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# The Effectiveness of Treatments for Trauma: A Quantitative Review

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THE EFFECTIVENESS OF TREATMENTS FOR TRAUMA: A QUANTITATIVE
REVIEW

by

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# ABSTRACT

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This quantitative review examined the published research on the effectiveness of treatments for trauma. The review included 31 studies that compared treatment for trauma with a wait-list or placebo control group and provided information about the time between the traumatic event and when therapy occurred. Findings from the review confirm there is a beneficial effect of therapy and suggest that gains made in therapy are maintained and increase after therapy ends. Cognitive-behavioral therapies appeared to be better than other treatments. Analysis also revealed that treatment was less effective, and sometimes harmful, for patients undergoing psychological debriefings. The differences between treatments did not depend on either how patients were recruited or when patients received therapy.

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CHAPTER 1

INTRODUCTION

Posttraumatic stress disorder is a greatly distressing and potentially disabling anxiety disorder that can occur following a traumatic event in which a person experiences either actual or threatened physical harm to themselves or another person (American Psychiatric Association, 2000). Many of those suffering from trauma-related symptoms report reexperiencing of the trauma through having unwanted thoughts or images of the event (Nixon et al., 2008; Speckens, Ehlers, Hackmann, Ruths, & Clark, 2007), dreams about the event (Mellman, Daniella, Bustamante, Torres, & Fins, 2001), flashbacks where the individual feels as though it is happening again (Bremner & Brett, 1997), and distress and physiological reactivity to cues related to the trauma (Wolfe et al., 2000). Those suffering may also try to avoid thoughts, people, or places that are associated with the trauma or by being unable to remember certain aspects of the trauma (Weems, Saltzman, Reiss, & Carrion, 2003). Many experience numbing symptoms in the form of feeling detached, lack of interest in activities, having limited affect, or sense of not being around for much longer (Weems et al., 2003). Other arousal problems such as increased startle response (Griffin, 2008), attention and concentration problems (Leskin & White, 2007), and hypervigilance (Stewart & White, 2008) can cause additional distress. Those who develop trauma-related symptoms are also at further risk for other disorders such as depression, drug and alcohol abuse, and other anxiety disorders (Miller, Fogler, Wolf, Kaloupek, & Keane, 2008; Schillaci et al., 2009; Thabet, Abed, & Vostanis, 2004).

According to the DSM–IV–TR, approximately 8% of the general population will develop posttraumatic stress disorder at some point in their lifetime (APA, 2000). Certain populations have an increased risk due to the nature of their location or work. For example, a study by Hoge et al. (2004) found that 11–17% of those returning from military operations in Iraq or Afghanistan screened positive for posttraumatic stress

disorder and Ichikawa, Nakahara, and Wakai (2006) found a prevalence of 44–46% in a sample of middle–eastern refugees.

For a person to be diagnosed with posttraumatic stress disorder, an individual must have been experiencing these symptoms for at least 1 month (APA, 2000). Interventions have been developed and therapies modified to be delivered as soon as possible after a traumatic event in the hopes of never having the distressing or debilitating symptoms of posttraumatic stress disorder ever appear or to greatly reduce their risk or magnitude. Examples include Brief Cognitive Behavioral Therapy, Assessment Conditioning, and Supportive Counseling (Foa, Zoellner, & Feeny, 2006). These interventions are generally aimed at providing help regardless of the presence of symptomotology and usually occur within a few days to a few weeks of the incident. Some of these are interventions may be mandated or patients may feel pressured to undergo treatment. Data on some of these interventions is conflicting which has led to controversy over their use.

One such intervention is Critical Incident Stress Debriefing (CISD). CISD was developed in 1983 by Mitchell (Mitchell, 1983) to meet the needs of first responders, such as the Red Cross, to provide psychological aid for those who have recently suffered a trauma. Originally developed as a six–step group plan to be administered within the first 72 hours of a traumatic event to reduce or stop the development of trauma-related symptoms, CISD has been used frequently since its inception (Oster & Doyle, 2000). In its original version there were four key components: (a) individual or group on–scene crisis intervention, (b) small group discussion, (c) the six–step group discussion, (d) and follow–up support (Everly, Flannery, Eyler, & Mitchell, 2001). Later (1999) Everly and Mitchell revised CISD into Critical Incident Stress Management (CISM). These authors argue that CISM differs in that it has a seven–stage group plan with ten key components. However, there are researchers who claim that there is not enough research to verify the two procedures as wholly the same or different (Devilly, Gist, & Cotton, 2006). CISM

was partly developed due to CISD being referred to and used only as the six or seven–step group discussion in other studies and out in the field (Everly et al., 2001).

Although originally used as the term for the group discussion, Psychological Debriefing (PD) has been used as another name for CISD/CISM (Litz, Gray, Bryant, & Adler, 2002; Rose & Bisson, 1998). PD has also been defined as a separate entity from CISD/CISM, with some researchers defining CISD/CISM based solely off of the whole process and steps of the program as outlined by Everly and Mitchell (Everly et al., 2001) and PD being defined as any intervention that occurs within three days of the traumatic event with emphasis on catharsis and education about symptoms that could develop (Devilly & Cotton, 2003).

The conflicting outcomes from this type of early intervention are where research has shown them as positive, negative, or nonexistent. Research has found either no effect or in some instances a negative effect for development of trauma-related symptoms when using CISD/CISM or PD (Emmerik, Kamphuis, Hulsbosch, & Emmelkamp, 2002; Mayou, Ehlers, & Hobbs, 2000; Rose, Bisson, & Wessely, 2003; Stallard et al., 2006) and research has also found a reduction in development of trauma-related symptoms when using these same interventions (Alexander & Wells, 1991; Bohl, 1991; Jenkins, 1996; Nurmi, 1999; Yule, Udwin, & Murdoch, 1990).

Other reviews have assessed more traditional forms of psychotherapy for trauma which occur after longer periods of time since the trauma. Of those reviews, most focus on either cognitive-behavioral therapies (Falsetti, 2000) or eye movement desensitization and reprocessing (MacCluskie, 1998). Research on these two forms of psychotherapy have shown that they generally provide better outcomes than other forms of therapy for trauma and that the two of them are comparable in treating trauma (Siegler & Wagner, 2006; Bisson et al, 2007). However, a more recent review by Benish, Imel, and Wampold (2008) suggests that all forms of treatment for trauma are equally effective.

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Although there is no doubt that there are benefits to psychotherapy for distressed individuals (e.g., Smith, Glass, & Miller, 1980), this does not mean that psychotherapists should operate under the assumption that psychotherapy must be given for those who could potentially become distressed, especially if research has shown the potential for adverse effects from this. Given that in normal populations, the lifetime prevalence rate for potentially traumatic events is low (Hepp et al., 2006), as is the lifetime prevalence of developing posttraumatic stress disorder (Kessler, Berglund, Demler, Jin, & Walters, 2005) this would seem to indicate that resilience for traumatic events is rather high. For example, Bonanno, Galea, Bucciarelli, and Vlahov (2007), found in a group of over 2700 patients that over 50% were considered resilient. It may be better for the individual to wait until symptoms appear and seek out help on their own instead of having it given to them when it is not needed or may cause negative effects.

Although some will argue that specific trauma interventions, such as those previously mentioned, are not psychotherapy (Everly, 1995), it is difficult to see how this is so if one uses the definition of psychotherapy as given by Metltzoff and Kornreich (1970):

> Psychotherapy is taken to mean the informed and planful application of techniques derived from established psychological principles by persons qualified through training and experience to understand these principles and to apply these techniques with the intention of assisting individuals to modify such personal characteristics as feelings, values, attitudes, and behaviors which are judged by the therapist to be maladaptive or maladjustive. (p. 6)

The primary purpose of this study is to assess if these early interventions or any better or worse than more traditional therapy. A wide range of time, from the day of the event to years later, will be used so as to assess if early interventions are any better, worse, or the same as waiting for the patient to seek therapy on their own later on.

Furthermore, because some early interventions can be mandated, such as for those in law enforcement or emergency responders, it would also be helpful to look at the relationship between being mandated to therapy for trauma and seeking it out voluntarily. Research on addiction disorders has shown it to be more helpful in reducing use and relapse for women who were mandated than those who voluntarily entered drug rehabilitation (Clark & Young, 2009).

Because women are more likely than men to have trauma-related symptoms following a traumatic event (Tolin & Foa, 2008) and some researchers have reported differences in symptomatology (Zlotnick, Zimmerman, Wolfsdorf, & Mattia, 2001) between men and women, it is possible that there may be differences in treatment between men and women. Another question is whether early interventions have a different impact for males and females.

Most early interventions are done in one session, and, in fact, the modal number of sessions for all psychotherapy is one session (Hoyt, 2003). Yet, Anderson and Lambert (2001), found that, in general, 11 sessions of psychotherapy are needed for 50% of the patients to reach what was considered clinically significant change. Does the number of sessions have an effect on outcome?

Additionally, previous research has suggested that therapist outcomes were comparable despite therapist training (Durlak, 1979; Hattie, Sharpley, & Rogers, 1984; Berman & Norton, 1985) while more recent studies have suggested that training is beneficial (Crits-Cristoph, 1991; Lyons & Woods 1991; Stein & Lambert, 1995). Therefore another aim of the review is to assess whether there is an effect for therapist training level.

Finally, the study will examine the relationship between year the study was published and the effect size for outcome. That is, have treatments for trauma become more effective as time goes on?

CHAPTER 2

METHOD

*Studies*

The analyses are based on 31 studies of PTSD interventions and psychotherapy. Study characteristics are reported in Table 1. There were 5 sexual trauma studies, 7 motor vehicle trauma studies, and 18 studies that listed various types of trauma. There were 8 studies that could be identified as psychological debriefings, 10 that did not match any category, and 13 that were cognitive behavioral therapies. The study publication dates range in time from 1991–2008. The studies were searched through PsycARTICLES and PsycINFO, by using such search terms including *trauma intervention, trauma outcomes, PTSD outcomes, PTSD prevention,* and *control*. Also, studies that were found were reviewed so as to find other studies cited but that did not turn up in the initial search.

Studies were excluded if no definite time since a particular trauma could be determined. Time ranges were excluded. For example, in some combat trauma studies the researcher might study an individual unit of armed forces. The researcher might list the amount of time of the tour of duty and give a list of some traumas experiences but because no definite amount of time since a specific trauma and when therapy was received could be determined, the study would be dropped from analysis. Studies were also excluded if there was not a control condition.

Outcomes were measured in the studies using a variety of both self–report and clinician administered scales for trauma symptomotology, such as the Impact of Events Scale and the Clinician Administered PTSD Scales.

*Coding of Treatment Outcomes*

Each outcome measure was reported via Cohen's *d*, a measure of effect size. It was calculated by subtracting the mean of one group from the mean of another and dividing

that number by the pooled standard deviation. All studies reported means and standard deviations, and effect size was calculated this way.

Table 1

*Characteristics of Psychotherapy Studies for Trauma*

| Study characteristic | *N* of Studies | *M* | Range |
|---|---|---|---|
| Patient age in years | 22 | 33.6 | 15–40.1 |
| Years since trauma | 31 | 1.9 | 0.003–10.7 |
| Proportion female patients | 24 | 0.6 | 0.18–1 |
| Number of sessions | 29 | 5.3 | 1–16 |
| Proportion recruited patients | 26 | 0.5 | 0–1 |
| Proportion professional therapists | 22 | 0.9 | 0–1 |

*Coding of Study Characteristics*

The most important criterion for inclusion in the analysis was the recording of time after trauma that the intervention or psychotherapy began. The diagnosis of posttraumatic stress involves symptoms that persist for at least 1 month. Therefore, studies were dichotomized into those based on patients receiving treatment within 1 month of the traumatic event (prediagnosis studies) and those in which treatment occurred after that point (postdiagnosis studies). For this study, 1 month was defined as 30 days.

In some studies the patient was recruited while in a hospital after an accident and would be approached by a doctor or nurse about the study. The patient might have felt that they had to join the study because their healthcare provider was asking them to. In

another study, after a convenience store robbery, patients are sent to the study by their employer. In these two examples, the patient is experiencing more pressure to join the study than someone who is recruited or referred by ads or by the researcher after the patient has left the hospital. Any recruitment that may induce excess pressure on the patient to join is being defined as enhanced recruitment whereas all other recruitment types will be referred to as regular recruitment.

Studies were initially classified into the type of treatment being assessed: cognitive-behavioral therapies, eye movement desensitization and reprocessing, debriefings, or other. Cognitive-behavioral therapies included those studies in which the active treatment could be classified as cognitive, behavioral, or both. Eye movement desensitization and reprocessing was strictly for eye movement desensitization and reprocessing. Debriefing included those studies that were classified as psychological debriefing, critical incident stress debriefing, and educational/emotional debriefing. Other consisted primarily of therapies invented by the author that did not fall into the other three categories. The eye movement desensitization and reprocessing and other categories did not have enough studies to be included in comparative analysis. Some studies used multiple types of active treatments compared to controls. For these studies, the final effect size was the average of the two active treatments. Studies that used this method were excluded from comparing categories of treatment because the full effect of one active treatment type could not be clearly shown.

Therapist training was coded in terms in whether the therapist was a professional or a paraprofessional. A paraprofessional for this study was a person with no graduate level education or training in psychology or psychiatry but who performed the role of therapist.

The number of sessions was a continuous variable. This number is the average number of sessions for a treatment group.

Control conditions were either wait-list or assessment conditions in which the subject received no form of therapy and was repeatedly assessed or placebo control in which the subject received common factors of psychotherapy but did not have any active ingredients of specific forms of psychotherapy. For this study, supportive counseling was the most common type of placebo control.

CHAPTER 3

RESULTS

Studies differed in the type of control group with which the active treatment was compared. One group of studies used a wait-list control condition, in which control patients received no treatment during the period of the study. A second group used a placebo control condition, in which control patients interacted with a therapist but no formal type of therapy was given. And a third group of studies used both wait-list and placebo types of control.

The top part of Table 2 shows the means and standard deviations of effect sizes indicating how much better or worse active treatment was compared to control for the three classes of studies. The mean effect sizes for the three groups did not differ reliably, $F(2, 30) = 0.80$, $p = .5$. However, it was surprising that the mean effect size for studies having only a placebo control was numerically higher than the mean observed for studies having only a wait-list control. Normally, one would expect that the effect of treatment compared to a placebo control, in which control patients receive some facets of therapy, would be smaller than the effect of treatment compared to a wait-list control, involving no form of therapy at all for the control patients.

One possible reason for the numerically larger mean effect size in studies using placebo controls might be a difference in the type of treatments investigated in those studies. In particular, all eight (100%) of the studies using only a placebo control were assessing a cognitive-behavioral therapy as the active treatment, whereas only 6 of the 14 studies (30%) using just a wait-list control examined cognitive-behavioral therapies, $\chi2(1, N = 31) = 15.49$, $p = .004$.

The middle section of Table 2 presents studies that involved cognitive-behavioral therapies. As before, the mean effect sizes for the three groups did not differ reliably, $F(2, 16) = 0.17$, $p = .9$. Moreover, the effect of treatment compared with placebo controls

was numerically smaller than for treatments compared with the wait-list controls, as would normally be expected.

Table 2

*Efficacy of Treatments for Posttraumatic Stress for Studies with Wait-list, Placebo, or Both Types of Control Groups*

| | | Effect Size | |
| Type of control | *N* of studies | *M* | *SD* |
| --- | --- | --- | --- |
| All Studies | | | |
| Wait-list | 20 | 0.33* | 0.69 |
| Both | 3 | 0.60 | 0.47 |
| Placebo | 8 | 0.65* | 0.43 |
| Only studies of cognitive-behavioral therapies | | | |
| Wait-list | 6 | 0.67* | 0.72 |
| Both | 3 | 0.60 | 0.47 |
| Placebo | 8 | 0.65* | 0.43 |
| Only studies with both control types | | | |
| Wait-list | 3 | 0.65* | 0.72 |
| Placebo | 3 | 0.55 | 0.23 |

*Note.* Asterisks indicate that the mean effect size differed reliably from zero ($p < .05$).

A more direct way of assessing comparisons of wait-list control and placebo control is to analyze the three studies that had both wait-list control and placebo control types. This method is more reliable because wait-list and placebo groups are being compared

with the same measures of outcome and under the same study procedures, which is not the case in the previous comparisons. When looking only at these three studies, as shown at the bottom of Table 2, comparison of treatments to wait-list controls and to placebo controls did not differ reliably, $t(2) = 0.34$, $p = .7$.

Given that the results of the comparisons of treatments with placebo controls and wait-list control were comparable after adjusting for differences between studies, all further analysis used the results of the wait-list and placebo studies combined. If a study had both types of controls, the effect size from those two types of comparisons were averaged together.

Table 3

*Efficacy of Active Treatments Based on Comparisons to Control at Pretreatment, Posttreatment, and Follow-up Assessments*

|  |  | Effect Size | |
| --- | --- | --- | --- |
| Assessment | *N* of studies | *M* | *SD* |
| Pretreatment | 31 | -0.04 | 0.23 |
| Posttreatment | 31 | 0.44* | 0.61 |
| Follow-up | 24 | 0.59* | 0.69 |

*Note.* Asterisks indicate that the mean effect size differed reliably from zero ($p < .05$).

Table 3 presents the mean effect sizes for comparison between active treatment and control at pretreatment, posttreatment, and follow-up. As can be seen, at posttreatment the active group was slightly more than four-tenths of a standard deviation higher than

the control group and at the follow-up assessment active treatment was better by almost six-tenths of a standard deviation than the control group.

As shown in Table 3, the mean effect size for comparisons of treatment to control increased from time point to time point. Analysis indicated, in fact, that this difference over time was statistically significant both from pretreatment to posttreatment $t(30) = 4.00$, $p < .001$, and from posttreatment to final assessment, $t(23) = 4.16$, $p < .001$. Therefore, improvement from treatment not only occurred during the treatment period but there were continued gains after therapy had ended.

Table 4

*Efficacy of Treatments of Patients Receiving Therapy Within 1 Month of the Traumatic Event (Prediagnosis) or Later (Postdiagnosis)*

| | | Effect Size | |
| | N of | | |
| Diagnostic status | studies | M | SD |
| --- | --- | --- | --- |
| Prediagnosis | 17 | 0.18 | 0.47 |
| Postdiagnosis | 14 | 0.76* | 0.63 |

*Note.* Asterisks indicate that the mean effect size differed reliably from zero ($p < .05$).

The diagnosis of posttraumatic stress involves symptoms that persist for at least 1 month. Therefore, studies were dichotomized into those based on patients receiving treatment within 1 month of the traumatic event (prediagnosis studies) and those in which treatment occurred after that point (postdiagnosis studies). As shown at the top of Table 4, the mean effect of treatments of prediagnosis patients was substantially smaller than the effect for treatments of postdiagnosis patients, $t(29) = 2.94$, $p = .006$. The number of

sessions was correlated with when therapy was received: The number of sessions for prediagnosis therapies was less ($M = 2.65$, $SD = 1.85$) than for postdiagnosis ($M = 8.54$, $SD = 3.78$), $t(27) = 5.48$, $p < .001$. When controlling for number of sessions, there was still a difference between prediagnosis ($M = 0.13$, $SD = 0.17$) and postdiagnosis ($M = 0.77$, $SD = 0.19$) studies, $F(1, 28) = 5.17$, $p = .01$.

Recruitment strategy was defined as being either enhanced-recruitment, in which the patient was intensely sought to enroll in the study, or regular-recruitment, in which the patient was directed to a source for help or information. Recruitment strategy did have an impact on outcome in that the effect sizes of studies using enhanced-recruitment patients ($M = -0.07$, $SD = 0.48$) were almost half of a standard deviation lower at posttreatment assessment compared to studies with regular-recruitment patients ($M = 0.47$, $SD = 0.50$), $t(24) = 2.21$, $p = .04$. The effect for recruitment strategy did not vary depending on whether treatment began within a month of the trauma or later, interaction $F(1, 22) = 1.11$, $p = .3$.

Studies were divided into three categories based on the type of trauma treated: sexual trauma, motor vehicle trauma, and mixed trauma type. The overall means and standard deviations for type of trauma can be seen in Table 5. Overall, the effect of treatment did not vary reliably by type of trauma, $F(2, 27) = 2.32$, $p = .1$. However, the effect size for motor vehicle accidents was numerically lower than the other types of trauma.

Table 5

*Efficacy of Treatments for Different Types of Trauma*

| Type of Trauma | *N* of studies | Effect Size | |
| --- | --- | --- | --- |
| | | *M* | *SD* |
| Sexual | 5 | 0.89* | 0.85 |
| Motor vehicle | 7 | 0.15 | 0.41 |
| Various | 18 | 0.46* | 0.62 |

*Note.* Asterisks indicate that the mean effect size differed reliably from zero ($p < .05$).

Some studies had patients who were diagnosed with acute stress disorder while others had patients with posttraumatic stress. However, there was no reliable difference in effect size at posttreatment for studies involving patients with acute stress disorder ($M = 0.61$, $SD = 0.47$) and those involving posttraumatic stress disorder ($M = 0.40$, $SD = 0.65$), $t(29) = 0.7$, $p = .5$.

Another issue is whether the effectiveness of treatments in different studies varied because these studies had patients with differing levels of distress. For example, if patients were not very distressed, they would have less room for improvement during treatment. However, analysis indicated that treatment effects did not vary across studies as a function of the initial distress of the patient sample for either the average of outcomes, $r = .19$, $p = .30$, measures of anxiety, $r = .20$, $p = .37$, or measures of trauma, $r = .06$, $p = .76$.

Table 6

*Efficacy of Different Types of Therapy for Posttraumatic Stress*

| Type of therapy | N of studies | Effect Size | |
| --- | --- | --- | --- |
| | | *M* | *SD* |
| Overall | | | |
| Cognitive-behavioral therapy | 13 | 0.76* | 0.53 |
| Debriefing | 8 | -0.14 | 0.26 |
| All others | 10 | 0.50* | 0.62 |
| CBT | | | |
| Cognitive-behavioral therapy | 13 | 0.76* | 0.53 |
| NonCBT | 13 | 0.20 | 0.65 |
| Debriefing | | | |
| Debriefing | 8 | -0.14 | 0.26 |
| Nondebriefing | 23 | 0.64* | 0.57 |

*Note.* Asterisks indicate that the mean effect size differed reliably from zero ($p < .05$).

Studies were divided in terms of the type of treatment being assessed: cognitive-behavioral therapies, psychological debriefings, and other treatments. The overall means and standard deviations of the therapy types can be seen at the top of Table 6. There was a difference between the three categories where debriefing differed from cognitive-behavioral therapies and all others, $F(2, 30) = 7.68$, $p = 0.002$. Those studies that could be identified as using cognitive-behavioral therapy did differ from those studies that did not use cognitive-behavioral therapy, $t(25) = 2.45$, $p = .02$, seen in the middle part of

table 6. Those studies that used cognitive-behavioral therapies performed better by a little over half a standard deviation than those studies not employing cognitive-behavioral therapies. As shown in the bottom part of Table 6, looking at the mean score for effect size, those patients who participated in a debriefing study were almost nine-tenths of a standard deviation below those patients not in a debriefing study, $t(30) = 3.70$, $p = .001$.

Did the difference between treatments depend on whether therapy was received within 30 days of the trauma, which is before there is a formal diagnosis of posttraumatic stress? For cognitive-behavioral therapies the difference did not depend on when therapy was received, interaction $F(1, 23) = 1.42$, $p = .3$. No debriefing studies occurred after diagnosis, so it was not possible to assess the difference before 30 days and after 30 days.

Table 7

*Efficacy of Treatments for Studies with Professional and Paraprofessional Therapists*

| Type of training | *N* of studies | Effect Size | |
| --- | --- | --- | --- |
| | | *M* | *SD* |
| All studies | | | |
| Professional | 20 | 0.51* | 0.19 |
| Paraprofessional | 2 | -0.36 | 0.59 |
| Debriefing studies | | | |
| Professional | 3 | -0.36* | 0.33 |
| Paraprofessional | 2 | -0.13 | 0.19 |

*Note.* Asterisks indicate that the mean effect size differed reliably from zero ($p < .05$).

Another question was whether differences between treatments depend on how the patients were recruited. Differences between cognitive-behavioral therapies and others did not depend on how patients were recruited, interaction $F(1, 23) = .57$, $p = .5$. The same held true for debriefing studies in that the difference with other therapies did not rely on how patients were recruited, interaction $F(1, 27) = .04$, $p = .7$.

Therapist training was categorized by those either receiving or who had received professional training in a graduate program or paraprofessionals. Looking at the top part of Table 7, professionally trained therapists performed better by over one standard deviation compared to paraprofessional therapists, $t(21) = 2.04$, $p = .05$. However, the two paraprofessional therapist studies were also debriefing studies. When looking within only debriefing studies, there is no longer a statistically significant difference between therapies conducted by professionals and paraprofessionals, $t(3) = 0.87$, $p = .5$, as shown at the bottom of Table 7.

The number of sessions a patient received was related to outcome: Studies in which patients received only one session of treatment fared worse than those studies in which patients received more than one session of treatment, $t(29) = 3.09$, $p = .004$.

Previous research has suggested that there is a difference in manifestation of symptoms for trauma based on sex. However, the percentage of female patients was not correlated with the outcome for treatment, $r = .13$, $p = .5$. Also, the year the study was published was not significantly correlated with effect size at posttreatment, $r = -.01$, $p = .9$.

Throughout the studies, the most common types of measures used could be classified as either specifically trauma measures, general anxiety measures, or depression measures. Shown in Table 8 are the means and standard deviations for the different types of measures. Additionally, some of these were self-report and others were clinician administered. Table 9 shows the effect sizes for how the measures were administered.

Table 8

*Efficacy of Treatments for Specific Types of Measures*

| | *N* of | Effect Size | |
| Type of measure | studies | *M* | *SD* |
| --- | --- | --- | --- |
| Anxiety | 22 | 0.39* | 0.49 |
| Depression | 26 | 0.43* | 0.53 |
| Trauma | 30 | 0.54* | 0.70 |

*Note.* Asterisks indicate that the mean effect size differed reliably from zero ($p < .05$).

Table 9

*Efficacy of Treatments for Different Sources of Measures*

| | *N* of | Effect Size | |
| Source of measure | studies | *M* | *SD* |
| --- | --- | --- | --- |
| Patient | 14 | 0.63* | 0.56 |
| Clinician | 30 | 0.45* | 0.63 |

*Note.* Asterisks indicate that the mean effect size differed reliably from zero ($p < .05$).

CHAPTER 4

DISCUSSION

As has been previously found, and as analysis demonstrates, psychological interventions are effective (e.g., Smith et al., 1980). This study indicated similar general effectiveness for the treatment of trauma. In studies comparing therapy to wait-list and placebo controls there was a positive effect of therapies for trauma. At the end of treatment, patients were functioning over four-tenths of a standard deviation better than controls. Furthermore, for studies that reported follow-up assessments, there was an increase over a tenth of a standard deviation from posttreatment. This would indicate that not only does therapy work but for the studies reviewed there were continued gains afterwards.

Diagnosis of posttraumatic stress disorder was found to have an effect for treatment. Those patients who received treatment before the 1 month diagnosis performed worse at the end of treatment than those who received treatment after the 1 month diagnosis time criteria. One explanation might be that waiting for symptoms to develop instead of trying to eliminate them from forming might be more effective for  patients. Another explanation could be that the majority of prediagnosis studies were debriefing studies, which did not to perform as well as other types of treatments. Along with that, a significant number of the postdiagnosis studies were cognitive-behavioral studies which performed better than other forms of treatment.

Analysis revealed that studies with patients who were recruited in a pressured capacity soon after a trauma improved less from treatment than those who were recruited by less intense means. One explanation for this might be that pressuring someone into a therapy instead of having that person seek it out on their own could be harmful. Indeed, for the most part those in the pressured category were worse at the conclusion of treatment than before they began. Another possibility is that of the five studies identified

as pressured four were psychological debriefing studies. Analysis showed debriefing treatments to have performed worse than other categories. In fact, the one study assessing a cognitive-behavioral treatment in this category did show improvement of about two-tenths of a standard deviation while on average patients in the debriefing studies became worse during treatment.

Analysis compared the effectiveness of different types of therapies, with the most frequent being cognitive-behavioral therapies and psychological debriefings. In addition to being numerically lower, debriefings also had a negative effect size and differed reliably from the cognitive-behavioral and all others groups. Cognitive-behavioral therapies performed better by over half a standard deviation than treatments that were not cognitive-behavioral. This is similar to the findings of van Etten and Taylor (1998), who found that cognitive-behavioral therapies are generally more effective for trauma than other types of therapies. Debriefings performed worse by over three-fourths of a standard deviation compared to nondebriefing studies. This is consistent with previous findings that psychological debriefings may be harmful or have no effect (Emmerik et al., 2002; Mayou et al., 2000; Rose et al., 2003; Stallard et al., 2006).

These differences could not be explained by how soon after a trauma therapy was received. Certain treatments were more likely to occur at different points in time. For example, no debriefing studies occurred after 30 days and very few cognitive-behavioral studies occurred before 30 days. However, analysis indicated that differences between treatments did not depend on when therapy was received.

In addition, these differences could not be explained by the way in which patients were recruited. Half of the debriefing studies used enhanced recruiting methods, whereas only one cognitive-behavioral study did. Again, analysis demonstrated that differences between the treatments did not depend on method of recruitment.

At first glance, treatments involving therapists who were professionally trained tended to have better outcomes than treatments using paraprofessional therapists. However, the number of studies involving paraprofessionals was very small, making it difficult to assess the function of training. The paraprofessional studies were also debriefing studies. When looking at only debriefing studies, treatment by professionals did not differ reliably from treatment by paraprofessionals. This finding is more in line with past research that shows that paraprofessional training is comparable to professional training (Berman & Norton, 1985; Bright, Baker, & Niemeyer, 1999).

Male and female clients did not differ in outcome. The finding of this review did not support the findings of some studies (Tolin & Foa, 2008; Zlotnick et al., 2001) that found differences between how men and women responded to treatments for trauma.

The number of sessions a patient received was related to the effectiveness of treatment. Those patients who received treatment that involved more than one session had higher outcomes than those who received treatment that consisted of only one session. There were 8 studies that only had one session of treatment and of those 8, 7 were debriefing studies. It is possible that receiving only one session of treatment leads to worse outcomes. However, it might also be that because the one session was a debriefing treatment, and debriefing treatments had worse outcomes, that is why one session treatments performed worse than treatments with more sessions. There were no cognitive-behavioral treatments with only one session.

The year that the study was published was not related to treatment outcome. Of the evaluated studies, those that were published 20 years ago were just as effective as studies published more recently. This finding would suggest that therapy for trauma has not become any better or worse as time progresses.

There was a positive effect of treatment for measures of anxiety, depression, and trauma; with treatment effects largest for measures of trauma. There was also a positive

effect for both measures of patient self-report and those rated by clinician. Measures that were self-report had numerically higher outcomes than measures that were clinician administered. It could be that for self-report measures patients overestimate how well they are doing, whereas with clinician-administered measures the therapist has a better sense of how the patient is doing.

The major finding of this study is that patients in debriefing treatments appear to perform worse than patients receiving other types of therapy, particularly cognitive-behavioral therapies. The performance of both types of treatment could not be found to rely on how early treatment was received or how patients were recruited into the study. Initially, the idea of examining recruitment was to assess whether the effectiveness of treatment would be affected if patients were required to receive treatment for a trauma. Unfortunately, no studies could be found in which such mandated therapy occurred and so the issue could not be analyzed in this review. Future research may want to evaluate the efficacy of therapy for trauma when treatment is mandated.

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APPENDIX

*Studies analyzed with type of intervention, recruitment method, and effect size*

| Study | N | Type of treatment | Recruitment | $d$ |
|---|---|---|---|---|
| Gersons et al. (2000) | 42 | Other | Regular | -.07 |
| Marchand et al. (2006) | 75 | PD | Enhanced | -.46 |
| Mayou et al. (2003) | 61 | PD | Enhanced | -.50 |
| Sijbrandij et al. (2006) | 236 | PD | Regular | -.12 |
| Stallard et al. (2006) | 158 | PD | Regular | .07 |
| Wagner et al. (2007) | 8 | CBT | Enhanced | .65 |
| Bisson et al. (1997) | 133 | PD | Enhanced | -.23 |
| Hobbs et al. (1996) | 104 | PD | Regular | -.21 |
| Bryant et al. (2003) | 24 | CBT | Regular | .82 |
| Bryant et al. (2008) | 90 | CBT | Regular | .49 |
| Ehlers et al. (2005) | 28 | CBT | Regular | 1.55 |
| Ehlers et al. (2003) | 85 | CBT | Regular | .02 |
| Foa et al. (1991) | 45 | CBT | Regular | .57 |
| Conlon et al. (1998) | 40 | Other | Regular | .10 |
| Bisson et al. (2004) | 152 | PD | Regular | -.12 |
| Brom et al. (1993) | 151 | Other | Regular | .47 |
| Foa et al. (2006) | 90 | CBT | Regular | .14 |
| van Emmerik et al. (2008) | 125 | CBT | Regular | .54 |
| Rose et al. (1999) | 157 | PD | Regular | .19 |
| Lee et al. (1996) | 39 | PD | Enhanced | .16 |
| Knaevelsrud & Maercker (2007) | 96 | CBT | Regular | .47 |
| Blanchard et al. (2003) | 78 | CBT | Regular | 1.08 |

*Studies analyzed with type of intervention, recruitment method, and effect size*
(*continued*)

| Study | N | Type of treatment | Recruitment | *d* |
|---|---|---|---|---|
| Resick et al. (2002) | 121 | CBT | Regular | 1.52 |
| Lange et al. (2003) | 101 | Other | Regular | .87 |
| Bryant et al. (1998) | 24 | CBT | Regular | 1.09 |
| Bryant et al. (1999) | 45 | CBT | Regular | .94 |
| Bryant et al. (2005) | 87 | CBT | Regular | .67 |
| Bryant et al. (2003) | 48 | CBT | Regular | 1.00 |
| Marks et al. (1998) | 87 | CBT | Regular | .00 |
| Taylor et al. (2003) | 60 | CBT, Other | Regular | -.01 |
| Rothbaum (1997) | 21 | Other | Regular | 2.04 |

*Note: CBT = cognitive-behavioral therapy, PD = psychological debriefings,*

*Other = any therapy not cbt or pd.*