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### The Efficacy of Personal Construct Therapy as a Function of the Type and Severity of the Presenting Problem

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## THE EFFICACY OF PERSONAL CONSTRUCT THERAPY AS A FUNCTION OF THE TYPE AND SEVERITY OF THE PRESENTING PROBLEM

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*A recent quantitative review of the personal construct therapy (PCT) outcome literature revealed reliable but somewhat modest effects for this type of therapy. Examination of moderator variables has shown that the efficacy of PCT might vary as a function of other factors, such as whether or not the treatment was tested with a clinical population. In the present study, these findings were expanded on by exploring the relation between the type and severity of presenting problems and treatment outcome as reported in the controlled PCT outcome literature. Overall, results revealed that the efficacy of PCT did not differ substantially across different types of problems, but effect sizes tended to be significantly smaller when more severe problems were being treated.*

Over the past 50 years, George Kelly's (1955/1991) psychology of personal constructs has developed into an internationally adopted form of psychotherapy that has been the subject of numerous texts, journal articles, and book chapters (Neimeyer, Baker, & Neimeyer, 1990; Neimeyer & Baldwin, 2005; Neimeyer & Martin, 1996; Winter, 1992). The growth and perseverance of personal construct therapy (PCT) may be attributed, in part, to its unique philosophical approach to understanding and treating problems, which is guided by a constructivist framework that emphasizes clients' subjective realities (Neimeyer & Bridges, 2003). Indeed, one of the primary tenets of PCT is that multiple viable constructions of reality can be creatively employed to make meaning out of even the most difficult life circumstances (Kelly, 1955/1991). Significantly, this way of thinking about life problems diverges markedly from more objectivist approaches, such as cognitive-behavioral therapy, that strive to achieve greater

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concordance between clients' thoughts and a knowable reality (Neimeyer, 1998).

Beyond these philosophical differences, PCT also draws on a variety of novel techniques that are designed to promote active experimentation with alternative life scripts and encourage clients to approach their lives in new ways. Specifically, PCT therapists often use interventions such as *fixed role therapy*, in which clients role play a character somewhat dissimilar from their current selves (Epting & Nazario, 1987; Kelly, 1955/1991), or *interpersonal transaction* groups, in which clients take turns interacting with other group members in rotating dyads, with the purpose of promoting greater exposure to different subjective realities (Landfield & Rivers, 1975; Neimeyer, 1988).

Despite the intuitive appeal of these novel interventions and other related techniques (Neimeyer & Bridges, 2003), a recent meta-analysis of the PCT outcome literature revealed an average effect size of .38 when PCT was compared to a waitlist control group, placebo, or standard care (Holland, Neimeyer, Currier, & Berman, 2007).<sup>1</sup> Although this effect was significantly greater than zero, it was notably smaller than those found for psychotherapy in general, which have shown average effect sizes ranging from about .75 to .85 (Wampold, 2001). In an effort to understand these findings, several moderating variables have been explored to see if the effect sizes calculated for a given study vary reliably as a function of other factors. For example, it has been found that PCT tends to yield substantially larger effect sizes when more traditional outcome measures are used whose content is predetermined and supplied by the researcher (e.g., paper-and-pencil questionnaires, behavioral observations) compared to more broad-based measures designed to tap into personal meanings of the client (e.g., repertory grids, content analysis scales; Holland et al., 2007).

In another meta-analytic study, it was revealed that PCT outcome studies conducted in clinical practice tended to produce smaller effects (Cohen's  $d = .34$ ) than studies that used non-clinical samples (Cohen's  $d = 1.04$ ; Metcalfe, Winter, & Viney, 2007). From a clinical perspective, this finding is of particular interest because it suggests that the effects of PCT may vary as a function of the problems being treated. Importantly, greater knowledge about PCT's relative performance across different

presenting problems could give clinicians valuable information about which clients are best suited for this type of treatment. Given the clinical implications of this research question, it is the purpose of this study to expand on Metcalfe and colleagues' (2007) findings and examine the efficacy of PCT (as reported in the published literature) across different presenting problems of varying types and levels of severity.

## Method

### *Studies*

This review was based on 22 studies that examined the effectiveness of PCT for a variety of physical and psychological problems. The studies were identified using two different methods. First, a comprehensive search was done through PsychInfo using combinations of search terms such as *personal construct*, *constructivism*, *fixed-role*, *interpersonal transaction*, and *treatment outcome*. In addition, reference sections of past reviews (Epting, 1981; Metcalfe et al., 2007; Viney, 1998; Viney, Metcalfe, & Winter, 2005; Winter, 2003) were consulted to obtain any missed studies.

Several criteria were used to select studies for inclusion in the review. First, only studies that explicitly stated that one of the tested treatments was based on personal construct theory or the work of George Kelly were included. Second, these outcome studies had to compare a PCT therapy to a group receiving no active treatment (i.e., waitlist control, no treatment, standard care, or inactive treatment) or to an alternate active treatment. Case histories and studies that used a pretest–posttest design (e.g., Leahy & O'Sullivan, 1987; Sheehan, 1985) were excluded from the review. Finally, as a way of permitting replication by future reviewers of the PCT outcome literature, only studies that appeared in the published literature and could be readily accessed were considered for inclusion in the current review.<sup>2</sup>

It should be noted that all controlled PCT outcome studies were included in the review, regardless of the method of assignment to conditions, as long as both groups were made up of broadly similar people. Although past research suggests that studies that nonrandomly assign participants to conditions (e.g., as with matched control groups) tend to yield smaller and more

variable posttest treatment effects (Shadish & Ragsdale, 1996), this trend did not hold in this sample of studies. Specifically, in this review studies that employed a nonrandom method of assignment were similar to those that used random assignment, in terms of both the magnitude of the effect and its variability. This finding mirrors results found in past reviews of the PCT outcome literature (Metcalf et al., 2007; Viney et al., 2005). Given the apparent similarity of these studies in the PCT literature, no distinctions were made in this review between studies that employed random versus nonrandom assignment to conditions.

Across these 22 studies, the typical client was a woman of 39 years of age who attended about 12 90-minute therapy sessions over a period of 5 months. More often than not, the client was seen by a female therapist. Further details about the descriptive characteristics of these studies have been reported in another publication (Holland et al., 2007).

#### *Moderator Variables*

In this review, both the type and severity of the problem treated were coded for each study. The studies selected for review examined the effectiveness of PCT with diverse populations of clients who were dealing with a variety of physical and psychological problems. These problems were grouped into five categories: (a) trauma and stress, (b) physical problems and aging, (c) anxiety and fear, (d) problematic behavior, and (e) psychosis and delusions. In this context, “trauma and stress” refers to the past or present experience of difficult life circumstances that might put one at risk for psychological distress. This category includes problems such as caring for someone with AIDS (Viney, Crooks, & Walker, 1995) or surviving childhood incest (Alexander, Neimeyer, Follette, Moore, & Harter, 1989). Studies that dealt with physical problems and aging worked with individuals who were potentially distressed but primarily identified as having some sort of physical problem such as chronic musculoskeletal pain (Haugli, Steen, Laerum, Nygard, & Finset, 2003) or medical hospitalization (Viney, Clarke, Bunn, & Benjamin, 1985a, 1985b). Problems categorized as “anxiety and fear” involved worry directed at some specific object (e.g., snakes; Lira, Nay, McCullough, & Etkin, 1975) or life situation (e.g., public

speaking; Karst & Trexler, 1970). The term “problematic behavior” was used to refer to concrete behaviors that are typically considered to be harmful to oneself (e.g., not exercising; Annesi, 2002) or to society in general (e.g., criminal behavior; Viney & Henry, 2002). Finally, one study tested the efficacy of a personal construct intervention with individuals exhibiting symptoms of formal thought disorder (e.g., tangential thinking, loose associations, word salad; Bannister, Adams-Webber, Penn, & Radley, 1975), and this type of problem was categorized as “psychosis and delusions.”

In addition to coding the types of problems addressed in each study, the severity of the problem was assessed by three advanced graduate students in clinical psychology and by one senior clinical psychologist. Raters indicated on a scale from 1 to 5 (1 being “not severe at all” and 5 being “extremely severe”) the degree to which they felt the problem being studied would bring about distress, impairment, or other negative consequences. These ratings were then averaged to obtain an overall severity score for each problem explored in a given study. The mean of these averaged ratings across all studies was 3.05, with a range of scores from 1.5 to 4.5. The reliability of these ratings was assessed using a two-way mixed model, intra-class correlation coefficient (ICC) for mean ratings of multiple judges, as proposed by Shrout and Fleiss (1979), and equaled .88.

### *Estimating Treatment Effects*

For all studies, each outcome measure was converted into a Cohen’s (1977)  $d$ , which is a standardized form of expressing the size of an effect. The posttest mean of the comparison group (e.g., placebos, waitlist controls, other treatments) was always subtracted from the posttest mean of the PCT group, and this difference was then divided by the pooled within = groups standard deviation. In some cases, high scores on an outcome measure indicated improvement, whereas for other outcome measures low scores indicated greater improvement. When lower scores on an outcome measure were considered signs of improvement, the positive or negative valence of the effect size was adjusted in such a way that a positive  $d$  always denoted an advantage for the PCT group.

All effect sizes within a study were calculated, with a few exceptions. Measures taken midway through the treatment were not converted into effect sizes. In addition, effect sizes were calculated only when a reasonable conclusion could be drawn about what constituted positive change on a given outcome measure. Thus, in one study of a PCT intervention for agoraphobia (Winter et al., 2006, seven scores derived from a repertory grid measure were not used, as it was not clear whether high or low scores indicated improvement. Finally, because the primary purpose of this investigation was to examine the relation between facets of the problem being treated and outcome, rather than estimating the overall efficacy of PCT—which has been reported elsewhere (e.g., Holland et al., 2007; Viney et al., 2005)—only posttest effect sizes were used to test these moderating variables. Specifically, only 14 studies reported follow-up data, which was considered insufficient for this analysis.

Overall, 217 effect sizes were used in these analyses, which were calculated across the 22 studies. Of these, 42.4% were calculated either directly from means and standard deviations or from other statistics such as a  $t$  statistic or an  $F$  ratio with 1 degree of freedom in the numerator and did not require the use of estimation procedures. When this information was not provided, effect sizes were estimated using a variety of techniques (Glass, McGaw, & Smith, 1981; Miller & Berman, 1983).

Specifically, in 18.9% of cases means and standard deviations were available, but the researchers only reported the overall number of participants. In this situation, Cohen's  $d$  was calculated by assuming that an equal number of participants were in each condition. Additionally, for 13.4% of cases results were simply reported as "nonsignificant" or researchers mentioned using an outcome measure in their methods section but did not report any findings for it in their results. When only limited information of this kind was given for a measure, the corresponding effect size was conservatively estimated to be zero, so as to not bias the results by excluding unreported findings that likely disconfirmed the experimenter's hypothesis. Although such an approach avoids excluding null results and overestimating the overall effect size, it should be noted that past researchers have found that this procedure can often have the opposite result of underestimating effects (Ray & Shadish, 1996). Nevertheless, in theory this approach

offers an unbiased estimate in that it can conceivably overestimate (i.e., in cases when the comparison group outperformed the PCT group) or underestimate the effect size.

Finally, 12% of the time, group means and number of participants were given, but the pooled standard deviation had to be estimated from an  $F$  ratio with more than 1 degree of freedom in the numerator or from a standard deviation reported for a different assessment period or treatment condition. The remaining 13.3% of effect sizes were estimated using a variety of other procedures, which derived estimates from probability values, change scores, and quantitative information for individual participants. Techniques for computing effect sizes based on this sort of information have been outlined in previous work (e.g., Glass et al., 1981; Miller & Berman, 1983). Overall, effect sizes computed directly from sample means and standard deviations were less than .01 of a standard deviation larger than those calculated using estimation procedures, indicating that any assumptions made did not seem to introduce systematic bias into the calculation of effect sizes.

Finally, it has been established that estimates of effect size based on small samples tend to overestimate the effect size (Hedges, 1984). To correct for this small-sample bias, Hedges' (1984, Formula 4) correction was applied to all effect sizes reported in this review.

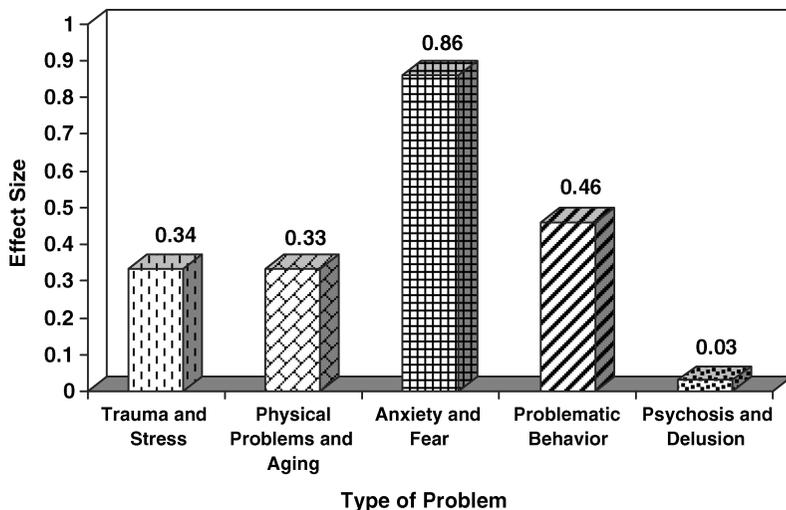
## Results

Effect sizes were first averaged across all measures of outcome within each study for PCT vs. no active treatment comparisons and PCT vs. active treatment comparisons.<sup>3</sup> The homogeneity of these effect sizes was then examined to determine if the  $d$ s varied beyond what would be expected by sampling error alone. The test for homogeneity of variance was not significant for PCT vs. no active treatment comparisons at posttest ( $Q_w = 25.26$ ,  $df = 18$ ,  $p = .12$ ) or for PCT vs. active treatment comparisons at posttest ( $Q_w = 3.82$ ,  $df = 7$ ,  $p = .80$ ). Although the variability among effect sizes did not significantly exceed what would be expected by sampling error, a random effects model was employed in this study, which assumes that the variability among effect sizes is attributable

to both sampling error and random study differences. This analytic strategy was chosen because it provides a more conservative estimate of the standard error, thereby providing an additional safeguard against making Type I errors. Above and beyond the variation due to sampling error and random study differences, systematic sources of variation were also explored through “mixed effects” analyses using the two proposed moderator variables. Although there was little empirical warrant to search for systematic variation (i.e., significant heterogeneity was not detected), when there is limited power to detect heterogeneity and sufficient justification for examining specific research questions, it is considered acceptable to test a limited number of moderator variables (Hedges, 1994).

Because studies based on larger samples tend to represent more stable estimates of the effect, all analyses were weighted by the inverse of the conditional variance (Shadish & Haddock, 1994). The mean weighted effect size across the 19 studies that compared PCT against a no-active-treatment group was .40, which was significantly greater than zero (95% confidence interval, .24–.57;  $p < .001$ ).<sup>4</sup> However, for the eight studies that compared PCT against an active treatment, the overall weighted effect size equaled .21 and was not statistically significant (95% confidence interval, -.01–.43;  $p = .06$ ).

Beyond these overall effects, differences in effect sizes across different types of presenting problems were examined using the analog to analysis of variance (ANOVA) test. Parameters for this test were estimated by means of a restricted information maximum likelihood (REML) procedure. When the effect sizes based on PCT vs. no-active-treatment comparisons were examined, this analysis did not reveal significant differences in effect size across the different types of presenting problems ( $p = .54$ ).<sup>5</sup> The mean effect sizes for each problem type are presented in Figure 1. Although significant differences were not found overall, it is noteworthy that PCT tended to yield particularly large outcomes ( $d = .85$ ) for problems relating to anxiety and fear, which were significantly greater than zero ( $p = .004$ ). In contrast, the effect size found for the treatment of psychosis and delusion was considerably smaller than the other effect sizes ( $d = .03$ )<sup>6</sup> and was not significantly greater than zero ( $p = .95$ ). Finally, for effect sizes based on PCT vs. active treatment comparisons, no significant



**FIGURE 1** Mean effect sizes for PCT vs. no active treatment comparisons across different types of presenting problems.

differences were found between different types of presenting problems ( $p = .84$ ).

Next, the severity of the problems treated was examined as a potential moderator variable using a weighted least squares regression analysis, which was weighted by the inverse of the conditional variance. A REML procedure was also used in this analysis. Interestingly, for PCT vs. no treatment comparisons, the rated severity of presenting problems was found to be negatively associated with the effect sizes, indicating that outcomes tended to be smaller when more severe problems were being treated. In this test, severity accounted for 29.3% of the variance in effect sizes, which was statistically significant ( $p = .007$ ). With regard to the effect sizes for PCT vs. active treatment comparisons, the severity of presenting problems was not found to be significantly associated with treatment outcome ( $p = .41$ ). However, there was a trend for effect sizes to be smaller when more severe problems were being treated ( $R^2 = .18$ ).

A final analysis was performed using an analysis of variance test to see if the type and severity of the presenting problem variables were confounded with one another. This test revealed that there were no significant differences in problem severity between

the different types of problems,  $F(4, 16) = 1.19, p = .35$ . However, it should be noted that the relative ordering of severity ratings did share some overlap with the magnitude of effect sizes. Specifically, problems related to anxiety and fear tended to be rated as less severe than other problems ( $M = 2.23$ ); problems related to psychosis and delusion were rated as more severe than other problems ( $M = 4.38$ ); and physical problems and aging ( $M = 2.97$ ), trauma and stress ( $M = 3.07$ ), and problematic behaviors ( $M = 2.99$ ) tended to cluster together with severity ratings in between these two extremes.

## Discussion

Overall, this review suggests that PCT seems to perform similarly across different types of problems. However, it is notable that there was a tendency for this therapy to perform well with problems related to anxiety and fear, exhibiting an average effect size of .86, which more closely matches the effects found for psychotherapy in general (Wampold, 2001). In personal construct terms, anxiety is associated with confrontation of situations in which a person lacks the ability to anticipate or meaningfully interpret events (Kelly, 1955/1991). In view of this, the tendency of personal construct strategies like fixed role therapy to offer clients carefully crafted temporary “make believe” identities while engaged in feared activities (e.g., public speaking or approaching a feared animal) might be particularly relevant, insofar as it offers a provisional structure for approaching the feared situation in a more meaningful way.

Additionally, although there is some anecdotal evidence suggesting that PCT might be beneficial for clients exhibiting symptoms of schizophrenia (e.g., Procter, 1987), this review has indicated that presently there are no experimental data supporting the efficacy of this treatment with this population of clients. Here, it might be the case that, even if a personal construct understanding of psychosis as loose and inconsistent construing of events (Bannister et al., 1975) is phenomenologically adequate, it either fails to address the underlying etiology of the symptoms in a useful way or is insufficiently redressed in the sort of brief and language-focused intervention that has been tested to date.

Perhaps the most interesting finding that emerged from this review was that the severity of presenting problems, as judged by independent raters, was found to be negatively associated with treatment outcome when PCT was compared against a waitlist control, placebo, or standard treatment group. In other words, PCT was found to be less efficacious when treating more severe problems. However, it should be stressed that this finding should not be taken as evidence suggesting that PCT is somehow less effective at treating severe problems compared to other therapies. Indeed, the relation between problem severity and treatment outcome was found to be nonsignificant when PCT was compared against an active treatment, although there was a modest trend in the same direction. Given this pattern of results, one explanation for why PCT has tended to yield lower effects overall compared to psychotherapy in general might be that researchers in this area have, in many cases, truly put this therapy to the test by examining its efficacy with a host of rather challenging presenting problems, such as deliberate self-harm (Winter et al., 2007), childhood incest (Alexander et al., 1989), and psychotic symptoms (Bannister et al., 1975).

Notwithstanding these thought-provoking findings, it should be noted that this study was constrained by several limitations. First, the small sample of available studies limited the statistical power in all analyses, making it somewhat difficult to reach many firm conclusions about relations among variables. Furthermore, even in this small sample of studies, in many cases information was insufficiently reported or there were other methodological shortcomings (e.g., nonrandom assignment to conditions) that limited the usefulness of the data. In this review, a best attempt was made to salvage all information by using estimation procedures when possible to derive an effect size for each measure used in a given outcome study. However, because a meta-analytic review is only as strong as the studies included within it, these findings should be interpreted with some caution.

It should also be noted that there was slight overlap between the two moderator variables, making it somewhat difficult to discern whether the relation between problem severity and treatment outcome was influenced by the type of problem. Likewise, it is possible that differences in outcome observed across different types of problems might have been affected by differences in

levels of severity. Thus, future studies would do well to examine this issue further, perhaps by investigating the efficacy of PCT with different problems of similar severity or by looking at treatment outcome among participants experiencing the same problem at different levels of severity.

Despite these limitations and possibilities for future research, this study represents a useful expansion of previous findings about the effectiveness of PCT in clinical settings (Metcalf et al., 2007) and highlights the relevance of symptom severity, rather than problem type, as a predictor of treatment outcome in the PCT literature.

### Notes

1. Standard care refers to a form of treatment that did not include an active psychological component and was customarily administered for the population of interest or the particular setting in which the study took place. For example, in one study of a PCT treatment for patients with musculoskeletal pain (Haugli et al., 2003), participants in the control group met with a physician and adhered to whatever treatment suggestions he or she made, which was standard protocol for patients with this problem.
2. The influence of publication bias in the PCT literature has been explored in previous publications (Holland et al., 2007; Metcalfe et al., 2007; Viney et al., 2005). In general, it appears that unpublished studies have yielded somewhat smaller effect sizes.
3. Waitlist, no treatment, standard care, and inactive treatment controls were all grouped as “no active treatment” because of the similarity of the effect sizes calculated across these different comparisons. In fact, effect sizes based on comparisons with placebo treatments and standard care on average tended to be somewhat larger than those based on waitlist or no treatment comparisons, which is the opposite of what might be expected.
4. This overall effect size differs somewhat from that reported in a previous publication (Holland et al., 2007). This difference is due to the updating of this data set with the results of a recent study by Winter and colleagues (2007).
5. Watson & Winter’s (2005) study was not coded for the type of presenting problem, because the sample included participants presenting with a multitude of diverse problems. Hence, this study was not included in any of the analyses involving this variable.
6. It should be noted that this effect size was based on one study (Bannister et al., 1975) and was calculated entirely with estimation procedures, which in this case mostly involved in-putting an effect size of zero for measures that were reported as not yielding significant differences between the treatment and control group at posttreatment. Thus, this effect size should be interpreted cautiously.

7. In References, \* indicates reference included information about a PCT outcome study that was used in the review. Some references represent the same study. In addition, Viney, Metcalf, and Winter's (2005) review provided extra information that had previously been unpublished for multiple PCT outcome studies. Superscripts indicate the problem type assigned to each study, whereby 1 = trauma and stress, 2 = physical problems and aging, 3 = anxiety and fear, 4 = problematic behavior, and 5 = psychosis and delusions.

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