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Efficacy of Psychotherapeutic Interventions to Promote Forgiveness: A Meta-Analysis

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Objective: This meta-analysis addressed the efficacy of psychotherapeutic interventions to help people forgive others and to examine moderators of treatment effects. ***Method:*** Eligible studies reported quantitative data on forgiveness of a specific hurt following treatment by a professional with an intervention designed explicitly to promote forgiveness. Random effects meta-analyses were conducted using $k = 53$ posttreatment effect sizes ($N = 2,323$) and $k = 41$ follow-up effect sizes ($N = 1,716$) from a total of 54 published and unpublished research reports. ***Results:*** Participants receiving explicit forgiveness treatments reported significantly greater forgiveness than participants not receiving treatment ($\Delta_+ = 0.56$ [0.43, 0.68]) and participants, receiving alternative treatments ($\Delta_+ = 0.45$ [0.21, 0.69]). Also, forgiveness treatments resulted in greater changes in depression, anxiety, and hope than no-treatment conditions. Moderators of treatment efficacy included treatment dosage, offense severity, treatment model, and treatment modality. Multimoderator analyses indicated that treatment dosage (i.e., longer interventions) and modality (individual > group) uniquely predicted change in forgiveness compared with no-treatment controls. Compared with alternative treatment conditions, both modality (individual > group) and offense severity were marginally predictive ($ps < .10$) of treatment effects. ***Conclusions***: It appears that using theoretically grounded forgiveness interventions is a sound choice for helping clients to deal with past offenses and helping them achieve resolution in the form of forgiveness. Differences between treatment approaches disappeared when controlling for other significant moderators; the advantage for individual interventions was most clearly demonstrated for Enright-model interventions, as there have been no studies of individual interventions using the Worthington model.

Keywords: forgiveness, interventions, efficacy, treatment, anger

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The psychological study of forgiveness has grown dramatically in the past two decades (Fehr, Gelfand, & Nag, 2010; Worthington, 2005), especially in the exploration of interventions designed explicitly to promote forgiveness. Initial evidence supports the efficacy of these forgiveness interventions, showing that they help participants increase their degree of forgiveness for an offense or injury, increase hope and psychological well-being, and decrease depression, anxiety, and anger (Baskin & Enright, 2004; Wade, Worthington, & Meyer, 2005).

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Definition of Forgiveness

What is "forgiveness"? According to the emerging consensus among intervention researchers, forgiveness can include both (a) the reduction in vengeful and angry thoughts, feelings, and motives that may be accompanied by (b) an increase in some form of positive thoughts, feelings, and motives toward the offending person (Wade & Worthington, 2003). Thus, forgiveness is understood as primarily an intrapersonal experience that does not include reconciliation with the offending person even though reconciliation might accompany it. Most researchers agree that forgiveness is not forgetting, condoning, or excusing the wrongdoing, nor is it simply the opposite or absence of bitterness and vengefulness (i.e., unforgiveness, Wade & Worthington, 2003; see essential agreement among 20 research teams in Worthington, 2005).

Given this definition, seeking to promote forgiveness in psychotherapy is more than simply reducing anger, bitterness, and vengeful rumination. With many clients, the simple reduction or elimination of negative thoughts and feelings would be considered a psychotherapeutic success. However, some psychotherapists have wondered, *What more can be done for my clients who have experienced significant hurts?* (e.g., DiBlasio & Benda, 1991). In response to this question, researchers and clinicians have proposed that helping clients

to forgive could be a useful focus of psychotherapy (Enright, 2001; Worthington, 2001). This is in line with a positive psychotherapy perspective that seeks to attend to and develop strengths rather than just minimize problems (Gelso & Woodhouse, 2003; Seligman, Rashid, & Parks, 2006). Thus, the promotion of forgiveness as a psychotherapeutic technique is more than simply reducing negative thoughts and feelings but also includes helping clients move toward more positive, even optimal functioning.

Explicit Forgiveness Interventions

As a result, several theoretical forgiveness models have been developed to promote forgiveness (e.g., Enright, 2001; Luskin, 2007; Worthington, 2001). Research groups headed by Enright and Worthington have led the way in investigating the efficacy of these interventions. Enright's treatment model contains 20 steps (Enright & Fitzgibbons, 2000), which are summarized in four phases: Uncovering (negative feelings about the offense), Decision (to pursue forgiveness for a specific instance), Work (toward understanding the offending person), and Discovery (of unanticipated positive outcomes and empathy for the offending person). Each of these phases includes several smaller steps within them. For example, within the Work phase, clients work toward understanding the offender, developing compassion, accepting/absorbing the pain, and considering giving a gift of forgiveness to the offender (Enright, 2001). The efficacy of the Enright model has been shown with groups as diverse as adult incest survivors (Freedman & Enright, 1996), parents who have adopted special needs children (Baskin, Rhody, Schoolmeesters, & Ellingson, 2011), and inpatients struggling with alcohol and drug addiction (Lin, Mack, Enright, Krahn, & Baskin, 2004).

The other primary research group has conducted research organized around Worthington's (2001) REACH Forgiveness model. Each letter in the acronym REACH represents a major component in the forgiveness process. In the first step of this model, participants recall (R) the hurt they experienced and the emotions associated with it. Next, participants work to empathize (E) with their offender, take another's perspective, and consider factors that may have contributed to their offender's actions. This is done without condoning the other's actions or invalidating the often-strong feelings the offended person has as a response. Third, participants explore the idea that forgiveness can be seen as an altruistic (A) gift to the offender. Participants learn that forgiveness can be freely given or legitimately withheld and recall times when others forgave them. Fourth, participants make a commitment (C) to forgive. This includes committing to the forgiveness that one has already achieved as well as committing to work toward more forgiveness, knowing that it is a process that often takes time to fully mature. Last, participants seek to hold (H) onto or maintain their forgiveness through times of uncertainty or a return of anger and bitterness (e.g., if they get hurt again in a similar way).

Previous meta-analyses have indicated that interventions of this nature can effectively promote forgiveness (Baskin & Enright, 2004; Wade et al., 2005). In one of the first meta-analyses of the efficacy of forgiveness interventions, Baskin and Enright found that in nine studies of individual and group therapy ($N = 330$ participants), explicit forgiveness interventions increased forgiveness, hope, and self-esteem, and reduced anxiety and depression. Baskin and Enright claimed that interventions that were process based (in which forgiveness is understood as a process that unfolds over time through a series of developmental steps) were more effective than interventions that were decision based (in which forgiveness is understood as a conscious choice made by the person who was injured). However, treatment categories were confounded with the amount of time spent intervening. That is, the individual counseling and process-based group forgiveness models had considerably longer treatment durations than the decision-based interventions. Thus, that meta-analysis left the question unanswered whether treatment model made a difference over and above time spent intervening. In earlier meta-analytic studies, Worthington, Sandage, and Berry (2000; 13 studies) and Worthington, Kurusa, et al. (2000; 25 studies) found that the duration of treatment and effect size were correlated about .75.

In another meta-analysis, Wade and colleagues (2005) examined the efficacy of forgiveness interventions, focusing on group treatments in 39 studies. This result was also confirmed in a separate meta-analysis of group treatments that examined 13 published studies up to 2006 (Rainey, Readdick, & Thyer, 2012). In addition, Wade et al. controlled for treatment duration. They reported that full forgiveness interventions (treatments that incorporate all components of an intervention model) were, in fact, more effective than partial interventions (dismantled treatments that used only certain components of a model), even when they controlled for treatment duration. Wade et al. also found that time spent on certain elements, for example developing empathy, was positively related to the efficacy of the treatments. However, the analyses of Wade et al. and Rainey et al. were limited to interventions provided in a group format. Therefore, neither meta-analysis assessed whether individual counseling interventions differed after controlling for treatment duration nor did they assess potential differences between counseling formats (e.g., individual, group, or couples). Finally, Wade et al. and Rainey et al. did not assess outcomes other than forgiveness. Therefore, the effects of group forgiveness treatment on outcomes such as depression, anxiety, and hope, while controlling for intervention duration, are still unknown.

Potential Moderators of Forgiveness Intervention Efficacy

Although forgiveness interventions appear effective for promoting forgiveness and perhaps even mental health, questions about moderators that affect the forgiveness process remain unaddressed. Specifically, what factors are likely to facilitate a participant's response to treatment? Are some treatment approaches better than others if the effects of treatment duration are controlled? Does treatment modality make a difference? How do these interventions affect different outcomes?

Treatment Duration

One of the most well-established moderators of treatment efficacy in the general psychotherapy outcome literature is treatment duration (Howard, Kopta, Krause, & Orlinsky, 1986). Dose–response curves have shown that improvement in client concerns increases considerably with more treatment, until about 28 counseling sessions, at which point it reaches an asymptote. Thus, in a short-term model (i.e., less than 28 sessions), duration should be expected to play an important role with forgiveness interventions

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as well. In fact, past summaries of the forgiveness outcome literature have indicated that this is the case (Worthington, Kurusa, et al., 2000; Worthington, Sandage, & Berry, 2000). Therefore, it is crucial to attend to the duration of the separate treatments to control for that factor when analyzing other moderators and to replicate the dose–response result with a more comprehensive list of forgiveness intervention studies.

Theoretical Intervention Model

The specific forgiveness model used to intervene is another important potential moderator of response to forgiveness treatment. Two forgiveness intervention models have been used in the majority of forgiveness intervention research: Enright's (2001) and Worthington's (2006). In addition, other researchers have developed and tested intervention models that are distinct from either the Enright or Worthington approach. For example, Luskin, Ginzburg, and Thoresen (2005) developed a cognitive behavioral model; Greenberg, Warwar, and Malcolm (2008, 2010) used an emotionally focused therapy approach; and DiBlasio and Benda (2008) used an explicitly decision-based model. Although the alternatives have not yet been investigated often enough to be examined separately, together they do provide an additional treatment model category against which Enright's and Worthington's models can be compared. An examination of the different forgiveness intervention models can provide information about the potential differences in treatment efficacy.

Individual, Couple, or Group Treatment Modality

A third potential moderator is treatment modality. Although the majority of forgiveness interventions have been conducted in group formats (Wade et al., 2005), there are a growing number of studies on individual and couple treatments. Previous reviews have attempted to answer this question as well. In their meta-analysis, Baskin and Enright (2004) compared individual process-based interventions to process- and decision-based group interventions. In that analysis, individual interventions were more effective than either group or couple interventions. However, this analysis did not consider treatment duration, which was confounded with treatment modality (process-oriented individual treatments averaged 36 hr; process-based group interventions averaged 7 hr; decision-based interventions averaged 4 hr). So, for forgiveness interventions, differences among modalities are possible but have not been effectively tested without substantial confounding of variables.

Offense Severity

Finally, another possible moderator of the forgiveness process is offense severity (McCullough & Hoyt, 2002). A variety of researchers have postulated that a relationship between these variables exists. Ohbuchi, Kameda, and Agarie (1989) examined the effects of offense severity, along with the importance of an apology from the offender, in encouraging forgiveness. They found that offenses that are more severe, even when an individual receives an apology from the offender, are more difficult to overcome and may require a more extensive and persuasive apology for anger and aggression to be mitigated. Fincham, Jackson, and Beach (2005) suggested that severe offenses are more difficult to overcome and may require a more complex understanding of forgiveness. Thus, it appears that the more severe the transgression, the more difficult it is to forgive. For example, Krumrei, Mahoney, and Pargament (2011) found that when divorce was considered a sacred loss or desecration of something sacred (i.e., marriage), it was particularly difficult to forgive. Although Exline, Worthington, Hill, and McCullough (2003) suggested that people maintained an intuitive cognitive accounting of the net injustice of offenses and their aftermath; there might be something qualitatively different about very severe transgressions, not just quantitatively different. This has not been investigated per se.

When applied to intervention research, the role of offense severity becomes even more nuanced. Often by design, participants recall a specific offense that was hurtful to them. The reported events vary in terms of external or objective severity, from murdered family members (Luskin & Bland, 2000) to feeling neglected by one's parents (Al-Mabuk, Enright, & Cardis, 1995). However, these events were recalled as something hurtful, and therefore, they often have little variability in self-reported severity. This is particularly evident in studies that recruited participants with a range of reported hurts (e.g., McCullough, Worthington, & Rachal, 1997; Wade, Worthington, & Haake, 2009). Typically, in studies that assess the perceived severity of the offense, almost all of the participants report that the offense was severe. Because of the design of such research, this result is understandable, but it does not provide information about the effect of offense severity on response to forgiveness treatment. In addition, no intervention studies of which we are aware address offense severity or attempt to investigate the role of offense severity that has been objectively rated by coders. One might suspect that participants with severe hurts would respond less favorably to forgiveness interventions or might need interventions of longer durations than those with less severe hurts (Worthington, Sandage, & Berry, 2000). However, several studies have shown that people experiencing significant offenses, including incest (Freedman & Enright, 1996) and murder of a family member (Luskin & Bland, 2000), respond favorably to treatment. Still, none of these studies have directly examined the influence of severity on response to treatment. One other consideration is important. In severe hurts, there is less possibility of a floor effect. Namely, in a mild hurt, effect sizes expected after intervention are limited by the amount of change that is achievable. However, with severe hurts, the amount of forgiveness to be achievable is considerable.

Purpose

The purpose of the present investigation was to expand the forgiveness intervention literature by (a) systematically comparing forgiveness and mental health outcomes between treatment and control groups from pre- to posttreatment and from pretreatment to follow-up, (b) examining the potential moderators of treatment efficacy that have been identified but not yet included in meta-analytic research, and (c) including research studies conducted since previous meta-analyses (about 7 years of research since Wade et al., 2005). We analyzed changes in forgiveness, anxiety, depression, and hope across time. In addition, we explored the moderating effects of treatment duration, psychotherapeutic model, treatment modality, and offense severity on forgiveness. Because considerably fewer studies included depression, anxiety and hope as outcomes, we did not analyze moderators of these outcomes.

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Method

This meta-analytic review was based on 54 published and unpublished reports of studies of forgiveness interventions. We searched the following areas to locate studies for inclusion: (a) computerized search of the PsycINFO (1872–2011) database using keywords *forgiveness*, *intervention*, *psychotherapy*, *and treatment;* (b) manual search of references listed in all located studies; and (c) contacting known forgiveness researchers for unpublished studies. Studies were included if they (a) examined effects of a psychotherapeutic intervention specifically designed to promote forgiveness, (b) offered the intervention in-person by a trained facilitator, (c) used a quantitative measure of forgiveness for a specific offense as an outcome, (d) were written in English, and (e) were completed prior to 2012. Studies were excluded if they were self-help rather than therapist-led (e.g., client-directed from an online resource or book); focused on developing forgiveness generally, but not for a specific offense; or did not measure forgiveness as an outcome. In Figure 1, we display the numbers of found, eligible, and ineligible studies.

A case can be made for excluding studies lacking a no-treatment control group from meta-analyses of interventions. When treatment effects in such studies are computed by comparing pre- and posttreatment means, they have been shown to yield inflated estimates of effect size (Lipsey & Wilson, 1993). Carlson & Schmidt (1999) showed that this bias was attributable to the failure to account for spontaneous improvement among untreated (control) participants, and Becker (1998) proposed that this source of bias could be obviated by computing an effect size (Δ) comparing

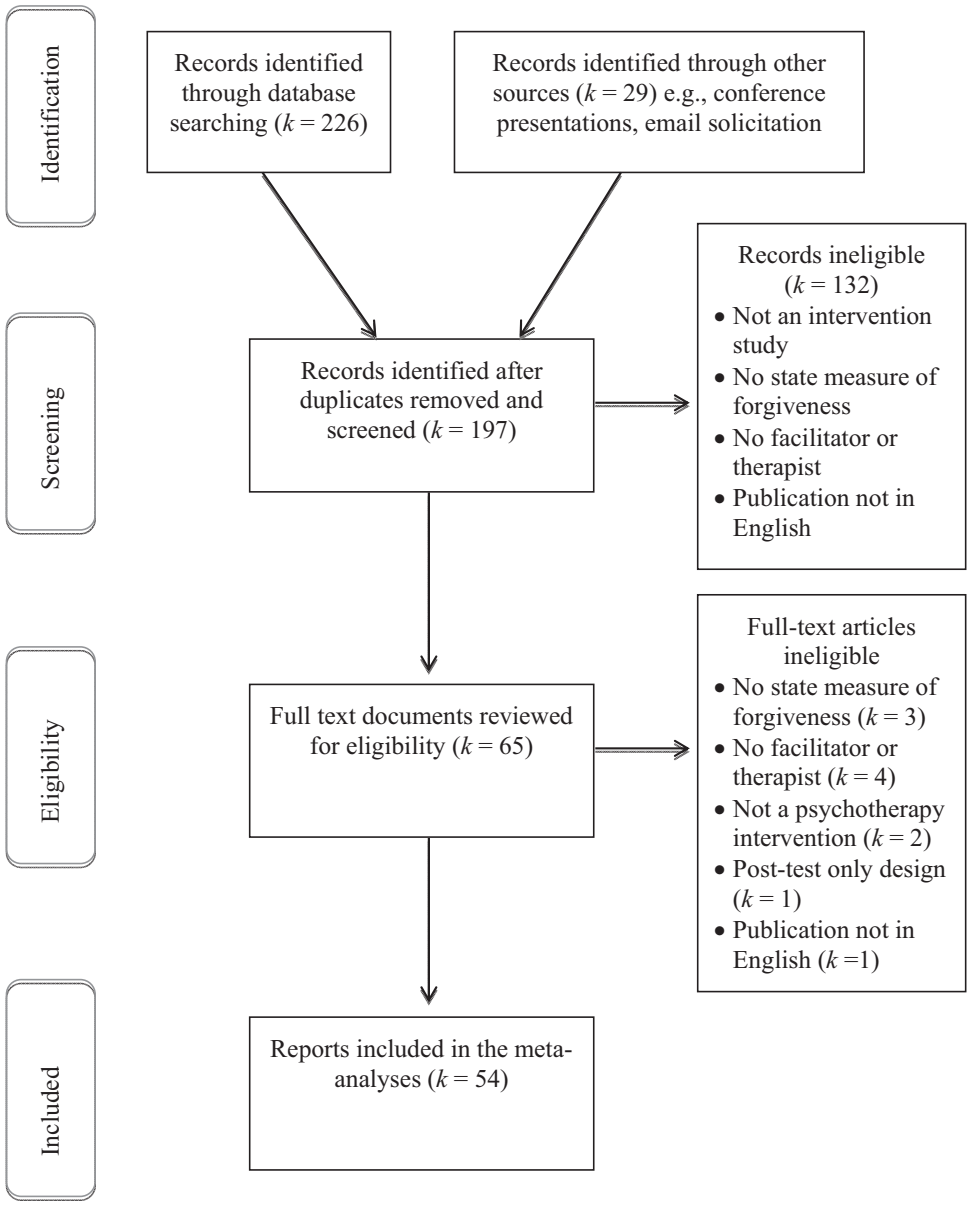

Figure 1. Flow diagram of studies included in the meta-analyses.

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improvement among treated participants to that among untreated participants, which makes possible imputation of control group data for studies without a control group, as described later. As described in the Results section, we tested to see whether there were systematic differences between studies with and without a control group after imputation and found none. We therefore included both controlled and uncontrolled studies in our analyses.

Assessing Methodological Quality

We assessed the methodological quality of the studies included in the meta-analysis with three ratings: whether the study was peer-reviewed, whether participants in the study were randomly assigned to treatment, and the retention rate of participants in the study. These three were chosen as estimates of the quality of each study's methodology because these are common indicators across different measures of quality (e.g., Downs & Black, 1998; Higgins & Altman, 2008). Peer review and random assignment were coded as dichotomous variables (yes or no) based on the report within the document to which we had access. To calculate the retention rate (i.e., the percentage of participants who remained in the study from pretest and treatment to posttest), we divided the number of participants who completed the treatments and pre–post questionnaires and were included in the analyses by the number of participants who were assigned to treatments. Because each of these characteristics can be considered an independent measure of study quality, we conducted a single-moderator analysis to determine whether each of these characteristics significantly moderated the reported effect size. None of these analyses revealed a significant difference in effect size based on these measures. (The *p* values for these tests in the forgiveness dataset with no-treatment comparisons—the largest and therefore most statistically powerful set of tests—were .94, .19, and .53 for published, random assignment, and retention rate, respectively.) These results indicated that treatment effect size was not related to the variables we used to assess methodological quality.

Coding of Moderators

Four raters, two males and two females, coded the severity of offenses described in each individual study. Offenses were coded on the basis of the Holmes–Rahe Social Readjustment Scale (Holmes & Rahe, 1967). The Holmes–Rahe provides estimates of the magnitude of stress for 43 particular life events, such as change in financial situation and death of a close friend. Many of these events directly reflect the offenses participants were experiencing (e.g., divorce). So, in many cases, the offenses that the treatment in a particular study was designed to address already had a numerical value on the Holmes–Rahe Scale. For those that did not (e.g., infidelity), a numerical value was determined prior to coding based on events on the Holme–Rahe that were judged by consensus of the raters and authors as similar in nature (e.g., sex difficulties). The greater the severity of the offense, the higher the offense was rated on a scale from 1 to 100. The four ratings were then averaged to produce a single index of offense severity for each study. Interrater reliability for this index (Shrout & Fleiss, 1979; intraclass correlation [ICC] 34) was .94. Treatment duration, psychotherapeutic model, and treatment modality were taken directly from the research report (i.e., article, chapter, dissertation, or manuscript) by the first author.

Computation of Effect Sizes

Outcome studies can use a variety of designs. Two common designs include posttest only with control group and pretest–posttest with control group. Each of these designs allows for a comparison of outcomes between the two groups. If participants are randomly assigned to groups (i.e., randomized controlled trial, RCT), this provides a sound basis for attributing group differences in outcomes to the effects of the intervention. The effect size computed from the well-designed, pretest–posttest RCT has been called the "standard of accuracy" (Carlson & Schmidt, 1999, p. 853) for intervention research, in that it controls for differences at pretest while comparing outcomes at posttest.

Another important consideration is the nature of the comparison group. Researchers commonly compare the intervention group to a wait-list or no-treatment control group. Another common strategy is to compare a forgiveness intervention to some alternative type of treatment (e.g., "placebo" condition; treatment as usual; partial forgiveness intervention). The effect size computed on this comparison estimates the additional gain in efficacy from employing a forgiveness-focused intervention in comparison to what would be expected under an alternative treatment approach. Because the two types of comparison groups address different research questions, we analyzed effect sizes of each type in separate meta-analyses.

Analyses

First, we computed within-groups (pre–post) standardized mean differences and sampling variances for both treatment and control groups. As recommended by Borenstein, Hedges, Higgins, and Rothstein (2009), these were then corrected to produce unbiased effect sizes (g_T and g_C) and sampling variances (v_{gT} and v_{gC}) for treatment and control groups, respectively. Becker's (1988) Δ is a comparison of change in the treatment group to change in the control group (taking baseline scores into account):

$$\Delta = g_T - g_C \tag{1}$$

$$v_\Delta = v_{g_T} + v_{g_C} \tag{2}$$

Becker's Δ was the basic unit of effect size for this meta-analysis, and studies were weighted by $1/v_\Delta$ in aggregation and significance testing (Becker, 1988; Hedges & Olkin, 1985).[1] Effect size computation and aggregation of dependent effect sizes was conducted using the R package "MAd" (Del Re & Hoyt, 2010), and omnibus analyses, heterogeneity tests, and moderator analyses were conducted using the R package "metafor" (Viechtbauer, 2010).

[1] In these calculations, the pre–post correlation is needed to compute the variance of the within-group (i.e., treatment or control) effect size. This correlation is rarely reported in research studies and therefore has to be estimated. This is essentially a test–retest correlation coefficient, although it may be somewhat smaller than a typical reliability coefficient because the effects of intervention likely decrease the stability of the scores. We conservatively assumed $r = .6$ for all outcomes. The sampling variance is a function of $(1 - r)$, so larger values of r lead to smaller sampling variances and standard errors. The value chosen for r does not change the effect size but does affect the sampling variance, which is the basis for weighting that effect size in computing the aggregate effect size. When the same value of r is used for all studies, the relative values of their sampling variances do not change radically for small changes in r.

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Effect sizes were treated as random effects in all analyses, based on our assumption that there were systematic differences among studies related to intervention efficacy, and our desire to generalize conclusions beyond the specific studies examined here (Hedges & Vevea, 1996).

Results

Preliminary Analyses

Studies lacking no-treatment controls. The 54 research reports included data on 62 interventions designed to promote forgiveness. However, 20 of these intervention groups were in studies that did not include a no-treatment comparison condition. Most of these studies compared a forgiveness intervention to a nonspecific treatment ($k = 9$) or to a placebo group ($k = 4$); the remaining studies ($k = 7$) used a single-group, pre–post design.[2] Placebo controls can enhance outcomes in their own right (Wampold, Minami, Tierney, Baskin, & Bhati, 2005), and nonspecific interventions are also expected to result in improvement in outcomes (Wampold, 2001). To produce a common effect size metric, one must avoid comparing some treatments to no-treatment controls (T–NT) and others to placebos or other alternative treatments (T–AT).

One option for computing comparable effect sizes from studies lacking a no-treatment condition is to compute the standardized mean difference comparing postscores to prescores for these treatments. Carlson and Schmidt (1999) noted that these effect sizes are likely biased relative to those computed by comparing treatment and no-treatment groups (Lipsey & Wilson, 1993). Instead, Becker (1988) recommended meta-analyzing control group effect sizes from studies that include a no-treatment control group. The results of this preliminary analysis permit imputation of control data for studies lacking a no-treatment control group.

Of the 42 studies that included data for a no-treatment condition, four were duplicates—two different interventions that were compared with the same no-treatment control group. When the 38 independent no-treatment control groups were meta-analyzed, they yielded an aggregate effect size of $g_+ = 0.05$, 95% confidence interval (CI) [0–.06, 0.17], reflecting the expected increase in forgivingness in the absence of treatment. Thus, expected spontaneous improvement over this relatively short interval ($Mdn = 6.33$ weeks; range = 1–57) was small and not statistically significant (95% CI includes 0). Although the control group effect sizes were heterogeneous—Q (37) = 75.17, $p = .0002$ ($I^2 = 53\%$)—we found no evidence of moderation by any of the study characteristics tested as moderators in the main analyses. We therefore used the aggregate effect size of $g_C = 0.05$ ($v_{gc} = .0033$) as the control group effect size for studies lacking a no-treatment control condition, so that T–NT effect sizes could be computed for all treatment groups. Because alternative-treatment conditions varied by study, we did not impute values for studies without alternative-treatment groups. Consequently, the T–AT analyses include only effect sizes derived from studies that included such conditions.

Examination of outliers. We followed recommended procedures (Hedges & Olkin, 1985; Viechtbauer, 2010) for outlier analysis to identify studies with effect sizes so deviant that it appears unlikely they were drawn from the same population as the remaining studies in the sample. We relied on the "externally standardized" residuals (Viechtbauer, 2010), based on the final models (i.e., models including significant moderator variables) reported. For the T–NT analysis, we identified two studies with $|z| > 2$, where z is the standardized residuals divided by its standard error: Alvaro (2001) and Sells, Giordano, and King (2002). The first study had an extreme positive residual of 3.53 ($\Delta = 3.80$) and the second had a negative residual of –3.69 ($\Delta = 2 –.92$). Visual inspection confirmed that each of these studies was well into the tails of the distribution of effect sizes. We therefore excluded these two studies from all reported analyses. A third study did not contribute to the final analysis because of missing moderator information but was a distant outlier in the omnibus analysis (Gambaro, 2002; standardized residual = 6.19; $\Delta = 6.81$). We therefore excluded this study from all analyses as well. A second outlier analysis in the new data set (with the three studies excluded) revealed no further outliers. Similar outlier analyses for T–AT comparisons identified a single outlier (Gambaro, 2002; standardized residual = 6.11; $\Delta = 6.56$), which was also excluded from reported analyses.[3] The outlier analysis of effect sizes derived from baseline-to-follow-up comparisons revealed no outliers in this data set.

Main Analyses: Forgiveness as Outcome

Omnibus analysis. In Figures 2 and 3 we provide effect sizes (with 95% CIs) and moderator information for the studies included in the T–NT and T–AT meta-analyses, respectively. In Table 1, we display the omnibus effect sizes and homogeneity tests for both T–NT and T–AT comparisons for forgiveness as an outcome. In comparison with untreated participants, those receiving forgiveness interventions reported substantially greater increases in forgiveness ($\Delta_+ = 0.56$). Put another way, the average participant in the intervention group showed greater improvement over the course of treatment than 71% of those in the no-intervention group. T–AT comparisons yielded an aggregate effect size of $\Delta_+ = 0.45$, almost as large as that for the T–NT comparisons. In addition, heterogeneity tests were statistically significant for both types of comparisons, with a high proportion of effect size variance ($I^2 =$ 72% and 77%, respectively) attributed to systematic sources beyond the variance expected due to sampling error. Thus, we conducted planned moderator analyses for both sets of effect sizes.

Single moderator analyses. We first tested each potential moderator variable individually, for both T–NT and T–AT comparisons. Results are shown in Table 2 (continuous moderators) and Table 3

[2] Because participants in single group, pre and post (SGPP) test design studies are not randomized to condition, such studies are not as rigorous at controlling for threats to internal validity (e.g., client self selection into treatment as a confound). However, inclusion of SGPP studies, after establishing their effect sizes in relation to imputed control group data to eliminate potential bias (Becker, 1988), is in keeping with the goal of meta-analysis to summarize all empirical literature relevant to the research question of interest. Based on a reviewer's concern that there may still be bias in these effect sizes, we conducted post hoc analyses in which we included a dummy variable (contrasting studies with and without imputed control group data) in the final model and found that, when significant moderators are accounted for, there are no systematic differences between these two sets of studies ($ps > .20$).

[3] The general pattern of findings does not change with the exclusion of outliers. As expected, the homogeneity statistics (Q; I^2) are substantially reduced when outliers are excluded. Also, because two of three outliers were positive (i.e., extreme, positive effect sizes), the omnibus effect size is somewhat reduced when they are excluded. For the full data set, $\Delta_+ =$ 0.62, 95% CI [0.45,0.80]; $Q(61) = 350$, $I^2 = 89\%$; after exclusion of outliers, $\Delta_+ = 0.56$, 95% CI [0.43,0.68], $Q(52) = 188.91$, $I^2 = 72\%$.

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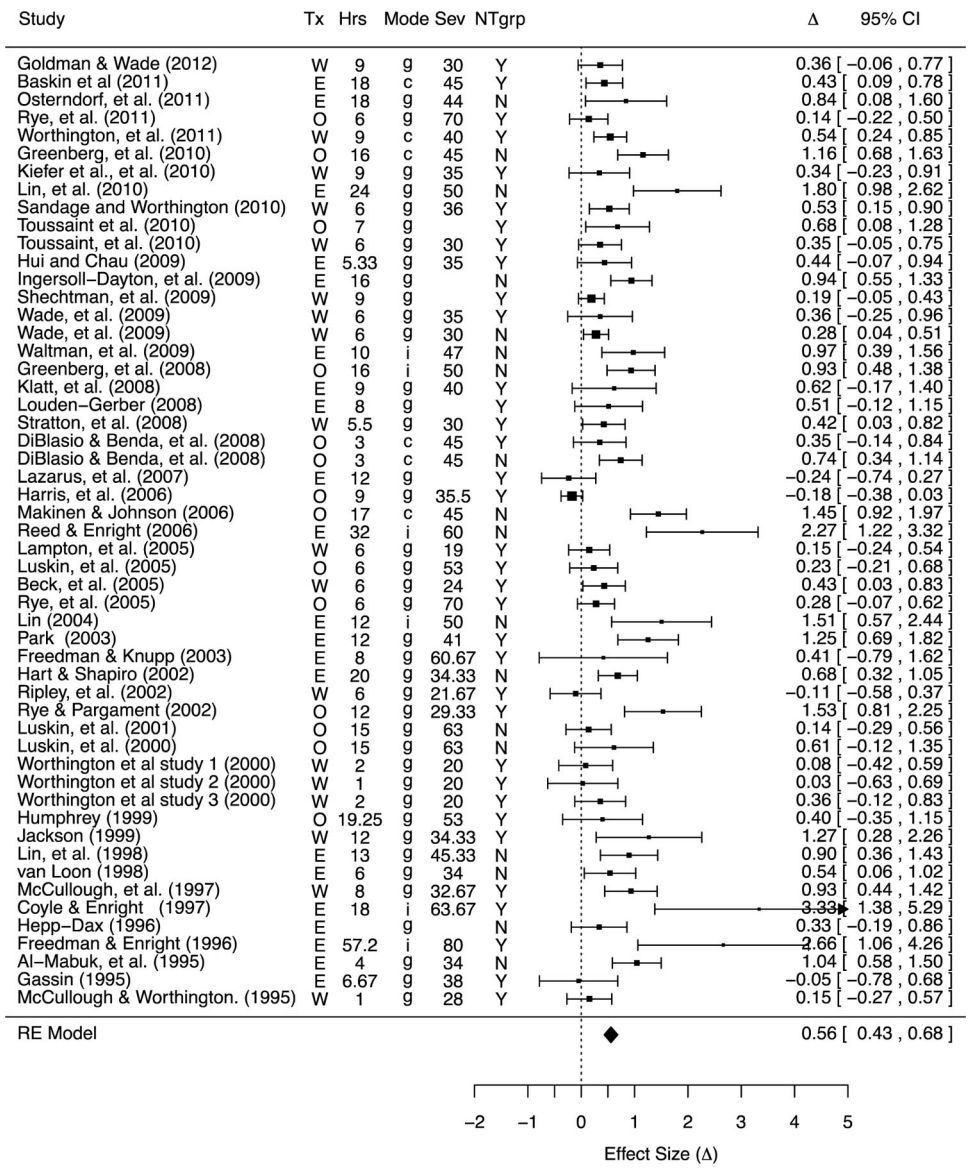

Figure 2. Forgiveness interventions: Treatment–control comparisons. Tx = treatment model; W = Worthington, E = Enright, and O = other treatment model. Mode = treatment modality; g = group, c = couples, and i = individual therapy modality. NTgrp = no-treatment control group; Y = yes and N = no. Sev = offense severity. Hrs = hours; CI = confidence interval; RE = random effects.

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(categorical moderators). For continuous moderators, in Table 2, we report intercepts (B_0) as well as slopes (B_1). It is the slope that quantifies the degree of association between the moderator variable and outcomes; the intercept reflects the estimated improvement when the value of the moderator is 0. For example, dosage significantly predicts treatment–control effect sizes, with $B_0 = 0.102$ and $B_1 = 0.047$. This means that the predicted effect size (Δ) is a function of the treatment duration: Predicted $\Delta = B_0 + B_1{}^*$(treatment hours).

Thus, the predicted Δ for an intervention of 1hr's duration is $0.124 + 0.046 = 0.17$ (a relatively weak effect size), whereas that for a 10-hr intervention is $0.124 + 0.46 = 0.584$ (a moderate effect size and close to the omnibus effect size reported in Table 1). The interventions in these studies varied widely in their duration (*Min* = 1 hr; *Max* = 57 hr; $M = 10.3$, $SD = 8.8$), and this variability helped to account for the variation in effect sizes between studies. For the T–AT comparisons, dosage was also a significant predictor of treatment efficacy, with a slope very similar to that observed for the T–NT effect sizes ($B_1 = 0.043$).

Offense severity was also a significant predictor of study effect size ($B_1 = 0.012$) in the T–NT comparisons. In addition, severity of offense was a significant moderator of T–AT effect sizes, with a much steeper slope ($B_1 = 0.041$) than that for T–NT effect sizes. As the severity of the offense (and presumably the difficulty of forgiving) increases, the advantage of forgiveness treatments over generic treatments increases.

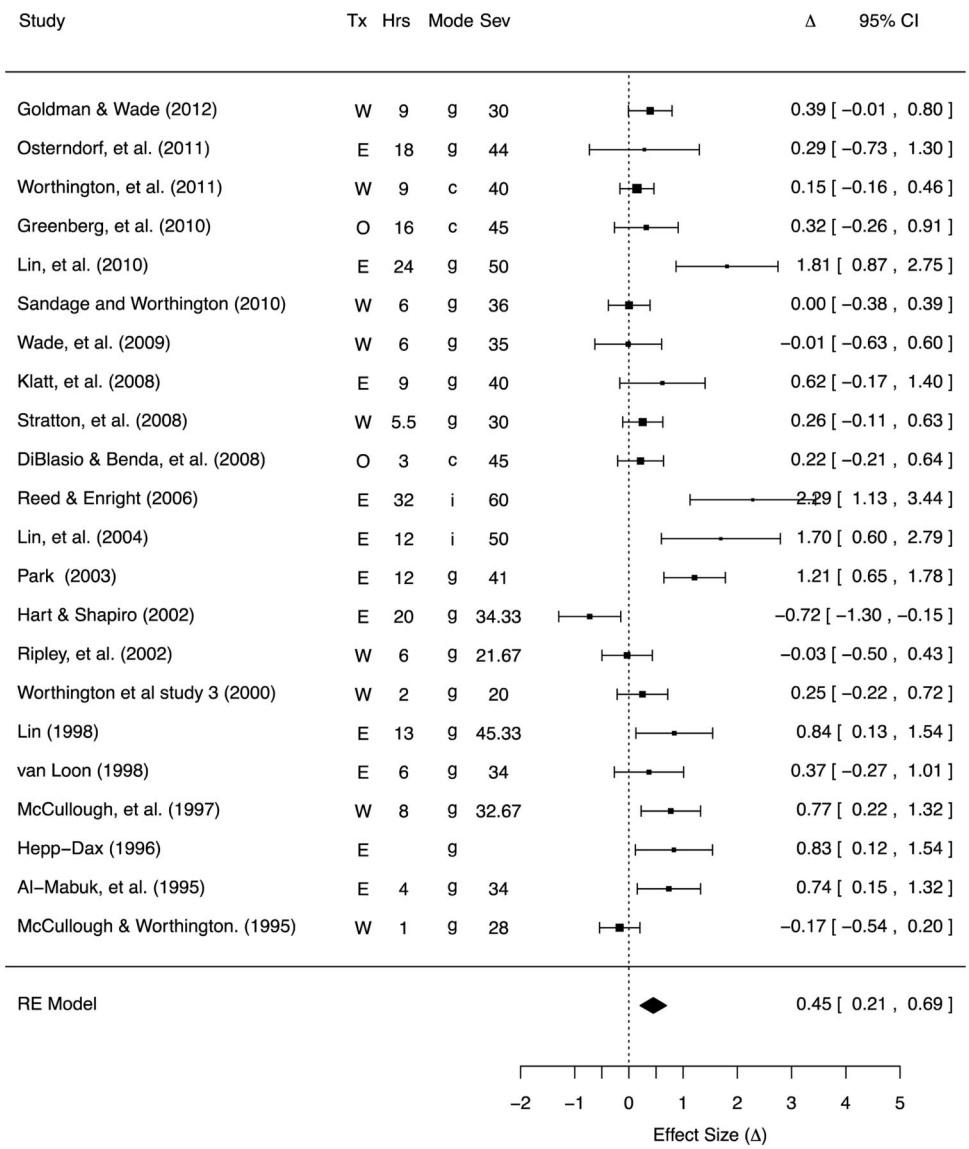

Figure 3. Forgiveness interventions: Treatment–alternative treatment comparisons. Tx = treatment model; W = Worthington, E = Enright, and O = other treatment model. Mode = treatment modality; g = group, c = couples, and i = individual therapy modality. Sev = offense severity. Hrs = hours; CI = confidence interval; RE = random effects.

In Table 3, we show the pattern of findings for significant categorical moderators. For both T–NT and T–AT comparisons, intervention model was a significant moderator of outcome. Interventions based on the Enright model were significantly more effective than those based on the Worthington model, and other interventions were intermediate. For both T–NT and T–AT comparisons, treatment mode was also a significant moderator. In both cases, individual interventions were more efficacious than either couple or group interventions.

Multiple moderator analyses. The results of single-moderator analyses may be misleading due to confounding among moderator variables. For example, in forgiveness interventions, it is common to offer longer treatments for more severe offenses (thus confounding treatment duration with offense severity) and also to offer individualized rather than group treatment for severe offenses (thus confounding treatment modality with offense severity). It is therefore recommended to use meta-regression to examine unique effects of a moderator variable, controlling for the effects of other study characteristics that may be correlated (Viechtbauer, 2007). In Table 4, we show the results of meta-regressions of effect size onto those moderators that emerged as significant in the single-moderator analyses, for both T–NT and T–AT comparison.[4] Categorical moderators were

[4] Prior to conducting the meta-regressions, we assessed for collinearity among the variables in the analyses. VIFs (variance inflation factors) for the final models displayed on Table 4 were all smaller than 5.0. By convention, many methodologists use the "rule of 10" to interpret the VIF statistic—that is, VIF $>$ 10 creates doubts about the results of the analysis and triggers steps to reduce multicollinearity before finalizing the model (O'Brien, 2007).

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Table 1
Omnibus Effect Sizes and Heterogeneity Tests With Forgiveness as a Dependent Variable

Comparison	k	Δ_+	95% CI	Q	p	I^2
No treatment	53	0.56	[0.43, 0.68]	188.91	<.0001	72%
Alternative treatment	22	0.45	[0.21, 0.69]	72.39	<.0001	77%

Note. Studies were modeled as random effects. k = number of studies; Δ_+ = effect size (standardized mean difference controlling for preintervention scores; Becker, 1988); CI = confidence interval; Q = homogeneity test; p = probability value for Q statistic under H_0 ($df = k - 1$); I^2 = percentage of variance in effect sizes that is attributable to systematic variation.

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dummy coded, with Enright-model interventions as the reference group for the treatment model variable and individual interventions as the reference group for the treatment modality variable.

For T–NT comparisons, only dosage (i.e., treatment duration in hours; $B = 0.033$) and modality (specifically, the contrast between group and individual treatment modes; $B = -0.57$) emerged as unique moderators of study effects. When dosage and modality were controlled, treatment model was not a significant predictor of study effect size. This means that the advantage for Enright-model interventions observed in the single-moderator analyses was an artifact of the confounding of those study characteristics with treatment dosage and treatment modality. Enright-model interventions tended to be substantially longer in duration than Worthington-model interventions (Ms = 15.4 hr and 5.6 hr, SDs = 12.3 and 3.2, respectively). In addition, nearly one third of studies using the Enright model involved individual interventions, compared with none of the studies using the Worthington model. When these differences were controlled, the intervention models did not differ in efficacy.

Figure 4 shows the practical import of the significant moderator findings for T–NT comparisons (Johnson & Huedo-Medina, 2011). The three panels of this figure depict separate predicted dose–effect trend lines for the three treatment modalities (group, individual, and couples), along with 95% confidence bands for these predicted effect sizes. In all three panels, the lower confidence band is above the zero point on the y axis, indicating that even for very low dosages (i.e., a single hour of intervention) the predicted effect size differs significantly from zero. Although all effect sizes in the data set contribute to the estimation of each meta-regression line, we highlight the points in each panel representing effect sizes for the relevant treatment modality, with the data points representing other intervention modalities shown in light gray. So we can see that, in the first panel, group interventions lasting 1–5 hr are predicted to have relatively weak (although still statistically significant) effects on forgiveness; interventions of about 10 hr in duration should have a moderate effect (i.e., $\Delta = 0.5$), and those lasting 18–20 hr a large effect (i.e., $\Delta = 0.8$). By comparison, an individual intervention lasting only 5 hr is predicted to have a large effect ($\Delta = 0.8$), with correspondingly higher effect sizes for interventions of longer duration.

For T–AT comparisons, none of the moderators emerged as significant ($p < .05$) in the multiple moderator analysis. Given the lower power of these tests ($k = 21$), we note two marginally significant findings ($p < .10$), which should be interpreted cautiously. Reflecting the T–NT studies, a marginally significant advantage was observed for individual treatments, this time relative to couple treatments ($B = -1.27$, $p = .067$). In addition, offense severity was found to be a marginally significant moderator of effect size in these studies ($B = 0.045$, $p = .068$), indicating a trend for forgiveness interventions to show more of an advantage over non-specific treatments when offense severity is higher.

Publication bias. We created funnel plots based on residuals from the final models for both T–NT and T–AT studies to examine the pattern of effect sizes for evidence of publication bias. The residuals (plotted against their SEs) form an inverted funnel; when publication bias is present, the base of the funnel (corresponding to studies with the smallest Ns and therefore the largest SEs) may show only extreme values (i.e., may show a dearth of residual values close to zero). This pattern was not observed for either T–NT or T–AT studies. In both cases, the funnel plot was symmetrical.[5]

Follow-up data. Follow-up intervals for the different treatments ranged considerably, from a low of 2 weeks to a high of 36 weeks ($M = 11.1$, $SD = 8.4$). For the subset ($k = 18$) of T–NT studies that included a follow-up assessment for both treatment and control groups, the effect size comparing change from baseline to follow-up was $\Delta = 0.45$, 95% CI [0.27, 0.62]. The baseline-to-postintervention $\Delta = 0.39$, 95% CI [0.22 to 0.55], and the postintervention- to-follow-up $\Delta = 0.06$, 95% CI [−0.07, 0.18], suggesting that treatment gains were maintained over the follow-up interval. Moderator tests for this subset of T–NT comparisons (baseline to follow-up interval) yielded a significant effect only for intervention dosage: $B = 0.08$, 95% CI [0.02, 0.13].

In Figure 5, we show separate average trajectories over time for treatment ($k = 41$) and control ($k = 18$) groups that provided follow-up assessments. For this larger subset of treatment groups, $g_T = 0.78$, 95% CI [0.60, 0.95], from baseline to follow-up, with no significant change in forgiveness from postintervention to follow-up, $g_T = 0.07$, 95% CI [0–.03, 0.16]. Improvement in the control group was significant from baseline to follow-up, $g_C = 0.14$, 95% CI [0.05, 0.23], and also from postintervention to follow-up, $g_C = 0.08$, 95% CI [0.02, 0.14], but not over the shorter interval between pre- and postassessments, $g_C = 0.06$, 95% CI [0–.02, 0.15]. In summary, the follow-up analyses suggest a pattern of strong improvement in the treatment group postintervention followed by maintenance of gains at the follow-up assessment, with much slower but still significant increases in forgivingness for the control group over time.

We also conducted moderator analyses for the effect size between pretreatment and follow-up for studies including both treatment and control data at these two time points. Treatment dosage was the only significant moderator for this reduced ($k = 18$) subset of studies, $\Delta_+ = 0.064$, 95% CI [0.005, 0.123]. (None of the studies of individ-

[5] Based on a request from a reviewer, we conducted significance tests for asymmetry in these funnel plots, based on procedures recommended by Egger, Smith, Schneider, and Minder (1997), regressing effect sizes converted to standard normal deviates (z scores) onto the inverse of the standard errors for these effect sizes. Sutton (2009) noted that this procedure is not appropriate when significant moderators are observed and proposed an alternative method (including these study characteristics as covariates in the regression model) –although he noted that evaluation of the performance of this approach "is an ongoing work" (p. 441). When following Sutton's proposal, we found no evidence of significant asymmetry in these plots (Bs = −0.09 and −0.39, ps = .47 and .36 for no-treatment and alternative-treatment plots, respectively).

Table 2
Significant Single-Moderator Analyses—Continuous Moderators

Variable	k	B_0	B_1	95% CI (B_1)	$z(B_1)$	p
Treatment–no treatment comparisons						
Dosage (hours)	51	0.102	0.047	[0.029, 0.064]	5.33	<.0001
Offense severity	47	0.120	0.012	[0.002, 0.022]	2.25	.024
Treatment–alternative treatment comparisons						
Dosage (hours)	21	−0.024	0.043	[0.009, 0.077]	2.45	.014
Offense severity	21	−1.072	0.041	[0.017, 0.065]	3.30	.001

Note. Univariate analyses used a mixed model (studies random, levels of moderator variables fixed). Significant categorical moderators are tabulated separately. k = number of studies; B_0 = intercept; B_1 = slope; CI = confidence interval; $z(B_1) = z$ statistic for B_1.

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ual or couples interventions included follow-up assessments, so modality could not be investigated as a moderator at follow-up.)

Additional Treatment Outcomes

In addition to assessing change in forgiveness, some studies included measures of psychological symptoms often associated with relational transgressions. Although those psychological symptoms were not targeted in the intervention, it is reasonable to hypothesize that if forgiveness was promoted, perhaps mental health symptoms would be mitigated. The most common of these symptoms were depression, anxiety, and hopelessness. Each of these outcomes was measured in only a subset of studies, so to understand the relative efficacy of forgiveness interventions for these other common outcomes, we compared the aggregate effect size for each outcome to the corresponding aggregate effect size for forgiveness in the same subset of studies. All outcomes were scaled so that a positive effect size reflects greater improvement for the intervention group relative to no-treatment controls.

For the subset of studies measuring depression ($k = 10$), the aggregate effect size for comparing change in depression between the forgiveness treatment and no treatment conditions was $\Delta_+ = 0.34$, 95% CI [0.17, 0.52]. In this same group of studies, the aggregate effect size for forgiveness was $\Delta_+ = 0.60$, 95% CI [0.26, 0.94]. The effect size for depression in these studies was 43% smaller than the effect size for forgiveness. Despite this numerical difference, a null hypothesis of no difference in outcomes for these two variables could not be rejected ($p = .09$). For the subset of studies measuring anxiety ($k = 7$), the aggregate effect size for anxiety was $\Delta_+ = 0.63$, 95% CI [0.0003, 1.26]. In this same group of studies, the aggregate effect size for forgiveness was $\Delta_+ = 1.34$, 95% CI [0.55, 2.12]. The effect size for anxiety was 50% lower than that for forgiveness; however this average difference was not significantly different from zero ($p = .21$). For the subset of studies measuring hope ($k = 6$), the aggregate effect size for hope was $\Delta_+ = 1.00$, 95% CI [0.38, 1.62]. For these same studies, the aggregate effect size for forgiveness was $\Delta_+ = 0.94$, 95% CI [0.16, 1.73]. Again, the difference was not statistically significant ($p = .96$).

In summary, forgiveness interventions, although not targeting mental health symptoms directly, resulted in reductions in depres-

Table 3
Significant Single-Moderator Analyses–Categorical Moderators

Variable	k	Δ_+	95% CI	Q	df	p	I^2
Treatment–no treatment comparisons							
Intervention model				8.54	2	.014	
Enright	20	0.82_a	[0.60, 1.03]	61.10	19	<.0001	69%
Worthington	18	0.35_b	[0.16, 0.54]	20.53	17	.248	17%
Other	14	0.55_{ab}	[0.33, 0.77]	78.30	13	<.0001	83%
Treatment mode				16.44	2	.0003	
Individual	6	1.44_a	[0.99, 1.89]	14.03	5	.049	64%
Couple	6	0.75_b	[0.44, 1.06]	16.60	5	.005	70%
Group	40	0.44_b	[0.31, 0.56]	115.59	39	<.0001	65%
Treatment–alternative treatment comparisons							
Intervention model				8.00	2	.019	
Enright	11	0.78_a	[0.46, 1.10]	44.60	10	<.0001	78%
Worthington	9	0.17_b	[−0.11, 0.46]	11.09	8	.197	28%
Other	2	0.26_{ab}	[−0.37, 0.89]	0.08	1	.773	0%
Treatment mode				11.28	2	.004	
Individual	2	1.98_a	[1.03, 2.92]	0.52	1	.47	0%
Couple	3	0.22_b	[−0.27, 0.71]	0.27	2	.87	0%
Group	17	0.37_b	[0.15, 0.60]	53.80	21	.001	70%

Note. Univariate analyses used a mixed model (studies random, levels of moderator variables fixed). Means that do not share a subscript differ significantly ($p < .05$). Significant continuous moderators are tabulated separately. k = number of studies; Δ_+ = effect size; CI = confidence interval; Q = homogeneity test. Q for the moderator assesses homogeneity between groups; Qs for the levels assess homogeneity within groups.

Table 4
Multiple-Moderator Analyses—Significant Single Predictors Only

Variable	*B*	95% CI	*p*
Treatment–no treatment comparisons			
Intercept	0.92*	[0.15, 1.70]	.020
Dosage (hours)	0.033*	[0.012, 0.055]	.002
Offense severity	−0.001	[−0.013, 0.012]	.873
Treatment model			
Worthington vs. Enright	−0.19	[−0.52, 0.14]	.270
Other vs. Enright	−0.25	[−0.58, 0.09]	.150
Modality			
Group vs. individual	−0.57*	[−1.05, −0.08]	.021
Couple vs. individual	−0.31	[−0.84, 0.22]	.251
Treatment–alternative treatment comparisons			
Intercept	−0.25	[−2.77, 2.28]	.849
Dosage (hours)	−0.01	[−0.057, 0.036]	.658
Offense severity	0.045[a]	[−0.003, 0.093]	.068
Treatment model			
Worthington vs. Enright	−0.03	[−0.69, 0.63]	.923
Other vs. Enright	−0.14	[−1.32, 1.03]	.810
Modality			
Group vs. individual	−0.79	[−2.00, 0.42]	.201
Other vs. Enright	−1.27[a]	[−2.63, 0.09]	.067

Note. Overall tests of model significance were $Q(6) = 35.74$ and 17.82; ps = < .0001 and = .007; ks = 46 and 21 for no-treatment and alternative-treatment comparisons, respectively. CI = confidence interval.
[a] Marginally significant ($.05 < p < .10$).
* $p < .05$.

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sion and anxiety and increases in hope; all effect sizes for mental health symptoms did not contain zero. Effect sizes comparing forgiveness interventions with no treatment ranged from .34 for depression to 1.00 for hope, indicating that forgiveness interventions may also help clients with psychological outcomes other than forgiveness. Direct comparisons of the effects for hope and the mental health symptoms showed that there were no significant differences between effects for these outcomes and effects for forgiveness. However, the significance test results must be interpreted with caution because of the small number of studies (and consequent lack of statistical power). Based on the numerical differences, effects of forgiveness interventions for reducing negative affect (depression and anxiety) were 40%–50% lower than those for forgiveness in the same subset of studies. Effects for increasing hope were similar in magnitude to those for forgiveness.

Discussion

Several notable findings emerged from this meta-analysis. First, interventions designed to promote forgiveness are more effective at helping participants achieve forgiveness and hope and reduce depression and anxiety than either no treatment or alternative treatments. Additionally, the specific treatment model used did not make a difference in outcomes. From our

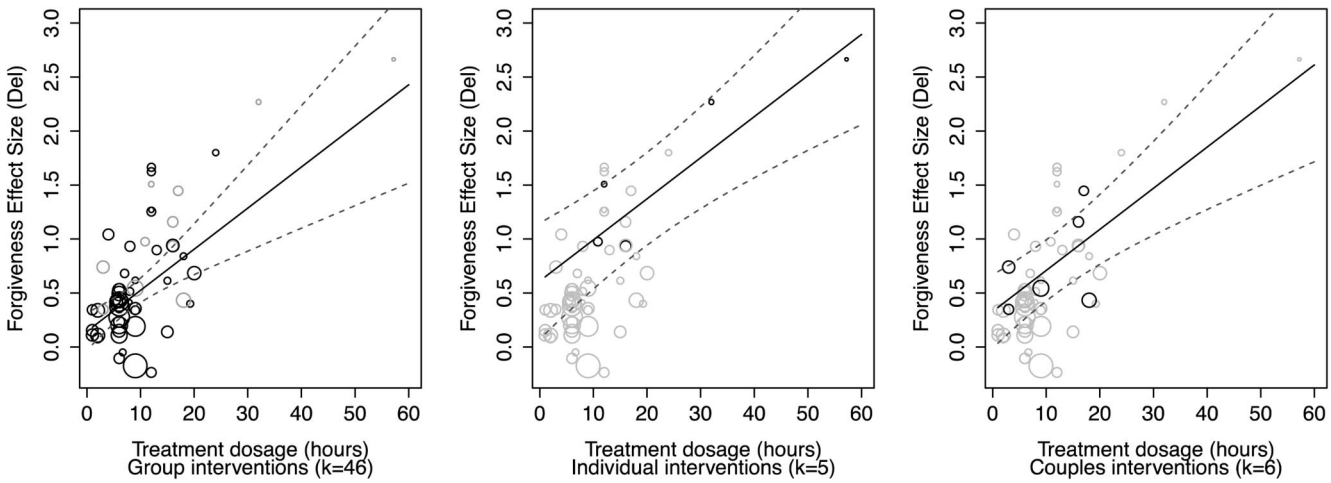

Figure 4. Dose–effect relationship for three treatment modalities (treatment vs. no treatment comparisons): group, individual, and couples interventions. Del = delta.

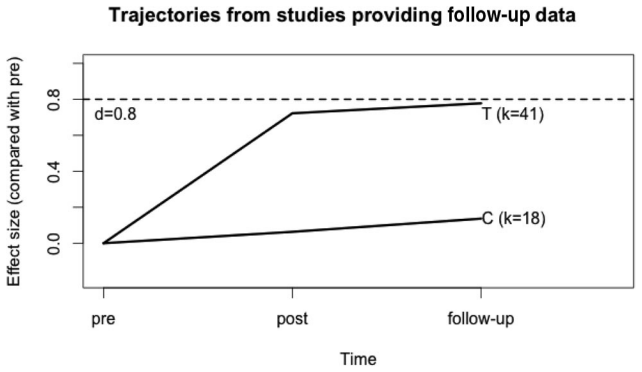

Figure 5. Growth in forgiveness for treatment (T) and no-treatment control (C) groups, baseline to follow-up.

results, it appears that using theoretically grounded forgiveness interventions is a sound choice for helping clients to deal with past offenses and helping them achieve resolution in the form of forgiveness.

Second, treatment dosage was an important predictor of forgiveness as an outcome when comparing forgiveness interventions to no treatment, though individual treatment (vis-a-vis group treatment) also makes a difference. As measured in this study, treatment duration was the amount of time that therapists worked specifically with the participants to help them forgive. This fits with findings from past meta-analyses (e.g., Worthington, Sandage, & Berry, 2000), research on typical patterns of forgiveness in college students (e.g., McCullough, Fincham, & Tsang, 2003), and similar changes following various interventions to promote forgiveness (e.g., Wade et al., 2009). With more time, people are generally able to develop more forgiveness, even those not receiving a forgiveness intervention (McCullough, Luna, Berry, Tabak, & Bono, 2010). For those who are receiving forgiveness treatment, shorter interventions promote less forgiveness than do longer interventions. However, the specific forgiveness model does not seem to make a difference when duration of treatment and modality are controlled. The relationship between duration and effect size also seems to account for other potential moderators such as severity of the offense.

Another finding of note was that offense severity was positively correlated with forgiveness as an outcome for the forgiveness versus alternative treatment comparisons. One possible reason for this correlation is that a confound exists between severity and duration of treatment. Severe transgressions tend to be treated longer. The results of the multiple moderator analyses do not fully support this explanation; a relationship between forgiveness outcome and offense severity was still suggested ($p = .068$) after including treatment dosage in the prediction model. If a relationship between offense severity and forgiveness outcomes does exist, another factor that might explain this relationship is that those who were more severely offended may have had more room to change in terms of forgiveness. If more severe offenses result in less forgiveness (which basic research supports they do; e.g., Fincham et al., 2005), then those with more severe offenses at the start of the intervention may have had the opportunity for greater changes in forgiveness than those who experienced less severe offenses. In addition, those with more severe offenses may have responded more positively to the explicit forgiveness interventions than those with less severe offenses. Perhaps those who have been more dramatically hurt may need more focused attention on the hurt and the healing and forgiveness process.

Finally, in the analyses of studies that included follow-up data collection, the overall delta estimating change in forgiveness indicated that on average clients achieve about .78 standard deviations of change at post-treatment and maintain that change at follow-up (see Figure 4). Furthermore, these changes appear to persist over time, suggesting that not only do forgiveness interventions help clients achieve forgiveness but that forgiveness is maintained following treatment (e.g., Blocher & Wade, 2010).

Implications

The present findings have several implications. First, many psychotherapists use forgiveness interventions, presumably because interpersonal difficulties are prevalent in most counseling. Thus, focus on forgiveness can be expected to provide not only an experience of increased forgiveness, but it can also provide psychotherapeutic benefit in treatment of depression and anxiety, and it can provide a benefit of hope, illustrating that forgiveness interventions might not only help to remediate problems (e.g., depression) but enhance human functioning as well.

Second, it seems not to matter as much which program is employed (i.e., Enright's, Worthington's, or some other) as how long the psychotherapist and client work on forgiving. That is not to say necessarily that any treatment is equally efficacious for developing forgiveness. Genuine forgiveness interventions showed clearly superior efficacy over alternative treatments. Unfortunately, the alternative treatments were not one single alternative treatment, but included a range of treatments, some of which were true alternative psychotherapies and some not. Still on average, explicit forgiveness interventions were more effective at promoting forgiveness than were the alternatives. Individual research projects indicate that forgiveness interventions might be more effective than typical psychotherapeutic interventions (e.g., Lin et al, 2004; Reed & Enright 2006) for dealing with some problems. Still only a few psychotherapeutic treatments that have been tested against explicit forgiveness interventions, so it is currently impossible to draw definitive conclusions about the superior efficacy of explicit forgiveness interventions.

Third, treatment duration seems to be a crucial element in promoting forgiveness and other mental health benefits associated with forgiveness interventions. In most general psychotherapy, attention to forgiveness takes a minor part of psychotherapy—perhaps as little as 2 or 3 hr (DiBlasio & Benda, 1991). Given the strong relationship between time spent in explicit forgiveness intervention and the promotion of forgiveness, general psychotherapy might be supplemented by adjunctive forgiveness-promoting treatments such as psychoeducational groups. General psychoeducational groups to promote forgiveness can include people with a variety of interpersonal transgressions within any group. Thus, adjunctive psychoeducational groups could be extend the amount of forgiveness and benefit to improving depression, anxiety, and hope. This is especially important given that modality of delivery (i.e., to individuals, couples, or groups) seemed to matter little in the amount of benefit participants derived.

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Fourth, the advantage of individual interventions over the more common group modalities was apparent for T–NT comparisons, and individual interventions were marginally superior to couples, but not group, interventions for T–AT comparisons. Relatively few studies have examined the effectiveness of individual counseling for forgiveness, and of the six we located for this meta-analysis, five (83%) used the Enright model (the sixth was a test of emotion-focused therapy to promote forgiveness; Greenberg et al., 2008). Thus, while these results are important and suggest that further study of individual forgiveness interventions is warranted, one should be cautious about generalizing beyond the types of treatments examined here. Perhaps the surest conclusion is that individual implementations of the Enright model have been substantially more effective than group implementations (although the group implementations themselves lead to significant improvement relative to what would be expected in the absence of treatment).

Fifth, questions arise about the active ingredients within forgiveness treatments. Wade and colleagues (2005) tried to describe the effects of seven elements that were roughly modeled on Worthington's REACH steps (plus explicitly defining forgiveness and incorporating other interventions like relaxation or anger-management methods). Given the strong dose–response relationship we have found, it is reasonable to inquire whether any particular technique contributes to outcomes more than any other. Yet common sense suggests that there would be some aspects of any treatment that are not therapeutically active. Future research must determine what might be omitted from treatment protocols to provide a more time-efficient intervention.

Sixth, much is still unknown about the nature of the transgressions that are most appropriate as targets for a forgiveness intervention. Is there an optimal time to intervene after a transgression is experienced? Is there an optimal amount of severity of initial harm? Given that time seems to decrease unforgiving emotions and motivations following a power law (McCullough et al., 2010), is there an optimal amount of residual unforgiveness that would be most responsive (or least responsive) to an intervention? Sixth, no effort was made to evaluate which treatments, if any, would be particularly effective for which types of transgression. For example, the emotionally focused treatment (EFT) by Greenberg, Warwar, and Malcolm (2008, 2010) might be particularly effective for couples in couple therapy, given the status of EFT as empirically supported (see Baucom, Shoham, Mueser, & Daituo, 1998), and Worthington's REACH model might be particularly effective for couple enrichment, given the status of his hope-focused couple approach as an empirically supported couple enrichment intervention (see Jakubowski, Milne, Brunner, & Miller, 2004). However, this is speculation because this was not tested in the present study. We note that Enright's model has successfully been used as psychotherapy for several severe problems. Even though statistically significant, our regression analyses show that duration of treatment might be the key variable in treatment success, we must note that Worthington's model has not been tested in long interventions with severe offenses. Thus, research is needed to test whether it would actually be as successful as Enright's or other models with such severe problems and long durations.

Limitations

Although we located an adequate set of studies that measured forgiveness as an outcome, the outcomes of depression, anxiety, and hope were reported less often. Therefore, our analyses assessing the effects of the interventions on these variables were limited. Although we were able to assess change over time, there were not enough studies to conduct the same moderator analyses used with forgiveness for these outcomes. The results showing change in depression, anxiety, and hope are based on a smaller, more select set of studies and therefore should be viewed with caution. Likewise, the follow-up assessments for the control groups, especially the no treatment conditions, were limited. Although a respectable percentage of studies included follow-up assessments of the treatment conditions, fewer reported follow-up data for control participants who often entered the treatment phase following the post-assessment. Therefore, this limits our confidence in our results about how people who are not in treatment or who are waiting for treatment change in terms of forgiveness over a longer time.

Another limitation of this review is that the only measures used to assess forgiveness in these studies were self-report measures. Although self-report measures are crucial for assessing internal and subjective experiences such as forgiveness, these measures may include biases from socially desirable responding or halo effects. Related to this limitation is the limitation of the offense severity rating system. Although we had strong reliability across raters, the degree of severity was grouped by study, making it a much broader measure than would be the case if individual client offenses were rated. Unfortunately, we did not have that data for most studies and could not provide that level of detail. Third, our measure of methodological quality was limited by the scope of the questions we assessed. Although our measures were not related to effect size, this does not mean that methodological quality was unrelated to outcomes. If assessed with different measures, effect size might be related to quality. In addition, we used methodological quality as a predictor of effect size whereas it could be used as a cut-off for only including studies in the meta-analysis with a certain level of quality. Because we wanted to cast a wide net on this literature and because quality was not related to effect size in our analyses, we included all studies that met our initial criteria.

A fourth limitation is that the moderator analyses are only correlational in nature. These were not included as part of an experimental design that would provide evidence of causation. Instead, the moderators we examined were only correlated with outcome and therefore do not indicate causation. For example, treatment duration is certainly related to outcome but we, cannot say from these data alone that the time spent intervening caused larger effect sizes. Finally, the failure of some studies to include a control or comparison group (i.e., use of the SGPP design) is a serious weakness, although we believe this design limitation can be largely overcome using the imputation procedures recommended by Becker (1988). This places severe restrictions on the ability to draw causal conclusions about improvement for these participants based on the findings of a single study, as the SGPP design fails to rule out several important threats to internal validity. However, the meta-analysis of control group improvement presented here provides a basis for comparison for these studies that helps to overcome this limitation and can eliminate the bias found by Carlson & Schmidt (1999) in simple pre-post effect sizes. The

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logic of this method is similar to that of benchmarking (using data from large-scale clinical trials as a basis for comparison; e.g., Minami, Serlin, Wampold, Kircher, & Brown, 2008), which is another technique for enhancing the validity of conclusions drawn from client data in the absence of statistical controls.

Future Research

Based on this review and meta-analysis, the results of research on basic outcome studies of individual and group treatments are apparent and robust; forgiveness interventions are helpful for many people and many different kinds of hurts. However, beyond the most basic questions, few questions have been answered. Specifically, researchers might conduct intervention studies that examine the relationship of counseling processes to forgiveness outcomes, comparing and contrasting what has already been established as important processes in the more general psychotherapy literature. In addition, future research could further specify the active ingredients of these interventions, focusing on common versus specific mechanisms of change (Wampold, 2001). Understanding who most benefits from these interventions is also a question that has not been thoroughly examined and would be very helpful for mental health professionals. Specifically, work with minorities (racial/ethnic, religious, sexual orientation) could be especially valuable to understand potential interactions between social justice, advocacy, and forgiveness intervention efforts. For example, researchers might examine interventions that help lesbian-gay-bisexual-transgender clients forgive experiences of discrimination in a way that promotes their individual mental health but does not limit their motivation to work for social change and to seek justice for themselves and others. Finally, forgiveness interventions provided in couple formats have not received near the research attention that individual and group modalities have. Therefore, more research into couple therapy that explicitly promotes forgiveness (e.g., Makinen & Johnson, 2006) and more general couple therapy would help to develop our understanding more in this important area.

In addition, future research utilizing different methods to measure forgiveness would help to advance the field. Although forgiveness is in many ways a subjective and internal experience, careful operationalization of forgiveness definitions and creative methodology to assess forgiveness in ways other than client self-report would be beneficial. Some intervention studies in the past have used an observer report (e.g., romantic partner, close friend; Rye et al., 2005). Measurements of behaviors associated with forgiveness might also be useful to validate self-reported forgiveness and add additional dimensions to the existing outcome studies.

Because forgiveness intervention appears successful in relieving depression and anxiety and promoting hope, researchers should consider including other psychological variables in future studies of forgiveness interventions. These might include posttraumatic stress, hostility, self-control, relationship satisfaction, well-being, spirituality, and job performance. Even physiological responses such as heart rate (and other peripheral physiological measures), heart rate variability (as a sympathetic nervous system variable), or brain activity might be fruitful avenues to pursue following forgiveness interventions. Because forgiveness seems to have a marked effect on people, it is likely that there are other factors associated with forgiveness and receiving a forgiveness intervention. In addition, research on other psychological interventions might include the development of forgiveness as a potential mediator or moderator of mental health. For example, researchers examining the efficacy of treatments for posttraumatic stress disorder (PTSD) could examine the development of forgiveness for an offender as a mediator of improved mental health (i.e., PTSD treatment improves forgiveness which in term reduces anxiety, intrusive thoughts, and the like). Future research could explore these and other ancillary benefits of forgiveness in more detail and specificity, which would further delineate possible benefits of receiving forgiveness interventions.

Conclusion

Overall, the status of the research to date suggests that forgiveness is a viable and evidence-based treatment for dealing with transgressions. These interventions are more effective than alternative treatments and no treatment in promoting forgiveness of the offender and hope for the future and reducing depression and anxiety. Results from this meta-analysis indicate that forgiveness treatments are robust and effects are maintained following the termination of the treatment. Although not enough research has been conducted to answer various specific questions about the efficacy of forgiveness interventions, it appears that the duration of the treatment is directly linked to amount of forgiveness achieved, whereas the specific treatment packages (e.g., Enright, Worthington) and modalities (e.g., individual, group) do not differentially predict outcome.

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