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A Meta-Analysis of Component Treatment Studies: Are the Parts as Good as the Whole?

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A META-ANALYSIS OF COMPONENT TREATMENT STUDIES:

ARE THE PARTS AS GOOD AS THE WHOLE?

by

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ABSTRACT

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Psychotherapy has been proven effective in treating a variety of mental health issues. However, there is disagreement in psychotherapy research about whether or not factors common to all psychotherapies or specific ingredients within a treatment package are responsible for successful treatment outcomes. Component studies are research designs specifically aimed at identifying the mechanisms of change in a full treatment package. Component studies do this by comparing the differences in outcome among dismantled treatment components and the full treatment protocol. The present meta-analytic review of component studies examined whether or not differences between these two treatment groups generally exist. Fifty-nine component studies were analyzed to determine the difference in the outcome between dismantled treatments and full treatment packages. Studies were coded on a number of variables including sample size, intervention type, treatment problem, gender, and age. As hypothesized, results from 59 comparisons of post-treatment score differences revealed that there are not differences between the full treatment package and the component treatment (d = -.005). These results mirrored the findings from Ahn and Wampold's (2001) original meta-analysis of 20 component studies. However, there was significant heterogeneity among these studies, with treatment effectiveness moderated by age (i.e., older clients benefiting more from

the full treatment packages; β = -.316). The present study also examined treatment outcomes at follow-up. The follow-up results from 44 comparisons of component groups and full treatment package groups indicated that the full treatment package was more effective than the component group (*d* = -.157). The results from this study suggest that specific ingredients in psychotherapy packages do have an effect, but these effects may not be apparent at post treatment, and, instead, will appear in follow-up outcomes.

DEDICATION

To my father, William Joel Clarke, who always told me that he was proud and gave me the freedom and encouragement to chase my dreams. You are truly missed.

To my friend, Earl William Bell, who has supported me throughout the past two years. I could not have done this without you.

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CHAPTER I

INTRODUCTION

Psychotherapy has been proven effective in treating a variety of mental health issues. However, it is still unclear how psychotherapy works. Component studies are designed to help researchers identify the active ingredients that are responsible for the benefits yielded by a full treatment protocol (Ahn & Wampold, 2001; Borkovec, 1990). A component design involves splitting the therapeutic elements comprising a treatment and delivering them in separate components of therapy. This design can be delivered either to different groups or to the same participants in sequence to evaluate the effectiveness of specific active ingredients (Longmore & Worrell, 2007). The aim of this project is to determine whether component studies generally find differences in outcome among dismantled treatment components and the full treatment protocol. This issue was addressed in an earlier meta-analysis by Ahn and Wampold (2001), but the present study provides a more comprehensive, updated meta-analysis.

The psychotherapy research field has moved from debating whether psychotherapy is effective in the treatment of psychological disorders (e.g., Eysenck, 1952) to debating how and why psychotherapy is effective. Although, meta-analytic investigations (e.g., Smith & Glass, 1977; Smith, Glass, & Miller, 1980) have shown that most therapeutic practices are generally effective for treating a wide range of clinical disorders (Task Force on Evidence-Based Practice in Psychology, 2006), there is still disagreement among researchers about whether client improvement is due to specific ingredients of treatments (e.g., Eysenck, 1994; Giles, Neims, & Prial, 1993; Strupp, 1986) or to factors common in all therapies (e.g., Ahn & Wampold, 2001; Frank & Frank,

1991; Garfield, 1998; Task Force on Evidence-Based Practice in Psychology, 2006; Wampold, 2001). Advocates for empirically supported treatments (ESTs) believe that specificity (i.e., attributing to specific ingredients) is key to the therapeutic process. The EST movement supports a process that begins with formulating theories to explain disorders, problems, or complaints; designing treatments that contain specific ingredients that are based on the theories formulated; and finally testing the treatments through randomized controlled trials (RCTs). In an RCT design, participants are randomly assigned either to an experimental treatment, a standard practice comparison, or a control condition. The control condition consists of either no treatment or a placebo treatment. At the end of the study, the outcomes of the experimental treatment group are compared to the control condition outcomes or the standard practice comparison to determine if the experimental treatment worked better (Sibbald & Roland, 1998). If the treatment is supported through RCTs, then the benefits of psychotherapy are attributed to the specific ingredients that were originally posited (Castelnuovo, Faccio, Molinari, Nardon, & Salvini, 2004; Wampold, 2001).

Critics of this approach believe that common factors (i.e., healing context, working alliance, belief in rationale for treatment and in treatment itself) are responsible for treatment benefits. Advocates of the common factor approach emphasize the commonalities among therapies. Wampold (2001) wrote:

> All therapies involve the relationship of a client and therapist, each of whom believes in the efficacy of the treatment. The therapist provides the client with a rationale for the disorder and administers a procedure that is consistent with that rationale. The client discusses the most intimate details of his or her life,

> confident that the therapeutic relationship will continue. The particular specific ingredients contained in the treatment, according to the contextual model, are not responsible for therapeutic benefits. (p. xii)

Critics of the specific ingredients approach, often point to the "Dodo Bird Verdict" to support their claims that common factors are the causal proponents of change in psychotherapy. The "Dodo Bird Verdict" derived from Rosenzweig's (1936) seminal survey when he concluded that there were "some implicit common factors in diverse methods of psychotherapy" that were so pervasive that the differences between types of therapy treatment would be very small (p. 412). He summarized this assertion by quoting *Alice in Wonderland*, "everybody has won, so all shall have prizes," which was the Dodo bird's verdict after judging the race (Luborsky et al., 2002, p. 1). Rosenzweig's theory that common factors are the key to psychotherapy success has gained support from meta-analyses of comparative studies (Luborsky et al., 2002; Luborsky & Singer, 1975; Wampold et al., 1997) in which one type of psychotherapy is compared to a different type of psychotherapy to see which one is more effective in treating clients. For example, a comparative study could compare cognitive behavioral therapy to interpersonal therapy for the treatment of depression. In general, these meta-analyses found that although psychotherapy was more effective than no therapy, the type of psychotherapy did not make a significant difference on treatment outcome measures.

In 1995, the APA Division 12 (Clinical Psychology) Task Force on Promotion and Dissemination of Psychological Procedures published criteria for identifying empirically validated treatments (eventually renamed empirically supported treatments) for particular disorders (Evidence-Based Practice in Psychology, 2006). The Division 12

Task Force described this publication as an effort to promote treatments delivered by psychologists, because there was a perception in the health field that psychological treatments were ineffective or inferior to pharmacological treatment (Evidence-Based Practice in Psychology, 2006). The criteria used by the Division 12 Task Force sparked controversy after they published a report identifying 18 treatments whose empirical support they considered to be well established. The criteria for being a well-established treatment included a treatment having been tested in RCTs with a specific population and implemented using a treatment manual (Evidence-Based Practice in Psychology, 2006).

Some psychologists argued that conducting research using RCTs and trying to identify specific active ingredients may harm research and the practice of psychotherapy because it underemphasizes the common variables that may be the agents for effectiveness in therapy. Garfield (1998) argued that although we should conduct research and the efforts of the Division 12 Task Force should be applauded, the scientific method being used is biased toward manualized treatments that dominate the field. Garfield (1998) went on to say that this approach is an "attempt to certify and mandate the use of such therapies prematurely… and by emphasizing the name or form of psychotherapy, essentially to minimize the importance of patient variability and therapist skill" (p. 121).

Although advocates of both sides want to learn about the active ingredients that lead to positive change in the therapeutic process, the proponents of specific factors believe that these specific factors or techniques may be responsible for the therapeutic successes of psychotherapy. It is understandable why some psychologists and researchers would not like the view that common factors and not specific factors are

responsible for psychotherapy success. As stated by Frank (1971), " little glory derives from showing that the particular method which one has mastered with so much effort may be indistinguishable from other methods in its effects" (p. 350).

Understanding the *active ingredients* in psychotherapy appears to be a goal that all psychotherapy researchers value. Component studies have become the *gold standard* for trying to accomplish this goal (Ahn & Wampold, 2001). The present meta-analysis of component studies examined whether differences are generally found between dismantled components and the full treatment protocol. If the meta-analysis were to reveal that there are no differences between components and the full treatment protocol, then there may be theoretical implications to consider. Specifically, results that show that there are no differences between components and the full treatment protocol may provide evidence in favor of common factors being responsible for change more than specific factors. Also, such findings may suggest that the research design currently utilized in most component studies is not as useful as believed.

History of Component Designs

Psychotherapy research is conducted using several research designs that have evolved over time. Psychotherapy designs have included comparing treatment to no-treatment conditions, comparing treatment conditions to placebo conditions, or comparing treatment conditions to alternative treatments. Establishing specific causal factors has been a struggle because, although most psychotherapy designs can draw a cause-and-effect conclusion, it remains unclear what specific causal factors within the therapy are responsible for the change (Borkovec & Castonguay, 1998).

In the 1960s, systematic desensitization became a widely accepted treatment for a variety of anxiety disorders. Even though there was a large amount of empirical support behind the use of systematic desensitization, it was unclear which of the different variables composing the treatment was causing the behavioral change. The developer of systematic desensitization, Wolpe, theorized that the change in behavior was due to an underlying neural process called "counter-conditioning" or "reciprocal inhibition" that required all components of the treatment to be effective (Davison, 1968, p. 92). The research supporting systematic desensitization used comparative designs comparing systematic desensitization to alternative therapy, placebo therapy, or no treatment. Although there was support that the treatment worked better than no treatment/placebo or alternate conditions, there was not clear evidence that counter-conditioning was the process causing change. In a review of laboratory studies, Lang (1969) suggested a method for analyzing desensitization. He wrote:

> Perhaps the most prevalent approach involves a sequential dismantling of the basic treatment unit. This approach is rather like that of a curious aborigine who hopes to understand a modern automobile. Clinical reports tell him that it runs. He has even taken it out for a spin. But he does not understand what makes it go. His plan is to start pulling things off it (perhaps starting with the shiny hood ornament) until it stops, hoping that he will come to know what parts are critical to its functioning- and that the owner will not mind too much the mess he has made of things. (pp. 161-162)

Davison (1968) was the first to test Wolpe's theory of how systematic desensitization worked using a dismantling approach. Davison asserted that until tested it

was possible that the favorable outcomes could be due to relaxation alone, to the gradual exposure to aversive stimuli, or to nonspecific relationship factors. He developed the first component study by "dismantling" systematic desensitization into different treatment groups composed of the different components of therapy and comparing them. Davison recruited 28 female volunteers from a junior college who reported being very afraid of nonpoisonous snakes. The study divided the women into four treatment groups. The first group received the whole desensitization package. The second group called pseudo-desensitization received relaxation but no exposure. The third group received an exposure treatment without relaxation, and the fourth group was not given any treatment. Davison found a significant difference between the groups that received the full treatment package and the groups that only received part of the package. The finding that the full treatment package worked better than any of the parts alone provided evidence for Wolpe's theory of counter-conditioning. Follow-up studies (e.g., Miller & Nawas, 1970; Nawas, Welsh, & Fishman, 1970) used the same dismantling design as Davison's (1968) study and provided evidence that relaxation was not an important component to systematic desensitization in the treatment of snake phobia. Furthermore, in 1975, Marks reviewed all of the empirical literature and concluded that it was the graded exposure in systematic desensitization that was the key element in treating phobias (as cited in Tryon, 2005).

This new dismantling design, termed a component study, provided a way to test theory and provide evidence for active ingredients in treatment packages. Component studies can be conducted by using either a dismantling design or an additive design (e.g., Ahn & Wampold, 2001; Borkovec, 1990; Borkovec & Miranda, 1999). As demonstrated

by the Davison (1968) study, dismantling studies are designed to help researchers identify the active ingredients that are responsible for the benefits yielded by a full treatment protocol (e.g., Ahn & Wampold, 2001; Borkovec & Miranda, 1999). A dismantling design involves splitting the therapeutic elements comprising a treatment and delivering them in separate components of therapy. If the treatment component without the "specific active ingredient" results in a less successful outcome than the complete treatment package, then the researcher can conclude that there is evidence that the active ingredient is key to the effectiveness of the treatment (e.g., Ahn & Wampold, 2001; Borkovec & Castonguay, 1998; Borkovec & Miranda, 1999). Additive designs add a specific ingredient to a treatment to see if this ingredient increases the effectiveness of a particular treatment. The idea behind the additive design is that each component in psychotherapy is "partially effective" so that the combination of these partially effective components will lead to a more powerful treatment than any component alone (e.g., Ahn & Wampold, 2001; Borkovec, 1990; Borkovec & Miranda, 1999).

Through these two types of designs, component studies can provide evidence of specificity. In an additive design, evidence of specificity is provided when the added component increases the overall effectiveness of the full treatment package. In a dismantling design, evidence of specificity is provided when dismantling one component reduces the effectiveness of the full treatment. Advocates of these designs argue that component designs are superior to designs in which two treatment packages are compared, because they provide specific conclusions about causal factors, and they provide greater control over many extraneous variables that hinder and confound interpretations of outcome (Borkovec & Miranda, 1999). Comparative designs can

provide evidence that a particular therapy caused a degree of change beyond the amount of change caused by factors common to all therapy and beyond the alternative therapy (Borkovec & Castonguay, 1998).

In addition to component studies being used to provide evidence for active ingredients, they also have been used to debunk certain components of a treatment. Most notably, component studies have been used to examine the eye movement component within Eye Movement Desensitization and Reprocessing (EMDR) therapy. Because many mainstream psychotherapy researchers have suspected that the eye movement component of EMDR is grounded in pseudo-science (e.g., Herbert, 2000), several component studies have been conducted to demonstrate that the eye movement component is not an active ingredient in treatment.

Controversy over the interpretation of component studies began after Jacobson et al. (1996) used a dismantling design to examine the active ingredients of cognitive-behavioral treatment for depression. Cognitive-behavioral therapy includes several interventions that address observable behavior, dysfunctional automatic thoughts, and inferred underlying cognitive structures or schemas. Cognitive-behavioral therapy is given in a sequence that starts with changing overt behavior (i.e., behavioral activation), then moves to teaching the client to assess and correct situation-specific distortions in thinking (i.e., modification of automatic thoughts), and finally teaching the client to identify and modify negative core beliefs (i.e., schema therapy; Jacobson et al., 1996). Jacobson and colleagues (1996) randomly assigned 150 outpatients with major depression to groups of Behavioral Activation (BA) alone, Behavioral Activation plus modification of automatic thoughts (AT), or the full Cognitive Therapy (CT) package.

Jacobson et al. (1996) found no differences between the treatment groups. They interpreted this finding as evidence that BA is the essential effective ingredient of CT.

Rehm (2009) disagreed with this logic, asserting that dismantling studies generally do not find differences between the dismantled components and the full treatment. Rehm (2009) suggested that the lack of differences could be due to methodological problems with the dismantling design. Specifically, Rehm (2009) suggested two possible reasons for the lack of differences. First, given that most studies have multiple, complex components, it is possible that clients may respond to the same component in different ways or may respond to different components in the same way. Second, Rehm stated that it might be difficult to find differences in components if they are presented sequentially. For example, Rabin, Kaslow, and Rehm (1984) compared three versions of self-control therapy for depression on 104 women who met criteria for a major affective disorder. The researchers examined the patterns of change for 13 symptoms of depression. They found that most symptoms diminished in the first 3 to 4 weeks of therapy, and several symptoms (e.g., sadness and suicidal ideation) showed major reductions prior to the first session. Rabin et al. (1984) found that components presented at the beginning of therapy led to greater reduction in symptoms of depression than those presented later in therapy. If components presented earlier in therapy contribute more to treatment outcome than those presented later, then components presented earlier may be deemed the active ingredient due to order effects rather than superiority over other components. In contrast, if the study is one of a direct comparison, results may be more likely to indicate that both treatments were effective. If component studies consistently fail to reject the null hypothesis (i.e., fail to find significant differences between treatment conditions), then

the use of component studies in general needs to be questioned as a method of evaluating treatment components. Further, conclusions by researchers about treatment components based on component studies showing no treatment differences would also need to be made with extreme caution.

Component Studies Reviews

To date, only one meta-analytic study (Ahn & Wampold, 2001) has examined differences between treatment components and the full protocol in dismantling and additive studies. Ahn and Wampold (2001) conducted a meta-analysis on 20 components studies published in *Behaviour Research and Therapy*, *Behavior Therapy*, *Journal of Consulting and Clinical Psychology*, and *Journal of Counseling Psychology* from the years of 1990 to 1999. Ahn and Wampold (2001) used these four journals after they referenced a meta-analysis of comparative studies conducted by Shapiro and Shapiro (1982) and found that most outcome research came from these journals. The researchers concluded that including all component studies published in these four journals would be a comprehensive search of the literature. After coding the various component studies, Ahn and Wampold (2001) used an aggregate of the effect sizes of 27 comparisons by weighting each study's estimate of the effect size by the inverse of the variance to yield the aggregate effect size estimate. Ahn and Wampold's 27 comparisons came from 20 studies; therefore, in some cases the same full treatment was compared to more than one dismantled treatment. This method violates the independence assumption. For example in the Jacobson et al. (1996) study, the full treatment package (Behavioral Activation and Automatic Thoughts) was compared to two different component treatments (Behavioral Activation or Automatic Thoughts). Instead of considering the Jacobson et al. study as

one study and averaging the Cohen's *d*s from each component, Ahn and Wampold considered each comparison as an independent study, despite the fact that the full treatment package in each comparison was the same group of participants. Using this aggregation strategy, Ahn and Wampold (2001) failed to find significant differences between treatment components and the full protocol (i.e., d_+ estimate of -0.20). Ahn and Wampold also failed to find evidence of heterogeneity among the effect sizes. This finding of homogeneity suggests that there were no important variables moderating the effect sizes. Ahn and Wampold (2001) concluded from this finding, "the benefits of treatments are probably due to the pathways common to all bona fide psychological treatments" (p. 255).

Longmore and Worrell (2007) conducted a literature review of 13 component analysis studies that examined cognitive behavior therapy (CBT). Specifically, they were interested in whether specific cognitive interventions increased the effectiveness of therapy. In their review, Longmore and Worrell found little evidence to support the idea that specific cognitive interventions significantly increase the effectiveness of therapy. In fact, "the review showed that, almost without exception, component studies found no difference in effectiveness between the cognitive and behavioral elements of CBT" (p. 184). Consistent with Ahn and Wampold's (2001) position, Longmore and Worrell (2007) suggested that common, nonspecific therapy factors, rather than the active ingredients, may be responsible for why results generally show little or no differences between conditions in treatment component studies. The Jacobson et al. (1996) study was the only study included in both Ahn and Wampold's (2001) meta-analysis and Longmore and Worrell's (2007) review of component studies, which means that there are at least 12

relevant component studies just on CBT that were not included in the Ahn and Wampold meta-analysis. A review of the EMDR literature by Herbert et al. (2000) provided a list of 12 EMDR component studies. Only one of these studies was included in Ahn and Wampold's meta-analysis, which means there are at least 11 relevant EMDR component studies that were not included in Ahn and Wampold's original analysis. The purpose of most of these component studies was to prove that eye movement is not an active ingredient in EMDR. Therefore, the inclusion of these additional EMDR studies could dilute findings and, although the EMDR component studies should be included, they also should be coded for analysis as a possible moderator.

Present Study

Given the presumption that component studies are a *gold standard* of experimental design in psychotherapy research (e.g., Ahn & Wampold, 2001; Wampold, 2001), it is important to examine the validity of this belief empirically. Component study designs may not provide researchers with as much information as generally thought. As mentioned earlier, Ahn and Wampold (2001) are the only researchers who have conducted a meta-analysis of this topic, and they failed to find significant differences between treatment components and the full protocol. Although the findings were noteworthy, the research literature has expanded in the past 10 years. Thus, the present review is an appropriate update and extension of the work by Ahn and Wampold (2001). The primary aim of this project was to determine whether component studies generally find differences in outcome among dismantled treatment components and the full treatment protocol. Given that the previous meta-analysis failed to find significant differences between the components and the full treatment package and that

psychotherapy research in general has failed to find significant differences between psychotherapies, it was hypothesized that there would be very small differences between treatment components and the full treatment protocol. The present study provides a more comprehensive, updated meta-analysis since the last study conducted by Ahn and Wampold (2001). They used 20 studies from four journals (*Behavioral Research and Therapy, Behavior Therapy, Journal of Consulting and Counseling Psychology,* and *Journal of Counseling Psychology*) published between 1990 and 1999. The present study included studies from a wider range of sources and included literature that has been published since the meta-analysis by Ahn and Wampold (2001). In summary, the present study evaluated the following hypotheses:

1. There are generally no differences between the outcomes in treatment studies comparing a dismantled component to the full treatment package as indicated by an effect size near zero.
2. Results from the studies will be homogeneous.
3. If heterogeneity is found it may be moderated by treatment type, specifically EMDR versus non-EMDR.

In addition, a planned exploratory analysis was conducted to determine if a difference in effect sizes for follow-up outcome data was found between treatment studies comparing a dismantled component to the full treatment package. Because this was an exploratory analysis, no *a priori* hypothesis was made.

CHAPTER II

METHOD

Compilation of Studies

There were no original participants in this study, as it is a meta-analysis of existing literature. All the studies included in the Ahn and Wampold's (2001) meta-analysis were included in the present study. Furthermore, the four journals identified in their meta-analysis were searched by hand, via a review of each table of contents, for any relevant articles published between the end of their review (1999) and 2010. Ten out of twelve studies from Longmore and Worrell's (2007) literature review on component studies also were included in the present meta-analysis. One study was excluded because it provided two-year follow-up data of a study already included in this study and the other study examine did not use a component design to examine the active ingredients. Because a number of studies have examined whether eye movement is an essential component of Eye Movement Desensitization and Reprocessing (EMDR), component studies that met criteria from the Herbert et al. (2000) literature review of EMDR were also included in the meta-analysis. Finally, a search for relevant literature was conducted with PsychINFO and Medline. The search terms originally planned in the proposal had to be adjusted and limited due to very large search returns. Therefore, the combination of the search terms "dismantling" and "treatment" was used. These combined searches yielded 142 possible references. Any studies that met inclusion criteria and were not already captured by the journal searches were included in the meta-analysis.

The study used the same criteria for identifying relevant component studies as Ahn and Wampold's (2001) study (see Appendix A for details). In addition to the study

criteria listed by Ahn and Wampold in Appendix A, studies must have been written in English, produced codable data, and included a measure of treatment effect for each component in the dismantling studies. Single case designs were excluded. Studies were not excluded if they did not specify a Master's level therapist (as referenced in Appendix A); rather, studies were included as long as the therapy was provided by a graduate level student or a therapist supervised by a professional in the field.

Fifty-eight studies met the inclusion criteria and were used in the meta-analysis. One of the articles (Nicholas et al., 1991) counted as two studies because it reported two different component studies within the one article. This one article by Nicholas et al. (1991) had two different full treatment packages that were compared to two different dismantled components. Thus the meta-analysis included 59 studies that included 3,213 children, adolescents, and adults. All studies included in the meta-analysis are listed in Table 1.

Procedure

Once included, the studies were coded on a variety of dimensions including sample size, study design, sample type, method of measuring outcome, problem type (e.g., depression, anxiety), intervention type (e.g., cognitive-behavioral therapy, interpersonal therapy), components, client age, client gender, location of therapy (i.e., inpatient, outpatient), and year of publication (see Appendix B).

For each DV, a sample effect size was obtained by calculating the difference in the means of the two conditions and dividing by the pooled standard deviation (more-component-group *M*- fewer-component-group *M*)/SD. Typically, higher scores on outcome measures (dependent variables; DVs) indicate greater distress or pathology;

however, on occasion a higher score would indicate something positive (e.g., self-esteem). In these cases, the scoring was reversed so that a higher score represented greater impairment (e.g., lower self-esteem) to maintain consistent direction of change across studies. Therefore, a negative effect size indicates that the full treatment was superior to the dismantled treatment. If a study provided more than one DV, the DVs were averaged, and the average was used instead of multiple DVs. If, however, a study provided some DVs specific to the problem and some DVs that were more general, only the DVs that were specific to the problem being treated were included in the average. In the situation in which a study only reported the composite results from several outcome measures, the composite was the value included in the meta-analysis. Effect sizes were weighted by the inverse variance so that studies with larger samples had greater weight. To determine whether effect sizes were from a single population, a *Q* test of homogeneity was calculated. If *Q* was statistically significant, the homogeneity hypothesis would be rejected. In addition to the Q test of homogeneity, I^2 (Higgons, Thompson, Deeks, & Altman, 2003) was calculated. I^2 describes the amount of variance attributable to heterogeneity across the studies. An I^2 value of 0 would indicate absolute homogeneity among the studies, whereas 100 would indicate complete heterogeneity. When the effect sizes were found to be heterogeneous, potential moderators—including client type, type of intervention (specifically whether the treatment was an EMDR dismantling study), gender, year of publication, and age—were examined. Hedges' (1982) meta-analytic equivalent to a one-way ANOVA was used to examine categorical moderators (e.g., client type), and Hedges and Olkin's (1985) meta-analytic equivalent to multiple regression was used to examine continuous moderators (e.g., age).

The choice of which type of model to use in a meta-analysis depends on the inferences the researcher wants to make. If the researcher wants to make inferences only about the studies included, a fixed effects model would be appropriate. However, the present meta-analysis aimed to infer beyond the included studies and generalize to the population of component studies. Therefore, a random effects model was the most appropriate choice (Field, 2003). That is, random effects models assume that the studies included in the meta-analysis do not constitute all the studies that could exist (Field, 2003). Additionally, a random effects model allows for sampling errors by including both between-study and within-study variance in the error term. In contrast, a fixed effects model includes within-study variance and "assume all other 'unknowns' in the model are constant" (Field, 2003, p. 107). All analyses were conducted using Lipsey and Wilson's (2001) SPSS statistical programs.

CHAPTER III

RESULTS

Is the Full Treatment Superior to the Dismantled Treatment at Completion?

The first hypothesis posited that dismantled components would not differ from the full treatment package in treatment outcomes. This hypothesis was confirmed. In studies where different components were removed and each variation was compared to the full treatment, the effect sizes were calculated for each variation compared to the full package and then averaged together to obtain the study's average component *d,* so that each study yielded a single *d*. This strategy was used to avoid violating the assumption of independence. The average *d* across all 59 studies was -.005, which was trivial and not statistically significant (95% *CI* = -.09 - .08; *Z* = -.102, *p* = .91).

The second hypothesis (i.e., that there would be no heterogeneity among the studies) was not supported. Indeed, there was considerable heterogeneity among these studies, *Q* (58) = 77.4717, *p* = .045, I^2 = 25.1, suggesting that they do not all derive from the same population. Because of the significant heterogeneity across the studies, further analyses examined potential moderators that may explain some of the variability.

The analyses were also conducted using the same method that Ahn and Wampold (2001) used in their meta-analysis. Ahn and Wampold did not average component comparisons within a treatment study (for an average *d*) and instead treated each component comparison variation as a different study outcome, violating the assumption of independence. There were 76 comparisons within the 59 studies. An analysis using Ahn and Wampold's method of calculating *d* did not change the results. Specifically, the average *d* across all 76 comparisons was -.025, which also was trivial and not statistically

significant (95% *CI* = -.11 - .06; *Z* = -.566, *p* = .574). Consistent with the previous analysis, there was considerable heterogeneity among these studies, *Q* (75) = 111.6903, *p* = .0039, I^2 = 32.85, suggesting that the studies do not all derive from the same population.

A final effect size was computed by selecting the *d* that most favored the full treatment against whichever variation had the poorest outcome. Although this approach risked overstating the superiority of the full treatment, by possibly capitalizing on chance, it was the most rigorous test of the first hypothesis (i.e., that there would be no difference between the full treatment and a dismantled treatment). The averaged *d* across the 59 studies was -.039, which was still trivial and not statistically significant (95% *CI* = -.13 - .06; *Z* = -.802, *p* = .423). Again, there was considerable heterogeneity among these studies, *Q* (58) = 84.3616, *p* = .0135, I^2 = 31.24, suggesting that they do not all derive from the same population. Overall, across all analyses, there was no evidence that the full treatment was superior to a dismantled treatment at treatment completion.

Can the Variations across Study Outcomes be Explained?

Several demographic variables were explored as potential moderators. The analyses of moderators were conducted using the data from the first method of analyses (i.e., the *d*s were calculated for each variation compared to the full package and then averaged together to obtain the study's average component *d,* so that each study yielded a single *d*). The role of client type (whether the participant was a client or a volunteer), gender, and age were all assessed as potential moderators. Client type (*k* = 59) did not account for the variability across studies, Q_B (1) = .236, *p* = .628; Q_W (57) = 63.465, *p* = .259. The percent of male participants (*k* = 57) was assessed as a potential moderator, but gender

did not moderate the difference between the full treatment package and the component packages (β = .038; *B* = .001 [*SE* = .0016]; *Z* = .291, *p* = .771). When average age of the sample (*k* = 50) was treated as a continuous moderator, the meta-analytic analogue to a regression yielded a significant effect for age (β = -.319; *B* = -.008 [*SE* = .0034]; *Z* = -2.419, *p* = .016): The full treatment was more likely to yield superior outcomes in those studies in which the clients were older.

Because age was found to significantly moderate the outcome differences between the component group and the full treatment package at the end of the treatment, a scatter plot graph was used to visually assess for any outliers that may have influenced these findings. One possible outlier (Nezu & Perri, 1989) was identified: This study's *z*-score was the only *z*-score that was more than three standard deviations from the mean. Therefore, an additional analysis was conducted excluding this study. Even when the outlier was excluded from the analysis, the regression remained significant, (β = -.296; *B* = -.007 [*SE* = .0034]; *Z* = -2.042, *p* = .0411). Since age was not found as a moderator in the original study by Ahn and Wampold (2001), an independent sample's *t*-test was used to compare average age in the two meta-analyses. The studies from the Ahn and Wampold meta-analysis had a mean of 34.92 (*SD* = 11.46) while the studies added for the current meta-analysis had a mean of 33.73 (*SD* = 9.76). No significant differences were found between the Ahn and Wampold (2001) meta-analysis (*t* = .393, *p* = .696) and the current meta-analysis for the average ages of clients participating in the studies included.

Year of study publication (*k* = 59) was examined as a possible moderator. The studies included in the meta-analysis were published between 1989 and 2010. The year

of publication was not a significant moderator, (β = .0058; *B* = .0003 [*SE* = .007]; *Z* = .046, *p* = .96).

As mentioned earlier, there was a concern that the EMDR debunking studies may have diluted any full treatment package superiority by washing out differences between full treatment packages and component treatments. In order to assess whether EMDR studies could have moderated treatment outcomes, the studies were coded categorically for treatment type. Specifically, studies were assigned a number to categorize them as an EMDR treatment study versus a non-EMDR treatment study. Treatment type did not moderate the results, $Q_B(1) = .088$, $p = .767$; $Q_W(57) = 62.26$, $p = .295$. Both the EMDR studies' findings, *d* (10) = .0308 (95% *CI* = -.21 - .27; *Z* = .257, *p* = .80) and the non-EMDR studies' findings *d* (49) = -.0080 (95% *CI* = -.10 - .09; *Z* = -.17, *p* = .87), indicated no difference between the full treatment package and the component package.

Is the Full Treatment Superior to the Dismantled Treatment at Follow-Up?

Forty-four of the studies that reported comparisons between components and the full treatment package also provided appropriate follow-up data. Therefore, although no *a priori* hypothesis was made, potential differences between components and the full treatment package at follow-up were explored. If a study reported several follow-up data points, the follow-up point closest to six months was used to compute the effect size. The average *d* across all 44 comparisons at follow-up was -.159, (95% *CI* = -.26 - .06; *Z* = -3.01, *p* = .003). Although -.159 is generally considered a small effect (Cohen, 1988), it was significant. Despite the findings that there were no differences at treatment completion, this significant effect suggests the clients who received the full treatment package did better at follow-up than the clients in the component groups. These studies

were not heterogeneous [Q (43) = 46.7187, p = .322, I^2 = 7.9%], suggesting that they were drawn from the same population. Therefore, no moderator analyses were conducted on the follow-up studies.

CHAPTER IV

DISCUSSION

Overall Findings of Present Meta-Analysis

From these results, it can be concluded that initially treatment dismantled components do not differ in effectiveness from the full treatment package. Individuals who were treated with a component treatment ingredient did not differ from individuals treated with the full treatment package at the end of treatment. These results supported the original hypothesis that there would be very small differences between the dismantled component and the full treatment package. The finding that there was considerable heterogeneity among the studies was not predicted and suggests that there may be moderating variables that explain why the studies yielded different outcomes. In the present analysis, age was identified as a moderator: The full treatment was more effective than the dismantled treatment in those studies that had older participants. In addition to the original hypotheses that were proposed, an exploratory analysis yielded an unexpected finding that full treatment packages had better outcomes than the dismantled treatment components at the follow-up assessments. It can be concluded from the follow-up data that, in general, full treatment packages have better outcomes than component treatment groups over time.

Comparison With Previous Literature

Although the present meta-analysis replicates some of the findings of Ahn and Wampold (2001), the study on which it was based, it is important to note that some inconsistencies were also found (e.g., homogeneity versus heterogeneity). Likewise, the present study examined some questions that were not addressed by Ahn and Wampold

(e.g., moderators, follow-up outcomes). Primarily, the finding that there were no differences between the full treatment package and the component treatment at termination was consistent with the Ahn and Wampold's (2001) meta-analysis. In Ahn and Wampold's meta-analysis, they concluded that a lack of differences between component treatments and full treatments was due to nonspecific treatment factors. Ahn and Wampold argued that the nonspecific factors in therapy are the casual ingredients for treatment gains. They contended that because nonspecific factors are present in both the full and component treatments, differences between these two groups would not exist. The present study's null findings when comparing components and full treatments lend support to the idea that nonspecific factors may be responsible for the progress seen at treatment termination. However, as discussed in more detail below, the present study examined potential differences in follow-up outcomes—something that was not evaluated by Ahn and Wampold—and found that specific factors in the full treatment package are more effective over time.

The finding in the present meta-analysis that the studies were heterogeneous and moderated by age was inconsistent with Ahn and Wampold's findings, which found homogeneity among the studies. It is important to note that, although the present meta-analysis included a relatively small number of studies (k =59), it did include more studies than the Ahn and Wampold (2001) meta-analysis (k = 20). Therefore, the larger sample should have more accurately reflected the literature and increased the likelihood that the studies came from diverse populations, which could explain why the present study found heterogeneity.

Because heterogeneity was found in the present study, several potential moderators were examined. However, only age emerged as a moderator. The finding that older individuals may benefit more from broader treatments is not a new finding in the research literature. It is important to note that the "older" group in this sample is referring to clients in the middle to late-middle age group. The "older" group of individuals would be in their late 50s. Kennedy and Tanenbaum (2000) reported that although older adults benefit from many of the standard procedures in therapy, they also have additional needs and could benefit from more individualized modifications in therapy to address a variety of areas that are new challenges for them (i.e., family functioning, social functioning, self-reliance). Additional needs for older individuals may explain why a less complete or component therapy (e.g., Behavioral Activation) may not be as beneficial to them as the full treatment package (e.g., Cognitive-Behavioral Therapy) in addressing more specific individual needs and challenges at a later stage in life. It is also possible that the clients that come to therapy later in life have more ingrained schemas or core beliefs. For these clients, the more single-faceted treatments may not address problems that a client has struggled with for years. A more complete treatment package may offer more tools in these multiple areas of need.

The present meta-analysis built upon the Ahn and Wampold (2001) study by exploring differences at follow-up outcomes. Results from the follow-up data indicated a small but significant effect in favor of the full treatment package. Because the effect size was small (d = -.159), the magnitude should be addressed when drawing conclusions about *how much* of a difference this effect is in terms of treatment. First, the typical effect sizes in the meta-analyses literature should be considered for the different treatment

outcome designs. Lambert and Bergin (1994) reported a summary effect size of .48 when comparing an active psychotherapy to a placebo group (inactive treatment). Effect size differences drop (typically ranging from just above .00 to less than .21) when the treatment design is comparing two active treatments (Wampold et al., 1997). Therefore, an even larger drop in effect size should be expected for the present study, which compares an active treatment to a variation of that same treatment minus one component. It is not surprising, then, that the significant effect size found for follow-up outcomes was small in magnitude.

In further evaluating what this small effect size means, it is helpful to compare it to other effect sizes in the psychological and medical outcome literature. A comment by Rosenthal (1990) provides insight on what small effect sizes may mean by providing several examples of medical research and psychological research (e.g., Barnes, 1986; Canadian Multicentre Transplant Study Group, 1983; Centers for Disease Control Vietnam Experience Study, 1988; Smith & Glass, 1977; Steering Committee of the Physicians Health Study Research Group, 1988) where studies were ended prematurely due to small effect sizes in favor of the experimental treatment because it would be unethical to continue providing a placebo to subjects. Although the effect sizes were very small statistically, the effects were clinically meaningful when considering the number of people benefiting from treatment compared to the placebo. For example, although $r = .10$ is typically considered a small effect size, a study examining the effect of aspirin on heart attacks ended early when an effect size of $r = .034$ was found because of the large numbers of heart attacks prevented by taking aspirin. That is, only 104 people in the aspirin group experienced a heart attack compared to 189 people in the placebo group.

So, although the effect size of the present study is very small, it is nevertheless statistically significant and may be quite clinically meaningful.

The unexpected finding that full treatment packages are better than component treatments at follow-up may offer a more complete picture of *why* and *how* the psychotherapy process works. The finding that there are no differences between treatment components and the full treatment package at the end of therapy seems to lend support to the argument against the specificity theory. However, the follow-up outcomes provide evidence in favor of the full treatment package. The idea that nonspecific factors alone cause therapeutic change must be challenged with the finding of better full treatment outcomes at follow-up. One explanation for better full treatment outcomes at follow-up considers time of measurement as a key factor in outcome results. It is possible that differences between full treatments and components will have a delayed effect on treatment outcomes. The positive outcomes seen at the end of therapy may be due to the nonspecific treatment ingredients. However, it is the long-term gains in outcomes that can be attributed to specific treatment ingredients.

Several studies (Bush, Kanter, Landes, & Kohlenberg, 2006; Stiles et al., 2003; Tang & DeRubeis, 1999a, 1999b) looking at treatment outcomes across time found that the majority of the treatment gains occurred after the first session. The comparative studies (Luborsky et al., 2002; Luborsky & Singer, 1975; Wampold et al., 1997) mentioned earlier concluded that there were no differences between different psychotherapies and that they all led to similar gains, which were attributed to the nonspecific factors rather than active ingredients. Finally, the results at treatment completion found in the present meta-analysis as well as the one conducted by Ahn and

Wampold (2001) also underscore nonspecific factors as paramount. However, there may be an alternative explanation in which specific active ingredients do lead to specific changes. First, nonspecific factors are likely responsible for the initial change seen in clients. If treatment outcomes are only measured once at the end of psychotherapy (often a 6- to 12-week process), differences between psychotherapy ingredients or treatments may not be found because it is too early to detect any change beyond the benefits of nonspecific factors. This possibility is supported by the present study's findings which showed that the full packages were more beneficial at follow-up assessment. In other words, assessment of the initial outcomes may not have allowed enough time for the clients to demonstrate their benefit from the specific ingredients. Anecdotally, this pattern mirrors what therapists often describe with their clients in therapy. For example, cognitive therapists (e.g., Beck, 1995) often use techniques that aim to teach the client to become their own therapist; however, the process takes practice and the implementation of many tools over time. With time, the client should become better at being their own therapist and effectively using the specific techniques, which may be reflected in follow-up assessments (i.e., as supported by the present study). As such, it may be unreasonable for researchers and practitioners to expect huge treatment successes in a short amount of time. A second possibility is that clients who only received dismantled component packages lost the benefits initially showed at outcomes over time. It could be these less complete packages do not have the staying power and, therefore, the full package out performed the component package over time.

The current study contributes to the literature by answering an important question about the utility of component treatment studies. Component treatment studies are able

to provide information on active ingredients, but the typical time of measurement (at the end of treatment) may not be as useful as previously thought. Although it is difficult to keep clients engaged in research over long time periods due to various challenges, the present study suggests that it is still important to attempt to obtain longer follow-up data. Neither component studies nor comparative studies may be able to provide useful information about particular components if treatment outcomes are limited solely to the end of treatment. Since clients generally benefit more from the full treatment package at follow-up, it may not be the best approach to offer only certain components. Additionally, it may be beneficial to contact clients after termination to collect follow-up data on their progress. As addressed earlier in this paper, there has been an ongoing theoretical debate over the mechanisms of change in psychotherapy. The present study provides evidence that both nonspecific factors (particularly during initial phases of therapy) and specific ingredients (particularly after time, which would allow practice of techniques) in therapy contribute to the change seen in treatment outcomes.

Limitations and Future Directions

Although the present study provides important information about component designs and research outcomes, there are some limitations to consider. First, this meta-analytic review was restricted by the small number of studies ($k = 59$) included in the analysis. However, this review did include more studies than the Ahn and Wampold (2001) review ($k = 20$). Additionally, the electronic literature search was limited by the useful search terms available. The search terms originally proposed were too broad, captured more studies than manageable, and returned irrelevant studies.

There are also limitations that are inherent to any meta-analytic review. In a meta-analysis, it is preferable to include studies similar enough to be combined. Although all of the studies included were component studies, which was the research design of interest, the studies were diverse in treatment approach and treatment problem. Although the diversity of these studies should increase the external validity or generalizablity of the findings, it also limits the present study's internal validity. In addition to ranging in treatment problem and approach, the studies most likely differ in the quality of the research conducted. It would have been beneficial to rank the studies on quality of research; however, doing so would risk introducing bias of the investigator and would not allow the literature to speak for itself. Finally, author allegiance may not have been evenly balanced across the studies included. For example, researchers who come from a certain theoretical perspective may interpret empirical results in a way that complements their own viewpoint. Although author allegiance is a limitation to any meta-analysis, there is not a realistic way to code studies for author allegiance.

Given the findings of the present study, future component research should focus on providing long-term outcome assessments to examine whether evidence to support specific ingredients exist after a delay in time. Comparative research studies may also find a difference between treatments with longer follow-up assessments. The discussion over *why* psychotherapy works is far from resolved, but the present study provides some suggestions to move future research in the right direction.

In conclusion, the present meta-analysis indicates that there are no differences in treatment outcomes between a therapy component treatment and the full treatment package directly following treatment. However, small but significant differences do exist

between components and full treatments at follow-up assessments. These differences favor the full treatment package, indicating that specific ingredients in psychotherapy may be more effective than just nonspecific factors common to all therapy. It should be clear from this review that future component studies should include long-term follow-up assessments.

Table A1

General Information on Studies That Examined the Difference between a Full Treatment Package and the Component

Study	Disorder	More components group (n)	Fewer components group (n)	Effect *d*
Applebaum et al. (1990)	Tension Headache	CT+PMR (n=17)	PMR (n=16)	.246
Barlow et al. (1989)	Panic Disorder	PMR+EX+CR (n= 16)	PMR (n=10)	.379
		PMR+EX+CR (n=16)	EX+CR (n=15)	-.108
				.136(average *d*)
Barlow et al. (1992)	GAD	CT+PMR (n=11)	PMR (n=10)	.300
		CT+PMR (n=11)	CT (n=13)	.228
				.264(average *d*)
Baucom et al. (1990)	Marital Discord	CR+BMT (n=12)	BMT (n=12)	.449
		EET+BMT (n=12)	BMT (n=12)	-.202
		EET+CR+BMT (n=12)	BMT (n=12)	.485
				-.200(average *d*)
Bauman & MeInyk (1994)	Test Anxiety	EMDR (n=15)	FTDR (n=15)	.029
Blanchard et al. (1990)	Tension Headache	CT+PMR (n=16)	PMR (n=19)	.127
Borkovec et al. (2002)	GAD	CT+SCD (n=23)	CT (n=23)	-.054
		CT+SCD (n=23)	SCD (n=23)	-.276
				-.165(average *d*)
Borkovec & Costello (1993)	GAD	CBT (n=18)	AR (n=18)	-.187
Bryant et al. (2003)	PTSD	IE +CR (n=20)	IE (n=20)	-.247
Bryant et al. (2005)	Acute Stress	CBT+hypnosis (n=30)	CBT (n=33)	-.037

Table A1 (continued).

Study	Disorder	More components group (n)	Fewer components group (n)	Effect *d*
Bryant et al. (2008)	PTSD	IE+In Vivo EX+CT (n=28)	IE (n=31)	-.832
		IE+In Vivo EX+CT (n=28)	In Vivo EX (n=28)	-1.009
		IE+In Vivo EX+CT (n=28)	IE+In Vivo EX (n=31)	-.775
				-.872 (average *d*)
Cusack & Spates (1999)	PTSD	EMDR (n=11)	EMD-R (n=11)	.004
Dadds & McHugh (1992)	Child Conduct	CMT+ Ally (n=11)	CMT (n=11)	-.185
De Jong et al. (2000)	Spider phobia	EX+CC (n=18)	EX (n=16)	.245
de Zwaan et al. (2005)	Binge eating	BT (VLCD)+CBT (n=36)	BT(VLCD) (n=35)	.173
Deffenbacher et al. (2002)	Angry drivers	CRCS (n=17)	RCS (n=16)	-.349
Deffenbacher & Stark (1992)	General anger	CRCS (n=16)	RCS (n=19)	.153
Devilly et al. (1998)	PTSD	EMDR (n=12)	EMDR-EM (n=12)	-.326
Dunn et al. (1996)	Anxiety	EMD (n=14)	No Eye Movement (n=14)	-.001
Emmelkamp & Beens (1991)	OCD	EX+CT (n=10)	EX (n=11)	-.323
Fesk & Goldstein (1997)	Panic disorder	EMDR (n=18)	EFER (n=18)	-.002
Flessner et al. (2005)	Nail biting	SHR (n=18)	SHR-social support (n=20)	-.144
Foa et al. (2005)	PTSD	PE+CR (n=74)	PE (n=79)	-.079
Foa & Rauch (2004)	PTSD	PE+CR (n=27)	PE (n=27)	-.218
Foley & Spates (1995)	Public speaking phobia	EMD (n=10)	EX w/ resting eyes (n=10)	-.437
		EMD (n=10)	EX w/movement to sound (n=10)	.526
				.045 (average *d*)
Gosselin & Matthews (1995)	Test anxiety	EMD (n=21)	EMD-EM (n=20)	-.120
Grunes et al. (2001)	OCD	ERP+FI (n=14)	ERP (n=14)	-.424
Halford et al. (1993)	Marital Discord	EBMT (n=13)	BMT (n=13)	.313
Hope et al. (1995)	Social phobia	CBT (n=13)	Exposure only (n=10)	.253

Table A1 (continued).

Study	Disorder	More components group (n)	Fewer components group (n)	Effect *d*
Jacobson et al. (1996)	Depression	BA+AT (n=49)	AT (n=42)	-.003
		BA+AT (n=49)	BA (n=55)	.127
				.062 (average *d*)
Kazdin & Whitley (2003)	Child Conduct	PSST+PMT+PPS (n=57)	PSST+PMT (n=70)	.323
Koch et al. (2004)	Small animal phobia	CBT+EX (n=20)	EX (n=20)	-.484
Marks et al. (1998)	PTSD	EX+CR (n=20)	EX (n=20)	.383
		EX+CR (n=20)	CR (n=19)	.333
				.358 (average *d*)
Mattick et al. (1989)	Social phobia	CBT (n=11)	CR (n=11)	-.426
		CBT(n=11)	EX (n=11)	-.216
				-.321(average *d*)
McKay et al. (2010)	Cocaine dependence	RP+CM (n=25)	CM (n=26)	.0
Nezu & Perri (1989)	Depression	PST (n=14)	APST (n=14)	-1.295
Nicholas et al. (1991)	Chronic back pain	BT+PMR (n=8)	BT (n=9)	.795
Nicholas et al. (1991)	Chronic back pain	CT+PMR (n=8)	CT (n=7)	-1.567
Öst et al. (2004)	Panic disorder	CBT (n=34)	EX (n=29)	.047
Öst et al. (1991)	Blood phobia	ATP (n=10)	Tension only (n=10)	-.356
		ATP (n=10)	EX (n=10)	.149
				-.104 (average *d*)
Paunovic & Ost (2001)	PTSD	CBT+EX (n=7)	EX (n=9)	-.300
Petry et al. (2008)	Gambling problems	MT+CBT (n=40)	MT (n=55)	-.309
Pitman et al. (1996)	PTSD	EMDR (n=16)	EMDR-EM (n=14)	.232
Porzelius et al. (1995)	Eating disorder	OBET (n=25)	CBT (n=21)	.068
Propst et al. (1992)	Depression	CBT-Religious (n= 10)	CBT (n=10)	-.364

Table A1 (continued).

Study	Disorder	More components group (n)	Fewer components group (n)	Effect *d*
Radojevic et al. (1992)	Rheumatoid arthritis	BT+social support (n=15)	BT (n=14)	-.224
		BT+social support (n=15)	Social support (n=15)	-.121
				-.180 (average *d*)
Resick et al. (2008)	PTSD	CPT package (n=42)	CPT-C (n=38)	.099
		CPT package (n=42)	Written Alone (n=38)	-.348
				-.125 (average *d*)
Roehrig et al. (2006)	Body image	CA+EX (n=30)	CA (n=36)	.339
Rohan et al. (2007)	SAD	CBT+Light Therapy(n=15)	Light Therapy (n=16)	.478
		CBT+ Light Therapy (n=15)	CBT (n=15)	.469
				.473 (average *d*)
Rosen et al. (1990)	Body image	CBTP (n=13)	CBT (n=11)	.183
Sanders & McFarland (2000)	Family behavior	CBFI (n=23)	BFI (n=24)	.980
Sanderson & Carpenter(1992)	Phobias	EMD (n=32)	EMD-EM (n=30)	-.086
Schmidt et al. (2000)	Panic Disorder	CBT+breathing retraining(n=32)	CBT (n=21)	-.368
Schmiege et al. (2009)	HIV/STD risk	GPI+GMET (n=163)	GPI (n=154)	.242
Taylor et al. (2003)	PTSD	EMDR (n=15)	EMDR-EM (n=15)	.633
Thackwray et al. (1993)	Bulimia nervosa	CBT (n=13)	BT (n=13)	.760
Walters et al. (2009)	Alcohol problems	MI+FB (n=70)	FB (n=57)	.054
		MI+FB (n=70)	MI (n= 59)	-.092
				-.019(average *d*)
Webster-Stratton (1994)	Parenting effectiveness	GDVM+ADVANCE (n=38)	GDVM (n=39)	-.122

Table A1 (continued).

Study	Disorder	More components group (n)	Fewer components group (n)	Effect *d*
Williams & Falbo (1996)	Panic Disorder	CBT (n=11)	BT (n=10)	.093
		CBT (n=11)	CT (n=13)	.050
				.072 (average *d*)

Note. Disorder: GAD = Generalized Anxiety Disorder, PTSD = Post Traumatic Stress Disorder, OCD = Obsessive-Compulsive Disorder, SAD = Seasonal Affective Disorder, HIV = Human Immunodeficiency Virus, STD = Sexually Transmitted Disease. Component Group: CT = Cognitive Therapy; PMR = Progressive Muscle relaxation; EX = Exposure Therapy; CR = Cognitive Restructuring; BMT = Behavioral Marital Therapy; EET = Emotional Expressiveness Training; EMDR = Eye Movement Desensitization Reprocessing; FTDR= Finger Tapping Desensitization Restructuring; SCD = Self-Control Desensitization; AR = Applied Relaxation; CBT = Cognitive-Behavioral Therapy; IE = Imaginal Exposure; EMD-R = Eye Movement Desensitization without Reprocessing; CMT = Child Management Training; CC = Counter Conditioning; BT = Behavior Therapy; VLCD = Very Low Calorie Diet; CRCS = Cognitive and Relaxation Coping Skills; RCS = Relaxation Coping Skills; EMD = Eye Movement Desensitization; EFER = Eye Fixation Exposure and Reprocessing; SHR = Simplified Habit Reversal; PE = Prolonged Exposure; ERP = Exposure and Response Prevention; FI = Family Involvement; EBMT= Enhanced Behavioral Marital Therapy; BA = Behavioral Activation; AT = Automatic Thoughts; PSST = Problem Solving Skills Training; PMT = Parent Management Training; PPS = Parent Problem Solving; RP = Response Prevention; CM = Contingency Management; PST = Problem Solving Therapy; APST = Abbreviated Problem Solving Therapy; ATP = Applied Tension Package; MT = Motivational Techniques; CPT = Cognitive Processing Therapy; CPT-C = Cognitive Processing Therapy without the written component; CA = Counter Attitudinal Therapy; CBTP = Cognitive-Behavioral Therapy with Perception Training; CBFI = Cognitive-Behavioral Family Intervention; BFI = Behavioral Family Intervention; GPI = Theory based sexual risk reduction intervention; GMET = Group Based Motivational Enhancement Therapy; MI = Motivational Interviewing; FB = Feed Back; GDVM= videotaped parent skills training program; ADVANCE = cognitive training social learning program.

Table B1

Follow-Up Information on Studies That Examined the Difference between a Full Treatment Package and the Component

Study	Disorder	More components group (n)	Fewer components group (n)	Effect *d*
Barlow et al. (1989)	Panic Disorder	PMR+EX+CR (n= 16)	PMR (n=10)	.189
		PMR+EX+CR (n=16)	EX+CR (n=15)	.567
				.378 (average *d*)
Baucom et al. (1990)	Marital Discord	CR+BMT (n=12)	BMT (n=12)	-.032
		EET+BMT (n=12)	BMT (n=12)	.043
		EET+CR+BMT (n=12)	BMT (n=12)	.026
				.012 (average *d*)
Borkovec et al. (2002)	GAD	CT+SCD (n=23)	CT (n=22)	-.174
		CT+SCD (n=23)	SCD (n=21)	-.359
				-.267 (average *d*)
Borkovec & Costello (1993)	GAD	CBT (n=17)	AR (n=17)	-.077
Bryant et al. (2003)	PTSD	IE +CR (n=20)	IE (n=20)	-.357
Bryant et al. (2005)	Acute Stress	CBT+hypnosis (n=23)	CBT (n=24)	-.017
Bryant et al. (2008)	PTSD	IE+In Vivo EX+CT (n=21)	IE (n=21)	-.899
		IE+In Vivo EX+CT (n=21)	In Vivo EX (n=21)	-.927
		IE+In Vivo EX+CT (n=21)	IE+In Vivo EX (n=21)	-.732
				-.853 (average *d*)
Dadds & McHugh (1992)	Child Conduct	CMT+ Ally (n=6)	CMT (n=5)	-.173
De Jong et al. (2000)	Spider phobia	EX+CC (n=12)	EX (n=12)	.237
Deffenbacher et al. (2002)	Angry drivers	CRCS (n=17)	RCS (n=16)	-.025

Table B2 (continued).

Study	Disorder	More components group (n)	Fewer components group (n)	Effect *d*
Deffenbacher & Stark (1992)	General anger	CRCS (n=12)	RCS (n=14)	.240
Devilly et al. (1998)	PTSD	EMDR (n=13)	EMDR-EM (n=12)	-.334
Emmelkamp & Beens (1991)	OCD	EX+CT (n=10)	EX (n=11)	-1.151
Fesk & Goldstein (1997)	Panic disorder	EMDR (n=14)	EFER (n=14)	-.075
Foa et al. (2005)	PTSD	PE+CR (n=42)	PE (n=47)	-.055
Foa & Rauch (2004)	PTSD	PE+CR (n=18)	PE (n=20)	-.102
Grunes et al. (2001)	OCD	ERP+FI (n=14)	ERP (n=14)	-.463
Halford et al. (1993)	Marital Discord	EBMT (n=13)	BMT (n=13)	-.058
Hope et al. (1995)	Social phobia	CBT (n=9)	Exposure only (n=8)	.285
Jacobson et al. (1996)	Depression	BA+AT (n=47)	AT (n=39)	-.057
		BA+AT (n=47)	BA (n=50)	.091
				.017 (average *d*)
Koch et al. (2004)	Small animal phobia	CBT+EX (n=20)	EX (n=20)	-.736
Marks et al. (1998)	PTSD	EX+CR (n=11)	EX (n=12)	.497
		EX+CR (n=11)	CR (n=12)	-.864
				-.183 (average *d*)
Mattick et al. (1989)	Social phobia	CBT (n=10)	CR (n=9)	.356
		CBT(n=10)	EX (n=10)	.046
				-.301(average *d*)
McKay et al. (2010)	Cocaine dependence	RP+CM (n=23)	CM (n=24)	-.546
Nezu & Perri (1989)	Depression	PST (n=14)	APST (n=14)	-1.208
Nicholas et al. (1991)	Chronic back pain	BT+PMR (n=6)	BT (n=4)	-.467
Nicholas et al. (1991)	Chronic back pain	CT+PMR (n=5)	CT (n=5)	-1.324
Öst et al. (2004)	Panic disorder	CBT (n=34)	EX (n=29)	.025

Table B2 (continued).

Study	Disorder	More components group (n)	Fewer components group (n)	Effect *d*
Öst et al. (1991)	Blood phobia	ATP (n=10)	Tension only (n=10)	-.424
		ATP (n=10)	EX (n=10)	-.061
				-.104 (average *d*)
Paunovic & Ost (2001)	PTSD	CBT+EX (n=7)	EX (n=9)	-.153
Petry et al. (2008)	Gambling problems	MT+CBT (n=34)	MT (n=48)	-.281
Porzelius et al. (1995)	Eating disorder	OBET (n=20)	CBT (n=19)	-.075
Propst et al. (1992)	Depression	CBT-Religious (n= 10)	CBT (n=10)	-.193
Radojevic et al. (1992)	Rheumatoid arthritis	BT+social support (n=15)	BT (n=14)	-.065
		BT+social support (n=15)	Social support (n=15)	-.283
				-.174 (average *d*)
Resick et al. (2008)	PTSD	CPT package (n=44)	CPT-C (n=36)	.012
		CPT package (n=44)	Written Alone (n=39)	-.173
				-.161 (average *d*)
Roehrig et al. (2006)	Body image	CA+EX (n=22)	CA (n=28)	-.005
Rohan et al. (2007)	SAD	CBT+Light Therapy(n=13)	Light Therapy (n=14)	-.156
		CBT+ Light Therapy (n=13)	CBT (n=11)	-.192
				-.173 (average *d*)
Rosen et al. (1990)	Body image	CBTP (n=13)	CBT (n=11)	.180
Sanders & McFarland (2000)	Family behavior	CBFI (n=19)	BFI (n=20)	.980
Schmidt et al. (2000)	Panic Disorder	CBT+breathing retraining(n=32)	CBT (n=21)	-.397
Thackwray et al. (1993)	Bulimia nervosa	CBT (n=13)	BT (n=13)	-.257

Table B2 (continued).

Study	Disorder	More components group (n)	Fewer components group (n)	Effect *d*
Walters et al. (2009)	Alcohol problems	MI+FB (n=67)	FB (n=54)	-.079
		MI+FB (n=67)	MI (n= 59)	-.281
				-.180(average *d*)
Webster-Stratton (1994)	Parenting effectiveness	GDVM+ADVANCE (n=22)	GDVM (n=24)	-.138
Williams & Falbo (1996)	Panic Disorder	CBT (n=11)	BT (n=10)	.148
		CBT (n=11)	CT (n=13)	.163
				.154 (average *d*)

Note. Disorder: GAD = Generalized Anxiety Disorder, PTSD = Post Traumatic Stress Disorder, OCD = Obsessive-Compulsive Disorder, SAD = Seasonal Affective Disorder, HIV = Human Immunodeficiency Virus, STD = Sexually Transmitted Disease. Component Group: CT = Cognitive Therapy; PMR = Progressive Muscle relaxation; EX = Exposure Therapy; CR = Cognitive Restructuring; BMT = Behavioral Marital Therapy; EET = Emotional Expressiveness Training; EMDR = Eye Movement Desensitization Reprocessing; FTDR= Finger Tapping Desensitization Restructuring; SCD = Self-Control Desensitization; AR = Applied Relaxation; CBT = Cognitive-Behavioral Therapy; IE = Imaginal Exposure; EMD-R = Eye Movement Desensitization without Reprocessing; CMT = Child Management Training; CC = Counter Conditioning; BT = Behavior Therapy; VLCD = Very Low Calorie Diet; CRCS = Cognitive and Relaxation Coping Skills; RCS = Relaxation Coping Skills; EMD = Eye Movement Desensitization; EFER = Eye Fixation Exposure and Reprocessing; SHR = Simplified Habit Reversal; PE = Prolonged Exposure; ERP = Exposure and Response Prevention; FI = Family Involvement; EBMT= Enhanced Behavioral Marital Therapy; BA = Behavioral Activation; AT = Automatic Thoughts; PSST = Problem Solving Skills Training; PMT = Parent Management Training; PPS = Parent Problem Solving; RP = Response Prevention; CM = Contingency Management; PST = Problem Solving Therapy; APST = Abbreviated Problem Solving Therapy; ATP = Applied Tension Package; MT = Motivational Techniques; CPT = Cognitive Processing Therapy; CPT-C = Cognitive Processing Therapy without the written component; CA = Counter Attitudinal Therapy; CBTP = Cognitive-Behavioral Therapy with Perception Training; CBFI = Cognitive-Behavioral Family Intervention; BFI = Behavioral Family Intervention; GPI = Theory based sexual risk reduction intervention; GMET = Group Based Motivational Enhancement Therapy; MI = Motivational Interviewing; FB = Feed Back; GDVM= videotaped parent skills training program; ADVANCE = cognitive training social learning program.

APPENDIX C

CRITERIA FOR INCLUSION

Ahn and Wampold set the following as the criteria for inclusion in their 2001 meta-analysis:

> To be included in this meta-analysis, a study had to (a) involve a psychological treatment intended to be therapeutic for a particular disorder, problem, or complaint and (b) contain the necessary statistics to conduct the meta-analysis. To determine that a treatment was intended to be therapeutic, we used the criteria developed by Wampold et al. (1997); specifically, a treatment had to involve a therapist who had at least a master's degree and who met face to face with the client and developed a relationship with the client. Moreover, the treatment had to contain at least two of the following four elements: (a) The treatment was based on an established treatment that was cited, (b) a description of the treatment was contained in the article, (c) a manual was used to guide administration of the treatment, and (d) active ingredients of the treatment were identified and cited. Finally, the study's research design had to involve a comparison of one group with another group, and one of the following two conditions had to be satisfied: (a) One, two, or three ingredients of the treatment were removed, leaving a treatment that would be considered logically viable (i.e., coherent and credible), or (b) one, two, or three ingredients that were compatible with the whole treatment and were theoretically or empirically hypothesized to be active were added to the treatment, providing a "super treatment." A study was excluded when treatment A was compared with treatment B, where B was a subset of A but both A and B were established treatments in their own rights. (p. 252-253)

APPENDIX D

CODING SHEET FROM META-ANALYSIS DATA COLLECTION

Descriptive Information

Reference (APA style):

1. Study Number
2. Type of Publication
    1 = journal
    2 = book chapter
    3 = other (specify)
3. Publication year:
4. Mean age of sample
    a. if subgroups *M* of each

5. Type of Client:
    1= Volunteer (like college students seeking extra credit)
    2=Clinical (real client)

5b. Age=
    1=Adult
    2=Adolescent
    3=Child

6. Sex ratio of sample (% female, % male)
    a. if subgroups % of each
7. Problem Type
    1 = depression
    2 = anxiety (i.e., generalized anxiety disorder)
    3 = grief
    4 = aggression
    5 = psychotic disorders (i.e. Schizophrenia, Schizoaffective Disorder, Psychotic Mood Disorder)
    6 = personality disorders/interpersonal problems (i.e., Borderline Personality treated with DBT)
    7= Substance Use
    8=PTSD
    9= Panic Disorder
    10=Phobias
    11=OCD
    12=Bipolar Disorder
    13=Eating Disorder
    14=Somatoform and Factitious Disorders
    15=Health Disorders (i.e., headaches, rheumatoid arthritis)
    16=Marital Discord

17=Child behavior problems
18=Parent-training
19=enuresis
20=Mixed
21=Other

8. Type of Study design:
1= Within subjects design
2=Between subjects design
3=both

9. Treatment Location
1=residential/inpatient
2=outpatient

10. Region
1 = American
2 = European (note country/language)
3 = Other (list)

11. Treatment Intervention:
1=CBT
2=Exposure
3=IPT
4=Psychodynamic
5=Behavioral Therapy
6-Cognitive Therapy
7=DBT
8=EMDR
9=Marriage and Family Therapy
10= Applied Relaxation or PMR
11=Other

12. Measures of Pathology/Distress:

13. Sample size:
1a. total sample (Use entire sample and not treatment completers when available):
2a. Is the this just treatment completers or entire sample?
b. component sample size
c. full treatment package sample size

14. Assignment to treatments=
1= random
2= nonrandom

15. Order of treatment components:
1=Module
2=Simultaneous administration

15a= If module, are they presented in the same order?
1=yes

2=no

16. Number of sessions:

17. Length of sessions:

18.Dismantling Design:
a. More Component Group (example BT+CT-BT; EET+CR+BMT–BMT):
b. Less Component Group:
c. Component being tested:

EFFECT SIZE DATA: Feel free to copy and paste tables and then indicate on the table the needed figure.
19. Type of data effect size is based on (provide the values including *df*).

Mean of more component group=
Mean of less component group=
SD=

Follow-up Data;
Provide Follow-up data at one time point, and this time point should be the follow-up data that is closest to 6 months. If you have several time points past and it is not clear which one is closest to the 6 month (3 month collected and 9 month collected, used the later time point so 9 months in this example).

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