programs, see Thomas Ron and Paris (2008).

forgiveness— defined formally as "letting go of negative thoughts, feelings, and behavior" (Rye et al. 2005)— can reduce trauma and improve mental health for individuals suffering from PTSD (Reed and Enright 2006). Individuals who let go of their resentment are less beholden to their past and can move on from the traumatic event (Enright and Fitzgibbons 2000).

Closure and forgiveness may, in turn, foster social capital through a number of channels. Individuals who have forgiven or felt closure may feel ready to restore fractured relationships. They may therefore interact more with others, including former combatants, giving rise to broader networks. In many post-conflict settings, victims and perpetrators may avoid certain places or activities where they are likely to come into contact with each other. Forgiveness releases them from this avoidance, opening up prospects for participating in a wider range of community activities. The expansion of social networks and increase in prosocial behavior that arise from these networks lie at the heart of Putnam's conception of social capital (Putnam 2000. p. 19).

Thus, the implied prediction under this view is that reconciliation will improve both psychological wellbeing and social capital, as measured by broader social networks and greater pro-community orientation in individuals' behavior.

Yet a second view posits that societal and individual healing are distinct processes that do not have to coevolve (Mendeloff 2009). So, reconciliation may still be able to promote social cohesion, regardless of its impact on psychological wellbeing.

Are there reasons why reconciliation may not promote individual psychological healing, or even worsen it? Recounting war events may bring up difficult and painful memories, which could lead to re-traumatization. In fact, psychology studies have found that one-session debriefing of patients suffering from PTSD has no therapeutic benefits and could even worsen the trauma (van Emmerik et al. 2002; Rose et al. 2002). As Brounéus (2010) notes, the reason for this may lie in the fact that one-session debriefing, as with truth-telling processes, involve short and intense exposure to traumatic events, which leave little time for gradual habituation and desensitization (Josephs and Gray, 2008; van Emmerik et al. 2002). Similarly, listening to others speak about traumatic

events can itself be traumatic. For example, a mental health expert in South Africa found that the commission staff of the South African TRC program "responded with classic post-traumatic stress symptoms" (Hayner 2001, p.150).

Can societal healing arise without individual healing? Individuals may forgive perpetrators without feeling psychologically better. As an example, a victim may describe an event where their family member was brutalized, and decide to forgive the perpetrator for this act. At the same time, discussing this very event may bring this memory to the forefront of an individual's psyche and create emotional stress or depression. As a result, worsening psychological health can coexist with the act of forgiving — though forgiveness itself can produce increased engagement and social cohesion.

In addition, direct impacts on cohesion can also arise through channels that do not operate via person-to-person forgiveness. Victims may develop a more positive outlook on their community for collectively acknowledging the truth of their experiences. Similarly, perpetrators may feel less shame and fear when there is acknowledgement of these actions without retribution. This more positive outlook can even emerge if individuals choose not to testify in the proceedings. Once crimes are acknowledged, individuals can begin forming social ties and participating in their community. As a consequence, acknowledgement itself can help spur social capital (Quinn 2010).

Thus this second view predicts that reconciliation will lead to increases in social capital, while either reducing psychological wellbeing or leaving it unchanged. We utilize our field experiment in Sierra Leone to examine which of these predictions are supported by the data.

3 Conflict and Reconciliation in Sierra Leone

Sierra Leone had a civil war between 1991 and 2002. The war had no overt ethnic or religious dimensions (Humphreys and Weinstein 2006). Rather, discontent over corruption and authoritarian rule lay the groundwork for rebellion. Kleptocratic rulers enriched themselves with illicit diamond mining throughout the 1970s and 1980s, but

few public services were provided over this time (Reno 1995). A one-party state was declared in 1978 which persisted into the 1990s.

The rebellion was launched in 1991 by the Revolutionary United Front rebels (RUF). Besides discontent over government ineffectiveness, controlling the country' diamond wealth also played an important motivating role (Keen 2005; Bellows and Miguel 2009). Diamonds financed and thereby prolonged the conflict.

The war was brutal. More than 50,000 people were killed and over the half the population was displaced. Thousands were also raped and amputated (Human Rights Watch 1999). The vast majority of atrocities were committed by the RUF (Conibere et al. 2004; Smith et al. 2004). But there were two other actors. The Sierra Leonean Army (SLA) gained notoriety for colluding with the rebels — sometimes, to share diamond profits, and at other times, to avoid direct battles. In the process, they also terrorized civilians. In response, local defense militias called the Civil Defense Forces (CDF) emerged during the conflict. These were rooted in the local chieftaincy system. At the end of the war, some CDF factions may also have targeted civilians, but generally, they were revered for defending the local population.

The violence was largely neighbor-on-neighbor, committed between people from the same community who knew each other (Keen 2005). Although the RUF was fighting to overthrow the state, the violence was often personal and motivated by grievances against local abuse of power in what is considered to be a strongly gerontocratic society. Chiefs — typically older men — held considerable power over resource allocation with direct consequences for development outcomes (Acemoglu Reed and Robinson 2014). Disenfranchised youths, who gained access to guns during the war, would often target chiefs or elders from their home village (Keen 2005).

The nature of this violence underscored the need for reconciliation when the war ended. Following the conflict, the Sierra Leonean government and international community created a Special Court to try the most notorious, high-profile perpetrators, indicting 13 such individuals over the next decade.[7] The government also set up a

[7] `http://www.sc-sl.org`, retrieved 16 May 2013

national-level TRC which heard over 7,000 statements.[8] However, this covered only a small fraction of all atrocities committed. Overstretched, the TRC did not include broad based participation from the rural population, and therefore, didn't reach many of the individuals most adversely affected by the war. This created a gap in the reconciliation process that Fambul Tok was created to fill. Its aim was to reach out to the communities whose war-time grievances remained unaddressed.

4 Intervention and Evaluation Design

4.1 Fambul Tok

Fambul Tok was started by a Sierra Leonean in 2007. It currently operates in five of 13 Sierra Leonean districts. Its program aims at airing wartime grievances and unifying the community.

The intervention occurs at the section level, which are clusters of up to ten contiguous villages. The NGO places great emphasis on the process being community-driven. At point of entry, Fambul Tok holds a consultation meeting with all village chiefs in the section to attain consent and support for the project. This is followed by several months of community organization. Two groups are established: a Reconciliation Committee consisting of village chiefs, religious and youth leaders, as well as some war survivors and former combatants; and an Outreach Committee, consisting mostly of youth. The Reconciliation Committee is trained in trauma healing and mediation, and tasked with reaching out to victims and perpetrators to participate in the truth-telling process. The Outreach committee helps in publicizing and planning, to ensure broad-based participation from all villages in the section. The process culminates in a two-day long bonfire ceremony where victims share their stories and perpetrators ask for forgiveness for their war-crimes. The ceremony draws from old hunting traditions and is combined with traditional and religious rituals, including prayers and dancing. It is capped by

[8] `http://www.sierraleonetrc.org`, retrieved 16 May 2013

a 'cleansing' ceremony designed to bring the community together after the difficult testimonials. The ceremonies are relatively inexpensive, costing $150-$200.

After the ceremony, Fambul Tok also establishes a series of local institutional structures to further heal the community. It sets up a Peace Tree which provides a focal point for resolving disputes. In some treated areas, it also sets up communal farms on land set aside as a pledge towards reconciliation. Finally, it helps establish a Peace Mothers' group that promotes women's economic activities and discusses gender-targeted atrocities perpetrated during the war.

This intervention could have some impacts other than reconciliation. For example, the farms may generate economic activity and the Mothers groups could have a galvanizing effect on female participation in the community. We will argue below (Appendix Tables A.7, A.10-A.11) that most of the observed effects are likely due to reconciliation rather than these other impacts.

4.2 Evaluation Design

As a first step in the evaluation, Fambul Tok identified sections which were willing to participate in the program in its five districts of operation: Bombali, Kailahun, Koinadugu, Kono, and Moyamba. These sections were identified prior to the start of the community consultation process, or any other parts of the program.

The evaluation then proceeded in waves, enabling Fambul Tok to work within its capacity. Table A.1 in the Online Appendix shows the program was well implemented: respondents in treatment communities were significantly more likely to have heard of Fambul Tok, to indicate that their section held a bonfire ceremony, and to report that Fambul Tok had helped to establish a communal farm, peace tree and peace mothers group in the community.

Forty communities comprised the first wave of the evaluation and 60 communities comprised the second wave of the evaluation. The program was also implemented in a third wave. However, data collection to evaluate this wave was interrupted by the

Ebola crisis in Sierra Leone. Our field staff had to be evacuated while we were in the midst of collecting behavioral measures.

Within each wave, we first surveyed the communities at baseline. We matched sections into pairs, stratified by district based on an 'optimal greedy' algorithm (Greevy et al. 2004; Imai et al. 2009), using baseline data on exposure to violence, conflict incidence, economic activity and psychological health. We then randomly assigned one section in each pair to treatment.

The second wave baseline collected a more pared down set of variables. We discuss the implications of this for our empirical strategy in the next section. The interventions in wave one villages were initiated with bonfire ceremonies taking place between late April and June 2011. In these sections, we were able to collect two rounds of endline data—the first round of the endline was administered approximately 9 months after the intervention, and the second round was administered 31 months afterward. In wave two villages, bonfire ceremonies occurred over March through June 2012. We administered one endline round for these sections, approximately 18 months after the intervention. We thus present results using these three sets of endline surveys.

We conducted household and village surveys in two villages within each section. One village was the section headquarters, where the ceremony is typically held, and the second a randomly chosen village within each section. Respondents within villages were randomly sampled by first sampling households then selecting an individual from each household. Typically 12 respondents were sampled in each village, although in some villages the number was 10 or 11.

In both waves, we sought to re-survey the same respondents who were interviewed at baseline. We went to great lengths to minimize attrition. We conducted several follow-up visits and also tracked down respondents who moved to neighboring villages. We develop two measures of attrition. The first equals one if a baseline respondent did not receive an endline survey at all. This attrition rate is 7 percent. The second applies only to round one and equals one if a baseline respondent was not available for both endline surveys. This conservative attrition rate is 13 percent. As shown in Appendix

Table A.2, neither attrition indicator is predicted by treatment, meaning attrition was not differential in treatment communities relative to control communities.

A village-level survey was also conducted at baseline and endline. Due to mechanical error in the hand-held devices used for data collection, baseline data is missing for six villages, and endline data is missing for an additional six villages. If a village-level variable is one of just a few key indicators in an index, we construct the index by dropping these villages rather than imputing missing values to them.

Importantly, our evaluation was independent of the intervention — there was no overlap between the individuals who implemented the program and the enumerators who collected the data.

4.3 Empirical Strategy

Our main specifications combine all three sets of endline surveys — two rounds of the first wave and one round of the second wave. We utilize an analysis of covariance (ANCOVA) estimator in assessing all outcomes for which we have baseline data. This controls for the baseline value of the dependent variable. We opt for this estimator as it accounts for the covariance between pre- and post-treatment outcomes (Frison and Pocock 1992; McKenzie 2012), and has more power than a difference-in-difference (DD) estimator. The improvement in power is greatest when the correlation between pre and post measures is very low,[9] which is the case for our study.[10]

We allow the baseline dependent variable to exert different effects across rounds and waves. The estimating equation can be represented as:

$$y_{rivspw} = \beta_0 + \beta_1 T_s + \rho_p + \beta_2 y_{0ivspw} + \delta_r + \delta_r y_{0ivspw} \ + \lambda_w y_{0ivspw} + \varepsilon_{rivspw} \qquad (1)$$

[9] McKenzie (2012) estimates that at $\rho = 0.25$, the sample size needs to increase by 60% for the difference-difference estimator to attain the same power as ANCOVA.

[10] For example, in our study, autocorrelation, ρ, ranges between 0.04 for psycho-social indicators and 0.27 for group membership.

where y_{0ivspw} and y_{rivspw} denote outcomes at baseline and endline round r respectively, for individual i in village v, section s, section-pair p and wave w. ρ_p denotes section-pair fixed effects, which account for section-level matching in the allocation of treatment (Bruhn and McKenzie 2009). T_s is assignment to treatment, and β_1 measures the treatment effect. δ_r is a round effect which equals one for the second round endline. $\delta_r y_{0ivspw}$ is the interaction of this round two indicator with the baseline outcome, and allows the baseline to exert different effects over time. λ_w denotes a wave effect which equals one for sections in the second wave. Since each wave includes different sections, these wave effects are subsumed by section-pair effects. $\lambda_w y_{0ivswp}$ denotes the interaction of the wave effect with the baseline outcome. This allows baseline variables to have different effects for the wave two sections. This is particularly important since we are only able to include the pared down baseline outcomes collected in the second wave baseline survey.

For outcomes where we do not have any baseline values, we utilize a simple cross-sectional specification:

$$y_{rivspw} = \beta_0 + \beta_1 T_s + \rho_p + \delta_r + \varepsilon_{rivspw} \tag{2}$$

In all specifications, we cluster the standard errors at the section level, the unit of treatment allocation. There is one section in wave one and two sections in wave two which do not match the treatment assignment: these sections were assigned to control and yet 6 of the respondents in one of the villages and 8 of the respondents in the two others reported attending a bonfire ceremony. However, we utilize assignment to treatment in all of our specifications. Thus, ceremony participation among control respondents may lead to an understatement of the effect.

Our outcomes are primarily mean indices that aggregate various indicators used to measure similar concepts. The aggregation was pre-specified in a pre-registered analysis plan (PAP).[11] Owing to differences in data collection across the two waves, we developed

[11] Our PAP can be found here: http://bit.ly/1AIG5b0. The plan was registered December 12, 2012.

a modified pre-analysis plan for the second wave. But in pooling data across the two waves for the results in this paper, we followed the aggregation specified in the plan for the first wave, which was developed before we had access to any endline data from either wave. In the Appendix, we also detail the few circumstances under which we deviate from the pre-specified grouping, owing primarily to issues aggregating conditional and unconditional outcomes.

We use two types of mean indices, following Anderson (2008) and Kling, Liebman and Katz (2007). Both indices standardize the indicators and sum across these measures. The Anderson (2008) approach weights the standardized outcomes by the inverse of the variance-covariance matrix. This places less weight on indicators that add no extra information, due to high variance or high correlation with other indicators. The Kling et al. (2007) approach accounts for missing values by imputing the mean of the control (treatment) group to missing values for the control (treatment) group. Given missingness in some of our key indicators, the loss of observations from aggregating across indicators without imputation is at times substantial. Thus, we use mean indices constructed with the Kling et al. approach for the main results, but also present all results using the Anderson (2008) approach in the appendix.

4.4 Total Treatment Effect

Our empirical strategy identifies the total treatment effect which stems from both the direct and indirect effects of the program. For example, direct effects will arise from those who participated directly in the bonfire ceremony, as 40% of our randomly sampled respondents reported doing. However, there may also be indirect effects arising from those who did not participate directly. For example, a household member may attend the bonfire ceremony and develop a more positive outlook on their community, and subsequently convince other household members to join community groups. These

As in Casey et al. (2012), we finalized the plan while the endline data was being entered and cleaned, and before any of the authors had access to the data. In particular, we asked our field staff to password protect the data and give us access to the password only after the analysis plan was posted.

spillovers underscore why the section is the stable unit of treatment, and why it is important to randomize this intervention at the community level.[12]

5 Data

We administered a rich set of survey questions. This section provides an overview of the measures used in our primary analysis—forgiveness, psychological wellbeing and social capital. The Online Appendix details measures used for additional results.

We constructed a forgiveness scale from the Enright Forgiveness Inventory (EFI) (Subkoviak et al. 1995), which consists of 12 questions on a 4-point Likert scale.[13,14] These questions were administered to respondents who reported that they were physically or emotionally hurt during the war. The EFI inventory has strong internal consistency and retest reliability (Subkoviak et al. 1995) and is a standard measure in psychology studies. While all endline surveys included these 12 questions, the second wave baseline included a subset of seven questions. This subset is used to form a pared down index which serves as a baseline control for second wave observations in ANCOVA specifications.

For psychological wellbeing, we aggregated three indices for PTSD, anxiety, and depression. The PTSD index is a list of 11 questions from the 4^{th} Diagnostic and Statistical Manual of Mental Disorders (DSM-IV) (American Psychiatric Association 2000), as developed by Foa et al. (2007). Our anxiety and depression metrics are comprised of 10 and seven questions from the Zung indices of anxiety and depression

[12] They also present a challenge to instrumenting ceremony attendance with treatment assignment, since the spillovers point to a potential violation of the exclusion restriction using this approach.

[13] This scale captures three dimensions of forgiveness (Enright and The Human Development Study Group 1991): (1) affect – which includes feelings against the perpetrator, such as resentment, anger, compassion and love; (2) judgement – opinions and beliefs about perpetrator; and (3) behavior – actions or expressed desired actions against perpetrators, such as revenge or acts of kindness.

[14] Respondents are asked if they strongly agree, agree, disagree or strongly disagree with questions such as: "Do you spend time thinking about ways to get revenge on the person who wronged you?" To give this scale an intuitive meaning, we code agreement in the negative domain, and disagreement on the positive domain symmetrically around 0. We follow the literature in coding across responses in steps of one, for resultant values ranging from -1.5 to 1.5. Since linear regression is unaffected by affine transformations, coding the variable on other values such as 1 to 4 does not affect the results.

(Zung 1971), respectively. These indices are known for their test-retest validity (Knight et al. 1983) and are commonly used by psychologists.[15] These responses are also measured on a 4-point Likert.[16] The second wave baseline included seven and five questions on anxiety and depression respectively, which again form the pared down baseline controls for ANCOVA regressions. We inverted these indicators, so a reduction indicates worse psychological health.

In the process of piloting the surveys, we adjusted the wording for the forgiveness and psychology indices to the Sierra Leonean context, so they better reflect informality in Krio language.[17] All of our psychological measures, including the subsets, show strong internal consistency, as measured by Cornbach's alpha.[18]

We also measured attitudes toward ex-combatants and war participation. We gauged beliefs on culpability of ex-combatants by asking how much respondents agree with the statements: "those that did bad things in the past would do it again if they had the chance" and "people who joined the RUF are not responsible for what they did since they were drugged". For war attitudes, we asked if the respondent would fight again, or believe that others would fight again.

To gauge impacts on social networks, we asked the respondents to list people from the 11 other respondents, whom they: consider a very good friend and would ask for advice and help.[19] We used this to construct how many times a respondent was named by someone else. We also asked the respondent to list all the people in the village they would ask to collect money for them and ask for help. We are only able to conduct

[15] These questions also correspond closely to other studies in post-conflict societies. For example, the Survey of War-Affected Youth (SWAY) conducted in Uganda (Blattman and Annan 2010) asks the same PTSD questions and six out of the seven anxiety questions.

[16] For example, one anxiety question asks: "In the last month, did you feel fear without cause?" The responses range from "never" which is coded as a zero, to "yes often" which is coded as a three.

[17] For example, one PTSD question is: "Have you had recurrent or intrusive distressing thoughts or recollections about the assault". We changed this question to: "In the last month, did you sit and think of bad bad things that happened to you even though you don't want to think of it?"

[18] Cornbach's alpha is 0.865 for the full forgiveness index and 0.918 for the full set of questions in the three psychological wellbeing measures. It is 0.824 for the subset forgiveness index, and 0.897 for the subset of questions in the psychological wellbeing index.

[19] The enumerator first named the other 11 villagers who are being surveyed, then asked whom the respondent would choose. The enumerators were trained to emphasize that the respondent should not simply name everyone.

cross-sectional analyses with these questions, since they were asked differently in the baseline and endline surveys (See Appendix for more detail).

We also collected a battery of social capital questions developed by the World Bank.[20] We separately administered questions on trust of ex-combatants and migrants, and four questions on trust of community members which comprise an index of generalized trust.[21] Migrants are of interest because many former combatants migrated out of their communities after the war, which creates ambiguity as to whether a migrant is in fact a former combatant. Also, migrants are typically considered marginalized groups, and so increases in trust toward them could reflect more inclusive attitudes.

Our group membership index considers both membership and meeting attendance for organizations such as Parent Teacher Associations (PTAs) and religious groups. Our public goods measure includes monetary and labor contributions given to public facilities and community groups; the number of community projects; contributing money to a family in need over the past three months; and participation in road brushing, a common form of road maintenance in Sierra Leone.

5.1 Descriptive Statistics and Baseline Balance

Table A.3 in the Online Appendix presents descriptive statistics of key variables. The population we survey resides under highly impoverished conditions. Over 70 percent have no formal education, and fewer than eight percent live in a village with a market. In addition, these individuals experienced extensive violence during the civil war. 54% had a family member killed. 33% were beaten. 2% report being maimed and 3% acknowledge they were raped. These latter numbers are likely to be an underestimates given the sensitivity of these outcomes. Table A.4 also shows that there is a negative relationship between the violence exposure variables and our psychological measures at baseline, including affect toward former combatants as captured by forgive-

[20] http://go.worldbank.org/BOA3AR43W0

[21] We examine trust for ex-combatants and migrants separately since these questions are only administered to individuals who know someone from these groups. The Online Appendix provides further discussion around this.

ness, PTSD, anxiety and depression. These correlations suggest that the psychological measures capture meaningful variation.

Table 1 shows balance statistics on main outcome variables at baseline.[22] (Table A.4 presents analogous statistics for the additional outcomes.) Most outcomes display balance, with the exception of trust. Statistically, we expect to observe imbalance in some indicators purely by chance. Moreover, the imbalance goes in different directions for different measures, which suggests that these are statistical aberrations: the index of generalized trust is higher while trust of migrants is lower in treatment communities. Both are also only significant at the 10% level.

In addition, ANCOVA estimation controls for the baseline dependent variable, and accounts directly for confounding effects of potential imbalance. Finally, for robustness, we also present specifications where we control for imbalanced baseline indicators in all outcome regressions.

6 Results

We first present results on the relationship between reconciliation, forgiveness and views on former war-combatants. We then examine societal and individual healing, as captured by social capital and psychological wellbeing. We close by determining the persistence of the effects, and summarizing additional results.

6.1 Forgiveness and Views toward Ex-Combatants

Table 2 assesses the relationship between reconciliation and forgiveness. The outcome in the first row measures whether those hurt during the war have forgiven their perpetrators, based on the Enright Forgiveness Inventory. The second row is the equivalent measure for the subset of questions in both baseline waves.

These results show that the reconciliation process increased forgiveness substantially. The coefficient in the second row is .277. At endline, the control group mean

[22] We present the pared down measures where we have have limited measures in the wave two baseline.

measure was .951. We benchmark the effect by considering how experiencing violence during the war influenced an individual's baseline forgiveness. Of course war exposure was widespread — over 80% of the sample reported being hurt in some way. To ensure that we benchmark against consequential war experiences, we examine how experiencing more extreme forms of violence— getting raped, maimed or having a family killed —influence baseline forgiveness relative to other forms of hurt. For example, Appendix Table A.5 shows that rape reduces baseline affect toward ex-combatants by 1.21. Comparing to .277 would suggest that the reconciliation process offset negative feelings toward perpetrators by 23%. Or, having a family member killed reduces this measure by .920, which implies an offsetting effect of 30%.

These forgiveness effects are based on attitudinal questions, which raises concerns that the respondents may simply be saying what they believe surveyors want to hear. There are three factors that mitigate this concern. First, our evaluation is independent, and our survey team remained completely separate from the implementing NGO over the course of the study. Second, we ask these questions from 9 to 31 months after the bonfire ceremonies take place, not in its direct aftermath — so talk of forgiveness was not fresh on respondent's minds. Finally, our respondents were victimized in traumatic ways, experiencing events such as amputations. Given these experiences, it is not psychologically costless for them to respond that they no longer feel hatred toward their perpetrator, or that they wish their perpetrator well, if this did not reflect a shift in underlying perspective.

Did these effects on forgiveness also shift individuals' attitudes toward the war? To gauge views on former fighters, we aggregated two questions. We first show the treatment effect on the mean index of these indicators. The coefficient on this regression captures the effect measured in standard deviation units. The rows underneath also show results from separate regressions of the component indicators. The results indicate that overall, there were no significant shifts in attitudes around the culpability of former combatants. This suggests that people can grant forgiveness even if they continue to feel that the combatants were responsible for their actions.

Next, we examine an index of attitudes toward future war participation. This includes three indicators of whether the respondent believes that they, or other members of their community, would fight in a future rebellion. We utilize a cross-sectional specification since these variables were not collected in the baseline of the second wave. The results indicate no significant impact on this outcome. This suggests that granting forgiveness for past violence doesn't necessarily shift individual's beliefs regarding future violence.

At the bottom of Table 2, we also examine impacts on trust. The first outcomes measure trust of two socially marginalized groups — ex-combatants and migrants.[23] The third measures generalized levels of trust in the community, indexing four variables around perceived honesty and trustworthiness of community members. The results show a clear pattern. The treatment significantly increases trust of the marginalized groups, without exerting significant impacts on general trust levels. Trust of ex-combatants increased by nine percent, while trust of migrants increased by four percent.[24]

6.2 Social Capital

Since reconciliation is aimed at forgiving perpetrators, it is reassuring to see that the process led to increases in trust toward this group. But since it doesn't produce shifts in general trust, this still leaves open the question as to whether the process influences people's willingness to engage with each other and form ties. To gain leverage on this question, Table 3 examines impacts on the strength of social networks, using an index that aggregates four outcomes. These network measures were collected comparably only

[23] As discussed in the Data section, sometimes migrants are perceived to be former combatants.

[24] These questions are administered to those who report knowing a former combatant or migrant. This could create concerns that the treatment changes who individuals know. However, the Ancova specifications are estimated for those who know one such group member at both baseline and endline. Also, we show that the treatment doesn't affect the chances of knowing someone from one of these groups (Table A.6). These are relatively small communities, and so knowing new types of individuals is not the margin through which the effect operates. Consistent with this, when we present the cross-sectional estimates on trust (which are not restricted to those who knew members of these groups at baseline and thus allow compositional changes in the sample), the estimates are statistically indistinguishable at the 5% level from those in Table 2.

at endline, so we are restricted to using cross-sectional specifications. Even without baseline controls, we observe significant increases in the index of network strength. The largest effects stem from the second and third indicators. The means indicate that each respondent is listed, on average, two or three times as a friend or as someone to seek help from in control areas. The reconciliation treatment boosts each network measure by 11 percent. In short, individuals rely more on each other and are more connected to each other in treatment communities.[25]

If the reconciliation process improves individual's outlook on their community, it may have the capacity to alter their engagement with the community more broadly. We have two ways for gauging the community orientation of individuals' behavior. First, Table 4 examines their participation in community groups, based on both membership and meeting attendance. The treatment exerts a clear, positive effect on this aggregate index. The coefficients on the individual indicators suggest that the largest increases occurred in PTA and religious group participation. For example, PTA membership and meeting attendance were 25 percent and 45 percent higher in treatment communities, respectively, while religious group membership and meeting participation were 20 and 31 percent higher, respectively. Youth group membership and women's group meeting attendance also increased. In fact, the coefficients are positive for almost all other groups, with the exception of secret societies. This effect is noteworthy: since secret societies have closed membership dominated by the elite (Murphy 1980), decreased participation in this group is consistent with substitution toward more broad-based community organizations.

The women's group effect may raise the concern that the effects on aggregate group participation are driven by membership in Peace Mothers Groups in treatment areas. However, when we remove both women's group variables from the index, the coefficient

[25]It is possible that attending the ceremony may have generated friendships through an alternate social channel beyond its impact on healing. However, we find no significant differential impact of ceremony attendance on the mean effect of social networks, which casts doubt on the importance of this account. (The coefficient on the interaction term of treatment with attendance is .005 and the standard error is .110).

remains significant and increases in magnitude, suggesting this is not the case.

Second, Table 5 examines effects on individual's contributions to public goods. This index aggregates a number of different measures, including: whether individuals contribute labor or money to building and maintaining public facilities (such as bridges, schools, wells and clinics); "road brushing," which is a common form of road maintenance; monetary contributions to needy families; and labor and monetary contributions to PTAs, village development committees, and youth and women's groups.

There is again a significant increase in the mean effect index. Among the individual indicators, the effect is most precisely estimated for PTA contributions, where the implied effect is approximately 32 percent. The effect is also marginally insignificant for contributing to public facilities broadly, with a p-value of .12 and an implied effect of 7 percent. While the estimates for the other variables are not individually significant at the 10 percent level, the implied effect for contributing to women's groups is also substantial (approximately 20 percent), as is the effect for decision to give to those in need (8 percent). However, we again verify that the mean effect is not driven by mechanical women's group effect. In fact, when we remove this indicator, the coefficient again increases in magnitude and precision.

These results on network strength, group participation and public goods contributions provide evidence that reconciliation process led to improvements in social capital. Individuals were more likely to view one another as a source of support and contribute charitably toward community needs. The impacts on behavior also suggest that the forgiveness effects do not arise from respondents saying what they believe surveyors want to hear — rather, more positive affect toward former combatants is coupled with actual behavioral changes in treated areas.

A natural question is whether the reconciliation element was required to generate improvements in social capital. There are two reasons why we believe that a simple gathering of the community without this element would not produce similar results. First, a large-scale CDD/R program was put into place in Sierra Leone over approximately

the same period. The program spent $100 per household and was well-implemented, successfully delivering economic benefits (Casey et al. 2012). It established new institutional structures and fostered ongoing gatherings of the community in village-wide meetings to promote inclusion and collective action. Yet, it but found no impacts on social capital outcomes such as community group participation. Second, if the mere act of gathering improved person-to-person interactions through channels unrelated to war-related reconciliation, then we should observe reductions in other types of conflicts, such day-to-day disputes or social tensions. However, as we discuss below and show in the appendix (Table A.8), we do not observe such effects.

6.3 Psychological Healing

Did the positive effects on community cohesion arise in conjunction with positive impacts on psychological healing? Table 6 examines impacts on our index of psychological wellbeing, which includes measures of PTSD, anxiety and depression. The first row presents the index of complete indicators (with pared baseline controls for wave two). The second row presents the index with pared down anxiety and depression measures at endline. Both indices show that respondents in treatment communities experienced a deterioration in these outcomes. The coefficients indicate that the indices fell by approximately .14 standard deviation units in treated areas. Regressions of the individual indicators suggest that this negative impact stems from a worsening of all three psychometric measures.[26]

The continuous PTSD measure can also be converted into a dichotomous measure of whether an individual suffers from clinical PTSD. We construct one such measure to discern the magnitude of the trauma effect.[27] As shown in Table 6, this dichotomous

[26] Note that control group means of the continuous psychometric indicators are less instructive for guaging magnitudes in percent terms since they are aggregations on a Likert scale. Under these scales, changing the value assigned to responses will not alter the regression coefficients, but it will alter the value of the control group mean.

[27] Following guidelines from the Clinician-Administered PTSD Scale (Weathers et al. 2013), we categorize a respondent as suffering from PTSD if he or she shows at least one symptom of re-experience, one symptom of avoidance, and at least two symptoms of increased arousal. For example, consider

measure suggests that the prevalence of PTSD increased by 36%: 8% of the individuals in the control group displayed signs of PTSD and this fraction was 2.9% higher in the treated areas. These numbers firstly indicate that a sizable fraction of individuals (in the control areas) continue to suffer from PTSD nearly a decade after the end of the war. Secondly, they suggest a substantial worsening of this psychological outcome owing to the reconciliation treatment.

We can also benchmark the estimates against baseline violence exposure (Table A.5). If we compare the treatment effects against the baseline effect of being maimed, the treatment is predicted to worsen PTSD by 28%, depression by 47% and anxiety by 37%.

These sizable negative impacts on psychological wellbeing suggest that talking about the past can bring up painful memories and re-traumatize individuals. This effect emerges even though individuals who went through the reconciliation process forgave their perpetrators and engaged more with the community afterward. These results challenge the idea that reconciliation promotes societal healing through psychological healing, and suggest that these two forms of healing do not have to move concurrently in response to reconciliation processes.

6.4 The Persistence of Societal and Individual Impacts

A key issue is whether these effects persist over time. For example, do the estimates reflect short-run effects on forgiveness? Or, is the trauma from reliving the past sharpest in the first months after the ceremony?

We are able to explore short-run and long-run effects using wave one data, where the endlines were administered 9 and 31 months after the ceremonies, respectively. Table 7 presents the results separately by these two rounds.

Since the first wave includes less than half the sections in the evaluation, this is a

the question: "In the last month, did you sit and think of bad bad things that happened to you even though you don't want to think of it?" Of the responses "never" "yes small small" "yes sometimes" and "yes often", we conservatively code a respondent as having a symptom if they report either "yes sometimes" or "yes often."

relatively under-powered sample, and some of the effects are not individually significant. Yet, the pattern in this table shows that most effects — both positive and negative — persist over time.

First, the forgiveness effect persists: to the extent that respondents have been able to let go of their anger toward former combatants, these changes don't appear to recede over time. While trust toward former combatants is individually insignificant in both rounds, these coefficients are not significantly distinguishable from each other at the 5% level, indicating that the effects do not diminish in a meaningful manner. Trust of migrants also persists, and there are even short run improvements in attitudes toward former combatants and the generalized trust measures, though these effects fall – and significantly so – over the longer horizon.

Notably, increased participation in community groups is also sustained for over two and a half years down the road. Public goods contributions and social networks are individually insignificant in both sub-samples, but the coefficients display a similar pattern, increasing in magnitude during the second round. As such, the reconciliation effects do not reflect a short run burst in community orientation and civic engagement that subsequently fade away.

At the same time, Table 7 reveals that the psychological impacts also persist over the course of the two rounds. These negative impacts also did not diminish with time.

6.5 Additional Results

We present a number of additional results in the Appendix, which we summarize briefly here. We find that economic outcomes and activities did not increase systematically in response to treatment (Table A.7). There is an interesting pattern whereby an objective assets indicator increased while perceptions of economic wellbeing decreased, which could reflect a more negative outlook from greater depression in treatment areas. However, as a whole, we see no consistent effects. In particular, there were no significant effects on farming related activities such as labor devoted to working others'

farms. This suggests a limited impact of the FT communal farms.

Also, there were no systematic reductions on crimes and conflicts, or improvements in their resolution, though we do observe greater resolution by chiefs and less by friends and family (Table A.8). The overall conflict effects suggest that the reconciliation process influences outcomes related to the war, but doesn't prevent the occurrence of other day-to-day disputes. These results reinforce the idea that war-related reconciliation is an important element of the intervention. If merely gathering the community was crucial in improving inter-personal dynamics, then we should observe other outcomes such as day-to-day disputes falling or economic activities increasing. We do see some improvements in attitudes toward women (Table A.8), though these effects are not robust to the inclusion of additional controls (Table A.10 and A.15).

We also present additional robustness checks. We control for the presence of FT communal farms (Table A.10), since only some treatment areas had a farm. Our core results are unaffected, suggesting our results are not driven by the communal farms. We also find no differential effects of gender (Table A.11), which suggests that Peace Mothers groups are not key drivers of the treatment effect. The results together suggest that, while Fambul Tok has several components, our results are likely due to the reconciliation component rather than auxiliary impacts on economic activity from communal farms or the Peace Mothers.

We also find no differential effects based on exposure to violence (Table A.12) or being a former combatant (Table A.13). Experiencing more violence may generate a greater need for reconciliation, or may make it more difficult to reconcile. The null violence interactions are consistent with this theoretical ambiguity and suggest neither effect dominates the other. Finally, Tables A.14 and A.15 show that constructing the mean indices using the approach of Anderson (2008) and controlling for baseline imbalance do not meaningfully affect our results.

7 Conclusion

Countries emerging from internal conflict face the challenge of rebuilding social capital and renewing psychological wellbeing within their population. Yet, we have a limited understanding of how to ignite these processes, or how they relate to one another. In particular, we know little about the workings of reconciliation efforts, which are frequently posited to heal on both levels.

Our paper provides insight into the relationship between societal and individual healing in the aftermath of war. We present results from a novel randomized control trial of community-level reconciliation in Sierra Leone. The reconciliation process increased forgiveness and improved social capital: social networks were stronger and people contributed more to their communities in treatment villages. These are important impacts since vast resources are spent trying to improve social capital outcomes in post-conflict contexts. For example, a well-implemented CDD/R program in Sierra Leone found no impacts on public goods provision (Casey et al. 2012).

Yet, our study shows that the reconciliation process also exerted negative psychological impacts, leading to greater trauma, anxiety and depression within the treated areas. These effects persisted for nearly three years after the reconciliation process was completed. Together, these findings indicate that psychological healing is not a prerequisite for societal healing. Reconciliation can restore fractured relationship and rebuild social capital even if the process proves emotionally painful and worsens psychological health. In short, one form of healing can come at the expense of the other.

Our findings highlight the long shadow of war along two dimensions. The program we study was implemented nearly 10 years after the end of the civil war. The positive effects on forgiveness and social capital therefore suggest that the need for reconciliation persists long after the violence ends. At the same time, bringing up war accounts through truth telling opened up psychological wounds, suggesting the potency of these memories. An important remaining question is whether these effects would differ had reconciliation occurred in the direct aftermath of the conflict. For example, the psy-

chological impacts may have been incrementally smaller if trauma levels were already high owing to more recent memories of the war.

The negative psychological effects are an important additional cost beyond the direct monetary cost of the reconciliation process – and these costs should clearly be taken into account in considering the design of such programs. In addition, combining truth telling with more sustained counseling could help mitigate the detrimental impacts on trauma. Most truth and reconciliation programs focus on getting participants to talk about the past, rather than working with them after this event. Sustained subsequent counseling may be a beneficial addition to current reconciliation approaches. Future research should seek to explore these issues.

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Table 1. Baseline Balance

VARIABLES	T-C	Std. Error	Obs.
Market in village	0.002	(0.033)	2,075
Village size	16.486	(30.803)	2,158
No formal education	-0.000	(0.015)	2,208
Forgive perpetrators	-0.107	(0.153)	1,862
Ex-combatants would not Fight	0.011	(0.034)	2,191
Trust of Ex-combatants	-0.022	(0.054)	1,546
Trust of Migrants	-0.059*	(0.032)	1,962
Index of Generalized Trust	0.052*	(0.029)	2,211
Index of Community Group Participation	-0.016	(0.020)	2,213
Index of Public Goods Contributions	-0.034	(0.021)	2,214
Index of Psychological Wellbeing	0.023	(0.038)	2,202

Notes. Each row represents a separate regression of the baseline variable shown in the first column on treatment assignment. All regressions include section pair fixed effects. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 2. Forgiveness, Trust and Attitudes Toward War

VARIABLES	Control mean	Coeff.	Std. Error	Obs.	R-sqr.	Specification
Forgiveness						
Forgive perpetrators	2.264	0.571**	(0.227)	2,010	0.131	Ancova
Forgive perpetrators (based on questions in both baselines)	0.951	0.277*	(0.145)	2,085	0.121	Ancova
Attitudes Related to the War						
Index of Attitudes toward Ex-Combatants	-	-0.007	(0.029)	2,980	0.075	Ancova
Indicators:						
Those who did bad things during the war would do it again	2.582	0.018	(0.030)	2,966	0.060	Ancova
Rebels are not responsible for their actions	2.832	-0.025	(0.029)	2,966	0.089	Ancova
Index of War Attitudes	-	-0.024	(0.030)	3,000	0.057	CS
Indicators:						
If another war, people would not fight	0.780	-0.023	(0.016)	3,000	0.099	CS
People would not be a part of another rebellion	0.853	-0.030**	(0.015)	3,000	0.070	CS
If another war, you would not fight	0.038	0.013*	(0.007)	3,000	0.040	CS
Trust						
How much do you trust ex-combatants?	1.875	0.177**	(0.079)	900	0.222	Ancova
How much you trust migrants to this community?	3.161	0.123***	(0.033)	2,203	0.172	Ancova
Index of Generalized Trust in Community	-	0.006	(0.027)	2,996	0.135	Ancova
Indicators:						
People are honest and can be trusted	2.598	0.014	(0.026)	2,994	0.126	Ancova
People in village are honest and can be trusted	2.858	-0.010	(0.020)	2,976	0.167	Ancova
People in community would not betray fellow community members	2.550	0.003	(0.028)	2,976	0.059	Ancova
Money left out accidentally will still be there an hour later	0.365	0.010	(0.020)	2,956	0.141	Ancova

Notes. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. CS denotes a cross-sectional specification. Variables not shown in all regressions include section pair fixed effects and the second round indicator. Ancova specifications also include the baseline outcome variable, and the interaction of this variable with both the second round indicator and the second wave indicator. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level. The control mean is the mean in the control group at endline.

Table 3. Social Networks

VARIABLES	Control mean	Coeff.	Std. Error	Obs.	R-sqr.	Specification
Index of Network strength	-	0.099***	(0.001)	3,008	0.061	CS
Indicators:						
Number of people respondent would approach for advice / help	2.894	0.148**	(0.033)	3,005	0.056	CS
Number of people respondent would ask to collect money for them	3.144	0.155	(0.279)	3,005	0.026	CS
Number of times respondent listed as good friend	2.123	0.232**	(0.013)	3,008	0.192	CS
Number of times respondent listed as someone to ask for advice / help	3.245	0.362***	(0.005)	3,008	0.199	CS

Notes. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. CS denotes a cross-sectional specification. Variables not shown in all regressions include section pair fixed effects and the second round indicator. Ancova specifications also include the baseline outcome variable, and the interaction of this variable with both the second round indicator and the second wave indicator. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level. The control mean is the mean in the control group at endline.

Table 4. Reconciliation and Participation in Community Groups

VARIABLES	Control Mean	Coeff.	Std. Error	Obs.	R-sqr.
Index of Participation in Community Groups	-	0.058***	(0.017)	3,004	0.160
Index of Participation in Community Groups - w/out women's membership / meetings	-	0.064***	(0.017)	3,004	0.162
Indicators:					
PTA membership	0.137	0.034**	(0.016)	2,732	0.223
Village development committee membership	0.091	0.013	(0.011)	2,737	0.141
Youth group membership		0.015*	(0.008)	2,738	0.144
Women's group membership	0.118	0.022	(0.014)	2,004	0.138
Secret society membership	0.358	-0.058***	(0.019)	2,770	0.338
Religious group membership	0.286	0.055***	(0.020)	2,729	0.179
PTA meeting attendance	0.082	0.037**	(0.015)	2,739	0.138
Village development committee meeting attendance	0.068	0.008	(0.010)	2,734	0.106
Youth group meeting attendance	0.066	0.007	(0.008)	2,739	0.090
Women's group meeting attendance	0.075	0.024*	(0.013)	2,004	0.095
Secret society meeting attendance	0.056	-0.005	(0.008)	2,766	0.057
Religious group meeting attendance	0.190	0.058***	(0.016)	2,714	0.103
Community meeting attendance	0.626	0.006	(0.013)	2,983	0.077

Notes. Each row represents a separate Ancova regression of the outcome shown in the first column on treatment assignment. Variables not shown include section pair fixed effects, the second round indicator, the baseline outcome variable, and the interaction of the baseline outcome variable with both the second round indicator and the second wave indicator. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level. The control mean is the mean in the control group at endline.

Table 5. Reconciliation and Contributions to Public Goods

VARIABLES	Control Mean	Coeff.	Std. Error	Obs.	R-sqr.
Index of Public Goods Contributions	-	0.042*	(0.022)	3,008	0.171
Index of Public Goods Contributions (w/out contributions to women's group)	-	0.046**	(0.023)	3,008	0.184
Indicators appearing in endline and baseline:					
Contributed to public facilities	0.397	0.029	(0.019)	2,911	0.078
Brushed roads	0.290	0.005	(0.014)	2,898	0.171
Number of community projects (village level variable)	0.527	-0.049	(0.055)	2,901	0.308
Contributed to PTA	0.066	0.023*	(0.013)	2,732	0.105
Contributed to village development committee	0.062	0.001	(0.008)	2,737	0.119
Contributed to youth group	0.069	-0.002	(0.006)	2,738	0.081
Contributed to women's group	0.064	0.021**	(0.010)	2,004	0.076
Contributed money to someone in need	0.196	0.015	(0.016)	2,866	0.097

Notes. Each row represents a separate Ancova regression of the outcome shown in the first column on treatment assignment. Variables not shown include section pair fixed effects, the second round indicator, the baseline outcome variable, and the interaction of the baseline outcome variable with both the second round indicator and the second wave indicator. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level. The control mean is the mean in the control group at endline.

Table 6. Psychological Wellbeing

VARIABLES	Control mean	Coeff.	Std. Error	Obs.	R-sqr.
Index of Psychological Wellbeing (All indicators)	-	-0.147***	(0.033)	2,982	0.115
Index of Psychological Wellbeing (Indicators in both baselines)	-	-0.138***	(0.031)	2,982	0.115
Indicators (in both baselines):					
Less PTSD	28.819	-0.683***	(0.197)	2,776	0.119
Less Anxiety	14.945	-0.441***	(0.117)	2,895	0.142
Less Depression	11.677	-0.289***	(0.069)	2,913	0.092
PTSD Symptoms present	0.080	0.029***	0.011	2776	0.057

Notes. Each row represents a separate Ancova regression of the outcome shown in the first column on treatment assignment. Variables not shown include section pair fixed effects, the second round indicator, the baseline outcome variable, and the interaction of the baseline outcome variable with both the second round indicator and the second wave indicator. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level. The control mean is the mean in the control group at endline.

Table 7. Persistence of Effects

Sample:	Round 1			Round 2		
VARIABLES	Coeff.	Std. Error	Obs.	Coeff.	Std. Error	Obs.
Forgive Perpetrators	0.986***	(0.272)	550	1.231***	(0.361)	521
Trust Ex-combatants	0.100	(0.073)	241	0.048	(0.198)	203
Trust Migrants	0.140**	(0.053)	653	0.119*	(0.069)	564
Index of Generalized Trust in Community	0.119**	(0.050)	878	-0.009	(0.038)	845
Index of Attitudes toward Ex-Combatants	0.115**	(0.052)	875	-0.065	(0.055)	841
Index of War Attitudes	0.015	(0.033)	828	-0.039	(0.063)	789
Index of Network Strength	0.015	(0.027)	885	0.119	(0.085)	850
Index of Community Group Participation	0.038*	(0.022)	884	0.084**	(0.040)	847
Index of Contributions to Public Goods	0.024	(0.033)	885	0.035	(0.046)	850
Index of Psychological Wellbeing	-0.166***	(0.052)	873	-0.170***	(0.058)	837

Notes. These results present separate estimates for the two endline rounds in Wave One. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. Variables not shown include section pair fixed effects and the second round indicator. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level. The control mean is the mean in the control group at endline.

A Appendix

A.1 Aggregation of Questions under PAP

A.1.1 Conditional and Unconditional Outcomes

Our Pre-analysis Plan (PAP) specified how particular indicators would be aggregated in our examination of various hypotheses. As in Casey et al. (2012) and Humphreys et al. (2012), some of our original hypotheses combined 'conditional' outcomes, that relate to a sub-sample of respondents, with 'unconditional' outcomes, that relate to the full sample.

However, this could create a bias in the Anderson (2008) index due to sample selection, and may induce false rejection of the null in the Kling et al. (2007) index, if the two sample sizes may differ substantially. For example, in measuring trust, all respondents answered how much they trust people in general, but only respondents who personally knew ex-combatants answered how much they trusted this group, which was less than half the sample.

The Anderson (2008) approach would create a composite trust index restricted to this latter half. But the selected sample of individuals who know ex-combatants could have different levels of generalized trust, and, their trust levels may also respond differently to treatment. So, this approach could produce a biased treatment effect that is not representative of the full sample. The Kling et. al (2007) approach would create a composite trust index by imputing values to half the sample, for whom one of three major indicators would be missing. Imputation at this scale could artificially reduce the standard errors because the sample size increases without increasing true underlying variation.

We therefore made changes to three of our hypotheses that contained both 'conditional' and 'unconditional' outcomes. We focused our mean effect index of trust on four community-wide measures that were administered to all respondents. We separately examine trust of ex-combatants and migrants which were asked of sub-samples that knew these sub-groups. These same issues apply to the forgiveness hypothesis. The original index attempted to aggregate across three sub-groups: those that experienced hurt in the past, those who personally know the perpetrator, and those whose perpetrator still resides in the village. But the sample sizes of the final two subgroups, constituting affirmative responses from among twelve randomly sampled respondents, are too small for any useful analysis. We therefore look separately at the first group– forgiveness of all people who experienced

hurt in the past.

A.1.2 Other Changes

Similar aggregation issues applied to our analysis of conflict resolution, which we present as auxiliary results in Appendix Table A.8 of the Appendix. Our original PAP proposed to examine both the change in proportion of conflicts were resolved, we well as: the proportion of conflicts that were resolved without a third party; and the proportion that were resolved satisfactorily. Of course, the latter two variables are conditional on the former. In addition, a separate grouping proposed to look at the proportion resolved by chiefs as well as fines levied by the chiefs, but fines apply to a much broader set of cases than conflicts resolved by chiefs. Finally, one of our indicators – resolution without a third party – displayed zero variation in the baseline sample, which makes it infeasible to create a mean index using this measure. Given this limitation, we simply show each of the indicators in the table. Since there was a reported increase in the fraction of conflicts resolved by chiefs, we thought it was informative to assess if there were decreases in resolution in other categories. So we additionally examined resolution via mediation with friends and family. These results are discussed further below.

Finally, we had to make some changes to the social network questions. The way the questions were asked in the baseline survey proved problematic because some answers displayed limited variation. In the wave one baseline, the questions were prompted too strongly: respondents were asked to consider, in turn, each of the 11 other individuals being surveyed. (For example: "Now consider John Koroma. Would you be willing to share a farm boundary with John Koroma?"). As a result most of the respondents were listed by everyone else, but this was not a meaningful measure of actual connectedness. In contrast, in the second wave baseline, the questions weren't prompted at all. (For example, "name all the people you will be willing to share a farm boundary with"). As a result, respondents were almost never listed by anyone else, which again did not serve as a meaningful measure. Given these issues, in the endline, we undertook several additional changes. First, we entirely dropped the farm boundary question since this showed the least variation. In one part of the survey, the questions were unprompted. (For example, "Say you have a problem. Think of all the people from this village who you would go to for advice and help. Who are these people?").

The unprompted questions were used to construct how many times the respondent lists someone else. In another part of the survey the questions were lightly prompted: the respondent was asked to consider, jointly, the 11 other respondents being surveyed. (For example, "Of the people I named, name the people (if any) who you would go to for advice or help.") The lightly prompted questions were used to construct an indicator for how many times the respondent was listed by someone else. Furthermore, since the social network questions were very time-intensive to administer, we no longer asked the "who would you ask to go collect money for you?" in the prompted format. As a result of these changes, we had to conduct cross-sectional analysis on a sub-set of the social network questions that were administered in the endline surveys.

A.2 Data on Additional Outcomes

Here, we provide an overview of the additional variables collected in our survey, that we use for results presented in the appendix. We tracked the incidence of conflicts related to loans, land, property and religion that the respondent had experienced over the past 6 months. We also recorded the method of conflict resolution and the degree of satisfaction with conflict resolution. We asked about fines levied as a form of punishment. In addition, we tracked the incidence of violent and non-violent crimes. Finally, we recorded inter-village disputes over this period, though this is a village level variable, and as discussed in the paper, we are missing a considerable number of village surveys.

Our measure of social tensions comes from the World Bank[1] includes an indicator of the extent to which divisions between non-marginalized and marginalized groups (migrants vs. non-migrants, the young vs. old, and the poor vs. rich) escalate into violence; as well as feelings of inclusion as measured by the extent to which respondents feel they would benefit from community resources such as donations and the extent to which they feel their voice is heard.

We also tracked various types of economic activity. Our index includes measures of the frequency and size of lending and borrowing; time spent working on other people's farms over the past 3 months;[2] the number of communal farms; and the number and use of traders in the community. To measure economic well-being, we constructed an index of household assets and housing quality, using

[1] http://go.worldbank.org/BOA3AR43W0

[2] In a poor agricultural society such as Sierra Leone, sharing of farm labor serves as an important form of economic exchange.

principal component analysis (PCA). This index includes 16 household goods and whether the roof is made out of straw and the walls out of mud. We also asked respondents to report their subjective assessments of their ability to meet basic household needs such as school fees and health care, and their perceptions on whether their household situation has improved within the past year. The assets measure, along with the two subjective assessments comprise an index of economic outcomes used in the analysis.

We additionally measured gender attitudes by asking about if the respondent agrees with seven circumstances under which it is acceptable for "a man to beat his wife," and whether a wife has a right to express her own opinions.

A.3 Additional Results

Table A.7 examines economic outcomes. Here, we observe that the treatment induces a significant increase in the objective measure of household wellbeing, which is an asset index constructed by PCA. In contrast, there are negative effects on the two perceptions-based measures, and the effect is significant for the perceptions of overall household economic situation. This pattern is consistent with the idea that lower psychological wellbeing, such as depression, reduced perceptions of economic wellbeing, even while households in treatment areas experienced economic improvements. Aggregated together, these effects produce a negative impact on the index of economic outcomes.

However, as we show in the second panel of this table, individual indicators within the economic outcomes index were imbalanced at baseline (although the aggregate index was not). When we control for these baseline indicators individually, the overall effect is insignificant. In the second part of Table A.7, we also examine effects on a number of different economic activities, such as borrowing and lending and farming. While six of eight indicators are positive, the overall effect is not significant. Thus, there is no clear evidence of the reconciliation process influencing economic outcomes.

In Table A.8, we examine effects on conflicts. We find no significant impacts on an index of social tensions that focuses on divisions between groups. To examine effects on day-to-day conflicts, we sum the number of crimes and disputes over matters such as loans and land at the village level; we add this to a measure of inter-village disputes to construct a mean index. We look at this index cross-sectionally since we are missing surveys in a number of villages at baseline, and the only way

of incorporating baseline values of these would be by imputing missing data for whole villages, which could exert bias in an indicator comprised of a few indicators. The impact on the overall conflict index is also insignificant. These results suggest that while reconciliation can facilitate forgiveness for violence committed during the war, it can't necessarily mitigate other types of conflicts between households and groups, or prevent people from committing crimes in their community.

While conflict incidence did not change, there were some compositional changes in how conflicts were resolved. A larger fraction of conflicts were resolved by the chiefs in treatment areas, while a smaller fraction were resolved through friends and family. As discussed previously, chiefs were often targeted by youths in their communities during the war. If the reconciliation process restored these relationships, this may influence the degree to which individuals rely on the chiefs for conflict resolution.

In the last panel of Table A.8, we examine attitudes related to gender. Our index of attitudes toward women captures views on domestic violence as well as the rights of wives to express their opinions.[3] We find that reconciliation communities experienced significant improvements in this indexed outcome. These outcomes could shift because the reconciliation process highlighted challenges faced by women during the war, or because the Peace Mothers groups empower women. However, the magnitude of this effect doesn't vary significantly for men versus women (see Table A.11). This suggests that it is a more community-wide effect than participation in the women's groups. However, this effect is not as robust to controls as other estimates (see Table A.10 and A.15).

Table A.9 examines the persistence of these additional outcomes over the first and second rounds of the Wave 1 communities. These results suggest that the positive impact on household assets is not restricted to the short-run. And, the negative impact of the two subjective indicators emerge across different rounds, which again shows the inconsistency in the impact of the perceptions measures. The gender attitude index is also stronger in the short run and appears to diminish over time.

In Table A.10, we control for the presence of FT communal farms. (Some treated areas had farms while others did not). Of course this is an over-control since it controls out for our treatment. Yet, none of the estimated effects are rendered insignificant in a meaningful manner with the inclusion of this control. The coefficient on trust of ex-combatants becomes insignificant but this coefficient is

[3]The wave two baseline asked a limited set of questions related to attitudes toward wife beating, and didn't include the opinion question, so we control for the pared down control set in the ANCOVA regressions of this outcome.

not statistically distinguishable at the 5% level from the estimate in Table 2. Overall, this suggests that the treatment effects do not stem primarily from the presence of communal farms in treatment communities.

Next, we examine heterogeneous treatment effects based on gender and history of violence. It is only meaningful to examine differential effects of individual characteristics on outcomes that vary at the individual level (versus at the household or village level). Since several of the economic activity variables were at the village level, we created another index of just individual economic activities. Also, assets are household level measures; but the subjective perceptions are asked of individuals, so we are also able to examine these outcomes.

Table A.11 reports differential effects by gender. None of the coefficients on the Treatment x Female interactions are significant, with the exception of the borrowing and lending measure, which shows a smaller treatment effect for females on this economic outcome. If the Peace Mother's Group were a key driver of impacts in treated areas, we would expect to observe larger treatment effects for women, especially on economic activities. This table presents evidence against that account.

Next, we examine heterogeneous effects based on exposure to violence. Theoretically, two effects are possible. On the one hand, someone who has experienced more violence may have a greater need for reconciliation, which implies that treatment effects should be larger for these individuals. On the other hand, a violent experience may also mean that they have more to forgive (as the baseline Table A.5 shows); and this implies that the treatment effects should be smaller. Table A.12 presents these estimates. We find no meaningful differential impacts. This is consistent with the two forces offsetting each other. Similarly, in Table A.13, we find no differential treatment effects associated with the subset of individuals who we are able to identify as ex-combatants.

Table A.14 shows the results for the mean effect indices as constructed by the approach of Anderson (2008). The only effect that changes in terms of statistical significance is the index for public goods contributions, which arises from a large loss of observations owing to missingness in individual indicators within the index. No one public goods indicator is missing for a large number of observations (see Table 5), but each of them is missing for some, and when they are aggregated together without imputation in the Anderson index, it produces an overall sizable loss. This is precisely the circumstance under which it is useful to utilize some form of imputation as the Kling et. al method-

ology does. Under this methodology, the number of imputed observations is relatively small for any one indicator in the public goods index. Moreover, note that the estimated effect for this outcome under the Anderson approach is not statistically distinguishable from the estimated effect under the Kling approach at the 5 percent level.

Finally, Table A.15 presents the results when we control for the variables that show imbalance. Note that we cannot control for trust of migrants variable at baseline without reducing the sample by nearly 30% since this question was only administered to those who knew migrants. However, we control for the generalized trust index and the two subjective economic indicators that showed some imbalance. We find that the index of attitudes becomes statistically insignificant, but all remaining results are unaffected by the inclusion of these additional controls.

Table A.1 Program Implementation

VARIABLES	Coeff.	Std. Error	Obs.	R-sqr.
Heard of Fambul Tok	0.426***	(0.030)	3,003	0.296
Fambul Tok held bonfire	0.689***	(0.057)	3,008	0.576
Fambul Tok communal farm	0.190***	(0.036)	3,008	0.343
Fambul Tok Peace Tree	0.265***	(0.033)	3,008	0.273
Fambul Tok Peace Mother's Group	0.406***	(0.046)	3,008	0.381

Notes. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. Variables not shown include section pair fixed effects and the second round indicator. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A.2 Attrition

	(1) Attrition-1	(2) Attrition-2
Treatment	0.009	0.002
	(0.012)	(0.005)
Sample	Wave 1	Wave 1 and 2
Obs.	2,382	952
R-sqr.	0.044	0.025

Notes. Each column represents a separate regression of an attrition measure on treatment assignment. Attrition-1 equals one if the respondent surveyed at endline was not re-interviewed at endline wave two, or re-interviewed in either of the two endline rounds in wave one. Attrition-2 equals one if the wave one respondent was not re-interviewed in both endlines. Variables not shown include section pair fixed effects. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A.3 Descriptive Statistics

VARIABLES	Obs.	Mean	Std. Dev	Min	Max
Individual and Village Characteristics (Baseline):					
Gender	2212	0.549	0.498	0	1
No formal education	2208	0.717	0.451	0	1
Occupation farmer	2345	0.744	0.436	0	1
Market in village	2075	0.085	0.279	0	1
Village size (number households)	2135	185.727	331.354	9	2811
Beaten	2097	0.329	0.470	0	1
Maimed	2099	0.020	0.138	0	1
Raped	2092	0.030	0.170	0	1
Family member killed	2157	0.535	0.499	0	1
Saw Violence	1749	0.440	0.496	0	1
Panel Outcomes (Baseline and endline):					
Forgive Perpetrators	4296	-0.217	3.901	-10.5	10.5
Ex-combatants would not Fight	5191	2.552	0.794	1	4
Trust of Ex-combatants	3016	1.823	0.981	1	4
Trust of Migrants	4484	3.074	0.775	1	4
Index of Generalized Trust	5212	0.014	0.686	-1.987	1.932
Index of Community Group Participation	5218	0.011	0.429	-0.573	2.437
Attitude toward Wife Beating	5185	10.468	1.930	4	12
Index of Economic Outcomes	5222	-0.017	0.555	-1.545	6.350
Index of Economic Activity	5222	0.002	0.481	-1.052	11.839
Index of Group Tensions	5212	0.004	0.581	-2.778	1.470
Index of Psychological Wellbeing	5205	-0.035	0.839	-5.506	1.907
Less PTSD	5067	26.769	5.746	0	33
Less Anxiety	5141	13.356	3.929	0	21
Less Depression	5158	10.988	2.380	0	15
Cross-sectional Outcomes (Endline):					
Forgive Perpetrators	2434	2.502	5.408	-18	18
Index of War Attitudes	3000	-0.011	0.692	-1.675	2.526
If another war, people would not fight	3000	0.770	0.421	0	1
People would not be a part of another rebellion	3000	0.838	0.368	0	1
If another war, you would not fight	3000	0.045	0.207	0	1
Index of Network strength	3008	0.047	0.817	-1.144	27.597
Number of people respondent would approach for advice / help	3005	2.961	2.193	0.000	47.000
Number of people respondent would ask to collect money	3005	3.214	5.223	0	244
Number of times respondent listed by others as good friend	3008	2.236	2.023	0	13
Number of times respondent listed by others for advice / help	3008	3.419	2.887	0	16

Table A.4 Baseline Balance on Additional Outcomes

VARIABLES	T-C	Std. Error	Obs.
Attitude toward Wife Beating	-0.350	(0.226)	912
Index of Conflict and Crime (Village level variable)	-0.071	(0.072)	190
Index of Economic Activity	-0.026	(0.024)	2,214
Index of Economic Outcomes	0.019	(0.031)	2,214

Notes. Each row represents a separate regression of the baseline variable shown in the first column on treatment assignment. All regressions include section pair fixed effects. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A.5 War Exposure, Baseline Forgiveness and Psychological Health

VARIABLES	(1) Forgive Perpetrators	(2) Less PTSD	(3) Less Anxiety	(4) Less Depression
Raped	-1.211**	-2.358***	-0.512	-0.529
	(0.544)	(0.849)	(0.575)	(0.462)
Observations	1,470	1,918	1,986	1,999
Maimed	-0.564	-2.471***	-1.193**	-0.613
	(0.803)	(0.928)	(0.494)	(0.536)
Observation	1,475	1,925	1,990	2,005
Family member killed	-0.920***	-1.140***	-0.402**	-0.330***
	(0.232)	(0.286)	(0.195)	(0.120)
Observations	1,500	1,972	2,039	2,056

Notes. Each cell represents a separate regression of the respondent's war exposure on the baseline measure of the variables Forgive perpetrators, Less PTSD, Less Anxiety and Less Depression. All regressions include section pair fixed effects. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A.6 Robustness: Trust of Ex-Combatants and Migrants

VARIABLES	Coeff.	Std. Error	Obs.	R-sqr.	Specification
Do you know any ex-combatants?	-0.034	(0.024)	2,970	0.186	Ancova
Do you know an migrants?	-0.012	(0.013)	3,008	0.116	Ancova
Do you know any ex-combatants?	-0.024	(0.024)	3,000	0.179	CS
Do you know an migrants?	-0.011	(0.013)	3,008	0.109	CS
How much do you trust ex-combatants?	0.145**	(0.066)	1,470	0.177	CS
How much you trust migrants to this community?	0.083***	(0.032)	2,522	0.167	CS

See Table A.1 for notes.

Table A.7 Economic Activity and Outcomes

VARIABLES	Control Mean	Coeff.	Std. Error	Obs.	R-sqr.
Index of Economic Outcomes	-	-0.036*	(0.019)	3,008	0.161
Index of Economic Outcomes - control for baseline of individual economic indicators	-	-0.027	(0.020)	2,839	0.227
Indicators:					
Objective indicator of household assets	0.047	0.145***	(0.055)	2,836	0.416
Perception that household needs are met	10.079	-0.117	(0.072)	2,835	0.102
Perception of overall household economic situation compared to one year ago	2.882	-0.131***	(0.037)	2,831	0.088
Baseline balance on individual economic outcome indicators					
Indicator: Household assets index	-	-0.031	(0.065)	2,205	
Indicator: Perception that household needs met	-	0.383***	(0.144)	2,131	
Indicator: Perceived satisfaction with household economic situation	-	-0.107*	(0.054)	2,133	
Index of Economic Activity	-	0.034	(0.026)	3,008	0.182
Indicators:					
Frequency of borrowing and lending	2.17	0.043	(0.036)	3,008	0.461
Monetary value of borrowing and lending	5.17	0.302	(0.221)	2,915	0.104
Respondent belongs to an *osusu* (savings group)	0.396	-0.015	(0.018)	2,950	0.144
Number of traders (village level indicator)	9.356	0.743	(1.513)	2,710	0.501
Respondent buys from trader	0.899	-0.011	(0.011)	2,956	0.076
Number of communal farms (village level indicator)	0.558	0.096	(0.103)	2,820	0.359
Respondent belongs to a labor gang	0.333	0.002	(0.016)	2,738	0.164
Days spent working on other's farms	7.96	0.473	(0.618)	2,418	0.130

Notes. In the first and third panel, each row represents a separate Ancova regression of the outcome shown in the first column on treatment assignment. Variables not shown include section pair fixed effects, the second round indicator, the baseline outcome variable, and the interaction of the baseline outcome variable with both the second round indicator and the second wave indicator. In the second panel, the baseline values of each indicator is regressed on treatment assignment. In all specifications, standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level. The control mean is the mean in the control group at endline.

Table A.8 Societal Conflicts

VARIABLES	Control Mean	Coeff.	Std. Error	Obs.	R-sqr.	Specification
Social Tensions						
Index of Social Tensions	-	0.028	(0.021)	2,996	0.085	Ancova
Indicators:						
Dominant groups do not benefit more from community resources	1.127	0.026	(0.040)	2,963	0.094	Ancova
Marginalized groups benefit from community resources	7.367	0.053	(0.056)	2,809	0.092	Ancova
Respondent feels included and respected in the community	0.95	0.000	(0.005)	2,960	0.050	Ancova
Perception that social divisions escalated into conflict	0.506	0.037	(0.037)	2,943	0.140	Ancova
Conflict and Crime						
Index of Conflict and Crime	-	0.112	(0.072)	274	0.275	CS
Indicators:						
Number of conflicts	0.158	0.002	(0.019)	274	0.320	CS
Number of crimes	0.039	-0.005	(0.007)	274	0.226	CS
Number of violent crimes	0.004	0.003	(0.003)	273	0.172	CS
Number of inter-village conflicts	0.124	0.122***	(0.042)	274	0.295	CS
Conflict Resolution						
Resolved	0.85	-0.057	(0.050)	172	0.330	Ancova
Satisfactory resolved	0.753	-0.107	(0.067)	172	0.456	Ancova
Resolved without third party	0.218	-0.036	(0.035)	172	0.603	Ancova
Resolved with mediation from family/friends	0.428	-0.141**	(0.055)	172	0.547	Ancova
Resolved by chief	0.43	0.103*	(0.058)	172	0.326	Ancova
Fined by chief	0.09	-0.007	(0.009)	280	0.255	Ancova
Gender Attitudes						
Index of Attitude toward Women	-	0.044*	(0.025)	2,982	0.036	Ancova
Indicators:						
Attitude toward wife beating	18.856	0.081	(0.115)	2,957	0.036	Ancova
Belief that a wife has a right to her own opinion	0.888	0.019**	(0.008)	2,957	0.055	Ancova

Notes. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. CS denotes a cross-sectional specification. Variables not shown in all regressions include section pair fixed effects and the second round indicator. Ancova specifications also include the baseline outcome variable, and the interaction of this variable with both the second round indicator and the second wave indicator. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level. The control mean is the mean in the control group at endline.

Table A.9 Persistence of Additional Outcomes

	Coeff.	Std. Error	Obs.	Coeff.	Std. Error	Obs.
Sample:	**Round 1**			**Round 2**		
VARIABLES						
Index of Economic Outcomes	-0.014	(0.029)	885	-0.014	(0.023)	850
Assets	0.195**	(0.092)	879	0.182**	(0.083)	842
Perception of household needs	-0.052	(0.142)	806	-0.146*	(0.080)	780
Perception of economic situatoin	-0.113*	(0.057)	811	-0.067	(0.060)	784
Index of Economic Activity	-0.025	(0.041)	885	0.021	(0.035)	850
Index of Conflict and Crime	0.201	(0.139)	80	0.130	(0.122)	78
Index of Social Tensions	0.021	(0.026)	878	0.060	(0.039)	845
Index of Attitudes toward Women	0.068*	(0.038)	877	0.007	(0.045)	844

Notes. These results present separate estimates for the two endline rounds in Wave One. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. Variables not shown include section pair fixed effects and the second round indicator. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level. The control mean is the mean in the control group at endline.

Table A.10 Controlling for FT Communal Farm

VARIABLES	Coeff.	Std. Error	Obs.	R-sqr.	Specification
Forgive Perpetrators	0.640**	(0.248)	2,010	0.132	Ancova
Trust Ex-combatants	0.122	(0.080)	900	0.227	Ancova
Trust Migrants	0.129***	(0.038)	2,203	0.172	Ancova
Index of Generalized Trust in Community	0.038	(0.030)	2,996	0.139	Ancova
Index of Attitudes toward Ex-Combatants	-0.001	(0.031)	2,980	0.075	Ancova
Index of War Attitudes	-0.015	(0.034)	3,000	0.058	CS
Index of Network Strength	0.132***	(0.036)	3,008	0.064	CS
Index of Community Group Participation	0.052***	(0.017)	3,004	0.161	Ancova
Index of Contributions to Public Goods	0.044*	(0.024)	3,008	0.171	Ancova
Index of Psychological Wellbeing	-0.161***	(0.036)	2,982	0.116	Ancova
Less PTSD	-0.732***	(0.224)	2,776	0.115	Ancova
Less Anxiety	-0.455***	(0.128)	2,895	0.139	Ancova
Less Depression	-0.300***	(0.074)	2,913	0.090	Ancova
Index of Economic Outcomes	-0.042**	(0.020)	3,008	0.161	Ancova
Assets	0.105*	(0.055)	2,991	0.403	Ancova
Perception of household needs	-0.167**	(0.080)	2,857	0.083	Ancova
Perception of economic situatoin	-0.125***	(0.042)	2,860	0.081	Ancova
Index of Economic Activity	0.029	(0.029)	3,008	0.182	Ancova
Index of Conflict and Crime	0.112	(0.072)	274	0.275	CS
Index of Social Tensions	0.056**	(0.023)	2,996	0.089	Ancova
Index of Attitudes toward Women	0.041	(0.027)	2,982	0.035	Ancova

See Table A.8 for notes.

Table A.11 Impacts by Gender

VARIABLES	T		T x Female		Obs.	R-sqr.	Specification
	Coeff.	Std. Error	Coeff.	Std. Error			
Forgive Perpetrators	0.249	(0.341)	0.534	(0.568)	2,009	0.138	Ancova
Trust Ex-combatants	0.165	(0.109)	0.024	(0.144)	900	0.222	Ancova
Trust Migrants	0.132***	(0.045)	-0.021	(0.061)	2,203	0.174	Ancova
Index of Generalized Trust in Community	-0.013	(0.035)	0.032	(0.048)	2,995	0.136	Ancova
Index of Attitudes toward Ex-Combatants	-0.054	(0.043)	0.083	(0.055)	2,979	0.077	Ancova
Index of War Attitudes	-0.024	(0.044)	-0.001	(0.051)	2,999	0.058	CS
Index of Network Strength	0.137**	(0.060)	-0.059	(0.076)	3,004	0.075	CS
Index of Community Group Participation	0.069***	(0.025)	-0.024	(0.031)	3,003	0.164	Ancova
Index of Contributions to Public Goods	0.035	(0.029)	-0.001	(0.033)	3,004	0.196	Ancova
Index of Psychological Wellbeing	-0.133***	(0.042)	-0.034	(0.066)	2,981	0.123	Ancova
Less PTSD	-0.526**	(0.260)	-0.328	(0.394)	2,775	0.124	Ancova
Less Anxiety	-0.534***	(0.174)	0.127	(0.266)	2,894	0.149	Ancova
Less Depression	-0.270***	(0.097)	-0.066	(0.156)	2,912	0.103	Ancova
Perception of household needs	-0.222*	(0.112)	0.153	(0.152)	2,856	0.086	Ancova
Perception of economic situatoin	-0.083	(0.060)	-0.092	(0.093)	2,860	0.084	Ancova
Index of Economic Activity (Individual level)	0.029	(0.035)	-0.032	(0.046)	2,099	0.157	Ancova
Frequency of borrowing and lending	0.093	(0.064)	-0.102	(0.084)	3,004	0.465	Ancova
Monetary value of borrowing and lending	0.770**	(0.338)	-0.860**	(0.430)	2,911	0.106	Ancova
Respondent belongs to an *osusu* (savings group)	-0.018	(0.028)	0.010	(0.042)	2,949	0.151	Ancova
Respondent buys from trader	-0.003	(0.018)	-0.016	(0.023)	2,955	0.078	Ancova
Respondent belongs to a labor gang	-0.004	(0.026)	0.002	(0.035)	2,738	0.178	Ancova
Days spent working on other's farms	0.830	(1.106)	-1.124	(1.385)	2,414	0.145	Ancova
Index of Social Tensions	0.020	(0.029)	0.009	(0.046)	2,995	0.088	Ancova
Index of Attitudes toward Women	0.034	(0.036)	0.005	(0.058)	2,981	0.054	Ancova

See Table A.8 for notes.

Table A.12 Impacts by Exposure to Violence

VARIABLES	T		T x Violence-exposed		Obs.	R-sqr.	Specification
	Coeff.	Std. Error	Coeff.	Std. Error			
Forgive Perpetrators	0.552	(0.424)	-0.105	(0.587)	1,945	0.136	Ancova
Trust Ex-combatants	0.229*	(0.116)	-0.045	(0.136)	873	0.226	Ancova
Trust Migrants	0.172***	(0.049)	-0.084	(0.066)	2,108	0.175	Ancova
Index of Generalized Trust in Community	-0.016	(0.036)	0.003	(0.046)	2,861	0.144	Ancova
Index of Attitudes toward Ex-Combatants	-0.009	(0.040)	-0.011	(0.053)	2,848	0.075	Ancova
Index of War Attitudes	-0.039	(0.039)	0.013	(0.049)	2,861	0.060	CS
Index of Network Strength	0.093**	(0.041)	0.040	(0.069)	2,868	0.068	CS
Index of Community Group Participation	0.072***	(0.026)	-0.021	(0.033)	2,865	0.163	Ancova
Index of Contributions to Public Goods	0.037	(0.028)	0.011	(0.032)	2,868	0.177	Ancova
Index of Psychological Wellbeing	-0.160***	(0.052)	0.011	(0.064)	2,852	0.121	Ancova
Less PTSD	-0.871***	(0.309)	0.298	(0.391)	2,662	0.123	Ancova
Less Anxiety	-0.476**	(0.213)	0.003	(0.268)	2,778	0.144	Ancova
Less Depression	-0.270**	(0.127)	-0.044	(0.162)	2,788	0.094	Ancova
Perception of household needs	-0.062	(0.128)	-0.138	(0.157)	2,732	0.085	Ancova
Perception of economic situation	-0.232***	(0.063)	0.154**	(0.076)	2,736	0.088	Ancova
Index of Economic Activity (Individual level)	-0.006	(0.033)	0.016	(0.046)	2,868	0.187	Ancova
Index of Social Tensions	0.014	(0.032)	0.017	(0.042)	2,861	0.084	Ancova
Index of Attitudes toward Women	0.019	(0.039)	0.027	(0.053)	2,847	0.039	Ancova

See Table A.8 for notes.

Table A.13 Impacts by Ex-Combatants

VARIABLES	T		T x Ex-Combatant		Obs.	R-sqr.	Specification
	Coeff.	Std. Error	Coeff.	Std. Error			
Forgive Perpetrators	0.462*	(0.234)	0.828	(0.854)	1,930	0.138	Ancova
Trust Ex-combatants	0.209**	(0.084)	-0.057	(0.209)	868	0.227	Ancova
Trust Migrants	0.128***	(0.035)	-0.009	(0.123)	2,081	0.178	Ancova
Index of Generalized Trust in Community	-0.014	(0.025)	-0.006	(0.094)	2,819	0.141	Ancova
Index of Attitudes toward Ex-Combatants	-0.015	(0.030)	0.001	(0.123)	2,806	0.076	Ancova
Index of War Attitudes	-0.029	(0.028)	-0.092	(0.126)	2,819	0.062	CS
Index of Network Strength	0.100***	(0.036)	0.525	(0.573)	2,826	0.073	CS
Index of Community Group Participation	0.060***	(0.019)	0.007	(0.095)	2,823	0.164	Ancova
Index of Contributions to Public Goods	0.043*	(0.023)	0.064	(0.082)	2,826	0.176	Ancova
Index of Psychological Wellbeing	-0.154***	(0.037)	0.136	(0.157)	2,810	0.125	Ancova
Less PTSD	-0.703***	(0.223)	0.355	(0.914)	2,626	0.126	Ancova
Less Anxiety	-0.495***	(0.127)	0.997	(0.604)	2,736	0.145	Ancova
Less Depression	-0.301***	(0.076)	0.317	(0.348)	2,747	0.095	Ancova
Perception of household needs	-0.112	(0.075)	-0.150	(0.313)	2,700	0.085	Ancova
Perception of economic situation	-0.134***	(0.041)	0.026	(0.213)	2,704	0.086	Ancova
Index of Economic Activity (Individual level)	0.004	(0.023)	0.064	(0.099)	2,826	0.187	Ancova
Index of Social Tensions	0.024	(0.023)	-0.064	(0.097)	2,819	0.082	Ancova
Index of Attitudes toward Women	0.040	(0.028)	0.093	(0.105)	2,805	0.041	Ancova

See Table A.8 for notes.

Table A.14 Impacts using Indices as Constructed by Anderson (2008)

VARIABLES	Coeff.	Std. Error	Obs.	R-sqr.	Specification
Index of Attitudes toward Ex-Combatants	-0.004	(0.029)	2,960	0.073	Ancova
Index of War Attitudes	-0.014	(0.028)	3,000	0.044	CS
Index of Generalized Trust in Community	0.015	(0.029)	2,915	0.121	Ancova
Index of Network Strength	0.076**	(0.012)	3,005	0.112	CS
Index of Participation in Community Groups	0.035**	(0.017)	1,930	0.159	Ancova
Index of Public Goods Contributions	0.022	(0.025)	1,853	0.206	Ancova
Index of Psychological Wellbeing (All indicators)	-0.143***	(0.034)	2,635	0.121	Ancova
Index of Psychological Wellbeing (Indicators in both baselines)	-0.133***	(0.031)	2,667	0.120	Ancova
Index of Economic Outcomes	-0.039*	(0.020)	2,831	0.134	Ancova
Index of Economic Activity	-0.018	(0.035)	1,861	0.239	Ancova
Index of Social Tensions	0.028	(0.021)	2,996	0.085	Ancova
Index of Conflict and Crime	-0.001	(0.064)	273	0.051	CS
Index of Attitudes toward Women	0.044*	(0.026)	2,920	0.037	Ancova

Notes. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. CS denotes a cross-sectional specification. Variables not shown in all regressions include section pair fixed effects and the second round indicator. Ancova specifications also include the baseline outcome variable, and the interaction of this variable with both the second round indicator and the second wave indicator. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A.15 Controlling for Baseline Imbalance

VARIABLES	Coeff.	Std. Error	Obs.	R-sqr.	Specification
Forgive Perpetrators	0.548**	(0.239)	1,919	0.143	Ancova
Trust Ex-combatants	0.222***	(0.076)	860	0.240	Ancova
Trust Migrants	0.110***	(0.034)	2,084	0.181	Ancova
Index of Generalized Trust in Community	0.003	(0.024)	2,832	0.145	Ancova
Index of Attitudes toward Ex-Combatants	-0.012	(0.030)	2,818	0.085	Ancova
Index of War Attitudes	-0.025	(0.029)	2,831	0.062	CS
Index of Network Strength	0.130***	(0.035)	2,839	0.077	CS
Index of Community Group Participation	0.060***	(0.018)	2,836	0.174	Ancova
Index of Contributions to Public Goods	0.044*	(0.022)	2,839	0.180	Ancova
Index of Psychological Wellbeing	-0.142***	(0.033)	2,820	0.130	Ancova
Less PTSD	-0.664***	(0.196)	2,628	0.135	Ancova
Less Anxiety	-0.395***	(0.118)	2,738	0.149	Ancova
Less Depression	-0.279***	(0.069)	2,839	0.228	Ancova
Index of Economic Outcomes	-0.026	(0.020)	2,839	0.228	Ancova
Assets	0.149***	(0.054)	2,836	0.418	Ancova
Perception of household needs	-0.115	(0.071)	2,835	0.102	Ancova
Perception of economic situatoin	-0.130***	(0.037)	2,831	0.088	Ancova
Index of Economic Activity	0.037	(0.026)	2,839	0.192	Ancova
Index of Social Tensions	0.030	(0.022)	2,832	0.094	Ancova
Index of Conflicts and Crime	0.117	(0.083)	259	0.333	CS
Index of Attitudes toward Women	0.043	(0.026)	2,818	0.044	Ancova

Notes. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. Indeces are constructed using the Kling et al. (2007) methodology. All regressions include section pair fixed effects and the second round indicator. Ancova specifications also include the baseline outcome variable, and the interaction of this variable with both the second round indicator and the second wave indicator. All specifications control for baseline measures of the trust index and the individual indicators comprising the economic outcomes index which showed imbalance (See Table A.7). Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.