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# School-Based Mental Health Intervention for Children Affected by Political Violence in Indonesia

## A Cluster Randomized Trial

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**M**ENTAL HEALTH AND PSYCHOSOCIAL interventions for children and adolescents globally have received little research attention, even though mental health problems are one of the most significant contributors to the global burden of disease.<sup>1</sup> Exposure to violence is a risk factor for adverse outcomes of child development in low-income settings,<sup>2</sup> and repeated calls have been made for research into the efficacy of mental health interventions in complex emergencies.<sup>3,4</sup>

However, in spite of a body of literature on the impact of armed conflicts on children's mental health<sup>5,6</sup> and in spite of increased implementation of and consensus on interventions to target children affected by armed conflict,<sup>7</sup> the evidence base for the efficacy of these interventions is weak.<sup>8,9</sup> In Bosnia, a noncontrolled study has shown preliminary evidence for the efficacy of school-based group psychotherapy for posttraumatic stress disorder (PTSD), grief, and depressive reactions for 15- to 19-year-olds.<sup>10</sup> Furthermore, a randomized controlled trial has shown promising results for 5-year-olds by working with mothers through

**Context** Little is known about the efficacy of mental health interventions for children exposed to armed conflicts in low- and middle-income settings. Childhood mental health problems are difficult to address in situations of ongoing poverty and political instability.

**Objective** To assess the efficacy of a school-based intervention designed for conflict-exposed children, implemented in a low-income setting.

**Design, Setting, and Participants** A cluster randomized trial involving 495 children (81.4% inclusion rate) who were a mean (SD) age of 9.9 (1.3) years, were attending randomly selected schools in political violence-affected communities in Poso, Indonesia, and were screened for exposure ( $\geq 1$  events), posttraumatic stress disorder, and anxiety symptoms compared with a wait-listed control group. Nonblinded assessment took place before, 1 week after, and 6 months after treatment between March and December 2006.

**Intervention** Fifteen sessions, over 5 weeks, of a manualized, school-based group intervention, including trauma-processing activities, cooperative play, and creative-expressive elements, implemented by locally trained paraprofessionals.

**Main Outcome Measures** We assessed psychiatric symptoms using the Child Posttraumatic Stress Scale, Depression Self-Rating Scale, the Self-Report for Anxiety Related Disorders 5-item version, and the Children's Hope Scale, and assessed function impairment as treatment outcomes using standardized symptom checklists and locally developed rating scales.

**Results** Correcting for clustering of participants within schools, we found significantly more improvement in posttraumatic stress disorder symptoms (mean change difference, 2.78; 95% confidence interval [CI], 1.02 to 4.53) and maintained hope (mean change difference, -2.21; 95% CI, -3.52 to -0.91) in the treatment group than in the wait-listed group. Changes in traumatic idioms (stress-related physical symptoms) (mean change difference, 0.50; 95% CI, -0.12 to 1.11), depressive symptoms (mean change difference, 0.70; 95% CI, -0.08 to 1.49), anxiety (mean change difference, 0.12; 95% CI, -0.31 to 0.56), and functioning (mean change difference, 0.52; 95% CI, -0.43 to 1.46) were not different between the treatment and wait-listed groups.

**Conclusions** In this study of children in violence-affected communities, a school-based intervention reduced posttraumatic stress symptoms and helped maintain hope, but did not reduce traumatic-stress related symptoms, depressive symptoms, anxiety symptoms, or functional impairment.

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semistructured group discussions aimed at psychoeducation and enhancing coping and mother-child interaction.<sup>11</sup> A study in Gaza reports that debriefing

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and a psychoeducation intervention showed no superior change over a control condition in a nonrandomized study.<sup>12</sup> Apart from studies focused on traumatic stress–related outcomes, a recent study examined interventions for depression of adolescents in northern Uganda through a randomized controlled trial. This study reports that group interpersonal psychotherapy was associated with improvements in depressive symptoms compared with the wait-listed group, which served as the study's control group; however, creative play was not associated with having any effect on depression compared with the control group.<sup>13</sup> The only randomized study on refugees in high-income settings has shown smaller positive gains for a group of adolescents engaged in drama therapy.<sup>14</sup> Cohen and coworkers<sup>15</sup> and Taylor and Chemtob<sup>16</sup> have reviewed the limited number of studies on interventions for trauma-exposed children and adolescents in high-income settings. These 2 review articles show empirical support for cognitive behavioral treatments (CBTs), with a larger evidence base for abused children. A study included in the review by Taylor and Chemtob showed the efficacy of group CBT with children affected by violence for symptoms of PTSD and depression in multiethnic Los Angeles neighborhoods.<sup>17</sup>

Our current study was aimed at evaluating the efficacy of a school-based secondary prevention group intervention for children exposed to traumatic stressors in Central Sulawesi, Indonesia. Central Sulawesi is a multiethnic region on the fourth largest island in the archipelago of Indonesia, with approximately a quarter of the population living below the poverty line. Agriculture is the main source of income. The district of Poso has known communal violence between Christian and Muslim groups since 1998, and a qualitative study has shown the importance of postviolence psychosocial concerns, including an indigenous posttraumatic stress construct (W.A.T. et al, unpublished data,

August–November 2005). The causes of armed violence are myriad and include changed economic relations, migration policies, and state restructuring processes.<sup>18,19</sup>

## METHODS

Our main research questions were “How does a secondary school-based intervention affect psychosocial well-being of violence-affected children?” and “What is the role of gender and age in the outcomes of treatment?” A cluster randomized trial design was chosen over an individually randomized trial design to avoid contamination within schools.

### Participants, Screening, and Setting

Randomization was completed on schools, using a government-provided list of schools in the Poso district of Central Sulawesi, the most affected district within the region. Single-religious and private schools were excluded. Meetings were organized in schools, including parents, teachers, community leaders, and principals, to explain research purposes and obtain consent. Out of 21 qualifying schools, 14 were randomly selected (W.A.T.) using the “select exact amount of cases randomization” function of SPSS version 15.0 (SPSS Inc, Chicago, Illinois). Because we estimated enrolling approximately 30 children per school, 7 schools per treatment group would lead to sufficient sample size. Selected and not selected schools were similar in terms of size, religious composition, and geographic location. If the combined grades 4 and 5 had more than 60 students, either grade 4 or 5 was randomly selected for screening (3 out of 14 schools) using the same method. A power analysis to determine an appropriate sample size was based on both PTSD and depression symptoms. We examined 2 previous studies using the Child Posttraumatic Stress Scale and Depression Self-Rating Scale. Based on reported mean changes by Cohen et al<sup>20</sup> and Layne et al,<sup>10</sup> we calculated effect sizes of 1.10 for PTSD and 0.78 for depressive symptoms. To detect changes with the same effect sizes, with  $\alpha$  equal

to .02 (2-sided) and  $\beta$  equal to .95, we calculated that we needed a minimum of 18 (PTSD symptoms) and 35 children (depressive symptoms) per treatment group. To account for intracluster correlation we multiplied 35 by  $1 + (m-1)\rho$ , with  $m=30$  (average cluster size),  $\rho=0.1$  (intracluster correlation), and a power of 95%, resulting in an appropriate sample size of 137. To ensure sufficient sample size, we aimed at oversampling to reach approximately 200 children per treatment group.

Screening within schools was performed using symptom checklists assessing exposure to violent events ( $\geq 1$ ), PTSD ( $\geq 11$ ), and anxiety complaints ( $\geq 5$ ). These were judged to be relevant based on previous qualitative research (W.A.T. et al, unpublished data, August–November 2005). Screening took place to ensure the enrollment of children with similar symptoms and to avoid the risk of including children with no symptomatology to preclude risk of adverse effects. Screening instruments' sensitivity and specificity were explored with all 20 children in a conveniently selected classroom, outside the study sites. They were assessed independently for the presence of psychopathology by a trained Indonesian psychologist through a clinical interview and by research assistants with symptom checklists (Child Posttraumatic Stress Scale area under the curve, 0.708; cutoff point, 17; sensitivity, 0.750; specificity, 0.667; Depression Self-Rating Scale area under the curve, 0.758; cutoff point, 19.5; sensitivity, 0.714; specificity, 0.692). Original cutoff scores were retained for an overinclusive screening, fitting the secondary prevention character of the intervention. Exclusion criteria included the inability to function in a group setting (eg, violent behavior, could not follow instructions, would harm others) and a group of psychiatric problems (mutism, mental retardation, substance abuse, dissociative disorders, epilepsy without medication, panic or phobic disorders, and child psychosis), which were expected to obstruct participation and benefit from a

group intervention. Trained psychosocial counselors from the intervention team determined exclusion using these criteria, before the start of intervention. Three children were excluded from the study based on these criteria.

Of 495 children, 403 children (81.4%) were included and approached for participation (FIGURE). Baseline assessments were completed between March and May 2006, and follow-up assessments 1 week and 6 months after intervention were conducted from May to July 2006 and October to December 2006, respectively. We reviewed outcomes at 6 months to assess the mid-term benefits of the intervention. Our mixed methods regression analyses took into account change between the 3 time points. Children who did not meet inclusion criteria were invited for nontherapeutic group activities (eg, sports activities) to reduce the risk of stigmatizing selected children. Children who were excluded based on the exclusion criteria were referred for individual treatment.

**Intervention**

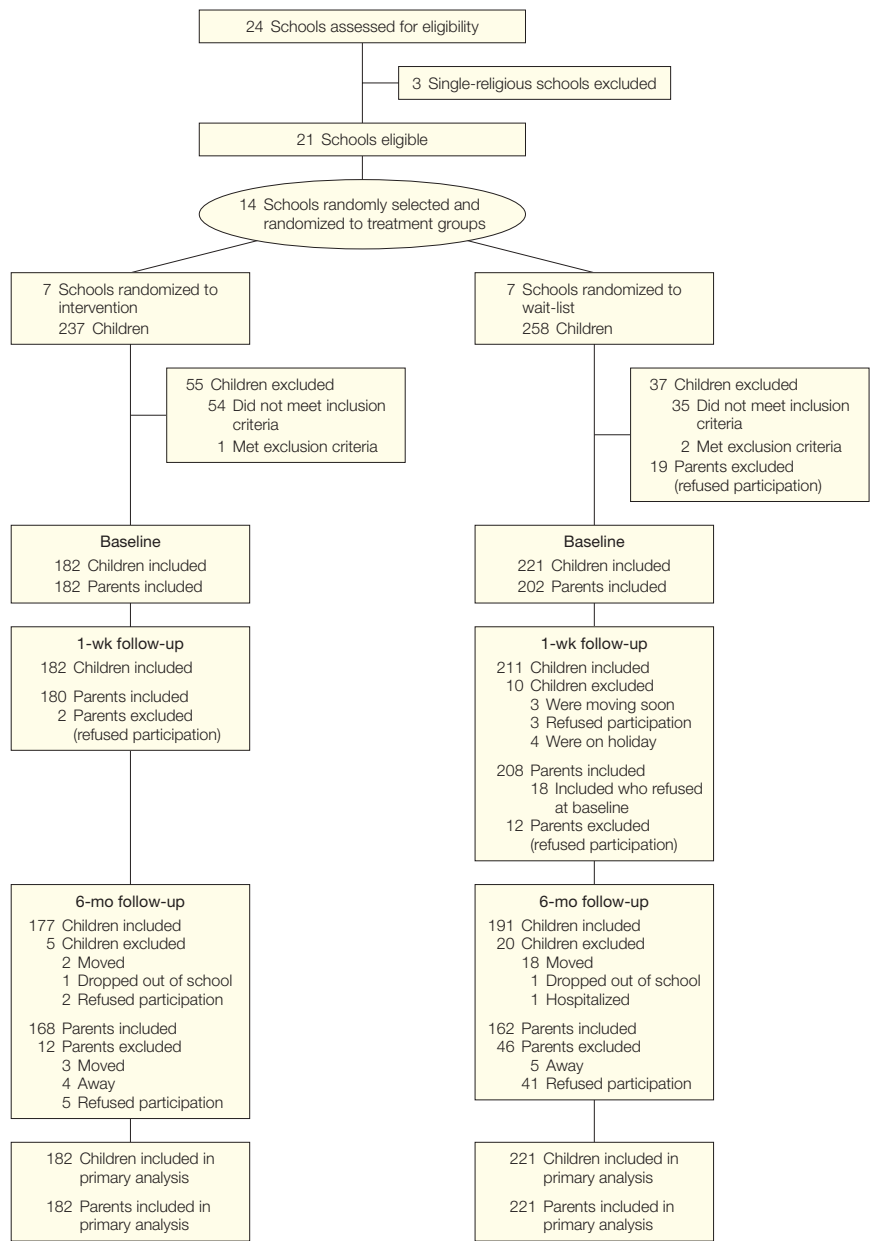
The intervention consisted of 15 sessions with groups of about 15 children over 5 weeks of a manualized classroom-based intervention. Interventionists, who had to be at least 18 years and have had at least a high school education, were selected from local target communities, based on a selection procedure assessing social skills through role-plays. Once selected, interventionists received a 2-week training program. They were generally people with no formal mental health training but had some experience as volunteers in humanitarian programs.

Classroom-based intervention was developed by the Center for Trauma Psychology in Boston<sup>21</sup> and conforms to current expert-based consensus and similar school-based interventions.<sup>7,22</sup> The intervention was part of a larger public mental health program, including primary and tertiary prevention interventions, implemented in partnership by HealthNet TPO and Church World Service–Indonesia. Classroom-

based intervention emphasizes the importance of integrating CBT techniques with cooperative play and creative-expressive exercises (drama, dance, and music) within a structured phased program: week 1, sessions 1 through 3, focuses on information, safety, and control (including psychoeducation); week 2, sessions 4 through 6, focuses on stabilization, awareness,

and self-esteem; weeks 3 and 4, sessions 7 through 12, focus on the trauma narrative; and week 5, sessions 13 through 15, aims at reconnecting the child and group to his/her social context using resiliency-based themes and activities (manual may be requested from R.D.M.). Trauma-focused elements in weeks 3 and 4 include non-forced sharing of trauma stories through

**Figure.** Participant Flow Diagram



Based on a model containing 2-way interaction terms: time × intervention and time × school.

art ("silent stories") and drama games. A randomized controlled trial with children in Palestine has shown promising results for children aged 6 to 11 years.<sup>23</sup>

Multiple independent research assessors judged fidelity of interventionists to the treatment manual, scoring 14 videotapes of randomly selected classroom-based intervention groups and 25 sessions, with a structured checklist containing dichotomous items on presence or absence of prescribed activities. Average treatment adherence was 89.76%. Interventionists did not have a role in assessments for this study.

### Outcome Measurements

Rating scales were chosen as instruments to assess the outcome variables because we were interested in changes of levels of complaints rather than psychiatric diagnoses. Rating scales were selected on the basis of relevance, brevity, ease of use or applicability, previous use in research with ethnocultural populations in war-affected settings, and psychometric properties during previous use. Average interrater reliability between all assessors was high ( $\kappa=0.901$ ) for all dichotomous items as it was for all continuous items in the complete interview schedule ( $\kappa=0.988$ , intraclass correlation). Instruments were translated with methods proposed by Van Ommeren and colleagues.<sup>24</sup> Use of this method ensures systematic use of generally advocated translation strategies through translation by an indigenous group of experts, conceptual review by an independent bilingual professional, review by targeted participants through focus groups, blind back translation, and piloting. To measure internal reliability, we used a Cronbach  $\alpha$  and for 2-week test-retest reliability, the Spearman-Brown coefficient. For parent-rated measures, we did not assess test-retest reliability.

### Traumatic Events and Primary and Secondary Outcomes

Exposure to violence events was assessed through a contextually constructed child-rated checklist of 9 di-

chotomous items (experienced, yes or no), including items such as having witnessed bomb blasts or sniper attacks, having been attacked, and having been displaced (test-retest reliability, 0.612; range, 0-9). It contained items both directly experienced and heard about but did not address structural problems related to residing in the region (eg, poverty). The list was constructed through a free-listing exercise with field-based personnel from the organization that implemented the intervention. For the assessment of posttraumatic complaints the child-rated Child Posttraumatic Stress Scale was used,<sup>25</sup> which measures the 17 PTSD symptoms described in the *Diagnostic and Statistical Manual of Mental Disorders* (Fourth Edition) on a 4-point scale from 0 to 4 ( $\alpha$ , 0.847; test-retest reliability, 0.650; range, 0-51). On the basis of qualitative research (W.A.T. et al, unpublished data, August-November 2005), 6 traumatic stress-related idioms, ie, pains, fainting, dizziness, trembling, stiffness, and fevers were included and scored on a 4-point scale ( $\alpha$ , 0.689; test-retest reliability, 0.566; range, 0-18). Depressive complaints were measured with the Depression Self-Rating Scale, a child-rated scale of 18 symptoms scored on a 3-point scale<sup>26</sup> ( $\alpha$ , 0.412; test-retest reliability, 0.517; range, 0-36). Anxiety symptoms were measured with the Self-Report for Anxiety Related Disorders 5-item version (SCARED-5).<sup>27</sup> This shortened child-rated version consists of those items that best predict the 5 subscales of the full 41-item SCARED ( $\alpha$ , 0.414; test-retest reliability, 0.663; range, 0-10). Aggression was measured with the Children's Aggression Scale for Parents,<sup>28</sup> a 33-item parent-rated scale that measures aggressive behavior on a 5-point scale, including verbal aggression, aggression against objects and animals, physical aggression, and the use of weapons ( $\alpha$ , 0.850; range, 33-132). Hope was measured with the child-rated Children's Hope Scale,<sup>29</sup> which consists of 6 items scored on a 5-point scale ( $\alpha$ , 0.622; test-retest reliability, 0.667; range, 6-36). A higher score de-

notes more hope. Impairment in functioning was measured through a contextually constructed 10-item checklist (child-rated,  $\alpha$ , 0.772; test-retest reliability, 0.783; parent-rated  $\alpha$ , 0.744; range, 10-40). The method proposed by Bolton and Tang<sup>30</sup> was applied and adapted for use with children (W.A.T. et al, unpublished data, 2007). In short, this entailed 2 weeks of participant observation, the collection of diaries ( $n=40$ ), and 2 focus groups with children to attend activities best representing normal daily activities of children at home, in the family, with peers, and at school. The checklist measures difficulties experienced in those activities on a 4-point scale, with a higher score denoting more difficulties in daily activities. Except for exposure, all above listed outcome instruments were included in the baseline and follow-up assessments. In addition, instruments addressing resilience constructs were included (coping, social support, and family connectedness). Because these were included to address underlying treatment mechanisms, they will be discussed elsewhere.

### Procedures and Ethics

Four local assessors with a bachelor's degree in a social science completed a 5-week training course to competently administer the instruments. It was not possible to blind them to treatment status because they needed to visit the selected schools, which is where child and parent interviews took place. A small number of parents who were not available for the assessment at schools were followed up through home visits. Informed consent for participation was sought from both parents and children. Written consent was obtained from parents before the start of research in school meetings. With children, written consent was obtained before the start of the interview, after reading out loud a description of the study and the assurance that nonparticipation would not lead to any negative consequences. The International Review Board of the Vrije Universiteit Amsterdam approved the design of the study.

**Statistics**

Baseline characteristics were compared with  $\chi^2$  tests, with continuity correction or Fisher exact test for comparison of frequencies, and independent sample *t* tests for comparison of mean scores of continuous variables. A small number of random missing values were replaced by group mean (treatment condition). For a descriptive analysis of changes in means between the 3 time points, pure change scores were calculated between baseline and each follow-up session at week 1 and at 6 months and from baseline to 6 months on an intent-to-treat basis, which were compared with independent sample *t* tests. Intent-to-treat analyses consisted of replacing values for missing assessments with the value of the last successful assessment (ie, last observation carried forward). For participants without baseline data, but with available follow-up data (10 children and 19 parents), we replaced missing values with group means. To establish the magnitudes of changes, effect sizes ( $\delta$ ) were calculated.<sup>31</sup>

As recommended for cluster randomized trials,<sup>32,33</sup> we used linear mixed-effects regression models, including fixed and random effects, to analyze the effects of the intervention. The random effects specified in these models provide the method needed to account for clustering or potential lack of independence that may exist between scores of children from the same school. We compared intervention and control groups, adjusting standard errors for clustering at the school level, by testing a random intercept model that included the fixed and random effects of time and intervention. Analyses took place in 2 steps, in accordance with our research questions. In a first step, we established the effect of treatment by examining 2-way interactions (time  $\times$  intervention). Subsequently, we assessed the role of sex and age in treatment by testing 2- and 3-way interactions (time  $\times$  sex, time  $\times$  age, time  $\times$  intervention  $\times$  sex, time  $\times$  intervention  $\times$  age). If 3-way interactions were significant, we repeated test-

**Table 1.** Demographic and Clinical Characteristics at Baseline<sup>a</sup>

Characteristic	Mean (SD)		P Value	Intraclass Correlation
	Treatment (n = 182)	Wait List (n = 221)		
Girls, No. (%)	99 (54.4)	97 (43.0)	.05	
Age, y	10.08 (1.39)	9.78 (1.21)	.02	
Displaced, No. (%)	37 (20.3)	75 (33.9)	.003	
Household members	4.35 (1.67)	4.58 (1.73)	.16	
Violent event types, No.	3.70 (1.82)	4.04 (1.81)	.06	
Child report				
PTSD symptoms <sup>a</sup>	20.92 (8.75)	22.35 (8.39)	.10	0.050
Trauma idiom	4.77 (3.12)	5.32 (3.61)	.11	0.018
Depressive symptoms	12.29 (3.33)	12.55 (3.47)	.44	0.025
Anxiety symptoms	4.38 (1.76)	4.46 (1.87)	.69	0.024
Function impairment	18.03 (5.61)	17.90 (5.39)	.20	0.081
Hope	15.07 (5.53)	16.15 (6.16)	.07	0.027
Parent Report				
	(n = 182)	(n = 202)		
Aggression	42.18 (9.09)	44.63 (12.08)	.03	0.098
Function impairment	14.04 (4.24)	14.20 (4.43)	.72	0.059

<sup>a</sup>On all outcome measures, except hope, a higher score denotes a less favorable state of well-being. See "Methods" section for test score ranges.

ing of 2-way interactions in separate age-sex groups to establish the efficacy of treatment per age-sex subgroup. We used SPSS version 15.0 for Windows mixed-methods regression analyses to test the mixed-effects models. An  $\alpha$  of .05 was considered statistically significant.

**RESULTS**

**Characteristics at Baseline**

The sample at baseline consisted of 207 boys (51.4%) and 196 girls (48.6%) between the ages of 7 and 15 years, with 79.8% between 9 and 11 years; 10.3% 12 years or older; and 10% younger than 8 years, for a mean (SD) age of 9.94 (1.21) years. One hundred thirty-three (31.1%) were Muslim; 189 (46.9%), Protestant; 52 (12.9%), Hindu; 7 (1.7%), Catholic; and 21 (5.2%), other religions. TABLE 1 shows comparisons at baseline of demographics and scores on outcome measures, respectively, including intraclass correlation. Demographics showed significant differences on sex (4% more girls in the treatment groups and 6% more boys in the wait-listed groups), average age (treatment group was 4 months older), and displacement (fewer people living

in their original village in the wait-listed condition). These differences might be attributed to the small number of clusters per group (n = 7). There were no significant differences between conditions on exposure and the outcome measures, except for parent-rated aggression. In addition, using independent sample *t* tests, we examined whether children (or their parents) who missed either the first or second follow-up were different at baseline than those who completed the study on child- and parent-rated outcome measures. We did not find any statistically significant differences.

The Figure shows the flow of participants over time during the assessment periods. More parents and children in the wait-listed group were lost to follow-up than those in the treatment group.

TABLE 2 reports independent sample *t* tests on pure change scores to illustrate changes in means over the 3 time points. These descriptive analyses showed significant differences between changes on means of the intervention condition vs the wait-listed condition on child-rated measures but not on parent-rated measures. Between

**Table 2.** Comparisons of Mean Changes Between Treatment Conditions (Intent-to-Treat)

Outcome	First Follow-up (1 Week After Intervention)						Second Follow-up (6 Months After Intervention)					
	Treatment		Wait List		P Value	$\delta$ (95% Confidence Interval) <sup>a</sup>	Treatment		Wait List		P Value	$\delta$ (95% Confidence Interval) <sup>a</sup>
	Mean Change (SD)	Change, %	Mean Change (SD)	Change, %			Mean Change (SD)	Change, %	Mean Change (SD)	Change, %		
Child report												
PTSD symptoms <sup>b</sup>	-9.10 (9.20)	-43.50	-4.85 (9.49)	-21.7	<.001	0.55 (0.35 to 0.75)	-10.35 (8.89)	-49.47	-6.15 (10.04)	-27.52	<.001	.44 (0.24 to 0.64)
Trauma idiom	-1.13 (3.21)	-23.69	-0.39 (3.65)	-7.33	.03	0.21 (0.02 to 0.41)	-1.64 (3.18)	-34.38	-0.89 (3.82)	-16.17	.03	0.21 (0.02 to 0.41)
Depressive symptoms	-0.80 (3.88)	-6.51	0.50 (4.33)	3.98	.002	0.31 (0.12 to 0.51)	-0.82 (3.82)	-6.53	0.16 (4.73)	1.27	.02	0.24 (0.04 to 0.43)
Anxiety symptoms	-0.97 (2.16)	-22.15	-0.65 (2.32)	-14.57	.15	0.14 (-0.05 to 0.34)	-1.06 (2.45)	-24.20	-0.96 (2.49)	-21.52	.15	0.04 (-0.16 to 0.24)
Function impairment	-3.30 (5.52)	-18.30	-1.11 (4.98)	-6.20	<.001	0.42 (0.22 to 0.61)	-3.48 (5.70)	-19.30	-2.06 (5.07)	-11.51	.008	0.26 (0.07 to 0.46)
Hope	-0.95 (6.84)	-6.30	1.10 (7.11)	6.81	.004	0.29 (0.10 to 0.49)	-0.60 (6.56)	-3.98	1.95 (6.90)	12.07	<.001	0.38 (0.18 to 0.57)
Parent report												
Aggression	-3.88 (9.22)	-9.20	-2.97 (9.13)	-6.65	.33	0.10 (-0.09 to 0.29)	-3.50 (9.13)	-8.30	-4.19 (10.63)	-9.39	.50	0.07 (-0.12 to 0.26)
Function impairment	-1.44 (4.72)	-10.26	-1.16 (4.23)	-8.17	.54	0.06 (-0.13 to 0.25)	-2.03 (4.71)	-14.46	-1.48 (4.69)	-10.42	.25	0.12 (-0.07 to 0.31)

Abbreviations: CI, confidence interval; PTSD, posttraumatic stress disorder.  
<sup>a</sup> $\delta$  Cohen effect size.<sup>31</sup> Effect sizes can be interpreted in terms of clinical meaningfulness as follows: 0 to 0.30, small; 0.31 to 0.59, moderate; 0.60 or higher, high effect size.  
<sup>b</sup>On all measures, except hope, a negative change is an improvement in well-being.

**Table 3.** Mean Treatment Group Differences Between Baseline and 6-Month Follow-Up Adjusted for Clustering of Participants in Schools

Outcome <sup>a</sup>	Mean (SE) at Baseline Adjusted for School Mean	Changes Adjusted for School		Mean (SE) Change Difference <sup>b</sup>	Mean Change Difference (95% CI)
		Mean (SE) Change	Mean Change, %		
PTSD symptoms	17.28 (0.84)	-7.47 (0.66)	-43.23	-2.78 (0.89)	(1.02 to 4.53)
Trauma idiom	4.33 (0.29)	-1.30 (0.23)	-30.02	-0.50 (0.31)	(-0.12 to 1.11)
Depressive symptoms	12.04 (0.36)	-0.66 (0.30)	-5.48	-0.70 (0.40)	(-0.08 to 1.49)
Anxiety symptoms	4.07 (0.17)	-0.86 (0.17)	-21.13	-0.12 (0.22)	(-0.31 to 0.56)
Function impairment	16.48 (0.61)	-2.28 (0.36)	-13.83	-0.52 (0.48)	(-0.43 to 1.46)
Hope	15.38 (0.49)	0.41 (0.49)	2.67	2.21 (0.66)	(-3.52 to -0.91)

Abbreviations: CI, confidence interval; PTSD, posttraumatic stress disorder.  
<sup>a</sup>On all measures, except hope, a negative mean change is an improvement in well-being.  
<sup>b</sup>A larger mean change difference is an additional positive improvement for the treatment group compared to the wait-listed group for all measures except hope.

baseline and the first follow-up, significantly different changes were found on all child-rated measures except anxiety. We did not find significant differences in scores between the first and the second follow-ups. At the 6-month follow-up, changes in general remained, although the magnitude of differences was smaller. Differences in mean change referred to moderate effect size for PTSD and hope between baseline and the first follow-up. At the second follow-up, depression and impairment in functioning differences in pure

change scores were small, but PTSD differences remained moderate. Also, differences in change on hope scores refer to moderate effect size at the second follow-up. Hope was retained in the intervention group, but decreased in the control group.

For a true test of our research questions, we performed mixed methods regression analyses (TABLE 3). We found a statistically significant effect of treatment on changes over time for PTSD symptoms and hope (mean change difference for PTSD symptoms, -2.78; 95%

CI, 1.02 to 4.53; for hope, -2.21, 95% CI, -3.52 to -0.91), but not for the other outcome instruments. In these mixed-methods regression analyses, we found only significant treatment effects for those variables that referred to a moderate effect size in the independent sample *t* tests (ie, PTSD and hope).

The analyses for our second research question showed significant effects of sex on change in PTSD symptoms (3-way interaction time×intervention×sex coefficient, 5.12; 95% CI, 2.27 to 7.96), as well as functioning (3-way interaction time×intervention×sex coefficient, 2.75; 95% CI, 1.15 to 4.34). Three-way interaction terms with sex for the other outcome instruments, including hope, were not significant. None of the 3-way interactions for age were significant. Subsequent 2-way interaction subgroup analyses (time×intervention) in subgroups to assess the effect of treatment on PTSD symptoms and function impairment per sex showed that treatment was effective in reducing PTSD and reducing function impairment for girls (2-way interaction terms coefficients: for PTSD, 4.76; 95% CI, 2.49 to 7.03; for hope, -2.71; 95% CI, -4.61 to -0.82; for function im-

pairment, 1.64; 95% CI, 0.27 to 3.02). No significant effects of treatment were found among boys on these 2 outcome variables.

## COMMENT

To our knowledge, this is the first cluster randomized trial on a school-based psychosocial intervention for children exposed to armed conflict in a low-income setting. Findings show a moderate reduction in PTSD symptoms and function impairment for girls and retained hope for boys and girls in comparison to a wait-listed condition between baseline, 1-week, and 6-month follow-up, in a situation of ongoing insecurity and instability. No changes were found on the other outcome variables; traumatic idioms, depressive, anxiety symptoms, and functioning (the latter for boys). It is noted that the intervention was implemented by paraprofessionals, because of the common lack of mental health professionals in complex emergencies.

Regarding our second research question, sex influenced both changes in PTSD symptoms and function impairment, but we found no effects for age. Girls benefitted more from the intervention than boys. The latter finding is in accordance with the efficacy of group interpersonal psychotherapy for adolescent girl survivors of war and displacement in Northern Uganda.<sup>13</sup>

In contrast to the lack of change shown in adolescents receiving creative workshops in Uganda,<sup>13</sup> this intervention, which includes structured creative activities as well as trauma-focused activities, did show effects on psychosocial well-being. It could therefore be considered a preliminary argument that increased structured interventions, which include trauma-focused activities, more effectively target PTSD symptoms. Corroboration of this argument can be found in the high-effect sizes of group CBT implemented in violence-affected schools in Los Angeles.<sup>17</sup> However, previously mentioned qualitative research has shown the importance of addressing wider social problems caused by war, rather than

purely focusing on PTSD complaints.<sup>34</sup> In addition, specialized mental health professionals to implement CBT are usually unavailable in low-income settings.<sup>35</sup> To resolve this tension, we propose that in complex emergencies, interventionists use a public health framework to tailor interventions to an appropriate population and referral level, based on investigated local needs, severity of complaints, available resources, and feasible and cost-effective interventions, while recognizing the importance of the social-ecological context.<sup>36,37</sup> On the basis of these findings, the classroom-based intervention then qualifies as an appropriate intervention to target larger groups of children (especially girls) at risk, when stress-related symptoms are relevant.

Although results were promising in terms of PTSD symptoms, hope, and functioning for girls, we did not find treatment effects for traumatic idioms, anxiety, and depressive symptoms for girls. Adaptations could be made to the intervention to address local, mainly somatic, expressions of trauma, for instance through focused psychoeducation and an increased inclusion of body-focused activities.<sup>38</sup>

Although PTSD symptoms and functioning decreased after the intervention for girls, we found only the maintenance of hope for boys in the treatment group. Sex differences may be due to differences in expression of and dealing with emotions in a group intervention setting, and further research should address appropriate interventions for boys.

In conclusion, we found treatment effects on 3 out of the 6 child-rated outcome instruments for girls and 1 out of 6 for boys. We hypothesize that these results may show that psychosocial interventions alone are unable to reverse the challenges to psychosocial well-being presented by chronic poverty and political instability. Further integration of psychosocial interventions with poverty reduction and conflict resolution might be indicated. Moreover, we did not find significant changes on parent-rated mea-

asures. We explain these findings by pointing to the number of parents that did not complete follow-up assessments (20.8%) and to indications from qualitative research (W.A.T. et al, unpublished data, August–November 2005) that parents' capacity to assess their children's well-being is affected by families' focus on rebuilding livelihoods destroyed during political violence.

Results of the study must be interpreted in light of the following limitations. First, some of the instruments had less than satisfactory internal reliability, notably the Depression Self-Rating Scale and SCARED-5. Low internal consistency of these instruments limits conclusions regarding interpretation of repeated measurements on these measures. It is therefore unclear whether the lack of change observed on these measures must be interpreted as a lack of efficacy of the intervention or a reliability problem. A recommendation following this concern is the need to strengthen measurement instruments in evaluation studies of this kind. Although rigorous studies are starting to appear in this field, further efforts must focus on developing locally constructed and validated measurement instruments. Second, assessors were not blinded to treatment status, and this could have biased results. Third, in terms of external validity, results of the study are only generalizable to school-going Indonesian children. Strengths of this study include its experimental nature within a community-based setting, the use of culturally relevant outcome measurements, assessment of fidelity, taking into account a broader definition of psychosocial well-being, and a longer-term follow-up.

In short, a school-based psychosocial intervention was able to moderately reduce PTSD symptoms, retain hope, and improve functioning for girls, and retain hope for boys affected by communal violence in a low-income context. Further adaptations and research to address the full range of post-traumatic outcomes and functioning are necessary.



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