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Can Psychological Interventions Increase Optimism? A Meta-Analysis

John M. Malouff and Nicola S. Schutte

University of New England, Australia

Please direct correspondence to John Malouff at University of New England Psychology, Armidale NSW 2351, Australia. Email: jmalouff@une.edu.au.

Abstract

Greater optimism is related to better mental and physical health. A number of studies have investigated interventions intended to increase optimism. The aim of this meta-analysis was to consolidate effect sizes found in randomized controlled intervention studies of optimism training and to identify factors that may influence the effect of interventions. Twenty-nine studies, with a total of 3,319 participants, met criteria for inclusion in the analysis. A significant meta-analytic effect size, g = .41, indicated that, across studies, interventions increased optimism. Moderator analyses showed that studies had significantly higher effect sizes if they used the Best Possible Self intervention, provided the intervention in person, used an active control, used separate positive and negative expectancy measures rather than a version of the LOT-R, examined only immediate effects, had a final assessment within one day of the end of the intervention, and used completer analyses rather than intention-to-treat analyses. The results indicate that psychological interventions can increase optimism and that various factors may influence effect size.

Key Words: optimism, intervention, meta-analysis, training

Can Psychological Interventions Increase Optimism? A Meta-Analysis

Optimism consists of having favorable expectations for the future (Carver, Scheier, & Segerstrom, 2010). Optimism can take the form of a trait or a state (Kluemper, Little, & Degroot , 2009). Greater optimism is related to both better mental health and physical health (Carver et al., 2010; Rasmussen, Scheier, & Greenhouse, 2009). For example, individuals high in optimism are less likely to become depressed and tend to have better immune functioning (Carver et al., 2010). They are likely to show more psychological growth after a trauma (Prati & Pietrantoni (2009). Greater optimism prospectively predicted lower mortality over the course of four years in one study (Galatzer-Levy & Bonanno, 2014) and over the course of 40 years in another study (Brummett, Helms, Dahlstrom, & Siegler, 2006).

Some researchers view optimism as a trait-like variable called dispositional optimism (Carver & Scheier, 2014). Measures of dispositional optimism have a test-retest reliability of .58 to .79 over short time periods and a range of test-retest stability of .35 to .71 over 10 years (Carver et al., 2010). However, studies have shown that optimism can change over time if a person's situation changes (Atienza, Stephens, & Townsend, 2004; Segerstrom, 2007).

Many researchers who have studied optimism interventions have tried to increase optimism for its own value (e.g., Breitbart et al., 2010), much as one would try to increase general self-efficacy (e.g., Betz & Schifano, 2000). Researchers have also targeted optimism in the hope of momentarily increasing pain tolerance (e.g., Hanssen, Peters, Vlaeyen, Meevissen, & Vancleef, 2013) or decreasing distress of some sort (e.g., Antoni et al., 2001).

Optimism interventions tested in studies have used various approaches. Many studies used the Best Possible Self intervention, which involves developing goals for and visualizing a best possible future self (e.g., Meevissen, Peters, & Alberts, 2011). The Best Possible Self intervention typically includes instructions similar to these, from Boselie, Vancleef, Smets, and Peters (2014, p. 335): "[I]magine yourself in the future, after everything has gone as well

as it possibly could. You have worked hard and succeeded at accomplishing all the goals of your life…"

Other intervention methods tested in studies include self-compassion training (Smeets, Neff, Alberts, & Peters, 2014); broad ranges of coping training, cognitive-behavioral therapy methods or positive-psychology methods, (e.g., Antoni et al., 2001; Chesney, Chambers, Taylor, Johnson, & Folkman, 2013; Drozd et al., 2014), including mindfulness (e.g., Schoenert-Reichl et al. (2015) and meditation (Rizzato (2014). One study used psychodrama methods to teach psychological skills (Tavakoly, Namdari, & Esmali, 2014). Also tested was Make a Wish activity for children with cancer (Shoshani, Mifano, & Czamanski-Cohen, 2015). Other tested interventions have not had obvious potential connections to optimism. These interventions include sensory isolation (Kjellgren, Erdfelt, Werngren, & Norlander, 2011) and lying on a bed of nails Kjellgren & Westman (2014).

The optimism intervention studies often have assessed optimism using the Life Orientation Test-R (LOT-R; Scheier, Carver, & Bridges, 1994), which asks about the respondent's usual positive and negative expectancies about the future. Studies typically use a total score for the LOT-R. Some studies used separate measures of present positive expectancies and present negative expectancies about the respondent's future (see, e.g., Hanssen et al., 2013).

Different optimism intervention studies have found different effect sizes, ranging from essentially no effect (Rizzato, 2014; Tak, Kleinjan, Lichtwarck-Aschoff, & Engels, 2014) to over a pooled standard deviation advantage for an intervention over a control group (Meevissen, Peters, & Alberts., 2011) The consolidated effect size across studies is unknown. It is also unknown what factors, if any, moderate the effect size. Does the type of intervention matter? The way optimism is measured? For how long do the intervention effects last?

The main aim of the present study was to aggregate findings of optimism intervention studies to determine whether it is possible to increase optimism with psychological

interventions. We used meta-analysis to combine the effect sizes for optimism found in randomized controlled optimism training studies. The analyses tested the prediction that optimism interventions lead to increases in optimism and examined several factors that might moderate effect size.

Method

The meta-analysis inclusion criteria for studies were: (1) a comparison of an intervention intended to increase optimism and a control group; (2) random assignment of participants to condition; (3) assessment of optimism; and (4) sufficient statistical results regarding between-groups results for optimism to allow the calculation of an effect size suitable for meta-analysis.

We did not search for studies that aimed to increase hope. Optimism is related to but different from hope in that hope, as typically assessed, involves anticipated level of success in achieving positive outcomes, as well as confidence in being able to bring about positive outcomes in the future (Ciarrocchi & Deneke, 2005). Research findings have indicated that optimism and hope are associated, yet distinct concepts (Bryant & Cvengros, 2004; Rand, 2009).

We searched five databases, Google Scholar, PsychINFO, Medline, Web of Science, and Cochrane, for the term optimism and one of the following terms: intervention, training, and randomized controlled trial. We searched the entire collection of each database, starting at the beginning of its content coverage. We searched for both published and unpublished studies and identified 2,847 records. We searched each included article for references to other articles that we might include. Also, we wrote to the corresponding author of each included study and asked for relevant unpublished results. The reference lists of published studies and colleagues provided five additional records. We completed the search in March, 2016.

We found a few studies (e.g., Fresco, Moore, Walt, & Craighead, 2009) that examined the effect of interventions on optimistic or pessimistic attribution style; we did not include

such studies as attribution style is not a direct measure of favorable expectations for the future. Some studies (e.g., Sergeant & Mongrain, 2014) employed interventions targeting optimism as a means of affecting other outcomes, but did not report between group effects for optimism and we did not include these studies. One study (Blackwell et al., 2015) assessed the effects of an intervention on optimism but used a control condition that could have suppressed optimism and we excluded that study. One study (Baghkheirati, Ghahremani, Kaveh, & Keshavarsi (2015) used eight classes as the unit of analysis. We excluded that study because it did not have random assignment of individuals.

The flowchart in Figure 1 shows the search process leading to the studies containing the information included in the meta-analysis. For all included articles, one of us coded the article and the other checked the coding. In order to obtain an estimate of inter-rater coding agreement, we set aside eight of the studies and coded them independently. We coded effect size data and 10 moderators for each of the studies, plus one additional effect size for one of the studies that had two control groups. Our coding agreed exactly on 82 of 89 coding decisions for an inter-coder agreement rate of 92%. In cases of disagreement with regard to coding the 29 studies, we made final decisions by consensus.

We examined as potential moderators study characteristics that (a) indicate the quality or meaning of results, (b) might have importance for designing future optimism interventions, and (c) could be coded for almost all studies. These criteria led to the following moderators: (1) type of intervention, including content, number of hours of in-person training, and whether the format of the intervention was in-person or online; (2) type of trainees, including whether the trainees were adults, whether they had an identified problem, and percentage of female trainees; and (3) aspects of the research design, including type of control group (active, added training versus waiting list or treatment as usual); type of optimism measure, whether the researchers paid participants, whether the between-group analyses were with persons completing the final assessment or were carry-forward intention-to-treat, and whether the

final assessment used for the meta-analysis was completed at a time later than near the end of the intervention.

For calculating effect sizes, we used pre and post data for both groups or related results such as *F* values. For analyses, we used the Comprehensive Meta-Analysis Version 2 (Borenstein, Hedges, Higgins, & Rothstein, 2006) statistical package.

For the overall meta-analysis, we used a random effects analysis in order to be able to generalize beyond the included studies. For studies with more than one optimism measure, we used the mean effect size for calculating a meta-analytic effect size. For the study of Fosnaugh et al. (2009), which include two experimental conditions and a control condition, we used the mean effect size of the two comparisons of experimental conditions and control. We used the *Q* statistic to evaluate nine categorical variables as possible moderators of effect size, and we used method of moments meta-regression to test two potential moderators with continuous data.

Results

The 29 included randomized controlled trials had a total of 3,319 participants. Table 1 shows the contributions of each study to the meta-analysis, along with information describing the study. Table 1 also provides information regarding significant intervention effects on outcomes other than optimism. Figure 1 shows a forest plot of weighted effect sizes for individual studies.

The meta-analytic Hedges' *g* for the difference between the optimism training group and the control group for all 29 analyses was 0.41 (95% CIs .29, .53), $p < .001$, indicating that the training produced a significant effect on optimism. To assess publication bias, we used the following standard methods. Duval and Tweedie's trim and fill bias analysis indicated no need to adjust the effect size, with no studies trimmed. Figure 2 shows the related funnel plot of effect size plotted on study size. The classic fail-safe N indicated that 645 studies with 0 effect size would be needed to make the meta-analytic result non-significant. Orwin's fail-safe

N, with a trivial *g* set at .15, indicated that 24 missing studies with 0 effect size would be needed to bring the overall effect size down to a trivial level.

The included studies showed significant heterogeneity in effect sizes, $Q(28) = 62.9$, $p < .001$, $i^2 = 55$, indicating the potential for moderators of effect size.

Table 2 shows the results for categorical moderator variables. Use of intention-to-treat analysis was associated with a much lower effect size. Expectancy measures other than a version of the LOT-R showed higher effect sizes than LOT-R measures. One study, Fosnaugh et al., 2009), reported results that could be converted into effect sizes for both expectancy measures and the LOT-R. The expectancy measures had higher effect sizes for two different experimental conditions ($g = 0.55, 0.33$) than the LOT-R ($g = 0.32, 0.20$). Studies with final assessment used in the meta-analysis completed less than a day after the end of the intervention had over twice the effect size of other studies.

Interventions that included the Best Possible Self method showed higher weighted mean effect sizes than other methods of increasing optimism, and interventions that were not entirely online showed higher mean effects than purely online interventions. Studies that used an active control group showed almost twice the effect size of other studies. Chesney et al. (2003) used both an active control group and a waiting list control, so we examined it to determine whether it would show a higher effect size for the comparison with the active control. It did. The active control had a higher effect size ($g = 0.33$) than the waiting list control ($g = 0.30$).

Two moderator analyses included potential moderators as continuous variable. We used method of moments meta-regression to examine percentage of females in a study as a moderator. The results showed a nonsignificant trend in the direction that the higher the percentage of females in a study, the higher the effect size, slope point estimate = .005, $p = .10$.

We used this same regression method to examine the association between number of hours of in-person intervention and effect size. We excluded the study of Shoshani et al. (2015) because it was not possible to code it for number of in-person intervention hours. The results showed a significant *negative* association between number of in-person intervention hours and effect size, slope point estimate = -.022, $p < .001$. Examining the in-person hours for each study showed a median of only 0.5 hour.

Discussion

The meta-analytic results show that it is possible to increase optimism through a psychological intervention. The meta-analytic effect size, $g = .41$, indicates a weighted mean difference of 41% of pooled standard-deviation in optimism at post-intervention between the experimental and control conditions. This effect size is small according to the standards suggested by Cohen (1988).

Higher optimism may have various physical and mental health benefits (Brummett et al., 2006; Carver et al., 2010; Galatzer-Levy & Bonanno, 2014; Prati & Pietrantoni, 2009; Rasmussen et al., 2009). In some of the studies included in the present meta-analysis, as well as increasing optimism, the interventions had beneficial effects such as decreasing negative affect, depression, and pain. These additional benefits accruing from interventions intended to increase optimism can be viewed in the context of a tenet of the positive psychology approach holding that while it is important to focus on positive characteristics such as optimism in their own right, this focus can also effectively be combined with a problem-based approach in that enhancement of positive characteristics can prevent or ameliorate distress (Seligman & Csikszentmihalyi, 2000).

Several variables were significant moderators of effect size. First, studies with interventions that included the Best Possible Self method showed effect sizes substantially higher than studies that did not include this method. The Best Possible Self method involves asking individuals to imagine clearly a future in which everything has turned out as well as

possible and they have achieved all their life goals. This intervention has been found to produce various benefits other than increasing optimism, such as increasing positive affect (Layous, Nelson, & Lyubomirsky, 2013; Renner, Schwarz, Peters, & Huibers, 2014).

Second, studies with between-group analyses that used only participants who completed the study had significantly larger effect sizes than studies that used carry forward pre-intervention scores for those lost to post-intervention assessment. That difference might be expected, given the conservative nature of intention-to-treat analyses.

A third significant moderator was whether the final assessment used for the meta-analysis was completed more than a day after the end of the intervention. Most studies collected the final data at or near the end of the intervention, and these studies had much higher effect sizes. This result raises a question about to what extent benefits of optimism intervention are maintained over time. For most purposes, a permanent increase would be ideal, although there are times, such as in stressful or especially challenging situations, where a temporary boost in optimism could be valuable.

The fourth variable that was a significant moderator was whether optimism was measured with a version of the LOT-R or with other expectancy measures. Expectancy measures had larger effect sizes compared to a version of the LOT-R. However, studies using the LOT-R did have a significant overall effect size. One study, Fosnaugh et al. (2009), provided usable results for both types of measures, with the effect size substantially higher for the non-LOT-R expectancy measures. These other expectancy measures assess positive expectancies about the future separately from negative expectancies about the future. The LOT-R combines positive and negative expectancies (reverse scored) in one measure. It also asks about the respondent's usual expectancies about the future, while other measures used in the studies ask about present expectancies. It might be easier to change present expectancies. However, it could be that it is best to measure positive and negative expectancies separately, as is done with positive and negative affect (Watson, Clark, & Tellegen, 1988).

Fifth, studies with an in-person intervention showed significantly higher effect sizes than studies with an entirely online intervention. In fact, the effect size for purely online interventions was not statistically significant. The difference between in-person and online intervention is surprising, but could be due to confounding effects of other variables. In randomized comparisons of in-person and online psychological interventions in general, the two formats usually are about equivalent in effects (see Christensen, Batterham, & Calear, 2014).

The sixth significant moderator was number of in-person hours of intervention. The more the hours of intervention, the lower the effect size. The median number of hours for the studies was 0.5 hour. It is hard to interpret this finding because studies with brief interventions tended to use the Best Possible Self intervention, to have the final assessment soon after the end of the intervention, and not to use intention to treat. This finding overlaps with the finding that a related moderator was significant: whether the intervention was online (0 in-person hours) or not.

Seventh, studies that used an active control group showed almost twice the effect size of studies with a waiting-list or treatment-as-usual control. One study (Chesney et al., 2003) used both an active control and a waiting list control, and the analysis with the active control had a slightly higher effect size than the waiting list control. This set of findings goes in the opposite direction of what one might expect.

Although the overall meta-analytic effect size was small, three moderator subgroups of studies showed a medium effect size, that is, one of at least $g = 0.50$: best possible self as the intervention, use of an expectancy group other than a version of the LOT-R, and active comparison group. If we apply a Bonferroni adjustment to set a conservative alpha level for the 11 moderator analyses, we would use a p value of .05/11 = .0045. Under this standard, only the three moderators listed above for having medium effect sizes would be significant.

In sum, the moderator findings suggest that optimism-intervention researchers might most wisely use the brief in-person Best Possible Self intervention, at least when they are seeking short-term effects. Using expectancy measures of optimism might show the larger effects than the LOT-R.

The results of all moderator analyses ought to be viewed cautiously because (1) with only 29 studies, the analyses had relatively low power to detect significant moderators, (2) moderator analyses are quasi-analytic (studies were not randomly assigned to a moderator condition), (3) 11 moderator analyses were done, creating a risk of alpha inflation, (4) findings with moderators that have one category including only a few studies may be especially likely to not be replicated in future studies, and (5) apparently important moderators can be confounded with each other or with other unexamined variables, that are the actual cause of differences in effect sizes. For instance, the expectancy measures were used mostly in studies that examined the brief Best Possible Self intervention. Hence, causal interpretations of moderator results are inappropriate, and the results are best viewed as providing hypotheses about possible moderating variables.

Overall, the meta-analytic results indicate that optimism interventions are successful in increasing optimism. Multiple studies found positive effects of the intervention on measures of positive affect, mental health and pain. The results of these studies and others (Brummett et al., 2006; Carver et al., 2010; Prati & Pietrantoni, 2009; Rasmussen et al., 2009) indicate that increases in optimism might have benefits for mental and physical health. How long intervention-induced improvements in optimism endure is not clear from the studies.

Future optimism-intervention research might examine, using experimental methods, what specific types of intervention content have the greatest effect on optimism; what types of online interventions, if any, produce increases in optimism; how long the benefits of optimism-focused interventions endure; and what additional clinical and other psychological benefits accrue due to an increase in optimism. Future research might also explore the

mechanisms through which optimism interventions lead to clinical outcomes such as decreased pain or depression, along with the consequences of increased levels of optimism. Finally, studies might examine qualities making individuals receptive to optimism interventions or allowing individuals to optimally benefit from optimism interventions.

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Table 1

Studies Included in Meta-Analysis

Authors	Training method	In-person hours of training	Trainees	Percent female	Control group	Participants paid	Optimism measure	Assessment weeks from training end	Country	N	Hedges' *g*	Other significant outcomes
Antoni et al. 2001	cognitive-behavioral stress management	20	cancer patients	100	active	no	LOT-R	28	U.S.	100	0.49	depression
Boselie et al. 2014	best possible self	0.35	college students	78	active	no	expectancy	0	Netherlands	74	0.33	exec task, pos & neg affect
Boselie et al. (in press) Study 1	best possible self	0.35	college students	79	yes	yes	expectancy	0	Netherlends	81	1.02	pos affect
Boselie et al. (in press) Study 2	best possible self	0.35	college students	74	yes	yes	expectancy	0	Netherlands	61	0.50	pos affect
Boselie et at. (under review)	best possible self	0.35	College students	92	Yes	yes	expectancy	0	Netherlands	61	0.81	performance while in pain
Breitbart et al. 2010	meaning-centered group therapy	12	cancer patients	51	active	no	LOT	0	USA	55	0.41	-
Chesney et al. 2003	cognitive-behavioral coping	15	HIV positive	0	1 group active and 1 not	no	LOT-R	0	USA	90	0.21	.
Drozd et al. 2014	Nine positive psychology methods	0	community	71	no	yes	LOT-R	0	Norway	95	0.18	affect
Fosnaugh et al. 2009	optimism word priming or positive future events	0.25[1]	college students	55	active	no	expectancy & LOT-R	0	U.S.	105	0.35	-

Hanssen et al. 2012	best possible self	0.35	college students	81	active	yes	expectancy	0	Netherlands	79	1.00	pain, pos affect
Kjellgren & Westman 2014	sensory isolation	9	community	78	no	no	LOT-R	0	Sweden	65	0.70	anxiety, depression
Kjellgren et al. 2011	lying on spike mat	0.67	pain	72	no	no	LOT	0	Sweden	36	0.18	-
Knaevelsrud et al. 2010	exposure and cognitive	0	trauma survivors	90	not active	no	LOT-R	0	German speaking	88	0.35	post-trauma growth
Koenig et al. 2015	Religious CBT	8.3	depressed, ill	68	active	no	LOT-R	12	USA	132	0.16*	-
Lee et al. 2006	meaning making	8	cancer patients	81	not active	no	LOT-R	.14	Canada	74	0.43	self-efficacy & self-esteem
Lengacher et al. 2009	mindful stress-management	8	cancer patients	100	not active	yes	LOT-R	0	USA	82	0.23	anxiety , depression
Littman-Ovadia & Nir, 2014	positive future events	0	community	61	active	no	LOT-R	4	Israel	77	0.17	neg affect, pessimism, emotional exhaustion
Meevissen et al. 2011	best possible self	0.5	college students	93	active	yes	expectancy	0	Netherlands	51	1.26	-
Peters et al. 2010	best possible self	0.35	college students	62	active	no	expectancy	0	Sweden	82	0.38	pos affect
Peters et al. 2013	best possible self	1.0	college students	84	active	yes	LOT-R	1	Netherlands	56	0.52	life satisfaction
Peters et al. 2015	best possible self	0.35	college students	57	active	yes	expectancy	0	Netherlands	56	0.19	-

Pietrovsky & Mikutta, 2012	best possible self, counting blessings	1.84[1]	depressed	53	active	no	LOT-R	0	Germany	17	0.38*	-
Rizatto 2014	loving kindness meditation	0	community	57	not active	no	LOT-R	0	Ireland	61	-0.02	-
Schonert-Reichl et al. 2015[3]	mindful cognitive-behavioral	9	children	44	active[2]	no	resiliency subscale	0	Canada	99	0.64	depression, aggression, prosocial behavior
Shoshani et al. 2015	Make a Wish	-	child cancer patients	41	no	no	adapted LOT-R	5	Israel	66	0.36	anxiety, depression, quality of life
Smeets et al. 2014	self-compassion	3.75	college students	100	active	yes	LOT-R	0	Netherlands	52	0.41	rumination
Tak et al. 2014[3]	cognitive-behavioral training	13.3	adolescents	47	no	yes	LOT-R	104	Netherlands	1341	0.05*	-
Tavokoly et al. 2014	psychodrama stress management etc.	12	troubled college students	100	not active	no	LOT-R	0	Iran	32	0.62	psychologi-cal balance
Wagner et al. 2007	writing disclosure and CBT	0	complicated grief	92	not active	no	LOT-R	0	German speaking	51	0.26	post-trauma growth

Note. In-person hours of training = 0 means intervention done online; Fosnaugh et al. training time is our estimate; students = university students; other significant outcomes include only outcome variables that might be affected by increased optimism; exec task means executive task performance after experiencing pain; pos & neg affect mean positive and negative affect. Some studies examined positive and negative affect and found no significant effect.

*Carry-forward intention-to-treat analyses, rather than completer analyses.

[1]We estimated training time.

[2]Control was CBT without religious elements.

[3]Random assignment of schools, rather than individuals.

Table 2

Moderator results

Moderator	*k*	*g*	Lower	Upper	*p*	Q^1	*p*	i^2
Intervention method, $Q(1) = 8.1$, $p = .004$								
Best possible self	10	0.64	0.42	0.86	<.001	15.7	.07	43
Not best possible self	19	0.28	0.18	0.39	<.001	23.4	.14	26
Intervention format, $Q(1) = 4.4$, $p = .04$								
In-person	24	0.46	0.32	0.61	<.001	61.0	<.001	62
Online	5	0.20	0.00	0.40	.056	1.3	.86	0
Trainee type, $Q(1) = 1.5$, $p = .22$								
Normal	17	0.48	0.29	0.66	<.001	58.9	<.001	73
Identified problem	12	0.33	0.19	0.47	<.001	3.0	.99	0

Trainee age, $Q(1) = 0.3$, $p = .59$								
Adult	26	0.43	0.32	0.54	<.001	33.6	.12	26
Child or adolescent	3	0.31	-.09	0.71	.12	9.0	.01	78
Type of control[1], $Q(1) = 7.2$, $p = .007$								
Active	17	0.51	0.36	0.66	<.001	25.4	.06	40
Not active	11	0.23	0.09	0.37	.001	13.6	.19	26
Participants paid $Q(1) = 1.4$, $p = .234$								
No	18	0.36	0.25	0.46	<.001	9.0	.93	0
Yes	11	0.53	0.26	0.81	<.001	30.3	<.001	83
Optimism measure[2], $Q(1) = 9.5$, $p = .002$								
LOT-R or related version	19	0.24	0.14	0.34	<.001	20.4	.31	12
Other expectancy measure	8	0.68	0.42	0.93	<.001	15.0	.04	53

Final assessment more than a day past end of training, $Q(1) = 5.0$, $p = .025$								
Assessment within one day of end of training	23	0.46	0.34	0.58	<.001	30.6	.11	28
Later assessment	6	0.22	0.04	0.40	.02	8.4	.14	40
Intention-to-treat analysis, Q(1) = 27.2, p < .001								
No	25	0.46	0.35	0.57	<.001	31.3	.14	23
Yes	4	0.07	-0.03	0.17	.19	1.0	.80	0

[1] The study of Chesney et al. (2003) was not included in this analysis because it used both types of control groups.

[2] The study of Fornaugh et al. (2009) was not included in this analysis because it used both types of optimism measure.

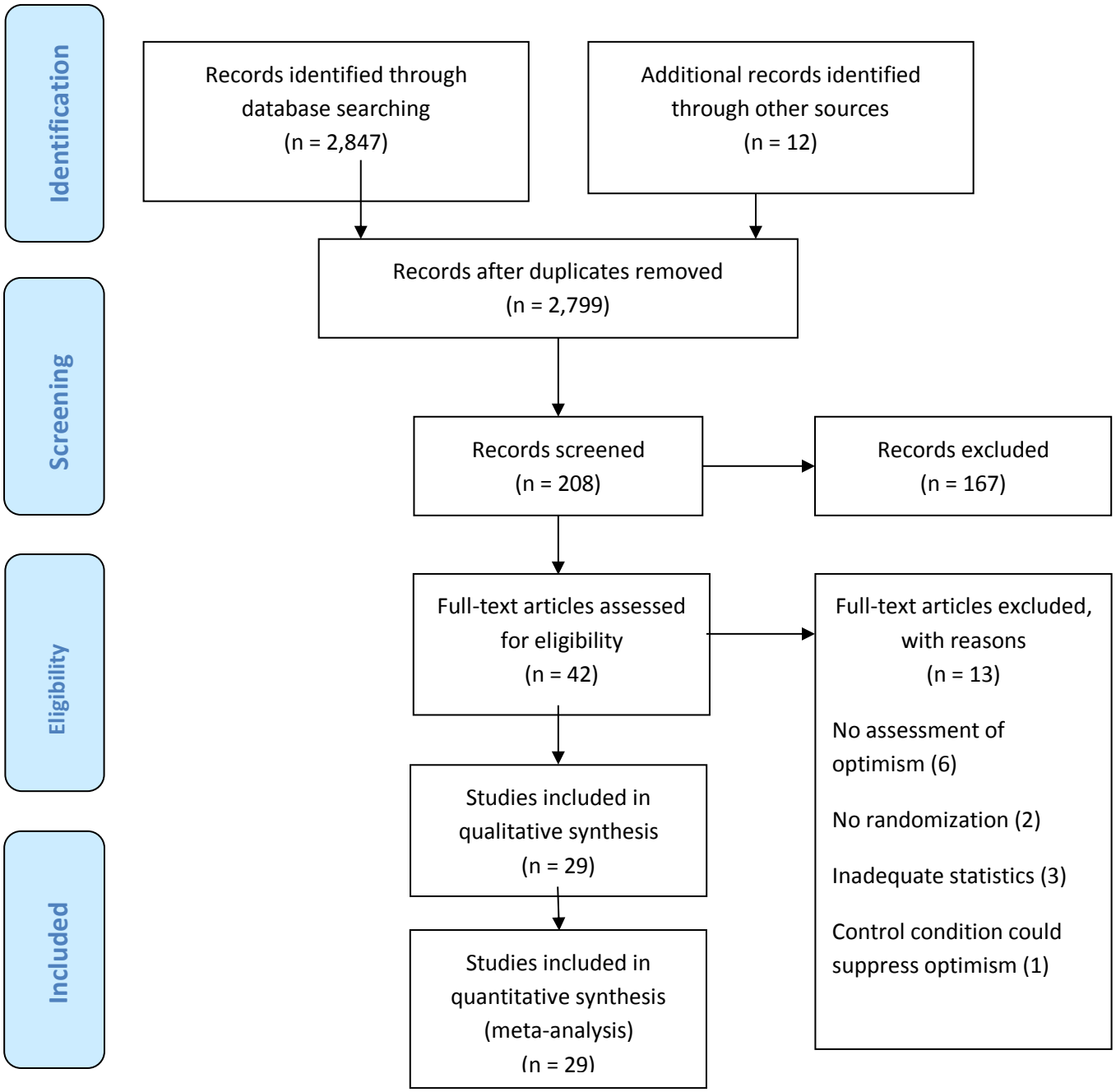
Identification
Screening
Eligibility
Included
Records identified through database searching (n = 2,847)
Additional records identified through other sources (n = 12)
Records after duplicates removed (n = 2,799)
Records screened (n = 208)
Records excluded (n = 167)
Full-text articles assessed for eligibility (n = 42)
Full-text articles excluded, with reasons (n = 13)
No assessment of optimism (6)
No randomization (2)
Inadequate statistics (3)
Control condition could suppress optimism (1)
Studies included in qualitative synthesis (n = 29)
Studies included in quantitative synthesis (meta-analysis) (n = 29)

Figure 1: Identification of Studies

Flow chart template from: Moher D, Liberati A, Tetzlaff J, Altman DG, The PRISMA Group (2009). *P*referred *R*eporting *I*tems for *S*ystematic Reviews and *M*eta-*A*nalyses: The PRISMA Statement. PLoS Med 6(7): e1000097. doi:10.1371/journal.pmed1000097

Study name	Comparison	Outcome	Hedges's g	Standard error	Variance	Lower limit	Upper limit	Z-Value	p-Value
Antoni et al. 2001	1 control gp	LOT or LOT-R	0.492	0.202	0.041	0.097	0.888	2.439	0.015
Boselie et al. 2014	1 control gp	Combined	0.331	0.232	0.054	-0.123	0.786	1.429	0.153
Boselie et al. (in press) Study 1	1 control gp	Combined	1.021	0.252	0.064	0.526	1.516	4.045	0.000
Boselie et al. (in press) Study 2	1 control gp	Combined	0.541	0.269	0.072	0.014	1.068	2.011	0.044
Boselie et al (under review)	1 control gp	Combined	0.807	0.281	0.079	0.257	1.357	2.875	0.004
Breitbart et al. 2010	1 control gp	LOT or LOT-R	0.408	0.286	0.082	-0.153	0.968	1.425	0.154
Chesney et al. 2003	Combined	LOT or LOT-R	0.313	0.214	0.046	-0.107	0.733	1.460	0.144
Drozd et al. 2014	1 control gp	LOT or LOT-R	0.175	0.204	0.042	-0.226	0.575	0.853	0.393
Fosnaugh et al. 2009	Combined	Combined	0.353	0.194	0.038	-0.028	0.733	1.816	0.069
Hanssen et al. 2012	1 control gp	Combined	1.001	0.237	0.056	0.537	1.464	4.228	0.000
Kjellgren et al. 2011	1 control gp	LOT or LOT-R	0.181	0.331	0.110	-0.468	0.830	0.546	0.585
Kjellgren & Westman 2014	1 control gp	LOT or LOT-R	0.697	0.255	0.065	0.198	1.197	2.736	0.006
Knaevelsrud et al. 2010	1 control gp	LOT or LOT-R	0.354	0.213	0.045	-0.064	0.772	1.660	0.097
Koenig et al. 2015	1 control gp	LOT or LOT-R	0.163	0.173	0.030	-0.177	0.502	0.938	0.348
Lee et al. 2006	1 control gp	LOT or LOT-R	0.427	0.233	0.054	-0.030	0.884	1.831	0.067
Lengacher et al. 2009	1 control gp	LOT or LOT-R	0.233	0.220	0.048	-0.198	0.663	1.059	0.290
Littman-Ovadia & Nir, 2014	1 control gp	LOT or LOT-R	0.172	0.227	0.051	-0.272	0.616	0.761	0.447
Meevissen et al. 2011	1 control gp	Combined	1.258	0.309	0.096	0.652	1.864	4.067	0.000
Peters et al. 2010	1 control gp	Combined	0.382	0.221	0.049	-0.052	0.816	1.724	0.085
Peters et al. 2013	Combined	LOT or LOT-R	0.520	0.271	0.073	-0.010	1.050	1.921	0.055
Peters et al. 2015	1 control gp	Combined	0.192	0.264	0.070	-0.326	0.710	0.727	0.467
Pietrowsky & Mikutta (2012)	1 control gp	LOT or LOT-R	0.337	0.465	0.216	-0.574	1.248	0.724	0.469
Rizzato (2014)	1 control gp	LOT or LOT-R	-0.016	0.259	0.067	-0.523	0.491	-0.063	0.950
Schoneri-Reichl et al. 2015	1 control gp	Resiliency Inventory subscale	0.640	0.205	0.042	0.239	1.041	3.126	0.002
Shoshani et al. 2015	1 control gp	LOT or LOT-R	0.360	0.245	0.060	-0.121	0.841	1.469	0.142
Smeets et al. 2014	1 control gp	LOT or LOT-R	0.411	0.276	0.076	-0.131	0.952	1.486	0.137
Tak et al. 2014	1 control gp	LOT or LOT-R	0.048	0.055	0.003	-0.059	0.155	0.878	0.380
Tavakoly et al. 2014	1 control gp	LOT or LOT-R	0.622	0.353	0.125	-0.071	1.314	1.759	0.079
Wagner et al. 2007	1 control gp	LOT or LOT-R	0.260	0.277	0.077	-0.283	0.803	0.940	0.347
			0.412	0.062	0.004	0.291	0.533	6.674	0.000

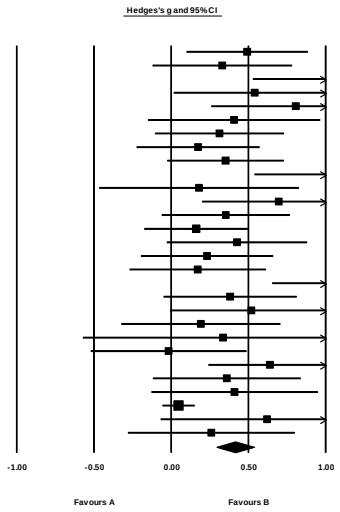

Figure 2. Forest plot of effect sizes.

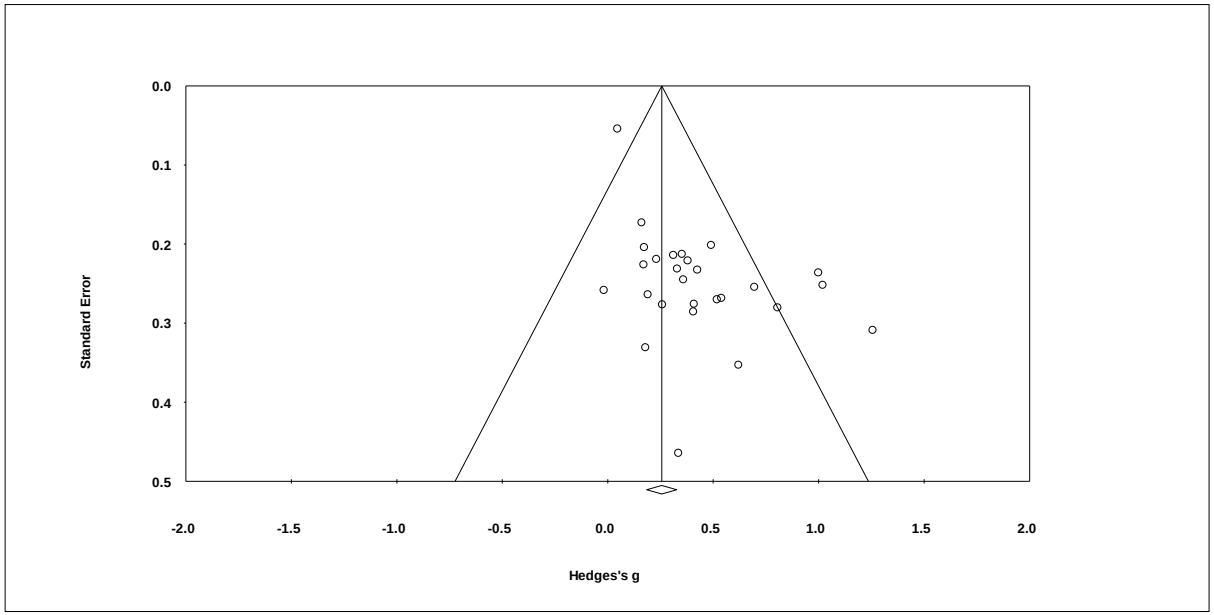

Figure 3:. Funnel plot of standard error by Hedges' *g*