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The efficacy of problem solving therapy in reducing mental and physical health problems: A meta-analysis

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Abstract

This paper describes a meta-analysis of 31 studies that examined the efficacy of problem solving therapy (PST). The meta-analysis, encompassing 2895 participants, showed that PST is significantly more effective than no treatment ($d=1.37$), treatment as usual ($d=0.54$), and attention placebo ($d=0.54$), but not significantly more effective than other bona fide treatments offered as part of a study ($d=0.22$). Significant moderators included whether the PST included problem-orientation training, whether homework was assigned, and whether a developer of PST helped conduct the study.

Keywords: Problem solving; Therapy; Treatment; Efficacy; Meta-analysis

Problem solving therapy (PST) developed out of a trend toward providing psychotherapy by teaching clients psychosocial skills (D'Zurilla & Goldfried, 1971). PST involves teaching a client how to use a step-by-step process to solve life problems. The usual process taught can be broken into two major parts: (a) applying a problem-solving orientation to life and (b) using rational problem-solving skills. Applying a problem-solving orientation usually involves appraising problems as challenges, thinking that the problems can be solved, and realizing that effective problem solving tends to require time and systematic effort (Nezu, 2004). Rational problem-solving skills include: (1) attempting to identify a problem when it occurs, (2) defining a problem, (3) attempting to understand the problem, (4) setting goals related to the problem, (5) generating alternative solutions, (6) evaluating and choosing the best alternatives, (7) implementing the chosen alternatives, and (8) evaluating the efficacy of the effort at problem solving (D'Zurilla & Nezu, 1999). If the efforts to solve the problem fail, one may return to any step and try again. PST typically involves oral and written presentation of the steps by the therapist, along with guided practice, both in session and as home assignments, in solving real problems. Developers of PST recommend that clients receive 8–16 sessions of 1.5 to 2 hr each (D'Zurilla & Nezu, 1999).

Over the past few decades dozens of articles have been published reporting evaluations of the efficacy of PST with a wide variety of problems, such as deliberate self-harm, depression, and obesity. D'Zurilla and Nezu (1999) summarized results of many of the older articles, including studies that included PST mixed in with other treatment components such as

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interpersonal skills training. D'Zurilla and Nezu concluded that the studies overall showed that PST was effective. A meta-analysis that included four studies of the efficacy of PST for preventing repetition of deliberate self-harm found only a non-significant trend for the effects of PST (Hawton et al., 1998). A meta-analysis by Townsend et al. (2001) evaluated efficacy of PST for hopelessness, depression, and problem resolution in deliberate self-harm clients. This analysis included the same four studies as in the prior meta-analysis plus another two studies. The comparison treatment was in most cases treatment as usual. Results showed that PST was more effective with regard to hopelessness, depression, and problem resolution than the comparison treatment. The two meta-analyses produced interesting results but with some important limitations: the analyses (a) reviewed only treatment for individuals who had engaged in intentional self-harm, (b) included only a small number of studies; and (c) showed different results with different types of outcome variables.

The mixed findings of prior meta-analyses suggest that there may be moderators of the efficacy of PST. For instance, in a very large and important meta-analysis, Wampold et al. (1997) found that various types of mental health treatment had sizeable but equivalent effect sizes, so one might wonder whether the effect of PST is moderated by the comparison group, with PST more effective than no treatment but no more effective than another bona fide treatment.

A study by Nezu and Perri (1989) showed that, as a group, depressed individuals who received the full PST, including problem-orientation training, experienced significantly greater decreases in depression than depressed individuals who received PST without problem-orientation training. Nezu (2004) commented later that the lack of problem orientation training might explain why some treatments described as PST are not effective. Presence or absence of problem orientation training as part of PST would therefore be worth examining as a moderator of effect size in a meta-analysis.

Burke, Arkowitz, and Menchola (2003) found that treatment studies of motivational interviewing had a greater effect size if they were done by a developer of the treatment method. Hence, one might wonder whether efficacy studies of PST would show larger effect sizes if done by a developer of PST. One might also think that more hours of PST would lead to larger effects, although studies of psychotherapy in general do not show an effect for number of hours of treatment (Bennett & Gibbons, 2000; Dobson, 1989; Feske & Chambless, 1995; Koss & Shiang, 1994). Giving PST clients home assignments might also affect treatment outcomes. A meta-analysis of home assignments in psychotherapy in general concluded that giving home assignments led to greater treatment effects (Kazantzis, Deane, & Ronan, 2000). Although no reason exists for PST to have greater effects with one gender than another or with young people versus adults, such a difference is possible and this information is common in research reports, so examination of these two variables seems warranted. Another variable of similar appeal and availability of information is whether the research participants were individuals seeking psychotherapy or recruited into psychotherapy for the study. The more important group for generalization would seem to be those who are seeking help on their own; very few people will ever be recruited into psychotherapy as part of a study. It is possible that PST works better for some types of problems than others; for instance, obesity problems tend to be resistant to substantial long-term improvement from any type of psychological treatment (Norris et al., 2004). It is also possible that individual PST differs in effect size from group PST, although psychotherapy in general seems to have similar outcomes with either format (McRoberts, Burlingame, and Hoag, 1998).

It is possible that subjective outcomes (e.g., self-report of depression) differ in effect size from more objective outcomes (e.g., interviewer ratings), so evaluating type of outcome as a moderator, such as Leichsenring and Leibing (2003) did in a meta-analysis, makes sense. Further, the effects of PST may fade out over time, as appears to happen with some psychological treatments of some disorders (Spiegler & Guevremont, 2003), although psychotherapy studies usually find no overall association between effect size and length of followup (Lambert & Bergin, 1994). Still, checking for an association between length of followup and treatment effect size seems wise.

1. Purpose of the present meta-analysis

The present paper reports the first meta-analysis of the efficacy of PST across all types of mental or physical health problems. The meta-analysis had as its objectives to evaluate (1) whether PST is more effective in helping individuals overcome health problems than no treatment, treatment as usual, attention–placebo treatment, and another bona fide treatment presented specifically as part of a study; (2) whether PST that includes problem-orientation training is more effective than PST without this element; (3) whether more hours of PST are associated with greater effects; (4) whether including home assignments in PST is associated with greater effects; (5) whether studies that involve one of the developers of PST show greater effects; (6) whether individual and group PST differ in efficacy; (7) whether effect size varies with gender; (8) whether effect size differs for children and adults; (9) whether the effect size varies with whether the participants are identified prior to the study as having a clinical problem or are recruited from the public; (10)

Table 1
Summary statistics for analysis (N_d=39)

Analysis	*N*	Problem treated	Depression related	PST developer	Adult	Clinical problem	Format	Hours of treatment	Home work	Comparison type	*d*	$CI_{-95\%}$	$CI_{+95\%}$
Alexopoulos et al. (2003)	25	Depression and executive dysfunction	Yes	No	Yes	No	Individual	na	No	Attention/placebo	0.963	0.130	1.790
Arean et al. (1993)	47	Unipolar depression	Yes	Yes	Yes	No	Group	18	No	BFTx reminiscence	0.493	−0.100	1.080
*Arean et al. (1993)	39	Unipolar depression	Yes	Yes	Yes	No	Group	18	No	Waiting list or no Tx	1.776	1.030	2.520
Azrin et al. (2001)	56	Conduct disorder/ substance dependency	No	No	No	No	Individual	19	No	BFTx family behav	0.141	−0.380	0.670
Barrett et al. (2001)	129	Dysthymia in adults	Yes	No	Yes	No	Individual	4	No	BFTx paroxetine	0.093	−0.250	0.440
*Barrett et al. (2001)	132	Dysthymia in adults	Yes	No	Yes	No	Individual	4	No	Attention/placebo	0.058	−0.280	0.400
Bennun (1986)	20	Outpatient families	na	No	Family	Yes	Family	na	No	BFTx Milan approach	−1.876	−2.930	−0.820
Biggam and Power (2002)	46	Vulnerable prisoners	No	No	Yes	No	Group	8	No	Tx as usual	0.734	0.140	1.330
Black and Scherba (1983)	14	Obesity	No	No	Yes	No	Group	18	Yes	BFTx behav	1.633	0.420	2.840
Carey et al. (1990)	22	Mental illness and chemical abuse	na	No	Yes	Yes	Group	6	No	Tx as usual	0.485	−0.370	1.340
Catalan et al. (1991)	47	Emotional disorders	na	No	Yes	Yes	na	na	No	Tx as usual	0.807	0.210	1.400
Chaney et al. (1978)	39	Alcoholism	No	No	Yes	Yes	Group	12	No	Tx as usual	0.468	−0.190	1.130
Coche and Flick (1975)	81	Psychiatric inpatients	na	No	Yes	Yes	Group	8	No	Tx as usual	0.487	0.040	0.930
Coche et al. (1984)	41	Psychiatric inpatients	na	No	Yes	Yes	Group	12	No	BFTx interactional	0.222	−0.400	0.850
Dowrick et al. (2000)	172	Unipolar depression	Yes	No	Yes	No	Individual	4	No	BFTx cog-behav education	0.178	−0.120	0.480
*Dowrick et al. (2000)	171	Unipolar depression	Yes	No	Yes	No	Individual	4	No	Waiting list or no Tx	0.017	−0.280	0.320
Epstein et al. (2000)	34	Child obesity	No	No	No	No	Group	na	Yes	BFTx family behav	−0.612	−1.300	0.080
Lerner and Clum (1990)	18	Suicidal ideation	na	No	Yes	No	Group	15	Yes	BFTx support/education	1.025	0.040	2.010
Malouff et al. (1988)	28	Divorce dysphoria	Yes	No	Yes	No	Group	6	Yes	BFTx rational emotive	−0.069	−0.810	0.670
*Malouff et al. (1988)	29	Divorce dysphoria	Yes	No	Yes	No	Group	6	Yes	Waiting list or no Tx	1.365	0.560	2.170
McLeavey et al. (1994)	33	Self-poisoning	na	No	Yes[a]	Yes	Individual	5	No	Attention/placebo	0.655	−0.050	1.360
#Mynors-Wallis et al. (2000)	226	Unipolar depression	Yes	No	Yes	No	Individual	4	No	BFTx fluvoxamine	0.151	−0.110	0.410

Mynors-Wallis et al. (1995)	56	Unipolar depression	Yes	No	Yes	Yes	Individual	4	No	BFTx amitriptyline	0.234	−0.290	0.760
*Mynors-Wallis et al. (1995)	55	Unipolar depression	Yes	No	Yes	Yes	Individual	4	No	Attention/placebo	0.864	0.310	1.420
Mynors-Wallis et al. (1997)	58	Emotional disorders	na	No	Yes	Yes	Individual	3	No	Tx as usual	−0.136	−0.650	0.380
Nezu (1986)	18	Unipolar depression	Yes	Yes	Yes	No	Group	14	Yes	Attention/placebo	4.056	2.440	5.680
Nezu and Perri (1989)	25	Unipolar depression	Yes	Yes	Yes	No	Group	18	Yes	Waiting list or no Tx	2.392	1.360	3.420
Nezu et al. (2003)	89	Distressed cancer patients	na	Yes	Yes	No	Individual	15	Yes	Waiting list or no Tx	2.747	2.170	3.330
Perri et al. (2001)	43	Obesity	No	No	Yes	No	Group	52	No	BFTx behave	0.636	0.020	1.250
*Perri et al. (2001)	38	Obesity	No	No	Yes	No	Group	52	No	Waiting list or no Tx	0.886	0.210	1.570
Pfiffner et al. (1990)	9	Externalizing	No	No	Family	Yes	Individual	4	Yes	BFTx behav parent management training	1.670	0.150	3.190
Sahler et al. (2002)	92	Mother of child with cancer	na	No	Yes	No	Individual	8	Yes	Tx as usual	0.400	−0.010	0.810
Salkovski et al. (1990)	20	Suicide attempt	na	No	Yes	Yes	Individual	5	Yes	Tx as usual	1.395	0.400	2.390
Schwartz et al. (1998)	341	Relative of breast cancer patient	na	No	Yes	No	Individual	2	Yes	Attention/placebo	0.031	−0.180	0.250
Spaccarelli et al. (1992)	37	Child behavior	No	No	Yes[b]	No	Group	6	No	BFTx behave	0.473	−0.190	1.130
*Spaccarelli et al. (1992)	38	Child behavior	No	No	Yes[b]	No	Group	6	No	Waiting list or no Tx	1.050	0.370	1.730
van den Hout et al. (2003)	76	Low back pain	No	No	Yes	Yes	Group	15	Yes	Attention/placebo	0.631	0.170	1.090
Williams et al. (2000)	219	Dysthymia in elders	Yes	No	Yes	No	Individual	4	No	BFTx paroxetine	0.122	−0.140	0.390
*Williams et al. (2000)	232	Dysthymia in elders	Yes	No	Yes	No	Individual	4	No	Attention/placebo	−0.270	−0.530	−0.010

Note. PST Developer = problem solving therapy (PST) developer one of the researchers; Clinical problem = whether the participants had been identified before the study as having a clinical problem; na = information not available; BFTx = bona fide treatment; Tx = treatment.

*Denotes a second analysis in a single study, this one comparing problem solving with a waiting list or attention–placebo. In order to eliminate any sample bias caused by using more than one analysis (effect size) per sample per study (Lipsey & Wilson, 2001) these second analyses were excluded from any meta-analysis that did not include comparison type as a variable.

#Using problem solving therapy by GPs versus medication alone.

[a] Included also teens.

[b] Parents to help child behavior.

whether the effect size varies with the type of mental or physical health problem; (11) whether PST has greater effects on self-report outcome measures than on objective outcome measures; and (12) whether the effects of PST diminish with increased follow-up time.

2. Method

2.1. Literature search

The PsycINFO and PubMed databases were searched using the term "problem solving." Relevant articles from this search were then used to obtain additional articles. We also evaluated every treatment study cited in D'Zurilla and Nezu (1999) and two meta-analyses of PST for self-harm treatment (Hawton et al., 1998; Townsend et al., 2001).

2.2. Characteristics of included and excluded studies

To be included, a study had to (a) evaluate PST as a treatment for a mental or physical health problem, (b) use a comparison condition, (c) include random assignment to condition; (d) either describe the problem solving steps that the participants were trained in or state that the PST followed D'Zurilla and Goldfried (1971) or D'Zurilla and Nezu (1982) or some closely related guidelines; and (e) report statistics essential to meta-analyses, such as the number of participants and the means and standard deviations. When, as in a few cases, some essential bit of statistical information was missing from a study, we attempted to obtain the information from the authors, but in all these cases we were unsuccessful. The meta-analysis included studies that compared PST in addition to another treatment versus just the other treatment.

Eight studies that were considered closely for inclusion in the meta-analysis were excluded because they did not clearly use PST but rather described vaguely a treatment focused on problem solving (e.g., Gibbons, Butler, Urwin, & Gibbons, 1978). Hence, their results may tell us nothing about problem solving therapy. Seven studies (e.g., Kazdin, Esveldt-Dawson, French, & Unis, 1987) were excluded because they combined problem solving therapy with training in means-end thinking, consequential thinking, and taking the perspective of others as recommended by Spivack, Platt and Shure (1976) in their description of interpersonal cognitive PST. Nine studies were excluded because they included in PST distinct, major therapy methods such as assertion training, relaxation training, communication training, systematic reinforcement for improved behavior, and extensive information booklets dealing with a specific problem of interest, such as a home care guide for individuals with cancer (e.g., Allen et al., 2002; Johnson & Greenberg, 1985). We excluded these studies because it is not possible to determine what contribution, if any, problem solving therapy made to the treatment outcomes. In excluding these studies for this reason, we followed the example of Moyer, Rounds, and Hannum (2004), who completed a meta-analysis of a specific therapy and excluded studies that included the specific therapy as part of a "combination treatment" (p. 8). One study was excluded because it had no outcome measure involving a mental or physical health problem (Intagliata, 1978). Three studies were excluded because they lacked adequate data for the meta-analysis (DeVellis, Blalock, Hahn, DeVellis, & Hochbaum, 1988; Graves, Meyers, & Clark, 1988; Hussian & Lawrence, 1981).

2.3. Coding studies

The studies were coded for the following variables: (1) type of comparison group: no treatment, treatment as usual, attention–placebo treatment, or another bona fide treatment presented specifically as part of a study; (2) whether the study report indicated that the PST used included problem-orientation training; (3) hours of PST; (4) whether home assignments were mentioned as part of the PST; (5) whether one of the developers of PST participated in the study; (6) whether the PST was presented in individual and group format; (7) gender of the participants: male, female, or mixed; (8) whether the participants were children or adults; (9) whether the participants were identified prior to the study as having a clinical problem or were recruited from the public; (10) the type of mental or physical health problem; (11) whether the outcome variables were self-report, objective, or a combination of self-report and objective; (12) length of the follow up; (13) the number of participants in each condition who completed the study; and (14) the key data for outcome measures. Treatment as usual characterizes the treatment individuals in a health care treatment program would have received if there was no study. Attention/placebo characterizes any study with a drug placebo or a psychological intervention intended to serve as an attention/placebo.

Table 2
Meta-analysis summary statistics employing a mixed effects model (method of moments random effects) analysis

Source	N_d	$N_{participants}$	d ($CI_{-95\%}$, $CI_{+95\%}$)	SE	z	p	Homogeneity analysis	Fail-safe N[a]
One analysis per study[b]	31	2161	0.54 (0.31, 0.77)	0.116	4.66	<.001	$Q(30)=171.06$, $p<.001$	136
All analyses	39	2895	0.56 (0.36, 0.76)	0.102	5.48	<.001	$Q(38)=228.75$, $p<.001$	179

Note. A significant Q value indicates that homogeneity should be rejected (i.e., effect sizes are heterogeneous).
[a] Reports the number of studies with $d=0.00$ needed to reduce the mean d to the d criterion value (±0.10).
[b] Excludes starred studies in Table 1.

2.4. Analysis

Some studies used two different groups of problem solving therapy, e.g., one administered by physicians and one administered by nurses. In such cases we randomly chose one of the two PST groups when the study did not report data for the two problem solving groups combined. In many cases a study included more than one comparison group for instance a behavioral treatment group and a waiting list control group. We completed separate analyses of comparisons of problem solving therapy with (a) a bona fide treatment offered specifically for the study, (b) treatment as usual, (c) attention/placebo treatment, and (d) no treatment or waiting list control. When a study provided outcome results for different periods after completion of treatment we used the longest followup period.

We included as dependent variables only measures of some mental or physical health condition such as depression or body mass index. We did not include self-report measures of problem solving strategies or ability, such as measured by the Problem-Solving Inventory (Heppner & Peterson, 1982), because these are not measures of mental or physical health. In this regard, we followed the model of Burke et al. (2003), who completed a meta-analysis of a therapy method called motivational interviewing, and included only measures of "the main behavioral and health outcomes" (p. 845). When two or more outcome variables were mathematically related (e.g., body mass index and weight), we chose the variable that seemed to best represent the desired outcome (e.g., body mass index).

Meta-analytic methods included (a) calculating effect sizes (d) based on means and standard deviations, between group statistics such as F, or percentages of participants who moved into the normal range (see e.g., Hedges, 1981, 1982; Lipsey & Wilson, 2001; Wolf, 1986); (b) using w, inverse variance weighting (Lipsey & Wilson, 2001), to compute descriptive and inferential statistics; (c) calculating an average effect size for studies with multiple outcome measures; (d) using the Q statistic (Lipsey & Wilson, 2001) to perform homogeneity analyses; and (e) examining effect sizes for univariate outliers using as a criterion $z=3.67$, $p=.001$ two-tail, and Normal Q–Q plots and Detrended Normal Q–Q plots, following the recommendations of Tabachnick and Fidell (2001). No effect sizes were identified as potential outliers.

3. Results

Thirty-one studies, producing 39 effect-size analyses, met all the criteria for the meta-analysis. Table 1 describes the studies, which encompassed a total of 2895 participants.

Table 3
Moderator analysis for four treatment comparison conditions, mixed effects model (method of moments random effects) analysis ($N_d=39$)

Source	Waiting list or no treatment	Treatment as usual	Attention-placebo	Other experimental treatment
Comparison treatment $Q_{between}$ (3)=16.54, $p<.001$				
d	1.37	0.54	0.54	0.22
SE	0.237	0.216	0.213	0.155
$CI_{-95\%}$	0.91	0.12	0.12	−0.08
$CI_{+95\%}$	1.84	0.96	0.95	0.52
z	5.79	2.49	2.51	1.42
p_d	<.001	.013	.012	.155
N_d	7	8	8	16
Q_{within}	14.68	3.12	17.70	17.49
df_Q	6	7	7	15
p_Q	.023	.874	.013	.290

Table 4
Moderator analysis, mixed effects model (method of moments random effects) analysis

Source[a]	$Q_{between}$	*df*	*p*	*d* ($CI_{-95\%}$, $Cl_{+95\%}$), SE	Q_{within}	*df*	*p*	*d* ($CI_{-95\%}$, $CI_{+95\%}$), SE	Q_{within}	*df*	*p*
				Not depression related				Depression related			
Depression related	0.01	1	.942	0.50 (0.14, 0.87), 0.186	9.03	8	.340	0.49 (0.16, 0.81), 0.164	27.14	9	.001
				No PST developer as one of the researchers				PST developer as one of the researchers			
PST developer	29.88	1	<.001	0.34 (0.15, 0.53), 0.098	30.85	26	.224	2.03 (1.45, 2.61), 0.294	16.78	3	<.001
				Non-clinical				Clinical			
Clinical participants	0.94	1	.332	0.63 (0.34, 0.92), 0.149	39.68	18	.002	0.39 (0.01, 0.77), 0.194	13.74	11	.248
				Individual				Group/family			
Treatment format	0.04	1	.846	0.51 (0.18, 0.84), 0.167	20.11	13	.093	0.56 (0.22, 0.89), 0.171	34.62	15	.003
				No homework				Homework			
Homework	8.29	1	.004	0.30 (0.01, 0.59), 0.146	11.62	18	.866	1.02 (0.62, 1.42), 0.204	34.00	11	<.001
				No orientation training				Orientation training			
Training in problem orientation	15.26	1	<.001	0.19 (−0.06, 0.45), 0.131	21.26	16	.169	1.00 (0.69, 1.31), 0.159	31.82	13	.003

[a] Excludes starred studies in Table 1.

Overall, PST had a significant effect size, using the best comparison group in each study, with another bona fide treatment the top choice, treatment as usual and attention–placebo treatment next, and then no-treatment (see Table 2). The homogeneity analysis in Table 2 indicates that the meta-analysis effect sizes tended to be heterogeneous, suggesting that random effects models should be employed for analyses (Lipsey & Wilson, 2001). Random effects models produce larger confidence intervals than fixed effects models, leading to more conservative conclusions about significant differences. We therefore used random effects models for all meta-analyses.

With regard to type of comparison group, Table 3 shows that PST was significantly more effective than being on a waiting list, treatment as usual, and an attention/placebo comparison group. However, PST was not significantly more effective than a bona fide comparison treatment, although there was a trend in that direction. No one bona fide therapy type was included in enough studies to allow comparison with a single specific type of therapy.

In order to search for moderators of effect size, we examined a number of coded variables to determine whether they were associated with effect size. Two variables, gender of participants and whether the participants were adults or not, had insufficient variability to support a meaningful analysis. Two variables involved continuous data, and their results were as follows. The effect size for number of hours of PST was $r(25) = .23$, $p = .12$ (one-tailed; a non-significant trend); the effect size for months of followup was $r(29) = -.09$, $p = .32$ (one-tailed). See Table 4 for the analyses of dichotomous variables and Table 5 for the analysis of type of assessment format, a tripartite variable.

Three variables were significantly associated with effect size: inclusion in PST of training in problem orientation, the statement in the report that PST included home assignments, and the participation in the study of one of the developers of PST (in all cases Arthur Nezu). There was no significant difference in effect size for the other variables examined: (a) individual versus group/family PST, (b) whether the participants had been identified before the study as having a clinical problem or were recruited from the public, (c) whether the problem involved some specific type of

Table 5
Moderator analysis for assessment type, mixed effects model (method of moments random effects) analysis (N_d=31)

Source	Self-report	Objective	Mixed
Comparison treatment $Q_{between}(2) = 2.85$, $p = .241$			
d	0.37	0.45	0.83
SE	0.196	0.219	0.209
$CI_{-95\%}$	−0.01	0.02	0.42
$CI_{+95\%}$	0.75	0.88	1.24
z	1.88	2.07	3.97
p_d	.060	.039	<.001
N_d	12	9	10
Q_{within}	25.23	5.53	19.65
df_Q	11	8	9
p_Q	.008	.700	.020

Table 6
Multiple regression analysis, random intercept, fixed slopes model (method of moments random effects) analysis (N_d=31)

Source	*B*	SE *B*	β	z	*p*
Constant	0.106	0.127	.000	0.837	.403
Home work	0.342	0.201	.197	1.696	.090
PST developer as one of the researchers	1.299	0.338	.478	43.84	<.001
Training in problem orientation	0.396	0.205	.239	1.927	.054

Note. This model had a d=0.50, R^2=.48, $Q(3)$=38.30, p<.001; residual $Q(27)$=41.24, p=.039.

disorder (only studies of depression-spectrum disorders were numerous enough to compare with other disorders), or (d) assessment format (i.e., self-report, objective or mixed).

In order to assess the relative importance of the three significant moderators, whether home assignments given, whether the PST included problem orientation training, and whether a PST developer was one of the researchers, we completed a multiple regression of effects sizes, calculated as suggested by Lipsey and Wilson (2001, pp. 138–142), with the three significant moderators entered. Table 6 shows that only whether a PST developer was one of the researchers made a significant independent contribution to effect size, with the other two variables close to significance. The three moderators together accounted for 48% of the variance in outcome.

To determine whether the three studies that lacked some essential statistical data needed for meta-analysis showed the same pattern as the studies that entered the meta-analysis, we looked closely at the results of each. Together the results of the studies (DeVellis et al., 1988; Graves et al., 1988; Hussian & Lawrence, 1981) showed no clear pattern. The studies of DeVellis et al. and Hussian and Lawrence showed no significant effect for PST, while the study by Graves et al. showed a significant effect.

4. Discussion

The meta-analysis showed that across all the studies PST had a significant effect size. PST was significantly more effective than no treatment, attention/placebo treatment, and treatment as usual, but not significantly more effective than other bona fide treatments with which it has been compared. The results, across 31 studies (39 analyses) and 2895 participants, provide strong evidence that PST tends to be effective in treating mental or physical health problems. Whether PST is more effective than other bona fide treatments is not perfectly clear because there was a non-significant trend in favor of PST. The present meta-analysis examined only 16 studies that used for comparison a bona fide treatment presented as part of a treatment study. It could be that with more studies, the trend in favor of PST would become a statistically significant difference.

Finding equivalency of bona fide treatments in psychological treatment outcome research, as in this meta-analysis, is common enough to have its own fanciful name, the Dodo Bird effect (Wampold et al., 1997). Equivalency may be common because different psychological treatments tend to have similar elements such as a healer, a ritual, and social support (Frank, 1973) and tend to lead to increased self-efficacy (Bandura, 1997). The finding of equivalent efficacy for PST and other psychological interventions suggests that PST may serve as a useful comparison treatment in evaluating whether new types of treatment are more effective than an existing treatment.

The meta-analysis also found three other factors that were associated with effect size: participation of one of the PST developers in the study, a clear statement in the study report that homework was assigned, and training in problem orientation. Studies conducted by a PST developer had significantly larger effect sizes than studies conducted by non-developers. This finding is similar to meta-analytic findings regarding a type of therapy called motivational interviewing (Burke et al., 2003). The present results could be interpreted in various ways such as that the treatment was done better or the therapists or clients had greater hope for the treatment. The main implication of the PST developer finding is that other therapists are unlikely to obtain such large effects with PST as those of one of the developers of PST. One might wonder whether a researcher's being a strong believer in a specific treatment might also tend to be associated with greater effect sizes, but that could not be tested within the meta-analysis, as the current tradition is not for researchers to describe their personal beliefs about the various treatments they test against each other.

PST studies that included training in problem orientation had larger effect sizes than studies that did not. This finding is consistent with the finding of Nezu and Perri (1989) using an experimental research method that depression improved more when clients received problem orientation training in addition to training in the rational steps of

problem solving. Although the moderator findings of the present meta-analysis do not show causation, they raise the possibility that the value of problem orientation training as part of PST extends to treatment of problems in general, as suggested by Nezu (2004). However, the multiple regression analysis showed that this moderator added no significant explained variance to that of assignment of homework and participation of a PST developer.

The finding that reporting the giving of home assignments was associated with larger effect sizes is consistent with findings in studies of psychotherapy in general (Kazantzis et al., 2000). The main implication of this moderator finding is that PST may be more effective if home assignments are given, as suggested by the developers of PST (D'Zurilla & Nezu, 1999). It is possible though that some or all of the studies that did not mention giving home assignments actually gave them. The meaning of the difference in effect sizes would then be murky. Also, the moderator findings do not show causation; some other factors may have led to both home assignments and greater effect sizes. Finally, the home-assignments variable did not add a significant amount of explained variance to that of the PST-developer variable and assignment of homework.

Several other variables were found *not* to be significantly related to effect size: the number of hours of PST (although there was a trend in favor of a higher number of hours being associated with greater effect size); individual versus group PST; whether the participants had been identified before the study as having a clinical problem; whether the problem involved a depression-spectrum disorder; whether the study used self-report, objective or both self-report and objective outcome measures; and the length of the followup.

The finding that number of hours of therapy was not significantly associated with effect size is similar to findings regarding other types of psychotherapy, which also have found no significant relationship between number of hours of treatment and outcome (Bennett & Gibbons, 2000; Dobson, 1989; Feske & Chambless, 1995; Koss & Shiang, 1994). The finding that group PST was not significantly less effective than individual PST is consistent with most research findings on individual versus group psychotherapy (McRoberts, Burlingame, & Hoag, 1998). The finding is important because group therapy can be much more cost effective. The finding that whether the participants were previously identified as having a clinical problem or were recruited for the study was unassociated with effect size suggests that the source of participants does not affect efficacy of PST. The finding that depression-spectrum problems and other problems had similar effect sizes shows that as of yet there is no evidence that PST is more effective with some specific class of problems than with others. Lack of a significant association between effect size and length of followup is consistent with most research on psychotherapy in general (Lambert & Bergin, 1994). The finding is important because it suggests no reason to expect a diminishing in PST effects over time.

Prior meta-analyses using some of the studies included in the present meta-analysis showed that PST had a *non*-significantly greater effect than treatment as usual on suicide attempt repetition in self-harm clients (Hawton et al., 1998) but did have a significantly greater effect than treatment as usual on self-reported depression, hopelessness, and improvement in problems in self-harm clients (Townsend et al., 2001). These two meta-analyses included some studies that we excluded for various reasons, such as that they examined a "PST" treatment involving both PST and some other treatment method. The findings of the present meta-analysis, which covers many more studies with a much broader range of psychological problems, are consistent with the significant findings of the meta-analysis of Townsend et al. (2001) and with the non-significant trend found in the smaller meta-analysis of Hawton et al. (1998).

Some cautions apply to interpreting the results. First, the meta-analysis included 31 studies, providing, as in most meta-analyses, limited power to search for moderators. Second, only published studies were included in the meta-analysis. Thus, the analysis may have a "publication bias" in that non-significant findings are less likely to be published than significant findings. However, the fail-safe analysis presented in Table 2 indicates that 136 unpublished studies would have to exist to negate the significant effect found in this meta-analysis for PST in general. Third, with many moderator analyses completed, there is the risk of inflated Type I error, leading to spurious findings. However, our use in analyses of the conservative random effects model reduces this risk to some extent. Fourth, evidence supporting the moderators is not experimental. For instance, the different studies were not randomly assigned to have assigned homework or not. Hence, association between whether homework was assigned and effects size may not be causal; some third variable may have led to both the assignment of homework and the effect level. This same sort of limitation applies to all the significant moderator findings. Hence, one would better think of significant moderator findings as suggesting promising avenues for future research with experimental methods than proving some causal link.

The present meta-analysis excluded seven studies of problem solving therapy combined with training in means-end thinking, consequential thinking, and taking the perspective of others as recommended by Spivack et al. (1976) in their description of interpersonal cognitive PST. If a few more outcome studies of interpersonal cognitive PST are

completed, the collection in its entirety might be large enough for a useful meta-analysis, assuming that the reports provide adequate statistical information.

The present meta-analysis excluded findings of changes in problem solving strategies used. Although such manipulation-check findings may not be as important as findings regarding client-sought health outcomes, manipulation-check findings can be important in judging whether PST achieves its intermediate goal of changing problem-solving behavior. A future meta-analysis could examine this issue.

Future research on PST might also profitably explore what efficacious therapeutic elements it shares with other types of psychotherapy and what unique efficacious elements it has. For instance it is not clear that PST leads to solving more real-life problems over time (Foxx & Faw, 2000; Mynors-Wallis, 2002; Tisdale & Lawrence, 1986). Do clients benefit merely from solving problems during therapy, from building their problem solving self-efficacy (Bandura, 1997), or from solving problems after therapy ends?

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