RESEARCH ARTICLE

PSYCHOLOGY

Reconciling after civil conflict increases social capital but decreases individual well-being

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Civil wars divide nations along social, economic, and political cleavages, often pitting one neighbor against another. To restore social cohesion, many countries undertake truth and reconciliation efforts. We examined the consequences of one such effort in Sierra Leone, designed and implemented by a Sierra Leonean nongovernmental organization called Fambul Tok. As a part of this effort, community-level forums are set up in which victims detail war atrocities, and perpetrators confess to war crimes. We used random assignment to study its impact across 200 villages, drawing on data from 2383 individuals. We found that reconciliation had both positive and negative consequences. It led to greater forgiveness of perpetrators and strengthened social capital: Social networks were larger, and people contributed more to public goods in treated villages. However, these benefits came at a substantial cost: The reconciliation treatment also worsened psychological health, increasing depression, anxiety, and posttraumatic stress disorder in these same villages. For a subset of villages, we measured outcomes both 9 months and 31 months after the intervention. These results show that the effects, both positive and negative, persisted into the longer time horizon. Our findings suggest that policy-makers need to restructure reconciliation processes in ways that reduce their negative psychological costs while retaining their positive societal benefits.

Most wars today are civil wars (*1*), which divide countries along ethnic, economic, and political cleavages. For example, the Hutus targeted the Tutsis during Rwanda's genocide (in 1994), and illicit diamonds sustained Sierra Leone's civil war (over 1991–2002), pitting one neighbor against another. Because conflicts like this sever social ties among individuals, their prevalence has spurred efforts to promote social cohesion and improve social capital as a part of postconflict recovery (*2*–*7*).

Truth and reconciliation processes are a common approach used around the world to promote this type of rebuilding (*8*). These processes are founded on the idea that airing wartime grievances is the key to restoring social ties. As such, they bring war victims face-to-face with perpetrators through forums in which victims describe war atrocities and perpetrators confess to war crimes without facing prosecution. Proponents of this approach claim that reconciliation processes are highly effective—not just in rebuilding social capital and promoting societal healing but also in providing psychological relief to participants, aiding individual healing (*9*–*15*). Yet, we have little knowledge of whether and how reconciliation processes help communities heal from conflict.

We have some evidence from past work that attitudes toward other groups can improve in the aftermath of nationwide Truth and Reconciliation Commissions (TRCs) (*16*) and with exposure to trauma counseling (*17*). Also, other types of interventions targeted toward individuals have been shown to reduce prejudice (*18*) and improve day-to-day dispute resolution (*19*). But what happens when we induce targeted, person-to-person forgiveness throughout a community? We lack rigorous evidence on how community-wide reconciliation influences either individual or societal healing (*20*, *21*).

Our study seeks to address this gap in the literature. We conducted a randomized control trial of a reconciliation process in Sierra Leone that was designed and implemented by a Sierra Leonean nongovernmental organization (NGO) called Fambul Tok. Fambul Tok's intervention has several features common to truth and reconciliation processes around the world: It initiates forums in which victims describe the violence they experienced and perpetrators seek forgiveness for their crimes. Also, no one receives monetary compensation or is punished for participating. However, Fambul Tok's approach is distinct from nationwide truth and reconciliation because it conducts community-level reconciliation, holding forums at the level of the section, which on average includes 10 villages. We used random assignment to evaluate the impact of its work across 100 sections of Sierra Leone. Our evaluation was independent, and we provided no input into the design of its program.

War and reconciliation in Sierra Leone

More than 50,000 people were killed during Sierra Leone's civil war. Thousands more were raped and had limbs amputated, and 2.6 million people—more than half the population of ~4 million people (*22*)—were displaced as a part of the Revolutionary United Front (RUF) rebel group's campaign of terror against the population.

Much of the violence was neighbor-on-neighbor and took place among members of the same village. Child soldiers were frequently recruited by the RUF. Sometimes, they willingly rose up against local authorities in their village, and at other times, they were forced to commit atrocities against fellow villagers. The other armed actors in the conflict included the Sierra Leonean Army (SLA) and local militias called the Civil Defense Forces (CDF), which emerged in response to widespread civilian abuses and came to be revered for protecting the population against the rebels. Although all armed actors inflicted civilian casualties, the vast majority of the atrocities were committed by the RUF (*23*, *24*).

After the conflict, the Sierra Leonean government set up a national TRC, but it only had the capacity to cover a small fraction of the war atrocities. Also, many rural Sierra Leoneans were unable to access the district capitals where the forums were held. As a result, a large part of the population was left out of the national reconciliation process.

Fambul Tok ("Family Talk" in Krio) was founded to address this gap in 2007, when it began initiating community-level reconciliation forums. As a part of its program, committees composed of community members were trained in trauma healing and mediation and conducted outreach to encourage victims and perpetrators to participate in the truth-telling process. This culminated in a 2-day bonfire ceremony in which victims described their experiences and perpetrators asked for forgiveness. The ceremonies were relatively cheap, costing between $150 and $200 in total, for all participants. They also incorporated traditional rituals to promote community healing. After the ceremony, Fambul Tok set up a symbolic Peace Tree in each village and, in some areas, communal farms to further sustain community healing. It additionally helped establish a Peace Mothers' group to discuss gender-targeted atrocities perpetrated during the war. As such, this intervention could have some impacts other than reconciliation—for example, on economic activity. Where we discuss alternative accounts, we lay out why effects on psychological health and social capital are likely due to reconciliation rather than these other impacts. (In supplementary text S3, we also discuss how local-level reconciliation processes such as the one implemented by Fambul Tok compare with national-level reconciliation processes.)

Healing through reconciliation

Reconciliation processes such as this one could theoretically have both positive and negative psychological consequences. On the one hand,

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they may improve psychological health if sharing war accounts has a cathartic effect (*10*, *11*, *22*) or leads to forgiveness, which has been shown to improve trauma, anxiety, and depression (*25*)—particularly when induced in the context of forgiveness therapies (*26*–*31*).

On the other hand, they may also prove traumatic because they evoke painful war memories without allowing for gradual habituation or desensitization (*32*, *33*). In this regard, reconciliation processes are similar to single-session debriefing (*34*), which seeks to counsel patients by exposing them briefly and intensively to traumatic events but has been shown to have limited therapeutic value (*34*, *35*). In contrast, gradual exposure therapy has been shown to be more effective for mitigating posttraumatic stress disorder (PTSD) (*32*, *36*). Negative psychological effects associated with reconciliation need not be concentrated among those who were directly victimized; for example, other community members may experience vicarious traumatization (*37*–*40*) as they hear about new atrocities committed during the war.

In fact, studies of those who have testified in national TRCs suggest that this participation produces mixed emotional responses (*41*–*43*), may not improve psychological health (*44*), or may even correlate with worse psychological outcomes (*34*, *45*). It is difficult to infer causal effects by comparing those who testified with those who did not because those who chose to testify may have experienced greater violence exposure or had a different psychological makeup. We use a randomized design to mitigate this type of endogeneity concern and better identify the effect of reconciliation on psychological outcomes, including trauma, anxiety, and depression. We also examined whether the effects vary systematically based on the degree to which individuals experienced war violence.

Reconciliation processes may also affect societal healing through their effects on social capital, which is conceptualized as social networks, and norms such as trust and reciprocity that arise from these network ties (*46*). Social capital effects could also arise as a consequence of forgiveness. For example, individuals may stop avoiding places and activities associated with perpetrators and form social ties with them after forgiving them for past actions. They could also arise as a consequence of acknowledgment (*47*): People may be more willing to contribute to communities that have recognized that they were victimized, or that have recognized that they perpetrated crimes without punishing them for these past actions. To determine impacts on social capital, we examined outcomes such as social networks, participation in community groups, and contributions to public goods.

Evaluation design

In 2011, when Fambul Tok was poised to expand into new sections in its five districts of operation (Kailahun, Kono, Bombali, Moyamba, and Koinadugu), we used random assignment to assign some sections to the Fambul Tok treatment group and other sections to serve as a part of the control group. Geographically, sections are units that lie within districts, whereas villages are even smaller units that lie within sections.

In the supplementary materials (figs. S1 to S3), we show that these five districts are similar to other districts in Sierra Leone along key dimensions such as exposure to war violence and other socioeconomic characteristics. These similarities suggest that the findings of the study are also likely to hold for other areas of Sierra Leone, which helps boost the external validity of the study.

The evaluation occurred in waves so as to allow Fambul Tok to work within its capacity. The first wave included 40 sections, and the second wave included 60 sections. Data collection for a third wave was interrupted by the Ebola crisis in Sierra Leone in 2014. Our field staff had to be evacuated while we were collecting behavioral measures. These 100 sections are also similar in key characteristics to other sections within the districts of study (table S1), which further bolsters potential generalizability to other areas of the country.

Within each section, we sampled two villages: One was the section headquarters, where the reconciliation ceremony was typically held, and the second was randomly chosen among what was on average nine remaining villages. Within each village, we interviewed a random sample of 10 to 12 adults, for a total of 2383 respondents across 200 villages. Almost all of our key outcome variables are individual-level responses from household surveys.

In wave one, we conducted endline surveys both 9 months and 31 months after the ceremonies took place, enabling us to determine both short-run and long-run effects. In wave two, endline surveys were conducted once, ~18 to 19 months after the ceremonies. The evaluation timeline, which spanned the 2011–2014 period, is shown in fig. S4.

For all endline rounds, we sought to resurvey the same respondents interviewed at baseline. We went to great lengths to minimize attrition, with repeat visits and by tracking respondents who had moved to neighboring villages.

The attrition rate of those who appeared in baseline but are missing from either endline round in wave one or the endline in wave two is 13% (315 out of 2382 individuals), and the attrition rate for those missing from both endline rounds in wave one or the endline in wave two is 7% (168 of 2382 individuals). As shown in table S2, neither of these attrition measures—nor the attrition measure of each endline round separately—is predicted by treatment (supplementary text S1).

We also used four village-level variables from a village survey. Because of a mechanical error in the hand-held devices used for data collection, this village-level survey is missing for five villages

Table 1. The impact of reconciliation on forgiveness and trust. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. All specifications include section pair fixed effects and the second-round indicator, the baseline outcome variable, and its interaction with both the second-round indicator and the second-wave indicator. SEs 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.

Variables	Control mean	Coefficient	SE	Observations	R^2
Forgiveness					
Forgive perpetrators	2.264	0.571**	(0.227)	2010	0.131
Forgive perpetrators (based on questions in both baselines)	0.951	0.277*	(0.145)	2085	0.121
Trust					
How much do you trust rebel excombatants?	1.901	0.177**	(0.079)	900	0.222
Indicator: Trust rebel excombatants somewhat or completely	0.328	0.073**	(0.036)	900	0.197
How much do you trust migrants to this community?	3.161	0.123***	(0.033)	2203	0.172
Indicator: Trust migrants somewhat or completely	0.861	0.058***	(0.012)	2203	0.094
Index of generalized trust in community	0	0.006	(0.027)	2996	0.135
Indicators					
People are honest and can be trusted	2.598	0.014	(0.026)	2994	0.126
People in village are honest and can be trusted	2.858	−0.010	(0.020)	2976	0.167
People in community would not betray fellow community members	2.550	0.003	(0.028)	2976	0.059
Money left out accidentally will still be there an hour later	0.365	0.010	(0.020)	2956	0.141

in baseline and a separate six villages in endline. As shown in table S3, whether a village is missing in either baseline or endline is also uncorrelated with treatment. The number of villages and individuals in our sample is shown in table S4, disaggregated by wave and round of data collection. (In supplementary text S1, we discuss additional robustness checks to confirm that missing village-level indicators do not affect our results.)

Empirical strategy

We used our baseline survey data to match sections into pairs stratified by district and randomly assigned one section in each pair into treatment and the other into control. The balance on key covariates is reported in table S5, and more details on matching and balance statistics are provided in supplementary text S3. To examine treatment effects, our main specification pools together endline surveys from both waves and rounds of the evaluation. For most outcomes, we have baseline data, which enables us to control for the baseline value of the dependent variable (*48*, *49*). This approach reduces noise and increases power and has been commonly used in recent experimental studies in the social sciences [for example, (*50*)].

We estimate regressions that can be represented as

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

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 (*51*). δ_r is a round effect that equals 1 for the second-round endline. The interaction term, $\delta_r y_{0ivspw}$, allows the baseline to exert different effects over time. λ_w is a wave effect that equals 1 for sections in the second wave. Because each wave includes different sections, wave effects are subsumed by section-pair effects. $\lambda_w y_{0ivspw}$ allows baseline variables to have different effects for the wave-two sections. This control is particularly important because we are only able to include pared-down baseline outcomes collected in the second-wave baseline survey (a point discussed further in the Data section). Last, T_s is assignment to treatment, and β_1 measures the treatment effect.

If we did not have baseline data for an outcome, we estimated cross-sectional specifications of the form

$$y_{rivspw} = \beta_0 + \beta_1 T_s + \rho_p + \delta_r + \varepsilon_{rivspw} \quad (2)$$

We clustered the standard errors (SEs) at the section level, which is the unit of treatment allocation. This accounts for the potential correlation of errors across individuals within a section (and implicitly, within a village, because a section is larger than a village).

There are three sections in which some responses do not match treatment assignment; these sections were assigned to control, and yet six of the respondents in one village and eight respondents in the other two reported attending a bonfire ceremony. However, we used assignment to treatment in estimating all of our specifications. Thus, ceremony participation among control respondents may lead to an understatement of the effect.

Many of our outcomes are mean effect indices that first standardize and then sum various indicators used to measure similar concepts. We used the methodology of (*52*), which imputes missing values before aggregation. The indicators are standardized by subtracting control group means and dividing by control group standard deviations, so that the control group means for the indices are zero by construction. In supplementary text S3, we provide greater detail on this method, and in table S6, we show robustness to an alternate method that does not first impute missing values (*53*).

To avoid fishing for significant effects (*4*, *6*), we registered a Pre-Analysis Plan (PAP) in the Evidence in Governance and Politics (EGAP) depository before analysis of any endline data from either wave one or wave two. The PAP outlines the indicators comprising each index and all the hypotheses to be tested. A copy can be found at http://egap.org/registration/622. All hypotheses specified in our PAP are listed in table S7. We present results for six of the hypotheses in Tables 1 to 6 and 10 others in the supplementary materials. In supplementary text S2, we discuss the PAP in more detail and also the few circumstances under which we deviated from the prespecified grouping, owing to issues aggregating conditional and unconditional outcomes or to changes in how the social network data were collected over rounds.

In addition, we show in tables S8 and S9 that adjusting for multiple comparisons by controlling for rates of false discovery (*54*–*56*) does not affect any of our main results (supplementary text S3).

Data

In terms of our dependent variables, we used the Rye Forgiveness Scale to construct an index of forgiveness toward former perpetrators (*57*). This is a sum of 12 questions [and a subset of the 60 questions in the Enright Forgiveness Inventory (*58*)], answered on a four-point Likert scale, which were administered to those who reported being physically or emotionally hurt during the war. These questions are designed to measure affect as well as cognitive and behavioral responses toward former perpetrators.

The questions in this index are listed in table S10. Whereas all three endline surveys and the first-wave baseline included these 12 questions, the second-wave baseline included a subset of seven questions, which serve as a pared-down baseline control for second-wave observations. However, both indices show high internal consistency: Cronbach's α is 0.865 for the full forgiveness index and 0.824 for the pared-down forgiveness index.

To measure trust, we aggregated four questions on perceived trust and honesty of community members into an index of generalized trust. We also asked separate questions on degree of trust toward former RUF rebel combatants (to whom we refer as "rebel ex-combatants," for brevity) as well as migrants, many of whom are former combatants who left their villages after the war. We also measured trust of former members of the SLA and the CDF (supplementary text S2). These trust questions are based on a 4-point Likert scale (with responses "trust completely," "trust somewhat," "distrust somewhat," and "distrust completely"). In order to aid the interpretation of our results, we also constructed a binary variable indicating whether the respondent trusts the relevant subgroup or not.

To gauge impacts on social networks, we asked respondents to identify people from the 9 to 11 other respondents in that village whom they

Table 2. Reconciliation and social networks. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. All specifications are cross-sectional because we do not have baseline measures of these dependent variables. All regressions also include section pair fixed effects and the second round indicator. SEs 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.

Variables	Control mean	Coefficient	SE	Observations	R^2
Index of network strength	0	0.099***	(0.028)	3008	0.061
Indicators					
Number of people respondent would approach for advice / help	2.894	0.148**	(0.069)	3005	0.056
Number of people respondent would ask to collect money for them	3.144	0.155	(0.142)	3005	0.026
Number of times respondent listed as good friend	2.123	0.232**	(0.091)	3008	0.192
Number of times respondent listed as someone to ask for advice / help	3.245	0.362***	(0.126)	3008	0.199

would consider a good friend and would ask for advice and help. We used this to construct a measure of 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 standardized and summed these four measures into a mean effect index. We were only able to conduct cross-sectional analyses with these questions because they were asked differently in the baseline and endline surveys (supplementary text S2).

We constructed a community group participation index based on whether respondents were members of organizations such as Parent Teacher Associations (PTAs) and religious groups and whether they attended group meetings. We also constructed an index of public goods contributions based on whether individuals contributed money or labor to community groups or to building public facilities (including bridges, schools, wells, and health clinics), gave money to a family in need, or participated in road-brushing (a common form of road maintenance), as well as the number of community projects in their village.

Turning to psychological health, we measured PTSD using 11 questions from the PTSD Symptom Scale that assesses the presence and severity of PTSD symptoms according to the 4th Diagnostic and Statistical Manual of Mental Disorders (DSM-IV). This scale has been validated for research purposes (*59*, *60*) and shown to have good psychometric properties, including high internal consistency and test-retest reliability (*59*). We also drew 7 depression and 10 anxiety questions from the Zung Depression and Zung Anxiety indices (*61*, *62*). The second-wave baseline included a subset of seven and five questions on anxiety and depression, respectively, which again form pared-down baseline-dependent variable controls. The indices for PTSD, anxiety, and depression are sums of questions answered on a four-point Likert scale (all the questions are listed in table S11). We further aggregated these three indices into a mean effect index of psychological health. We inverted the indicators so that a reduction in the index indicates worse psychological health.

The psychometric scales from which we drew our questions have typically been assessed in developed country contexts, which raise questions around whether they are culturally relevant and valid for a developing country such as Sierra Leone. We piloted our survey instruments extensively and adapted the wording of the psychological measures to the Sierra Leonean context so that they better reflect the informality of Krio language. Furthermore, our scales correspond closely to scales used in other recent studies set in postconflict parts of sub-Saharan Africa, where they have demonstrated good psychometric properties. For example, 15 of our 17 questions on anxiety and depression are also a part of the Johns Hopkins 25-Item Checklist for Anxiety and Depression (*63*). An adapted version of this scale shows strong internal consistency among adults who were formerly child soldiers in Sierra Leone (*64*–*66*). Although our PTSD scale has not been applied in Sierra Leone, it uses the same questions as the Child Posttraumatic Stress Disorder Reaction Index (CPTSD-RI) (*67*, *68*), which has been tested on a population of Ugandan and Congolese child soldiers (*69*). Moreover, the psychological wellbeing questions we used also exhibit high internal consistency in our sample, with a Cronbach's α ranging between 0.831 and 0.936 (supplementary text S1).

We also converted the continuous PTSD measure into a dichotomous indicator of whether an individual suffers from clinical PTSD or severe trauma, following guidelines from the Clinician-Administered PTSD Scale (*70*). This is coded as 1 if the individual showed at least one symptom of reexperience, one symptom of avoidance, and at least two symptoms of increased arousal. We did not prespecify that we would look at this indicator in our PAP but do so to better gauge the magnitude of the effects on PTSD.

In terms of our sample, because the 10 to 12 respondents were randomly chosen in each village, some may have been victims during the war and others perpetrators. Our main results examine average impacts on all respondents. However, we also collected data on the ways in which respondents were exposed to violence to determine whether the treatment effect varies based on victimization. In our PAP, we defined a violence-exposed individual as one who was beaten, raped, maimed, abducted, or saw violence during the war. We discuss alternate measures in supplementary text S5. We also define someone as an excombatant based on a self-reported measure and whether they indicated that they were abducted and forced to carry a gun after getting abducted. There is likely to be extensive underreporting in both measures, which means the excombatant variable is likely measured with noise.

Descriptive statistics of key variables are presented in table S12. The surveyed respondents reside in impoverished conditions. More than 70% have no formal education, and less than 8% live in a village with a market. They also experienced extensive war violence: 54% had a family member killed, 33% were beaten, 2% report being maimed, and 3% report that they were raped. These latter numbers are also likely to be underestimates, given the sensitivity of these measures.

Respondents in treatment sections were very familiar with Fambul Tok's reconciliation program

Table 3. Reconciliation and participation in community groups. 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, 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. SEs 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.

Variables	Control mean	Coefficient	SE	Observations	R^2
Index of participation in community groups	0	0.058***	(0.017)	3004	0.160
Index of participation in community groups, without women's membership or meetings	0	0.064***	(0.017)	3004	0.162
Indicators					
PTA membership	0.137	0.034**	(0.016)	2732	0.223
Village development committee membership	0.091	0.013	(0.011)	2737	0.141
Youth group membership	0.101	0.015*	(0.008)	2738	0.144
Women's group membership	0.118	0.022	(0.014)	2004	0.138
Secret society membership	0.358	−0.058***	(0.019)	2770	0.338
Religious group membership	0.286	0.055***	(0.020)	2729	0.179
PTA meeting attendance	0.082	0.037**	(0.015)	2739	0.138
Village development committee meeting attendance	0.068	0.008	(0.010)	2734	0.106
Youth group meeting attendance	0.066	0.007	(0.008)	2739	0.090
Women's group meeting attendance	0.075	0.024*	(0.013)	2004	0.095
Secret society meeting attendance	0.056	−0.005	(0.008)	2766	0.057
Religious group meeting attendance	0.190	0.058***	(0.016)	2714	0.103
Community meeting attendance	0.626	0.006	(0.013)	2983	0.077

(table S13), indicating that the intervention was well implemented.

Results

Our findings on reconciliation and the forgiveness of former perpetrators are presented in Table 1, top. The forgiveness index (of 12 questions) is 0.571 higher in treatment areas (SE = 0.227, P = 0.013), over the control group mean of 2.264. The pared down forgiveness index (of seven questions) is 0.277 higher (SE = 0.145, P = 0.059) than that of its control group mean of 0.951.

Because the forgiveness indices are summed on a Likert scale, the coefficients cannot be interpreted in percent terms by comparing them with control group means. Under these scales, changing the value assigned to responses will not alter the regression coefficients but will alter the control group mean, yielding a different implied percent effect. Moreover, no one valuation is necessarily more appropriate than another because units have no inherent meaning in Likert scales (supplementary text S1) (*71*).

To gauge whether the effects on forgiveness are large, we instead benchmark the treatment effect against how exposure to specific forms of war violence affected feelings toward perpetrators, as reflected in the forgiveness index at baseline. For example, having a family member killed lowered baseline forgiveness by 0.920 (SE = 0.232, P < 0.001) (table S14). Thus, the reconciliation program can be said to offset this effect and increase forgiveness by 30% (0.277/.920 = 0.301). This approach is speculative because we cannot observe the causal effect of violence exposure on forgiveness, and so we are benchmarking our treatment effect against a correlation. As such, the interpretation of magnitudes in this manner should be taken as suggestive.

These forgiveness effects are based on survey responses, which raise potential concerns that respondents may say what they believe surveyors want to hear. But there are four factors that mitigate the concern that the results are driven by social desirability bias. First, our surveyors are completely independent of the implementing NGO, so they would not be associated with messages of reconciliation. Second, we asked these questions 9 to 31 months after the reconciliation ceremonies take place, so talk of forgiveness is not fresh on respondents' minds. Third, respondents are not simply asked whether they have forgiven the perpetrator, but rather asked a series of questions designed to gauge their feeling and behavior toward excombatants (such as avoidance), which are arguably less subject to this type of bias. Last, our respondents experienced traumatic forms of victimization, such as amputations and the killing of family members, so it is not psychologically costless for them to say that they no longer feel anger toward their perpetrator, unless this reflects an underlying change in their perspective. However, to further bolster this interpretation, we also discuss whether these forgiveness effects go hand-in-hand with changes in the community orientation of individuals' behavior.

Next, we examine impacts on trust. As shown in Table 1, bottom, the reconciliation treatment increases trust toward both rebel excombatants and migrants. Looking at the binary indicator, respondents are on average 7.3 percentage points (SE = 0.036, P = 0.046), or 22.2%, more likely to trust a rebel excombatant and 5.8 percentage points (SE = 0.012, P < 0.001), or 6.7%, more likely to trust a migrant. Higher trust of migrants suggests greater inclusion of this marginalized group, whose members are sometimes difficult to distinguish from excombatants. In contrast, there is no discernible impact on trust toward former members of the SLA or CDF (table S8). This indicates that the reconciliation process led to changes in trust toward those who perpetrated atrocities during the war, namely former members of the RUF. Although all of these trust questions are administered to subsets of individuals who know members of each of these groups, our specifications restrict the sample to those who knew group members at both baseline and endline because they include controls for the baseline-dependent variable. In table S15, we further verify that these results are not driven by compositional changes in who knows members of these groups.

Because reconciliation is aimed at forgiving former war perpetrators, it is reassuring to see that the process did increase trust toward former rebel combatants. Yet, at the same time, there is no significant impact on the index of trust toward community members generally (Table 1, bottom). Moreover, the reconciliation process also did not alter individuals' beliefs that former combatants and other community members would fight again in the future (table S16). Both null effects raise questions as to whether the treatment altered individuals' interactions with other community members.

To further investigate this question, we examine impacts on social networks (Table 2). The coefficient on the mean effect index implies that the index of network strength is 0.099 standard deviation (SD) units larger in treatment sections than control sections (SE = 0.028, P = 0.001). Because the index is an aggregation of various indicators, effect sizes have a more intuitive meaning if we look at the individual indicators constituting the index.

For example, the number of individuals whom respondents would ask for advice or help increases by 0.148 above the control group mean of 2.894 (SE = 0.069, P = 0.033), implying a 5% increase. The tendency to be listed as a good friend and as someone to ask for advice or help both also increase by 11% (SE = 0.091, P = 0.013 and SE = 0.126, P = 0.005, respectively). As we discuss in supplementary text S5, we see no significant differential impact of ceremony attendance on the

Table 4. Reconciliation and contributions to public goods. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. All specifications include section pair fixed effects and the second-round indicator. All specifications also include the baseline outcome variable and its interaction with both the second-round indicator and the second-wave indicator, except for "Contributed money to someone in need." Because we do not have the second-wave baseline-dependent variable for this outcome, we instead control for the other baseline measures of public goods contributions and their interaction with the second-round indicator and the second-wave indicator in the regression of this outcome. *** 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.

Variables	Control mean	Coefficient	SE	Observations	R^2
Index of public goods contributions	0	0.042*	(0.022)	3008	0.171
Index of public goods contributions (without contributions to women's group)	0	0.046**	(0.023)	3008	0.184
Index of public goods contributions (indicators in both baselines)	0	0.046**	(0.022)	3008	0.171
Indicators					
Contributed to public facilities	0.397	0.029	(0.019)	2911	0.078
Brushed roads	0.290	0.005	(0.014)	2898	0.171
Number of community projects (village-level variable)	0.539	−0.060	(0.057)	2841	0.356
Contributed to PTA	0.066	0.023*	(0.013)	2732	0.105
Contributed to village development committee	0.062	0.002	(0.009)	2737	0.119
Contributed to youth group	0.069	−0.002	(0.006)	2738	0.081
Contributed to women's group	0.064	0.021**	(0.010)	2004	0.076
Contributed money to someone in need	0.178	0.010	(0.019)	2039	0.100

social networks index, which suggests that this effect does not arise as a mere consequence of social interactions generated at the ceremony.

We next examine whether the reconciliation process altered the community orientation of individuals' behavior. Estimates on participation in community groups are presented in Table 3. The mean effect index is significantly higher in treatment sections by 0.058 SD units (SE = 0.017, P = 0.001). This overall effect reflects two different types of impacts among individual indicators. Membership and meeting attendance for almost all of the individual community groups increase, with effect sizes ranging from 11% above the control group mean of 0.10 for youth group membership (SE = 0.008, P = 0.066) to 45% above the control group mean of 0.08 for PTA meeting attendance (SE = 0.015, P = 0.017). In contrast, membership and meeting attendance decrease for secret societies by 16% (SE = 0.019, P = 0.004) and 9% (SE = 0.008, P = 0.516), respectively. These effects are interesting because these groups have a closed membership dominated by the elite (72). Thus, these decreases are consistent with substitution toward more broad-based community organizations.

Because the Peace Mothers' Groups are initiated as a part of the intervention, we verify that women's groups do not mechanically drive this result: Dropping women's group membership and attendance from the index does not meaningfully affect the estimate.

The effects on contributions to public goods are gauged in Table 4. The mean effect index is 0.042 SD units larger in treatment villages (SE = 0.022, P = 0.055). Dropping the women's group indicators and the indicator for giving to someone in need (for which we do not have second-wave baseline data) does not substantively alter the estimate. Also, dropping the indicator of the number of community projects in a village from the index does not meaningfully affect the estimate (table S17), suggesting that imputation of missing village-level data does not drive this result.

The coefficient on public goods contributions is the smallest of our significant effects, among the mean effect indices. However, looking within the index again shows that effects on underlying indicators vary in magnitude. The effects are most precisely estimated and largest for contributions to PTAs and women's groups, where implied increases are 32% (SE = 0.013, P = 0.097) and 20% (SE = 0.01, P = 0.045), albeit from relatively small control group means of 6.6 and 6.4%, respectively. The implied effect for contributing to public facilities is 7% (SE = 0.019, P = 0.126), but from a relatively large control group mean of 40%.

These effects on networks, participation, and contribution also support our interpretation that the forgiveness effects are not driven through socially desirable responses regarding anger toward perpetrators because they are coupled with changes in the community orientation of individuals' actions. They also indicate that the reconciliation process boosted social capital as individuals formed more friendships and contributed more to their communities, although these changes were not accompanied by increases in general trust, which increased specifically for migrants and former rebel combatants.

Next, we turn from societal healing to individual healing. The effects on psychological well-being are presented in Table 5. The first row presents the index of complete indicators (with pared baseline controls for wave two). The second row presents the index with just the subset of indicators appearing in the wave two baseline. Both versions show that psychological health was significantly lower in the treatment villages, by 0.147 and 0.138 SD units, respectively (SE = 0.033, P < 0.001 and SE = 0.031, P < 0.001). This overall negative impact stems from a worsening of all three psychological measures.

The dichotomous indicator of clinical PTSD indicates that severe trauma was 36% higher in treatment sections, above the control group mean of 8% (SE = 0.011, P = 0.006). The control group means of the continuous psychometric indicators are again not useful for gauging magnitudes in percent terms because they are also aggregations on a Likert scale. If we instead take the alternate approach of comparing the treatment effect against baseline effects of being maimed (table S14), the treatment is predicted to worsen PTSD by 28%, depression by 47%, and anxiety by 37%. Thus, both the percent effects with the dichotomous PTSD indicator and the more speculative approach of benchmarking against violence exposure are consistent with one another and imply substantial effects.

We found that all of these effects, both positive and negative, are also robust to alternate specifications, as discussed in supplementary texts S3 and S6 and presented in tables S18 and S19.

The negative impacts on psychological well-being suggest that confronting the past through reconciliation processes may be deeply distressing. But are these effects concentrated among victims, specifically? This is important for gauging distributional consequences of the program.

To examine whether the psychological impacts are larger for those who were victimized during

Table 5. Reconciliation and psychological well-being. The top portion of the table examines the average treatment effect. Each row represents a separate regression of the outcome shown in the first column on treatment assignment. The control mean is the mean in the control group at endline. The bottom portion examines how the treatment effect varies according to individuals' exposure to violence. All specifications include section pair fixed effects and the second-round indicator, the baseline outcome variable, and its interaction with both the second-round indicator and the second-wave indicator. SEs 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.

Variables	Control mean	Coefficient	SE	Observations	R^2	
Average effect						
Index of psychological well-being (all indicators)	0	−0.147***	(0.033)	2982	0.115	
Index of psychological well-being (indicators in both baselines)	0	−0.138***	(0.031)	2982	0.115	
Indicators (in both baselines)						
Less PTSD	28.819	−0.683***	(0.197)	2776	0.119	
Less anxiety	14.945	−0.441***	(0.117)	2895	0.142	
Less depression	11.677	−0.289***	(0.069)	2913	0.092	
Clinical PTSD symptoms present	0.080	0.029***	0.011	2776	0.057	
Effect by violence exposure (saw violence, was raped, maimed, beaten, or abducted)						
	T		***T* × violence-exposed**			
	Coefficient	**SE**	**Coefficient**	**SE**	**Observations**	**R^2**
Index of psychological well-being	−0.160***	(0.052)	0.011	(0.064)	2852	0.121
Index of psychological well-being (indicators in both baselines)	−0.147***	(0.052)	0.005	(0.064)	2852	0.121
Less PTSD	−0.871***	(0.309)	0.298	(0.391)	2662	0.123
Less anxiety	−0.476**	(0.213)	0.003	(0.268)	2778	0.144
Less depression	−0.270**	(0.127)	−0.044	(0.162)	2788	0.094
Clinical PTSD symptoms present	0.038**	(0.018)	−0.010	(0.022)	2662	0.058

the war, we interact treatment with our prespecified measure of violence exposure (Table 5). We have limited power to identify these heterogeneous treatment effects, so we considered the magnitudes of the coefficients instead of focusing solely on statistical significance. However, the coefficients on the interaction terms are not just imprecisely estimated but also differ in sign across indicators. (The coefficient is negative for depression, indicating worse effects for victims, but positive for PTSD and anxiety.)

These results are consistent with the idea that even nonvictims may experience a worsening of psychological health from going through a reconciliation process. For example, other community members may experience vicarious traumatization from hearing about atrocities done to others (*36–39*).

Another way of gauging the distributional consequence of the reconciliation treatment is to see whether the impact on social capital is smaller (or larger) for victims. These interaction effects are examined in table S20 with two measures of violence exposure. The coefficients on the interaction term of treatment and victimization are mixed in sign, small in size, and imprecisely estimated for outcomes such as social networks, public goods contributions, and community group participation. Thus, victims do not appear to partake systematically less in social capital improvements.

We examine in table S21 whether effects vary for excombatants. Here, some of the interaction terms are quite large in magnitude; for example, the effect on the psychological well-being index implies that the negative effect on those who are not excombatants is nearly offset for excombatants. These effects are imprecisely estimated in part because the excombatant variable is likely to be underreported and measured with noise. Thus, it is difficult to draw definitive conclusions on the basis of these heterogeneous effects, and future work should probe this further.

A key issue is whether these effects persist over time. We present in Table 6 short-run and long-run effects using the two rounds of wave-one data. Because wave one includes fewer than half the sections in the evaluation, this is a relatively underpowered sample, and some of the effects are individually insignificant. Yet, the broad pattern implied by the coefficients indicates that both the positive and negative effects are sustained.

First, the impacts on all three psychological measures persist up to 31 months. This suggests that the war memories invoked by the reconciliation process are powerful and do not fade quickly.

At the same time, the effects on forgiveness and social capital outcomes also persist. Although the effect on trust of former rebel combatants is individually insignificant in both rounds, the coefficients are not significantly distinguishable from each other at the 5% level, indicating that they do not recede over time. Trust of migrants also persists, and there are even short-run improvements in generalized trust measures, although these effects fall, and significantly so, over the longer horizon. The coefficients on public goods contributions and social networks, if anything, increase in magnitude, suggesting that the effects do not recede. The effect on community group participation is also individually significant in both rounds. As such, reconciliation appears to boost the community orientation of individual behavior in a manner that does not subsequently fade away.

We interpret the results above as indicating that the reconciliation process itself affects both individual and societal healing. We also consider and present evidence against two alternative accounts, drawing on data for additional outcomes.

The first alternative account posits that the reconciliation ceremony may be relatively unimportant, whereas other components of the intervention—such as the Peace Mother's Group, Communal Farms, or Peace Tree—actually drive the estimated effects. We think that this is unlikely because treatment effects on forgiveness, social capital, and psychological health are not statistically distinguishable for men and women (table S22), nor if we include a control for communal farms (table S23). The effect on economic outcomes is even negative in sign (table S24), further suggesting limited impacts of communal farms. The coefficient capturing effects on the resolution of day-to-day disputes, which was the focus of the Peace Tree, is also negative and imprecise (table S25). Moreover, it is difficult to see how the negative effects on psychological well-being could emerge as a response to these other components. Together, these results suggest that the reconciliation component of the intervention is an important driver of the estimated effects.

A second alternative account posits that the reconciliation component may be driving the psychological effects, but the social capital outcomes arise from simply getting community members together in a gathering. We think that this is unlikely because it has proven incredibly difficult to move social capital outcomes in Sierra Leone. For example, a large-scale Community-Driven Restoration (CDR) program was implemented in Sierra Leone in 2008 in one of the same districts as in our study. This program spent $100 per household and fostered ongoing gatherings of the community in village-wide meetings in order to promote inclusive governance and collective action. A randomized evaluation found it successfully delivered economic benefits but had no effects on social capital outcomes such as community group participation, as measured with indicators similar to those used in our study (*4*). Given that social capital outcomes did not move in response to a well-implemented and well-resourced intervention, it is hard to see how a 2-day gathering initiated by Fambul Tok could deliver persistent effects on similar outcomes for up to nearly 3 years after the intervention, unless it entailed a deeper transformation of person-to-person interactions.

Second, if simply getting people together improved person-to-person interactions, then we should also have observed reductions in societal tensions and the incidence of other day-to-day disputes. But again, little support for this idea is provided in table S25. For example, the coefficient on the number of conflicts is only 0.002 (relative to a mean of 0.16) (SE = 0.019, P = 0.894). Rather, we observe improvements in

Table 6. Persistence of effects. 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. The final column indicates whether the specification also includes the baseline outcome variable, and its interaction with both the second-round indicator and the second-wave indicator. SEs 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.

	Round 1			Round 2			
Variables	**Coefficient**	**SE**	**Observations**	**Coefficient**	**SE**	**Observations**	**Baseline-dependent variable controls?**
Forgive perpetrators	0.986***	(0.272)	550	1.231***	(0.361)	521	Y
Trust rebel excombatants	0.100	(0.073)	241	0.048	(0.198)	203	Y
Trust migrants	0.140**	(0.053)	653	0.119*	(0.069)	564	Y
Index of generalized trust in community	0.119**	(0.050)	878	−0.009	(0.038)	845	Y
Index of network strength	0.015	(0.027)	885	0.119	(0.085)	850	N
Index of community group participation	0.038*	(0.022)	884	0.084**	(0.040)	847	Y
Index of contributions to public goods	0.024	(0.033)	885	0.035	(0.046)	850	Y
Index of psychological well-being	−0.166***	(0.052)	873	−0.170***	(0.058)	837	Y

outcomes that are specific to the war, such as forgiveness of war perpetrators and trust of former rebel combatants. This reiterates the idea that talking about the war is important in giving rise to the observed effects. In the supplementary materials, we examine these additional outcomes further by gauging their long-run impacts (table S19) and discussing them in greater detail.

Conclusion

Our findings highlight the long shadow of war along two dimensions. The reconciliation forums we analyzed were held nearly a decade after the end of Sierra Leone's civil war. Yet, the positive effects on forgiveness and social capital suggest that the need for reconciliation persists long after the violence ends. At the same time, the negative psychological impacts indicate that truth-telling opened up psychological wounds, pointing to the potency of these war memories when they are evoked suddenly (*32–34*).

These psychological effects do not preclude the possibility that individuals who forgave in response to reconciliation gained a psychological benefit—but they do suggest that these gains were offset by other negative impacts, such as the difficulty of coping with negative memories. In that regard, they corroborate the idea that forgiving is not the same as forgetting (*73*). They also suggest that forgiveness stemming from an intense, one-time event that evokes negative memories may differ in its psychological impact relative to forgiveness stemming from ongoing therapy (*74*).

Overall, our results indicate that the gains in societal healing associated with reconciliation came at a substantial cost in individual psychological healing. As such, they imply that policymakers need to find ways of holding reconciliation processes that reduce these psychological costs, while retaining the societal benefits. For example, it is possible that the negative psychological impacts may be smaller or even reversed if reconciliation efforts are held in the direct aftermath of the war, when trauma symptoms are high and people have yet to move on in their own way (*75*). A second possibility lies in combining reconciliation with other types of complementary interventions. For example, coupling these programs with sustained counseling—as used by forgiveness therapies (*26–31*), exposure therapy (*32*, *36*), or trauma healing interventions (*17*)—may help mitigate the detrimental impacts. Given the global prevalence of conflict and postconflict reconciliation, future research should explore alternate designs for efforts aimed at unifying societies in the aftermath of war.

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SUPPLEMENTARY MATERIALS

www.sciencemag.org/content/352/6287/787/suppl/DC1
Supplementary Texts S1 to S6
Figs. S1 to S4
Tables S1 to S26
References (*76–98*)

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Reconciling after civil conflict increases social capital but decreases individual well-being
Jacobus Cilliers, Oeindrila Dube and Bilal Siddiqi (May 12, 2016)
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Editor's Summary

The psychological cost of reconciliation

During civil wars, individuals and communities who were previously good neighbors can end up fighting each other. One approach to reknit these sundered social ties is to bring perpetrators and victims together in truth and reconciliation forums. Cilliers *et al.* found that these forums have helped to reestablish social bonds in Sierra Leone, but that they have also imposed a cost on the victims' mental health (see the Perspective by Casey and Glennerster).

Science, this issue p. 787; see also p. 766