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Do one-shot preventive interventions for PTSD work? A systematic research synthesis of psychological debriefings☆

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Abstract

Psychological debriefings (PDs) have proliferated throughout the trauma and mental health community under the auspice as a one-shot preventive intervention to mitigate acute stress reactions and prevent the development of posttraumatic stress disorder (PTSD). The author presents the results of a systematic research synthesis (SRS) of the available literature on the many variants of PDs. Fifteen empirical studies were systematically analyzed to determine the empirical status of PDs, examine the contention that debriefings prevent PTSD, and investigate the context and with whom PDs demonstrate effectiveness. In toto, PDs lack empirical support. Furthermore, the studies examined had a number of methodological shortcomings, which accent the need for further, well-controlled research of the components of debriefings, experience and training of debriefing providers, and the appropriate context and population prescriptive of PD.

Keywords: Psychological debriefing; Critical incident stress debriefing; Systematic research synthesis; PTSD

1. Introduction

The origins of psychological debriefings (PDs) are often traced to the work of the military historian S.L.A. Marshall who, during World War II, employed a technique labeled "historical

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event reconstruction debriefings'' to obtain, from the fighting forces, the details of battle and describe the reactions of ''soldiers under fire'' (Fitzgerald et al., 1993, p. 159). From Marshall's structured group debriefings and detailed recall of battle grew the belief, among mental health providers, that similar activities, with occupational groups frequently exposed to trauma (i.e., emergency services workers), would mitigate pathogenic stress reactions and prevent posttraumatic stress disorder (PTSD) (Everly, 1995, p. 279). The modern-day version of PD was introduced by Mitchell (1983) as the critical incident stress debriefing (CISD).

Originally designed as a group intervention technology for emergency services workers, CISDs have proliferated throughout the mental health community, in both group and individual forms, under various labels (e.g., critical incident debriefing, traumatic event debriefing, posttrauma interventions, and PD) (Rank, 1997), and across a broad range of contexts for both primary and secondary victims of trauma (e.g., military, disasters, humanitarian aid, motor vehicle accidents, robbery, and miscarriage) (Everly, 1995; Hobbs, Mayou, Harrison, & Worlock, 1996; Lee, Slade, & Lygo, 1996; Rank, 1997). Despite the phenomenal growth of PDs, there exists a dearth of empirical literature examining the claims of debriefing proponents. In addition, recent critical reviews have called for rigorous research of the intervention (Bisson, 1997; Deahl & Bisson, 1995; Raphael, Meldrum, & McFarlane, 1996). Therefore, this paper presents results of a systematic research synthesis (SRS; Rothman, Damron-Rodriguez, & Shenassa, 1994) of the available literature on PDs to provide a conceptual integration and discuss the status of emerging intervention research.

The SRS is a research review methodology that incorporates strengths of both meta-analytic methods and the flexible integrative qualities of traditional literature reviews (Rothman et al., 1994). In brief, SRS methodology involves five primary steps. These include: (a) establish clearly defined literature search parameters (to include language, date, and source limits); (b) identify electronic databases to be searched; (c) classify the results of the search into clearly defined categories; (d) systematically examine the conceptual and empirical literature for conceptual commonalities and empirical rigor; and (e) synthesize the findings into a strategic plan for further examination. In contrast to meta-analytic methodology, the SRS, promotes a conceptual synthesis of both qualitative and quantitative findings rather than only seeking a statistically summative conclusion of a given body of literature. The present study seeks to address several key questions: What is the empirical status of PDs? Do PDs prevent PTSD? In what form (individual or group) should PDs be performed? Lastly, how effective are PDs across differential trauma populations?

2. Review of the literature

To answer these questions, results of the initial literature search were separated into four categories: commentary literature, conceptual/theoretical literature, empirical literature, and technological literature (Klein & Bloom, 1994). Klein and Bloom described the primary purposes of commentary literature as a way to stimulate interest in a given topic, as well as to suggest structured methods for examining the problem. Much of the current literature on the subject of PDs can be categorized as commentary literature. This commentary literature can

be separated into three categories: proponents of the intervention, critics of the intervention, and personal "war stories" with no empirical data.

2.1. Proponents of the intervention

Proponents of debriefings extol its putatively preventive characteristics. Bell (1995) described the power of the intervention as one that can help victims change their perceptions from victim to survivor and "ameliorate their stress symptoms [to] hasten recovery" (p. 42). In his seminal article, Mitchell (1983) described debriefings as having "*enormous potential* [emphasis added] to alleviate overwhelming emotional feelings and potentially dangerous physical symptoms" (p. 39). However, Mitchell and Everly (1995) strongly argue that while PDs are intended as a form of secondary prevention, they are not a substitute for psychotherapy.

Borrowing from the crisis intervention literature, most proponents suggest that the rapid delivery of the intervention during the crisis phase of the traumatic phenomenon also helps to prevent individuals from developing PTSD. Rank (1997) asserted that the amount of time that passes between the event and intervention are critical. Rank argued that delayed treatment results in a distortion of critical details and events that impair recovery from the trauma. Armfield (1994) recommended that due to the delayed nature of PTSD, early secondary preventive interventions (i.e., PDs) are needed to mitigate acute stress reactions thereby preventing the onset of chronic conditions like PTSD.

Finally, the supporters of PDs point to a common thread that "participants liked the debriefing." Deahl, Gillham, Thomas, Searle, and Srinivasan (1994), despite finding no difference in PTSD symptomatology between debriefed and nondebriefed Gulf War body handlers concluded:

> We remain committed to the principle of debriefing. Our clinical experience suggests that many soldiers valued the opportunity to express feelings of anger and guilt and derived comfort from the realisation [sic] that these were normal emotional response to trauma (p. 64).

In addition, in their study of participants of CISDs, Robinson and Mitchell (1993) claimed that they found PD participants rated the positive value of the debriefing concomitantly with the severity of the stressor. They claim that their findings lend further support to the notion that participation in a debriefing is beneficial to the participants.

2.2. Critics of the intervention

Critics of PDs point to its lack of well-tested results in preventing PTSD (Deahl & Bisson, 1995; Gist, Lubin, & Redburn, 1998; Raphael et al., 1996; Rose, Wessely, & Bisson, 2001). Furthermore, some argue that debriefings only serve the desire of the mental health community to help rather than serving to meet the actual needs of survivors of trauma (Gist et al., 1998; Raphael et al., 1996; Stuhlmiller & Dunning, 2000). Finally, there are those who raise the possibility that potential iatrogenic consequences occur from debriefing participation

based upon slight but significant negative postadjustment for those who attend debriefings in comparison to those who did not receive services (Gist et al., 1998).

Inherent in the debate about PDs is the controversy over what constitutes a traumatic or critical incident? Mitchell (1983) described critical incident as, "any unplanned, unexpected or unpleasant situation faced by emergency services personnel that causes them to experience unusually strong emotional reactions and which have the potential to interfere with their ability to function either at the scene or later" (p. 36). Upon careful examination of this definition, one could ascribe any event emergency services personnel respond to as a critical incident worthy of debriefing, since all calls are both unplanned and unexpected situations. On the other hand, Green (1990, p. 1635) proposed eight generic stressor "dimensions" that serve to both set boundaries around a range of events that qualify as traumatic, and promote consistency across research studies. Finally, Morris (2000) delineated between critical incidents (i.e., experienced as part of the job of emergency workers) and traumatic stress (unexpected, disaster-type events) and excluded traumatic incidents that might be expected in everyday life (e.g., bereavement or miscarriage). Current debate abounds concerning what is a trauma and what is a routine facet of life, and a resolution to this question is essential to the fundamental issues addressed in this study, however critical examination of this debate is beyond the scope of this study (Davis, 1999; Summerfield, 2001). Nonetheless, it is clear that inconsistencies in definitional terms obfuscate the context and characteristics of those who are presumed to benefit from PDs. Hence, a better understanding of the conceptual literature from which debriefings were developed may serve to clarify the debate by examining the nature of traumatic stress and how PDs seek to mitigate traumatic stress reactions.

3. Conceptual underpinnings

Gottfredson (1984, as cited in Gist et al., 1998, p. 39) stated, "Good theory not only guides formulations of strategies and structures; it provides for their refinement through generation of theory relevant hypotheses and their testing in the most application relevant of all social laboratories; the field and community." The problem definition, desired outcomes, and interventions common to the variants of PDs are derived from an eclectic admixture of crisis theory, stress theory, group theory, and cognitive–behavioral theories of trauma (Bisson, McFarlane, & Rose, 2000; Busuttil & Busuttil, 1997; Canterbury & Yule, 1999; Dalgleish, 1999). This paper argues that the theoretical underpinnings of the intervention lack clarity and integration, similar to the question of what constitutes a trauma.

3.1. The nature of traumatic stress

The underlying assumption of PDs is that traumatic stress reactions are "normal, expected reactions being experienced by a normal person in response to an abnormally challenging situation" (Mitchell & Everly, 1996, p. 71). Borrowing from the stress and crisis literature, Mitchell and Everly (1996) incorporate biological stress response models hypothesized by Selye (1956), cognitive appraisal models of stress (Folkman & Lazarus,

1980; Lazarus, 1984), and cognitive processing models into their own model of traumatic stress responses. In brief, Mitchell and Everly proposed that excessive stress arousal along with unsuccessful coping strategies to include cognitive–affective integration, results in disease and dysfunction.

Cognitive processing models of trauma also promote a similar assumption. Horowitz (1986) posited that PTSD symptoms of intrusion and avoidance or denial represent an innate "completion tendency" of cognitive operations that are naturally predisposed to merge new trauma-related information with preexisting cognitive structures. Foa and Kozak (1986), building on the earlier work of Lang (1979), proposed a similar theory whereby the experience of a traumatic event results in the formation of a traumatic fear network. Modification of the fear structure involves activation of the fear structure (exposure) and introduction of information incompatible with the existing fear structure. Creamer, Burgess, and Pattison (1992) performed a path analysis that tested the elements of the trauma fear network and found empirical support for the model with interpretive elements of the trauma accounting for 37% of the variance of intrusive symptomatology at 4 months postevent.

Critics of the normal stress reaction model of trauma contend that recent empirical biological findings of PTSD differ from those that would be predicted from the Selye (1956) stress model (Shalev, 2000; Yehuda & McFarlane, 1995). Furthermore, Yehuda and McFarlane (1995) argue that the role of the stressor is not the true etiological factor in the development of PTSD, but rather a combination of premorbid functioning, biological hypersensitivity and posttraumatic factors such as social support and exposure to subsequent reactivating stressors. In addition, critics of Horowitz's model point out that while it amply describes cognitive processes concomitant with stress reactions, it does not indicate why some individuals develop PTSD and others exposed to the same trauma do not (Dalgleish, 1999). Finally, there is evidence that not all individuals experience an initial episode of denial or oscillate between stages of denial and intrusion (Creamer et al., 1992; Dalgleish, 1999).

3.2. PD models

The previously described "normal stress reaction to abnormal events" theoretical framework promotes the notion that psychological interventions can be provided, which aim to stimulate a healthy way of coping, encourage early recognition of disorders, and ultimately prevent the onset of chronic disorders, such as PTSD (Brom & Kleber, 1989). Within this rationale, PDs have burst forth. A review of the different variants of PDs is beyond the scope of this study, nonetheless, each of the models share three common elements (Busuttil & Busuttil, 1997):

1. Cognitive factors: Detailed disclosure of expectations, facts, thoughts, emotional reactions, and sensory impressions of the event or incident.
2. Coping factors: Education about traumatic stress responses, normalization and anticipatory guidance, and future planning.
3. Group support factors (group debriefing models): Reassuring and supportive environment and credible group leader.

Given the controversial theoretical frameworks of traumatic stress and the different variants of debriefings, a systematic analysis of the empirical literature was performed to answer the primary research question: Do PDs prevent PTSD?

4. Methods

4.1. Literature search parameters

The search terms utilized included: (a) MAJOR HEADINGS: PTSD, acute stress disorder; and (b) SUBHEADINGS: Early psychological intervention, prevention, stress debriefing, crisis intervention, PD, CISD, effectiveness, evaluation. The literature search was performed using the following on-line databases: (a) Veterans Administration National Center for PTSD, PILOTS database; (b) PSYCINFO; (c) Sociological Abstracts; (d) Social Sciences Citation Index; and (e) MEDLINE. Date and definitional and source limits were established to maximize the quality of the literature search.

Empirical literature (defined as studies that use standardized research methods) published before the introduction of CISDs by Mitchell (1983) was not included. Empirical literature with a primary population of children or adolescents was not included. Empirical literature employing either group or individual PDs was included. In addition, due to the limited number of empirical studies, both primary victims, as well as emergency response personnel as recipients of the debriefing, were included. Moreover, due to the ambiguous definition of trauma, previously discussed, no limits were placed on the source of trauma. Lastly, empirical literature was limited only to those appearing in peer-reviewed journals. The literature search yielded 15 studies that qualified as empirical research evaluating the effectiveness of PDs. Two of the studies utilized the same sample and outcome measures and are therefore presented as one study (Hobbs et al., 1996; Mayou, Ehlers, & Hobbs, 2000).

4.2. Evaluation criteria

The PD empirical literature was systematically critiqued using the following definitional categories, which are presented in summary form in Table 1:

1. *N* — sample size.
2. *Trauma type and recipient of intervention* — refers to the stated source of trauma that prompted the intervention, and recipients of the debriefing (e.g., motor vehicle accident, crash victims; mass shooting, EMS personnel, etc.).
3. *Design* — presence of a control group, (random assignment) contrast group, (nonrandom assignment) delayed treatment, or no control group.
4. *Intervention type and timing* — type of debriefing (individual or group), timing of the debriefing, use of treatment fidelity measures, and manualized treatment.

Table 1
Overview of empirical studies on psychological debriefing

Study	*n*	Trauma/recipient	Design	Intervention type and timing	Standardized measures	Findings
Bisson et al., 1997	133	Acute burn; Victims	R	Individual and couple (2–19 days)	Yes	Neg
Carlier et al., 2000	243	Multiple traumas; Police	NR	Unknown (24 h, 1 month and 3 months)	Yes	Neg
Carlier et al., 1998	105	Plane crash; Police	NR	Group (unknown)	Yes	None
Chemtob et al., 1997	43	Hurricane; Relief workers	R	Group (6 and 9 months)	Yes	Pos
Conlon et al., 1999	40	MVA, Victims	R	Individual (7 days)	Yes	None
Deahl et al., 2000	106	PK; Military	R	Group (unknown)	Yes	Pos
Deahl et al., 1994	62	Combat; Body handlers	NR	Group (unknown)	Yes	None
Hobbs et al., 1996; Mayou et al., 2000	106/61	MVA; Victims	R	Individual (1–2 days)	Yes	Neg
Jenkins, 1996	36	Mass shooting; EMS and Fire	NR	Group (1 day)	Yes	Pos
Kenardy et al., 1996	195	Earthquake; EMS and Relief	NR	Group (unknown)	Yes	Neg
Lee et al., 1996	39	Miscarriage; Victims	R	Individual (2 weeks)	Yes	Neg
Robinson and Mitchell, 1993	172	Multiple traumas; Hospital and EMS	NC	Group (unknown)	No	Inconcl.
Rose et al., 1999	157	Violent crime; Victims	R	Individual (9–31 days)	Yes	Neg
Shalev et al., 1998	39	Combat; Military	NC	Group (2–3 days)	Yes	Pos

PK = peacekeeping; MVA = motor vehicle accident; R = random assignment; NR = nonrandom assignment; NC = no control group; None = statistically nonsignificant findings on all outcome measures ($P > .05$); Pos = statistically significant ($P < .05$) positive finding for PDs on at least one outcome measure; Neg = statistically significant ($P < .05$) negative finding for PDs on at least one outcome measure; Inconcl. = inconclusive findings due to methodological shortcomings.

5. *Standardized measures* — refers to the type of measure used (standardized or nonstandardized) and use of blind assessors.
6. Percentage of subjects diagnosed with PTSD following intervention.
7. *Findings* — significant findings and confounding variables.

These categories also incorporate the seven criteria for evaluating PTSD treatments proposed by Foa and Meadows (1997), which include: (a) identified target symptoms; (b) valid and reliable measures; (c) use of blind evaluators; (d) assessor training; (e) manualized, replicable, and specific treatment; (f) random assignment to treatment groups; and (g) treatment fidelity/adherence. A narrative critique of the strengths and weaknesses of the

empirical literature by category, which incorporate the aforementioned evaluation criteria, are presented in Section 5.

5. Discussion

5.1. Sample size, trauma type, and recipients of debriefing

Studies of emergency services personnel make up approximately 33% of empirical research (Carlier, Lamberts, Van Uchelen, & Gersons, 1998; Chemtob, Tomas, Law, & Cremniter, 1997; Jenkins, 1996; Kenardy et al., 1996; Robinson & Mitchell, 1993). The sample sizes of the studies reviewed ranged from 36 to 243 (mean = 102.5, S.D. = 65.7, Md = 105). Given the unpredictable nature and magnitude of traumatic events, it is understandable that the dispersion of the samples is sizable. The recipients of debriefings were predominately adults except for the Bisson, Jenkins, Alexander, and Banninster (1997), Conlon and Colleagues (1999), and Hobbs et al. (1996) studies, whose samples included adolescents.

5.2. Design

Half of the studies employed experimental designs with random assignment to treatment condition (Bisson et al., 1997; Chemtob et al., 1997; Conlon, Fahy, & Conroy, 1999; Deahl et al., 2000; Hobbs et al., 1996; Lee et al., 1996; Rose, Brewin, Andrews, & Kirk, 1999). Two studies used treatment only designs (Robinson & Mitchell, 1993; Shalev, Peri, Rogel-Fuchs, Ursano, & Marlowe, 1998). The remainder did not randomly assign study participants into treatment and control groups (Carlier et al., 1998; Carlier, Voerman, & Gersons, 2000; Deahl et al., 1994; Jenkins, 1996; Kenardy et al., 1996).

Study designs that employ nonrandomization assignment strategies attempt to demonstrate that selection bias into the treatment group did not occur by comparing the two groups on demographic or pretreatment outcome measures. However, any findings from nonrandmization studies must be interpreted with extreme caution. For example, Kenardy et al. (1996) found significant group differences between those who received debriefing and those who did not. Self-selected debriefing participants were more often women who provided disaster relief assistance, were employed in professional occupations and had higher educational attainment than those whom did not participate. Using another example, in their most recent study of police officers, the participants in the Carlier et al. (2000) study self-selected into treatment and no treatment groups. Furthermore, they found that those in the treatment group were significantly less experienced police officers with significantly fewer police related traumas. Deahl et al. (1994) stated that naturally occurring group operational constraints (i.e., military duty, official absence, etc.) limited what groups were available for debriefing. Therefore, they claim that participation in the debriefing was not a function of individual selection bias. Shalev et al. (1998) claimed that representativeness was achieved based on the group's proximity and participation in combat operations. Furthermore, the groups had equal success and failure during the combat operations under study. Robinson and Mitchell's (1993) study

suffered from a number of notable methodological flaws. In their study, Robinson and Mitchell gathered data from emergency medical service (EMS) and hospital personnel who participated in CISDs over an 8-month period. Participants were asked to anonymously return questionnaires 2 weeks after the debriefing. Since there was apparently no way of identifying those who returned questionnaires, there was no mechanism to account for multiple responses by the same individual. Therefore, the 60% return rate is potentially misleading. Furthermore, no data were provided to identify the representativeness of the sample in relation to all who attended debriefings.

Selection bias also represented a potential internal validity threat in three of the experimental designs. Bisson et al. (1997) found that those assigned to the treatment group had a higher percentage of burn trauma. Hobbs et al. (1996) and Mayou et al. (2000), in their study of motor vehicle accident victims, also found that those assigned to the treatment group suffered greater severity of physical injuries and were hospitalized for longer periods of time. They found that injury severity and initial scores on the Impact of Events Scale (IES; Horowitz, Wilner, & Alvarez, 1979) were better predictors of postevent IES scores than was treatment condition. Deahl et al. (2000) randomly assigned UN peacekeepers into debriefing and nondebriefing treatment groups. They found that those assigned to the nondebriefing group had significantly higher baseline (i.e., pretreatment) IES and Hospital Anxiety and Depression scores (HADS; Zigmond & Snaith, 1983) than those assigned to the debriefing group. Finally, Lee et al. (1996), who treated women following a miscarriage, found that the percentage of participants with children was significantly higher in the treatment group than those in the control group. Lastly, experimental mortality for all the studies ranged from 0% to 22%. However, there was no apparent discrepancy between study completers and noncompleters in all studies with attrition.

5.3. Intervention type and timing

Group debriefings used the Mitchell (1983) CISD model (or modified version), Dyregrov (1997) PD model, or Historical Group Debriefing model (Shalev et al., 1998). Three of the individual debriefing studies used a variant of Mitchell's (1983) group model, adapted for individuals (Bisson et al., 1997; Lee et al., 1996; Rose et al., 1999). Hobbs et al. (1996, 2000) and Conlon et al. (1999) did not explicate the debriefing model tested in their studies. In addition, Carlier et al. (2000) did not only omit the context of the debriefing (i.e., individual or group PD), the subjects in their study received three debriefings. The first debriefing occurred within 24 h of the incident, the second was performed approximately 1 month after the incident, and the final debriefing was conducted approximately 3 months following the incident. In terms of debriefer experience and qualifications, all but four of the studies used mental health providers to conduct the debriefing. In two of the studies, the credentials of the debriefer were unknown (Carlier et al., 1998; Kenardy et al., 1996). The other two utilized peer debriefers (Carlier et al., 2000; Robinson & Mitchell, 1993).

Only two studies made mention of recorded intervention sessions (Rose et al., 1999; Shalev et al., 1998). However, neither study sought to measure treatment adherence as an intermediate outcome goal (Rosen, Proctor, & Staudt, 1999). Moreover, only one study

(Kenardy et al., 1996) assumed participants participated in a debriefing based on self-report, "[i]t was assumed that all subjects who reported having been debriefed did in fact receive a posttrauma debriefing" (p. 47). In describing negative perceptions of the debriefing intervention, Robinson and Mitchell (1993) invoked ad hoc arguments blaming experience of the debriefer or lack of procedural adherence for negative perceptions despite the lack of evidence that either of these variables were measured by the researchers. The lack of treatment fidelity and measurement of treatment adherence potentially undermines the validity of the outcome claims. Both Foa and Meadows (1997) and Rosen et al. (1999) cite the importance of treatment adherence and manualized treatment to ensure interventions are conducted in a replicable manner and to reduce the potential for confounds within the treatment condition.

5.4. Measures

All of the studies, except for Robinson and Mitchell (1993), used standardized measures to assess PTSD symptomatology. Four used standardized, structured interviews to assess specifically for PTSD diagnosis (Bisson et al., 1997; Carlier et al., 1998, 2000; Conlon et al., 1999). Three of these studies (Bisson et al., 1997; Carlier et al., 1998, 2000) used blind assessors and interviewer training to control for the possibility of instrumentation threats to internal validity. The remainder of the studies (sans Robinson and Mitchell, 1993) used self-report indices of anxiety, intrusion, avoidance, and global measures of distress symptomatology. In sum, there were 13 different outcome measures used, of which only two studies reported psychometric properties of the instrument (Carlier et al., 1998; Chemtob et al., 1997).

Robinson and Mitchell used a nonstandardized (i.e., no known psychometric properties), unpublished outcome measure, which was modified at an unspecified time during the study. Furthermore, a copy of their questionnaire did not accompany their findings. Measurement was conducted over numerous time periods ranging from immediately postintervention to up to 3 years with no discernible pattern. Although the heterogeneous mix of standardized measures identified in Table 1 are widely used in PTSD research, they demonstrate the seeming lack of consensus of definitional terms and conceptual clarity of the theories underlying debriefings.

5.5. Percentage of subjects diagnosed with PTSD following intervention

For an intervention whose process "... is considered one of the most important mechanisms to reduce the potential of PTSD" (Mitchell & Everly, 1996, p. 79), less than half attempted to directly assess for the presence of PTSD posttreatment. Those studies that did assess for PTSD found little evidence for the claim of PTSD prevention. Six of the studies (Bisson et al., 1997; Carlier et al., 1998, 2000; Conlon et al., 1999; Deahl et al., 2000; Rose et al., 1999) specifically measured PTSD using commonly accepted measurement instruments (Keane, Weathers, & Foa, 2000). None of these studies showed a significant positive treatment effect. On the contrary, Bisson et al. (1997) found a significantly higher percentage

of PTSD positive subjects at 13 months posttrauma. The remainder found no significant differences between treatment and no treatment groups on measures for PTSD (Carlier et al., 1998, 2000; Conlon et al., 1999; Deahl et al., 2000; Rose et al., 1999).

5.6. Findings

The data presented strongly suggest that the preventive properties, as well as effectiveness of debriefings, remain at best inconclusive. Of those studies that employed randomization techniques (ostensibly the most rigorous designs), only two (Chemtob et al., 1997; Deahl et al., 2000) detected positive statistically significant differences between PD and non-PD groups on at least one outcome measure. The remainder found either negative treatment effects, with those randomly assigned to the treatment condition performing significantly worse on at least one outcome measure (Bisson et al., 1997; Hobbs et al., 1996; Lee et al., 1996; Mayou et al., 2000; Rose et al., 1999) or no treatment effect (Conlon et al., 1999).

Furthermore, there appears to be no pattern of findings to support the use of debriefings on any one population. Of the studies with emergency services personnel, two found no significant effect for debriefing (Carlier et al., 1998; Jenkins, 1996), one found a significant effect for debriefing (Chemtob et al., 1997) and two were indiscernible (Kenardy et al., 1996; Robinson & Mitchell, 1993). In sum, there is no substantive support to employ debriefings with any one-trauma population, let alone the range of populations who are currently recipients of debriefings.

The diverse models and the varied contexts of debriefings further complicate any claims for the positive effects of PDs. While group debriefings (of various models) were predominant in the literature, less than half found a significant positive effect for treatment on at least one outcome measure (Chemtob et al., 1997; Deahl et al., 2000; Jenkins, 1996; Shalev et al., 1998). The remainder demonstrated a significant negative effect for treatment (Carlier et al., 1998; Kenardy et al., 1996), or no effect for treatment (Deahl et al., 1994). Individual debriefings also failed to demonstrate a statistically significant positive treatment effect. Two studies found a significant negative effect for treatment on at least one outcome measure (Bisson et al., 1997; Hobbs et al., 1996; Mayou et al., 2000), and three no treatment effect (Carlier et al., 2000; Lee et al., 1996; Rose et al., 1999; Conlon and Colleagues (1999)).

6. Conclusions and recommendations for further study

The SRS (Rothman et al., 1994) provided a framework from which to systematically analyze the available literature on treatment interventions. Nonetheless, there are limitations of SRSs, which require acknowledgement. The analysis of empirical literature did not include a discussion of the appropriateness of the statistical analyses conducted by the researchers, as it was assumed researchers met the basic assumptions requisite of the analytic technique applied. From a methodological standpoint, this synthesis relied upon available electronic (on-line) databases to identify the primary quantitative research on debriefings. The search

was limited to those studies that appeared in the databases. Dissertations and conference proceedings were not included in the quantitative research synthesis. Therefore, the study represents only a sample of the available empirical literature on debriefings for adults. Nevertheless, this study appears to have captured a significant majority of empirical literature on PDs in comparison to recent meta-analyses (Everly, Boyle, & Lating, 1999, $N=9$) and research reviews (Rose & Bisson, 1998, $N=6$).

While there is scant empirical evidence supporting the claims of debriefing proponents (Chemtob et al., 1997; Jenkins, 1996; Kenardy et al., 1996; Shalev et al., 1998), those studies that found negative treatment results were also hampered by possible confounding variables (Bisson, 1997; Hobbs et al., 1996; Mayou et al., 2000), which can be attributed to some of the challenges of conducting research with trauma victims identified by Robinson and Mitchell (1993). These challenges include lack of baseline data, the unpredictable nature of traumatic events, access to trauma populations, and the immediate needs of trauma survivors taking precedence over research interests. However, despite these challenges, critical inquiry into treatment methods for those exposed to human tragedy should not be abandoned. Therefore, several recommendations are made.

In addition to further rigorous randomized controlled trials in both group and individual contexts, the generic components of PDs should also be critically tested to identify which, if any, components contribute to positive posttrauma adaptation and reduction in posttrauma stress symptoms. Borkovec (1993) argued that dismantling research designs, whereby different components of the treatment are tested independently, are optimal in efficacy studies since they provide a way to control for nonspecific factors, and provide more certainty about the mechanisms of action behind a therapeutic intervention. These studies should also entail a no treatment control group as has been done with the randomized controlled trials of individual PDs (e.g., see Bisson et al., 1997).

Inherent to conducting dismantling research is the imperative to manualize PDs and incorporate fidelity measures into the design of a study. The growing number of proprietary organizations disseminating their debriefing model(s) to uncritical acolytes is evidenced by the number of times the media concludes a community disaster story with "And counselors are at the scene." Without independent testing of the numerous debriefing protocols, there is little chance of gaining clarity on the mechanisms allegedly involved in debriefings. Furthermore, that it took the creator of CISDs nearly 10 years to publish his first efficacy study of the intervention may suggest a trend in the field of trauma necessitating further study. Those who stand to profit most from the unanticipated tragedies of mankind must be held to the highest standards of scientific rigor.

Examination of the debriefing models creates the imperative to study the training and supervision of debriefing providers. For example, there is no evidence that formal CISD basic or advance training offered by the International Critical Incident Stress Foundation (2001) affects treatment outcome. While Morris (2000) identified the need for standardized education and accreditation, there exists no mechanism to actually evaluate the benefits of training. Without a critical analysis of the numerous programs propagating "trauma specialists," survivors of acutely traumatic events are prey to legions of professionals and paraprofessionals who hold pseudo-scientific titles (e.g., "certified traumatologist," "certified

master traumatologist'' [The Traumatology Institute, 2001], ''board certified expert in traumatic stress,'' or ''board certified in emergency crisis response'') (American Academy of Experts in Traumatic Stress, 2001).

Further research should be conducted to examine the appropriate context and population to deliver PDs, if any. The widespread application of debriefings in military contexts, hospitals, emergency services, disasters, individual traumas, and criminal victimization (e.g., see Wilson & Raphael, 2000) illuminates the need to examine with whom (victims, direct service providers, witnesses), in what context (individual/group), and for which general categories of trauma (Green, 1990) do PDs promote adaptation. Unless and until nonspecific and characteristic features of PDs are subject to rigorous empirical research, the utility of PDs will not only be suspect by the scientific community, but also prone to suspicion and disavowal by the communities they serve to enhance.

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