Single session debriefing after psychological trauma: a meta-analysis

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Summary

Background Despite conflicting research findings and uncertain efficacy, single session debriefing is standard clinical practice after traumatic events. We aimed to assess the efficacy of this intervention in prevention of chronic symptoms of post-traumatic stress disorder and other disorders after trauma.

Methods In a meta-analysis, we selected appropriate studies from databases (Medline Advanced, PsychINFO, and PubMed), the *Journal of Traumatic Stress*, and reference lists of articles and book chapters. Inclusion criteria were that single session debriefing had been done within 1 month after trauma, symptoms were assessed with widely accepted clinical outcome measures, and data from psychological assessments that had been done before (pretest data) and after (post-test data) interventions and were essential for calculation of effect sizes had been reported. We included seven studies in final analyses, in which there were five critical incident stress debriefing (CISD) interventions, three non-CISD interventions, and six no-intervention controls.

Findings Non-CISD interventions and no intervention improved symptoms of post-traumatic stress disorder, but CISD did not improve symptoms (weighted mean effect sizes 0·65 [95% CI 0·14–1·16], 0·47 [0·28–0·66], and 0·13 [–0·29 to 0·55], respectively). CISD did not improve natural recovery from other trauma-related disorders (0·12 [–0·22 to 0·47]).

Interpretation CISD and non-CISD interventions do not improve natural recovery from psychological trauma.

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Introduction

After traumatic events such as the Sept 11 attacks, offers of emotional and practical support to victims are thought to be appropriate and caring human responses. Psychological debriefing is a formal type of post-traumatic care, for which several models have been developed in the past two decades. Everly and colleagues[1] describe three stages in the development of these models. The earliest forms of debriefing included many individually applied techniques, termed "crisis intervention approaches".[1] "Group psychological debriefing"[2] has been used to reduce immediate distress, prevent later adverse psychological sequelae including post-traumatic stress disorder,[3] and identify individuals who were at risk of development of chronic problems and who needed referral for further treatment. There are three types of group psychological debriefing: critical incident stress debriefing (CISD) also known as the Mitchell model,[4] the Raphael model,[5] and process debriefing.[6]

In typical CISD, within 1 week of a traumatic event, a group of victims are led through seven stages in a single 1–3 h session. Process debriefing and the Raphael model are variations on the CISD model, differing in their emphasis on structure and in certain aspects of content.[2] CISD was integrated in the more comprehensive critical incident stress management model (CISM).

Psychological debriefing has received increasing attention from the scientific community. A search of the PsychINFO-database for English language journal articles with the word "debriefing" in the title identified 206 hits for the 1990s, compared with 79, 47, and 11 hits in the 1980s, 1970s, and 1960s, respectively. Many interventions are offered as treatments and described as debriefing, including CISD or CISD-like interventions, interventions that share only some elements with CISD, and interventions that have very little to do with CISD in its original form. Furthermore, these interventions are delivered by professional and non-professional workers with different backgrounds, at different time-intervals (sometimes months after a traumatic event), and are assessed with different instruments.

Despite the large number of research publications on this issue, debate continues on the efficacy of single session debriefing in prevention of symptoms of chronic post-traumatic stress disorder and other negative psychological outcomes after trauma. Several narrative reviews have been published on single session debriefing.[1,2,7] Conclusions varied from "there is no current evidence that psychological debriefing is a useful treatment for the prevention of PTSD [post-traumatic stress disorder] after traumatic incidents"[7] to "crisis intervention procedures, group debriefings, and especially CISM approaches are effective in reducing the negative psychological aftermath of a variety of critical incidents".[1] Thus, there is still no consensus on whether single session debriefing can contribute to the prevention of symptoms of chronic post-traumatic stress disorder.

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Narrative reviews of research have several limitations;[8] meta-analysis is a useful alternative. However, an earlier meta-analysis on the efficacy of psychological debriefing also had several limitations.[9] First, only studies of group psychological debriefing were included, although in clinical practice individual debriefing is the rule rather than the exception. Thus, the conclusions drawn by the authors cannot be generally applied to clinical practice. Second, in two studies, psychological debriefing was done at 6 and 9 months after trauma, and at 6 months after trauma, respectively, thus, these interventions could hardly have been preventive.

We have done a meta-analysis of studies designed to assess the efficacy of single session debriefing in preventing post-traumatic stress disorder and non-post-traumatic stress disorder psychopathology. We included studies of group and individual debriefing interventions that had been administered within 1 month of a traumatic event. The fourth edition of the *Diagnostic and Statistical Manual of Psychiatric Disorders* (DSM-IV)[3] states that to meet criteria for a diagnosis of post-traumatic stress disorder, symptoms have to persist for at least 1 month. Interventions done more than 1 month after trauma are therefore curative rather than preventive.

Methods

Procedures

We searched for studies on databases: Medline Advanced (1973–2000), PsychINFO (1967–2000), and PubMed (1970–2000). Keywords used were "posttraumatic", "stress", "debriefing", "prevention", and "intervention", and names of authors working in debriefing. We also did a manual search of all volumes of the *Journal of Traumatic Stress*. We searched reference lists of articles and book chapters identified by the searches for other relevant studies.

Inclusion criteria were that single session debriefing been done within 1 month after a traumatic event, psychological symptoms had been assessed with widely accepted clinical outcome measures, and data from psychological assessments that had been done before (pretest data) and after (post-test data) interventions and were essential for calculation of effect sizes had been reported for at least one outcome measure. Since we were not only interested in the effect of single session debriefing on symptoms of post-traumatic stress disorder, but also in the effects on general psychopathology, we included studies that contained reliable and valid psychological outcome measures for symptoms other than those of post-traumatic stress disorder. Outcome measures assessing symptoms of post-traumatic stress disorder included the impact of event scale,[10] clinician-administered post-traumatic stress disorder scale,[11] and post-traumatic stress disorder symptom scale.[12] Outcome measures assessing non-post-traumatic stress disorder symptoms included the hospital anxiety and depression scale,[13] brief symptom inventory,[14] and state-trait anxiety inventory.[15] Because of the ethical and practical difficulties in doing research after traumatic events and the resulting scarcity of such studies, we also included studies that fell marginally short of the highest methodological standards (eg, non-randomised allocation of participants to intervention and control groups). In studies that included more than one post-test assessment, data for the last measurement are presented.

We grouped interventions into CISD-type interventions and non-CISD interventions (30-min counselling, education, and historical group debriefing).

Study	Intervention	n*	Age (years, mean [SD])	Sex male	Dropouts	Measures	Type of trauma	Intervention		
								Timing (first)	Length	Timing (final)
Bisson[18]	CISD†	57	37·9 (13·1)	74%	26%	IES, HADS-A, HADS-D	Burns	Mean 6·3 days (SD 3·6) after trauma	Mean 44·3 min (SD 17·4)	13 months after trauma
	No-intervention control	46	36·7 (13·9)	76%	18%					
Carlier[19]	CISD	86	28·9 (5·6)	70%	NR	STAI-S	Misc (police officers)	About 24 h after trauma	Mean 41·4 min (SD 24·9)	24 h after trauma (shortly after first debriefing session)
	No-intervention control	82	31·7 (7·1)	65%	NR					
Conlon[20]	30-min counselling	18	32·9 (10·8)	39%	0 (40 on CAPS)	IES, CAPS				
	No-intervention control	22	34·7 (13·2)	55%	0 (5 on CAPS)		Road traffic accident	Mean 7 days after trauma	About 30 min	Mean 99 days after trauma
Mayou[21]	CISD	30	29 (NR)‡	57%‡	44%	IES, BSI	Road traffic accident	Within 24–48 h of the accident	1 h debriefing	36 months
	No-intervention control	31	26 (NR)‡	67%‡	40%					
Lee[22]	CISD	21	NR	0	7 (overall)	IES-I, IES-A, HADS-A, HADS-D	Early miscarriage	About 2 weeks after miscarriage	1 h session	About 4 months after miscarriage
	No-intervention control	18	NR	0	..					
Rose[23]	CISD	29	35·4 (13·8)	69%	46%	IES, PSS	Violent crime	Mean 21 days (SD 5·6) after trauma	Debriefing about 1 h Education about 30 min	11 months
	Education	35	34·9 (13·2)	75%	33%					
	No-intervention control	28	37·3 (13·8)	82%	45%					
Shalev[24]	Historical group debriefing	39	19·4 (1·8)	NR	5	STAI-S	Combat exposure	Within 48–72 h of combat	Mean 2·5 h (SD NR)	Immediately after debriefing

NR=data not reported. IES(-I/-A)=impact of event scale (-intrusion/-avoidance). HADS(-A/-D)=hospital anxiety and depression scale (-anxiety/-depression). BSI=brief symptom inventory. CAPS=clinician-administered post-traumatic stress disorder scale. STAI-S=state-trait anxiety inventory-state version. PSS=post-traumatic stress disorder symptom scale. *Number of participants who completed assessment. †CISD according to Mitchell's seven-stage model, or closely corresponding to CISD. ‡Reported at 4-months' follow-up, not available at 36-months' follow-up, but no significant differences between intervention and control groups at 36 months or between people who did and did not complete preintervention assessments.

Table 1: **Description of studies included in the final sample**

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Measures used to assess symptoms were grouped into those used to assess symptoms of post-traumatic stress disorder and those used to assess other symptoms (mainly of general anxiety and depression).

Statistical analyses

Before calculation of mean weighted effect sizes and comparison of 95% CIs, we investigated whether effect sizes should be weighted on quality of the study and duration of intervention as well as on sample size. The analytic strategy was based on work by Van Etten and Taylor.[16] Within-study effect sizes refer to the magnitude of change assessed with continuous measures between preintervention and postintervention assessment results within each intervention and control group (ie, rather than differences in post-test results across interventions). Effect sizes were calculated for each measure using Cohen's *d* statistic,[17] with the magnitude of change defined as the difference between preintervention and postintervention assessment group means divided by the pooled SD. Positive effect sizes indicate reductions in symptom severity; negative effect sizes indicate worsening of symptoms. If a study included more than one assessment after the intervention, effect sizes were calculated from results of all assessments. Long-term outcome was considered to be most relevant to our study. Therefore, reported effect sizes are for means and SDs obtained from the last assessment. Since most studies reported data only for participants who had completed both preintervention and postintervention assessments, effect sizes were based on these participants rather than on end-point or intent-to-treat analyses. If participants had completed more than one measure in a symptom group, effect sizes for these measures were averaged to obtain an aggregate effect size.[16]

Mean effect sizes were calculated across intervention types (CISD and non-CISD) and no-intervention control groups for both symptom groups. Since effect sizes of large studies are more likely to be reliable estimates of the efficacy of single session debriefing than those of small studies, sizes were weighted by the number of participants who completed assessments in each intervention group. 95% CIs were calculated for these weighted mean effect sizes to establish whether they were significant at $p<0{\cdot}05$. Weighted mean effect sizes without overlapping 95% CIs differ significantly at $p<0{\cdot}05$. Fail-safe N statistics were calculated[16] to investigate whether significant mean effect sizes might have been inflated by a publication bias—ie, a bias towards publication of studies reporting significant findings and large effect sizes.

Role of the funding source

The sponsors of the study had no role in study design, data collection, data analysis, data interpretation, or writing of the report.

Results

We identified 29 relevant outcome studies. We excluded 22 studies in which the intervention consisted of more than one session (three studies), the interval between the traumatic event and intervention was more than 1 month or was unclear (six studies), the intervention was

Study	Intervention	Measures	Effect size	Aggregate effect size (95% CIs)	
				PTSD symptoms	Other symptoms
Bisson[18]	CISD	IES	–0·18	–0·18 (–0·80 to 0·44)	–0·13* (–0·29 to 0·03)
		HADS-A	–0·19	··	··
		HADS-D	–0·07	··	··
	No-intervention control	IES	0·39	0·39 (–0·27 to 1·05)	0·24* (0·08 to 0·40)
		HADS-A	0·21	··	··
		HADS-D	0·27	··	··
Carlier[19]	CISD	STAI-S	0·38	N/O	0·38 (0.18 to 0·58)
	No-intervention control	STAI-S	0·01	N/O	0·01 (–0·21 to 0·23)
Conlon[20]	30-min counselling session	IES	0·99	0·99 (–1·28 to 3·26)	N/O
		CAPS	1.36	··	··
	No-intervention control	IES	0·73	0·73 (–0.87 to 2·33)	N/O
		CAPS	0·50	··	··
Mayou[21]	CISD	IES	–0·07	–0·07 (–1·15 to 1·01)	–0·31 (–0·35 to -0·27)
		BSI	–0·31	··	··
	No-intervention control	IES	0·19	0·19 (–0·77 to 1·15)	0·13 (0·11 to 0·15)
		BSI	0·13	··	··
Lee[22]	CISD	IES-I†	0·63	0·62* (–0·50 to 1·74)	0·37* (–0·15 to 0·89)
		IES-A	0·61	··	··
		HADS-A	0·25	··	··
		HADS-D	0·48	··	··
	No-intervention control	IES-I	0·57	0·53* (–0·84 to 1·90)	0·37* (–0·32 to 1·06)
		IES-A	0·49	··	··
		HADS-A	0·28	··	··
		HADS-D	0·46	··	··
Rose[23]	CISD	IES	0·79	0·61* (–0·49 to 1·71)	N/O
		PSS	0·43	··	··
	Education	IES	0·46	0·47* (–0·44 to 1·38)	N/O
		PSS	0·48	··	··
	No-intervention control	IES	0·80	0·66* (–0·53 to 1·85)	N/O
		PSS	0·50	··	··
Shalev[24]	Historical group debriefing	STAI-S	0·36	N/O	0·36 (0·12 to 0·60)

N/O=data not obtained. IES(-I/-A)=impact of event scale (-intrusion/-avoidance). HADS(-A/-D)=hospital anxiety and depression scale (-anxiety/-depression). BSI=brief symptom inventory. CAPS=clinician-administered post-traumatic stress disorder [PTSD] scale. STAI-S=state-trait anxiety inventory-state version. PSS=post-traumatic stress disorder symptom scale. *Aggregate effect size across measures. †Scores reported for the IES subscales only; we combined these to an aggregate effect size, as with the other aggregate effect sizes. Hence IES total scores are not reported.

Table 2: **Aggregate effect sizes and 95% CIs for type of intervention and symptom group**

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	n	PTSD symptoms M (95% CIs)	Other symptoms M (95% CIs)
Intervention			
CISD	5	0·13 (–0·29 to 0·55)	0·12 (–0·22 to 0·47)
Non-CISD interventions	3	0·65 (0·14 to 1·16)	0·36 (··)*
No-intervention control	6	0·47 (0·28 to 0·66)	0·13 (–0·02 to 0·28)

M=mean weighted effect sizes for the difference between results of preintervention and postintervention assessments. Since studies used different measures, mean weighted effect sizes were calculated from different numbers of separate effect sizes. *95% CI could not be calculated because only one effect size was available.

Table 3: **Effects of interventions on post-traumatic stress disorder (PTSD) and other symptoms**

exclusively pharmacological (one study), no preintervention psychological assessment was done (9 studies), or the data needed to calculate effect sizes (eg, means and SDs) were not available (three studies); studies excluded for multiple reasons are counted under the main reason for exclusion. A list of the excluded studies is available from the authors. The final sample included seven studies (table 1)[18–24] with eight interventions and six control groups in which no intervention had been done (ie, control studies showed spontaneous recovery or assessment only). Five studies were randomised controlled trials, one was a non-randomised controlled trial,[19] and one did not include controls.[24] One study[24] was of group debriefing and six were of individual debriefing. Carlier and colleagues[19] did a three-session intervention, thus their study was initially excluded. However, as the first CISD-type session was done and followed up by a preliminary post-test within 1 month after the traumatic event, the preintervention assessment and first post-test results met our criteria for inclusion.

Weighting effect sizes by relevant aspects of research quality was considered; specifically the effect of design and duration of intervention. Study designs were very similar, whereas other quality indices were too scarce or unclear to calculate meaningful weights. Duration of intervention and effect sizes were not correlated. Accordingly, additional weighting of effect sizes was dismissed.

Table 2 shows aggregate effect sizes and 95% CIs. For the study by Conlon and colleagues,[20] an aggregate effect size across the results from the IES (impact of event scale) and CAPS (clinician-administered PTSD [post-traumatic stress disorder] scale) could not be calculated since different numbers of participants completed each measure. Since the CAPS was the only observer-rated measure with a 40% dropout rate in the intervention, its effect size was not included in further analyses.

Table 3 shows mean weighted effect sizes. One way to interpret effect sizes is to consider values of 0·2, 0·5, and 0·8 as corresponding to small, medium, and large effects, respectively.[17] In accordance with this rule, the no-intervention condition (controls) resulted in a medium reduction in the severity of symptoms of post-traumatic stress disorder and a small reduction in other symptoms. Non-CISD interventions resulted in a medium-to-large reduction in the severity of symptoms of post-traumatic stress disorder and a small-to-medium reduction in other symptoms (but only one effect size was available for other symptoms). CISD interventions resulted in a small reduction in severity of symptoms of post-traumatic stress disorder and other symptoms.

95% CIs show that the effect sizes for CISD were not significant (table 3). The 95% CI for the non-CISD interventions was positive (ie, symptoms improved) and did not contain zero. The 95% CI for the no-intervention control category was greater than zero for symptoms of post-traumatic stress disorder, but contained zero for other symptoms. Comparisons between 95% CIs showed that they all overlapped—ie, there were no significant differences between effect sizes for CISD, non-CISD, and no intervention for both symptom groups. Fail-safe N statistics indicated that significant effect sizes were unlikely to have been inflated by a bias towards publication of studies reporting significant findings and large effect sizes.

Discussion

Despite the intuitive appeal of the technique, our results show that CISD has no efficacy in reducing symptoms of post-traumatic stress disorder and other trauma-related symptoms, and in fact suggest that it has a detrimental effect. In both groups of symptoms, 95% CIs for CISD overlapped with those for non-CISD interventions and no intervention controls. Thus, CISD was no more effective than non-CISD interventions or even than not intervening at all. In fact, the mean weighted effect size for symptoms of post-traumatic stress disorder was lower for CISD than for non-CISD interventions and for not intervening.

In the other group of symptoms, mean weighted effect sizes for CISD and for no intervention were equal. Stated differently, effect sizes for CISD were not significant in either symptom group, whereas effect sizes for non-CISD interventions and for no intervention indicated improvement in symptoms of post-traumatic stress disorder. This finding suggests that CISD does not improve psychological outcome after traumatic events.

A more lenient analysis with 90% CIs did not change the pattern of results. At $p<0·10$, mean weighted effect sizes for CISD were again not significant for either symptom group. Also, analysis of data obtained in the first rather than the last psychological assessment done after the intervention did not substantially change results. This result is not surprising since only three studies[18,21,23] included more than one such test and there was no significant correlation between effect size and duration of the interval between intervention and assessment. The only change in results was that the mean weighted effect size for symptoms of post-traumatic stress disorder was not significant for no intervention.

Most events in the studies included in our meta-analysis are major life events, and qualify as potential traumatic events. However, whether early miscarriages[22] are traumatic events is disputed, but exclusion of this study did not change the pattern of findings. In sum, the findings were robust even with varying statistical stringency, timing of assessment after the intervention, or stringency of the definition of trauma.

There are several explanations for the lack of efficacy of CISD. CISD might interfere with the alternation of intrusion and avoidance that characterises the natural processing of a traumatic event.[25] It might also interfere with natural processing in a broader sense—ie, inadvertently leading to victims bypassing the support of family, friends, or other sources of social support. CISD probably increases awareness of normal manifestations of distress after trauma. Although normalisation of these reactions is the aim of CISD, the suggestion that such reactions warrant professional care and must therefore be maladaptive might be an unintended result. Alternatively or additionally, exposure to trauma-related internal and external stimuli in CISD might not allow victims

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adequate time for habituation, thereby further sensitising them to these stimuli.[8] This hypothesis can be tested by collecting data that reflect habituation (eg, by use of subjective units of disturbance),[26] but to our knowledge no such data exist.

A third explanation could be that if CISD is offered after trauma, both victims at risk and victims not at risk for chronic psychological symptoms can participate. This factor might obscure a true beneficial effect of CISD on the development of chronic symptoms for individuals at risk. Although increasing numbers of risk factors for chronic symptoms after trauma are being identified,[27] their clinical and practical use is untested. Studies should be done to assess whether targeting the CISD intervention to at-risk individuals is warranted. Finally, CISD was never designed to be a stand-alone intervention, but rather part of a broader, multi-component CISM-type intervention that included training in being prepared for a crisis, follow-up, and referral.[9,19] The efficacy of this type of intervention was not the subject of our meta-analysis, and we suggest that it be convincingly proven in empirical research before large-scale implementation.

A limitation of our study is that, similarly to the meta-analysis by Everly and colleagues,[9] our analysis includes only a small number of studies because of our exclusion criteria and because some studies did not report preintervention assessment data, rendering impossible calculation of within-effect sizes of change in symptom severity. However, we realise that preintervention assessment is difficult in the aftermath of trauma. A possible solution to the small number of studies would have been to widen our inclusion criteria to include studies in which more than one intervention session was done or in which interventions were done more than 1 month after trauma. However, we do not think that this solution would have been useful, since it would not have answered our original question about the efficacy of single session debriefing in preventing chronic symptoms. Furthermore, meta-analyses based on small numbers of studies are not unusual and do not preclude drawing meaningful conclusions.[8]

Adaptations to enhance single session debriefing have been suggested. Symptoms of post-traumatic stress disorder in victims of robbery markedly improved after immediate CISD (<10 h), whereas participants in delayed debriefing (>48 h) benefited only slightly.[28] Brief cognitive behavioural programmes have produced promising results.[29,30] These programmes typically consist of four to five weekly individual sessions, starting within the first month after the traumatic event, and homework assignments. Interventions include education, imaginary and real (but introduced on a graded scale) exposure to traumatic situations, and cognitive therapy.

Should single session debriefing be made available routinely after trauma? Prevention of later adverse psychological sequelae such as post-traumatic stress disorder is only one aim of psychological debriefing. Other aims include reduction of immediate distress and identification and referral for further treatment of individuals at risk for development of chronic problems.[2] The decision to provide debriefing is not necessarily based on findings from only empirical research. Reports of satisfaction[17] or perceived helpfulness by participants[18,22] might be sufficient reasons to continue to offer debriefing. However, claims that single session psychological debriefing can prevent development of chronic negative psychological sequelae are empirically unwarranted.

Contributors

P M G Emmelkamp, A A P van Emmerik, J H Kamphuis, and A M Hulsbosch conceived and designed the study. A M Hulsbosch and P M G Emmelkamp did the literature search and identified eligible studies. A A P van Emmerik, J H Kamphuis, and A M Hulsbosch coded articles and decided on their inclusion. J H Kamphuis and A A P van Emmerik did statistical analyses and interpreted results. All drafts of the report, including the final version, were written by A A P van Emmerik and J H Kamphuis and revised by P M G Emmelkamp and A M Hulsbosch.

Conflict of interest statement

None declared.

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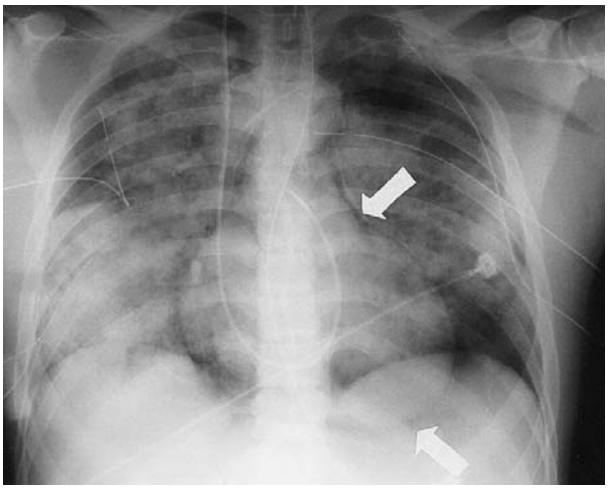
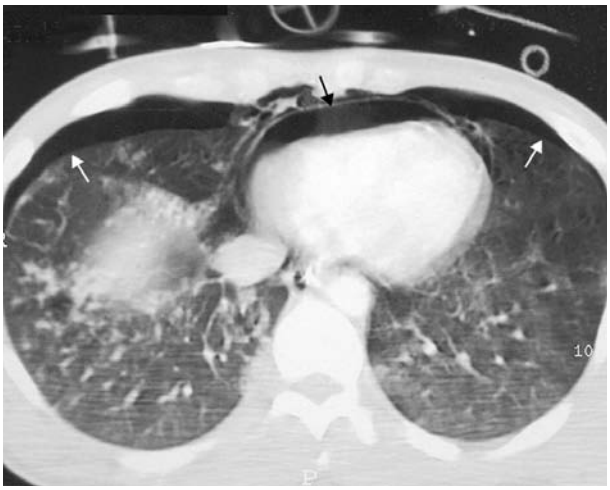
Clinical picture

Pneumopericardium

Isabelle Gerard, David Verhelst





A 27-year-old man was admitted to the hospital after a fall from approximately 10 m. He had multiple bone fractures, head trauma (Glasgow Coma Scale: 4/15), bilateral pulmonary contusions and pneumothoraces. We placed bilateral tube thoracostomies, and treated his other injuries. 1 day later, because of severe haemodynamic instability (hypotension and low cardiac output with high central venous pressure), we did transoesophageal echocardiography and found right ventricular compression in the absence of a pericardial effusion. Repeat chest radiographs (figure, left) showed the existing bilateral lung contusions and a new lucent outline of the heart (arrows). Computed tomography of the chest confirmed the diagnosis of post-traumatic pneumopericardium (figure, right, black arrow), bilateral pneumothoraces (white arrows) and lung contusion. The pneumopericardium resolved after we repositioned the left-sided interthoracic tube.

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