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Published in:
PLoS One

DOI:
10.1371/journal.pone.0021819

Citation for published version (APA):
The Effect on Mental Health of a Large Scale Psychosocial Intervention for Survivors of Mass Violence: A Quasi-Experimental Study in Rwanda

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Abstract

Background: War has serious and prolonged mental health consequences. It is argued that post-emergency mental health interventions should not only focus on psychological factors but also address the social environment. No controlled trials of such interventions exist. We studied the effect on mental health of a large scale psychosocial intervention primarily aimed at social bonding in post-genocide Rwanda. The programme is implemented at population level without diagnostic criteria for participation. It is open to any person older than 15 years, and enables participation of over 1500 individuals per year. We postulated that the mental health of programme participants would improve significantly relative to non-participants.

Methods and Findings: We used a prospective quasi-experimental study design with measurement points pre and post intervention and at 8 months follow-up. 100 adults from both sexes in the experimental condition entered the study; follow-up measurements were taken from 81. We selected a control group of 100 respondents with similar age, sex and symptom score distribution from a random community sample in the same region; of these, 73 completed the study. Mental health was assessed by use of the Self Reporting Questionnaire (SRQ-20), a twenty item instrument to detect common mental disorders in primary health care settings. Mean SRQ-20 scores decreased by 2.3 points in the experimental group and 0.8 in the control group (p = 0.033). Women in the experimental group scoring above cut-off at baseline improved with 4.8 points to below cut-off (p < 0.001). Men scoring above cut-off at baseline showed a similar trend which was statistically non-significant. No adverse events were observed.

Conclusions: A large scale psychosocial intervention primarily aimed at social bonding caused a lasting improvement of mental health in survivors of mass violence in Rwanda. This approach may have a similar positive effect in other post-conflict settings.

Trial Registration: Nederlands Trial Register 1120


Editor: James G. Scott, The University of Queensland, Australia

Received January 15, 2011; Accepted June 11, 2011; Published August 8, 2011

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Funding: This study was partly supported by a grant to FV from the Health Research Development Counsel, Department Prevention Programme (ZonMW), OOG-Geestkracht (ZonMW: 60-60105-98-117), partly by Cordaid and partly by Prins Bernhard Cultuurfonds through a grant to FV. The funders had no role in study design, data collection and analysis, decision to publish, or preparation of the manuscript.

Competing Interests: The authors have declared that no competing interests exist.

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These authors contributed equally to this work.

Introduction

Violent conflict has serious and prolonged mental health consequences [1–6]. Post-emergency mental health interventions are mostly aimed at persons at risk of psychiatric disorder, particularly post-traumatic stress disorder (PTSD) [7]. However, opinions differ regarding the value of such psychological trauma-focused care [8]. As organized violence affects individuals as well as communities and social institutions, it is argued that mental health interventions should not only focus on internal psychological factors but also address aspects of the social environment which could promote healing and adaptation [9]. Unlike trauma-focused approaches, psychosocial interventions focus primarily on stressful environmental conditions such as the division within communities, the destruction of social networks and the resulting loss of social and material support. Altering these conditions may foster people’s inherent capacity to recover [10], cause improvement in nonspecific symptoms among persons with and without specific disorders and, in some cases, be enough to reduce symptoms below the threshold of clinical disease [11]. Psychosocial interventions are preferably implemented at population level and directed at groups rather than
individuals. Group interventions have shown to have a positive impact on health outcomes in other areas of public health [12,13]. In war-affected societies a particular objective may be the restoration of social connectedness and mutual support. To date, the literature on humanitarian responses to disaster does not reflect any substantive discussion of comprehensive psychosocial interventions [14], and no controlled trials of such interventions exist. We carried out a controlled study to assess the effect on mental health of a psychosocial intervention programme which makes use of a therapeutic group approach called sociotherapy. It primarily aims at social bonding and secondarily at mental health improvement. We postulated that the mental health of programme participants as assessed with use of the Self Reporting Questionnaire (SRQ-20) would improve significantly relative to non-participants.

The intervention has been taking place since early 2006 in Gicumbi district (the former Byumba province) in the north of Rwanda, and is presently still running. The population of Rwanda experienced extreme violence during the genocide of 1994, when within a three month period about 800,000 people were killed; roughly two million refugees left the country, and around one million people were internally displaced. Only a few studies examined the mental health status of Rwanda’s post-genocide population, but all show high rates of mental health disorders, particularly depression and PTSD [15–21].

The sociotherapy programme is community-based, that is, it is carried out by trained Rwandan community leaders, and is implemented at population level. It enables over 1500 beneficiaries per year to participate. No diagnostic criteria for participation have been defined, as the programme aims to be accessible to all community members.

This study took place from October 2007 to September 2008, preceded by a pilot study over 2005-2006. Measurements were taken pre and post intervention and at 8 months follow-up.

Methods

The protocol for this trial and supporting CONSORT checklist are available as supporting information; see Checklist S1 and Protocol S1.

Ethics statement

Approval for this study was gained from the Medical Ethics Committee of the Academic Medical Center in Amsterdam, Netherlands. This included approval for the consent procedure used (see under paragraph ‘Interviews’).

Intervention

Sociotherapy has its roots in England during the second world war, when society had to cope with many psychiatric casualties [22]. The technique therapeutically uses interaction between individuals and their social environment to help subjects to re-assess and re-define values, norms, relations and possible collaborations. The principal premise is that reaching a certain level of mutual respect, trust and care in group interaction helps to increase the problem solving capacity and subjective mental health in individual group participants. In sociotherapy with survivors of systematic violence, safety and the setting of democratic rules are additional primary objectives. The intervention does not specifically aim at sharing or processing traumatic memories. Trauma symptoms are addressed through psycho-education and advice. Key elements of the working methods are debates and the exchange of experiences and coping strategies among participants, exercises, games and mutual practical support.

We (back-)translated the SRQ-20 to the local language, Kinyarwanda, and validated it for the actual context. The capacity of the SRQ-20 to identify probable psychopathology proved to be sufficient for men (AUC = 0.74) and women (AUC = 0.76).
Reliability was considered to be good (Cronbach’s α = 0.83). The optimal cut-off point was 7/8 for men and 9/10 for women (manuscript under review). We also validated the SRQ-20 for its capacity to assess change in symptom severity over time. The instruments factor structure proved to be time invariant; the number of factors, factor loadings and covariances of factors remained equal over time.*

Participants

A pre to post intervention test performed as a pilot study among sociotherapy participants (n = 77) showed a decrease of 2.7 (sd 4.2) of SRQ-20 mean scores (effect size 0.6). To establish a 2.7 effect with a standard .80 power, a minimum of 30 respondents in both the experimental and control group would be needed. To be on the safe side we aimed at larger numbers (n = 100) per study group. We did not aim at even higher numbers because of limited time, the large distances between the areas where respondents resided, and the low drop out rate during our pilot study.

Experimental group. Out of 45 sociotherapy groups starting simultaneously, the sociotherapy programme staff selected 10 groups through convenience sampling, balancing the gender ratio. These appeared to be large groups, and an unexpected high number of 133 participants showed up at the interview sites. At T0 we interviewed all 133, but at T1 and T2 we had to restrict ourselves due to limited time and human resources. Therefore, we invited a random selection of 100 out of the 133 to form our experimental group.

Control group. We applied the following procedure to compose a control group that was equivalent at baseline with regard to our main outcome measure, the SRQ-20 score. During our pilot study, 2.5 times more respondents in the experimental group (n = 97) had baseline scores above cut-off than in the control group (n = 229). For the actual study we therefore aimed to interview 2.5 times (n = 250) more respondents than in the experimental group, to later select 100 out of these to compose a control group. We identified five regions within Gicumbi district where the programme was not or had not been running so far, or for practical reasons would not start over the upcoming year. It could be assumed the inhabitants of these regions had experienced similar trauma exposure. Here, we randomly selected respondents through convenience sampling. Interviewers started at the top of a hill or in the centre of a village and each walked down a different footpath towards scattered houses or huts. An equal number of men and women, at home or in the fields, were randomly chosen and asked to participate. Finally 251 respondents were interviewed. After analysis of the data collected, we selected a group of 100 out of these for which the distribution of SRQ-20 scores matched that of the intervention group. For this purpose we used 8 clusters of scores (0–1, 2–3, 4–5, 6–7, 8–9, 10–12, 13–15, 16–20) and from each cluster randomly selected a number of respondents equal to the corresponding cluster in the experimental group. This final selection of 100 constituted our definite control group.

Interviews

Eight local interviewers were recruited; all were sociology students at the ‘Institut Polytechnique de Byumba’ in Gicumbi. Their one-week training addressed the principles of a longitudinal study design, interviewing techniques and our measuring instrument. They were involved in making the wording of the questions acceptable and understandable for people in Gicumbi [28]. Informed consent was obtained by use of an explanatory text, which because of the high illiteracy rate was read aloud. In case of refusal, demographic data and reasons for refusal would be requested and documented, but no-one refused.

For determination of the socio-economic status (SES) our interviewers approached respondents of the control group at, or near, their homes, and scored the SES by judging the state of the houses. Participants of the experimental group, however, were interviewed at the spot of their meetings, and were asked to describe the state of their houses themselves.

Statistical analysis

The repeated measures of our primary outcome, the total SRQ-20 score, were analyzed with a linear mixed-effects model [SPSS 16.02] using intervention (participants versus controls), time (T0, T1, T2) and their interactions as fixed-factors. Sociotherapy groups and control group areas, and respondents within the sociotherapy groups and the control group areas were random factors. An intention to treat analysis was employed, in which all available measurements of all respondents were analyzed according to the mixed-effects model. Missing data were considered to be missing at random in the repeated measures model. No assumptions on the covariances between the repeated measures were made (covariance type: unstructured). The primary hypothesis on the effectiveness of the intervention was tested with the p-value of the interaction test between time and intervention; a p-value of 0.05 or less was interpreted as statistically significant. We analysed for the sample as a whole, and stratified for sex and separate sociotherapy groups. Total SRQ-20 scores are presented with the mean. Significant interaction effects are presented with

| Table 1. Socio-demographic characteristics of experimental and control group at baseline. |
|---------------------------------|---------------------------------|----------------|----------------|
|                                | Experimental group (n = 100)     | Control group (n = 100) |
| **Sex**                        |                                 |                        |
| Male                           | 45 (45%)                        | 47 (47%)               |
| Female                         | 55 (55%)                        | 53 (53%)               |
| **P-value (Chi2)**             | 0.78                            |                        |
| **Mean Age**                   |                                 |                        |
| years                          | 34.9                            | 38.5                   |
| min-max                        | 16–76                           | 16–73                  |
| **P-value (T-test)**           | 0.10                            |                        |
| **Education**                  |                                 |                        |
| nil                            | 48 (48%)                        | 54 (54%)               |
| primary                       | 42 (42%)                        | 34 (34%)               |
| secondary 1–3                  | 9 (9%)                          | 9 (9%)                 |
| secondary 4–7                  | 1 (1%)                          | 3 (3%)                 |
| **P-value (Chi2)**             | 0.53                            |                        |
| **SES**                        |                                 |                        |
| marginal                      | 6 (6%)                          | 13 (13%)               |
| poor                          | 83 (83%)                        | 66 (66%)               |
| sufficient                    | 11 (11%)                        | 21 (21%)               |
| **P-value (Chi2)**             | 0.022                           |                        |
| **SRQ-20 score**               |                                 |                        |
| mean                          | 8.41                            | 8.26                   |
| standard deviation             | 5.05                            | 4.83                   |
| **P-value (T-test)**           | 0.83                            |                        |

P-value (T-test) 0.83

P-value (Chi2) 0.022

P-value (Chi2) 0.53

P-value (T-test) 0.10

P-value (T-test) 0.78

P-value (T-test) 0.022

P-value (T-test) 0.83

P-value (Chi2) 0.022

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P-value (T-test) 0.83

P-value (T-test) 0.022
estimates of the differences in mean SRQ-20 scores relative to baseline with 95% confidence intervals. We also present the reliable change index according to Jacobson and Truax [29].

In addition, we extended the same repeated measures model with the baseline SRQ-20 score as a variant, and then analyzed SRQ-20 score changes. Since the results were comparable, we do not present them here.

Table 2. Mean SRQ-20 scores, standard deviations, effect sizes (T0–T2) and P-values for experimental and control group.

<table>
<thead>
<tr>
<th></th>
<th>Total (n = 200)</th>
<th>Men (n = 92: exp 45, contr 47)</th>
<th>Women (n = 108: exp 55, contr 53)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>T0 T1 T2 Cohen’s d p</td>
<td>T0 T1 T2 Cohen’s d p</td>
<td>T0 T1 T2 Cohen’s d p</td>
</tr>
<tr>
<td>Mean (sd)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Experimental</td>
<td>8.4 (5.0) 7.2 (4.6) 6.1 (3.9) 0.51</td>
<td>6.6 (4.6) 5.8 (4.8) 5.3 (4.0) 0.30</td>
<td>9.9 (4.9) 8.3 (4.3) 6.8 (3.8) 0.70</td>
</tr>
<tr>
<td>group</td>
<td>Experimental</td>
<td>Control group</td>
<td></td>
</tr>
<tr>
<td>Control group</td>
<td>8.3 (4.8) 8.1 (5.7) 7.5 (4.8) 0.17</td>
<td>6.5 (4.0) 5.8 (5.0) 5.6 (4.0) 0.22</td>
<td>9.8 (5.0) 10.0 (5.8) 9.2 (5.2) 0.18</td>
</tr>
<tr>
<td></td>
<td>0.033</td>
<td>0.852</td>
<td>0.011</td>
</tr>
</tbody>
</table>

Figure 1. Flow chart of the composition of the study population at three measurements.

doi:10.1371/journal.pone.0021819.g001
not report these. To quantify the variability of the score change between the different sociotherapy groups we calculated the within and between sociotherapy group variances of the score change between T0 and T2 per participant as well as the average score change per sociotherapy group with its 95% confidence interval; here, we calculated the expectation of the posterior distribution of random effects with an empirical Bayesian analysis.

**Results**

**Baseline characteristics**
Baseline measurements took place in September 2007. The two study groups matched on SRQ-20 score distribution, sex and age at baseline (see Table 1). At T1, in January 2008, 90 subjects from the experimental group and 81 from the control group were interviewed, and at T2 (eight months later) 81 and 73, respectively. Of these, only 76 and 66 had been interviewed at both T0 and T1 (see Figure 1). The study groups showed no significant difference in level of education. They differed slightly in SES, with an overrepresentation of both lowest and highest SES groups in the control group.

**Drop out**
Drop out was unexpectedly higher than during the pilot study (see Figure 1). Drop out from the experimental group was mainly caused by illness, leaving the programme for unknown reasons and communication problems about day and time of interviewing. One particular sociotherapy group contained scholars; at T2 they had finished school and had moved to different areas. Drop out from the control group was also caused by illness and communication problems, but mostly by moving house. Drop out did not differ significantly between the experimental and the control group ($p = 0.79$). Drop-outs at T1 or T2 from either study group did not differ significantly in sex, age or level of education. Neither was there a difference in sex, age, level of education and SRQ-20 scores between actual respondents and drop-outs at T1 and T2.

**Changes in SRQ-20 scores**
Linear mixed-effects model analysis yielded a significant difference between the two study groups in decrease in mean SRQ-20 scores at follow-up (see Table 2 and Figure 2). From baseline to T2 the mean decrease was 2.3 in the experimental group versus 0.8 in the control group, meaning a difference in decreases of $-1.59$ (95% CI: $-2.81$ to $-0.38$). The reliable change index was 0.61 for the experimental group and 0.20 for the control group.

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**Table 3. Numbers of possible cases in experimental and control group at each measurement.**

<table>
<thead>
<tr>
<th></th>
<th>Men</th>
<th>Women</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>T0</td>
<td>T1</td>
</tr>
<tr>
<td><strong>Experimental group</strong></td>
<td>16</td>
<td>11</td>
</tr>
<tr>
<td><strong>Control group</strong></td>
<td>18</td>
<td>9</td>
</tr>
</tbody>
</table>

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**Figure 2. SRQ-20 score changes between T0 and T2 in experimental and control group.**
doi:10.1371/journal.pone.0021819.g002
Discussion

Our results suggest that the mental health of all survivors of mass violence studied here improved over time. Those who participated in the sociotherapy programme, however, showed an increased improvement over the duration of the intervention. This improvement continued after the intervention, and the difference in scores between the experimental and the control group was even larger at follow-up. This effect is significant in women, and seems to have clinical relevance: the mean SRQ-20 score of female possible cases in the experimental group dropped significantly, ending below cut-off. This corresponds with our finding that the individual scores of 56% of this subsample dropped to below cut-off, a substantially larger proportion than of female possible cases in the control group (21%). A significant improvement was not noted in male possible cases. However, improvement to below cut-off in male possible cases was more frequent in participants in the intervention programme than in the control group: 44% versus 28%.

As no quantitative outcome data of comprehensive psychosocial programmes in post-conflict settings exist so far, we relied on data from our pilot study to establish an appropriate study sample size. Our trial’s methodological strengths include adequate follow-up rates, and use of a measure that was locally validated for use as a screening instrument as well as for measuring symptom change over time. We used a quasi-experimental design, composing a control group equivalent to the experimental group with regard to our main outcome measure and to sex and age. Although the latter group could be considered as help seeking while the first is a community sample, the demand for the programme has shown to be widespread from the start, and its existence was not yet known to control group respondents. Besides, we think that the ‘one-time opportunity’ character of the intervention starting at a certain location was a key determining factor for participation, rather than a worse-than-usual mental state or greater openness in candidate participants at the start. Additionally, given the similarity of the living conditions of both study groups, the risk of confounding bias may be considered minimal. Yet, as this is not a randomized trial, one cannot completely rule out the existence of hidden systematic group differences. A difference between the study groups was noted in SES at baseline. We do not think that this seriously impacted the actual equivalence of both groups. Gicumbi’s population is extremely poor in general, and actually there is little real variety in SES. Possibly, the difference is caused by the method of SES determination. Contrary to the control group, participants of the experimental group described the state of their houses themselves. This may have resulted in a less divergent SES score distribution in the experimental group. A limitation of this study is that interviewers were not blind to the treatment condition, which may have affected the results. They were, however, in no way linked to the sociotherapy programme. Another limitation concerns the lack of detailed data on the proceedings of separate sociotherapy groups.

The applicability of the intervention may have been facilitated by its community-based and contextual sensitive nature, by the local prestige of its coordinators (EER), and by Rwanda’s long history of organizing communities in group structures. The programme’s impact may have been constrained, however, by the country’s still paranoid atmosphere and the prevailing tendency of its inhabitants to keep problems inside, especially in men. Qualitative information consistently pointed out that men in Rwanda generally do not share their emotional problems. This may have affected the results. They were, however, in no way linked to the sociotherapy programme. Another limitation concerns the lack of detailed data on the proceedings of separate sociotherapy groups.

Trials on mental health interventions in post-conflict contexts are rare. The few interventions studied vary from those carried out by multi-disciplinary teams and targeting all help-seeking
clients or only clinically indicated clients [30,31], to interventions directed at psychiatric cases [32–34], or school-based programmes focusing on children [35]. To our knowledge no controlled trials exist of large scale, population level, psychosocial interventions for survivors of mass violence. Such interventions are in line with the IASC Guidelines on Mental Health and Psychosocial Support in Emergency Settings, a consensus document endorsed by all relevant players [36]. This is the first controlled trial of a psychosocial intervention of this kind. The intervention is community-based in the sense that it is owned and carried out by members of the local population. Its sustainability is shown by its ongoing implementation for over 4.5 years now, with over 7,000 participants so far. Our study findings indicate that such an intervention may be clinically relevant and beneficial to mental health problem cases, and that the programme as well as this study deserve replication in other post-conflict contexts. Future studies may establish if the difference in effect between the two sexes found here is related to the actual context or to the intervention method. By collecting data on methods used per group, future studies may also seek to identify favourable and adverse factors within the intervention’s working methods.

**Supporting Information**

**Checklist S1**  CONSORT Checklist.  
(DOC)

**Protocol S1**  Trial Protocol.  
(PDF)

**Acknowledgments**

We thank Prof. JM Richters of the division of Culture, Health and Illness of the LUMC in Leiden for her contributions in qualitative research, and C. Dekker, who trained and supervised the sociotherapy group leaders. We thank Ngendahayo Emmanuel, Sarabwe Emmanuel and Hategekimana Balthazar for helping us organise and conduct the research. We also thank our loyal interviewers: Rukundo Ange, Mugisha-Bitanuzire John-Peter, Umuhoro Adelin, Uwizeye Beata, Ingahire Bernadette, Uwizeymana Albertine, Mukanyiligira Marie Grace, and Muhongwansekob Scholastique.

**Author Contributions**

Conceived and designed the experiments: WFS FV AMK TR. Performed the experiments: WFS FV TR. Analyzed the data: FV AK AHZ KS. Contributed reagents/materials/analysis tools: AMK AHZ. Wrote the paper: WFS FV TR AHZ KS.
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