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Psychological treatment of depression in inpatients: A systematic review and meta-analysis

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ABSTRACT

Research on psychological treatment of depression in inpatients is not conclusive, with some studies finding clear positive effects and other studies finding no significant benefit compared to usual care or structured pharmacotherapy. The results of a meta-analysis investigating how effective psychological treatment is for depressed inpatients are presented. A systematic search in bibliographical databases resulted in 12 studies with a total of 570 respondents. This set of studies had sufficient statistical power to detect small effect sizes. Psychological treatments had a small ($g = 0.29$), but statistically significant additional effect on depression compared to usual care and structured pharmacological treatments only. This corresponded with a numbers-needed-to-be-treated of 6.17. Heterogeneity was zero in most analyses, and not significant in all analyses. There was no indication for significant publication bias. Effects were not associated with characteristics of the population, the interventions and the design of the studies. Although the number of studies was small, and the quality of many studies was not optimal, it seems safe to conclude that psychological treatments have a small but robust effect on depression in depressed inpatients. More high-quality research is needed to verify these results.

Contents

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1. Introduction

It is well-established that psychological interventions are effective in the treatment of depressive disorders in adults (Churchill et al., 2001; Cuijpers, van Straten, Warmerdam, & Smits, 2008), although the effects may have been overestimated because of publication bias (Cuijpers, Smit, Bohlmeijer, Hollon, & Andersson, 2010) and because of the relatively low methodological quality of many studies in this area (Cuijpers, van Straten, Bohlmeijer, Hollon, & Andersson, 2010). Most research on psychological interventions for depression have been conducted in outpatients with mild to moderate depressive disorders (Churchill et al., 2001; Cuijpers, van Straten, Warmerdam, & Andersson, 2008; Cuijpers, van Straten, Warmerdam and Smits, 2008), often recruited via advertisement or in the general public. Psychological treatments have been found to be less effective in outpatients with chronic depression (Cuijpers, van Straten, Schuurmans et al., 2010), and possibly severe depression (Elkin et al., 1989), although evidence is not conclusive (Driessen, Cuijpers, Hollon, & Dekker, 2010).

Apart from these studies in depressed outpatients, several studies have examined the effects of psychological treatments in depressed inpatients in the past decades. The number of studies in this area, however, is not as large as the number of studies examining psychological treatments for depressed outpatients, presumably because most patients are treated in outpatient settings. Inpatient treatment remains an important treatment option for patients with more severe and chronic depression, who cannot safely stay in their own environment (Wolpert, 2001).

Inpatients belong to the most severe and disabled patient populations. Many of these patients suffer from severe and chronic forms of depression, and better treatment options may improve their recovery and reduce the suffering from themselves as well as their relatives. It is important, therefore, to examine the possibilities of psychological treatments to contribute to the reduction of the suffering of depressed inpatients.

Although some studies found positive effects of psychological treatment for depressed inpatients (De Jong, Treiber, & Henrick, 1986; Hopko, Lejuez, Lepage, Hopko, & McNeil, 2003; Lemmens, Eisler, Buysse, Heene, & Demyttenaere, 2009), several other studies did not find significant effects (Barker, Scott, & Eccleston, 1987; Bowers, 1990; De Jong-Meyer & Hautzinger, 1996; Miller, Norman, & Keitner, 1989). Meta-analysis can be used to integrate the results of these studies to get a better estimate of the overall effect size. Because no meta-analysis has attempted to integrate the results of the studies examining the effects of psychological treatments of depressed inpatients until now, this study is aimed at presenting the results of such a meta-analysis. Our hypothesis for this study was that psychological treatments would result in better outcomes compared to the care usually given to depressed patients in inpatient settings.

2. Methods

2.1. Identification and selection of studies

A database of 1120 papers on the psychological treatment of depression was used. This database has been described in detail elsewhere (Cuijpers, van Straten, Warmerdam, & Andersson, 2008) and has been used in a series of 25 earlier published meta-analyses (www.evidencebasedpsychotherapies.org). The database is continuously updated and was developed through a comprehensive literature search (from 1966 to January 2010) in which 10,346 abstracts in Pubmed (1831 abstracts), PsycInfo (2943), Embase (3087) and the Cochrane Central Register of Controlled Trials (2485) were examined. These abstracts were identified by combining terms indicative of psychological treatment and depression (both MeSH-terms and text words). For this database, the primary studies from 42 meta-analyses of psychological treatment for depression were also checked to secure that no published studies had been missed (www.evidencebasedpsychotherapies.org). For the current study, the full texts of these 1120 papers were examined. The reference lists of earlier reviews of psychotherapies for chronic depression and dysthymia were also examined (Stuart, Wright, Thase, & Beck, 1997; Cole, Elie, McCusker, Bellavance, & Mansour, 2000; Huber, 2005), as well as the references of the included primary studies.

We included (a) randomized trials (b) in which the effects of a psychological treatment (c) was compared to the effects of a control group (d) in adults who were hospitalized in a psychiatric setting during the treatment and (e) who had a depressive disorder (established with a diagnostic interview) as the primary presenting problem. Only studies were included in which structured and standardized psychotherapies referring to a protocol or clearly defined method were used, which were clearly different from the standard care. Studies in patients with comorbid substance use disorders and depression in substance use disorders units were excluded (Bowman, Ward, Bowman, & Scogin, 1996; Daughters et al., 2009), because depression was not the primary disorder in these patients, and the treatment units differed too much from other psychiatric inpatient settings.

2.2. Quality assessment

The validity of included studies was assessed with four criteria of the 'Risk of bias' assessment tool, developed by the Cochrane Collaboration (Higgins & Green, 2008). This tool assesses possible sources of bias in randomized trials, including the adequate generation of allocation sequence; the concealment of allocation to conditions; the prevention of knowledge of the allocated intervention; and dealing with incomplete outcome data. The two other criteria of the 'Risk of bias' assessment tool were not used in this study. One is aimed at selective outcome reporting (which is only possible in the tool if the study protocol is available, or other very clear indications of reporting only a selection of outcomes; none of studies reported publication of a study protocol), the other criterion is a rest category of possible problems that could put the study at a high risk of bias (but we did not find any indication for this).

2.3. Meta-analyses

For each comparison between a psychological treatment and a control group (or another active treatment), the effect size indicating the difference between the two groups at post-test was calculated (Cohen's d or standardized mean difference). Effect sizes were calculated by subtracting (at post-test) the average score of the psychological treatment group from the average score of the comparison group, and dividing the result by the pooled standard deviations of the two groups. Effect sizes of 0.8 can be assumed to be large, while effect sizes of 0.5 are moderate, and effect sizes of 0.2 are small (Cohen, 1988). Because several studies had small sample sizes we corrected the effect size for small sample bias according to the procedures suggested by Hedges and Olkin (1985).

In the calculations of effect sizes, we only used those instruments that explicitly measured symptoms of depression, such as the Beck Depression Inventory (Beck, Ward, Mendelson, Mock, & Erbaugh, 1961) and the Hamilton Rating Scale for Depression (HRSD; Hamilton, 1960). If more than one depression measure was used, the mean of the effect sizes was calculated, so that each study only provided one effect size. If means and standard deviations were not reported, we used the procedures of the Comprehensive Meta-Analysis software (see below) to calculate the effect size using dichotomous outcomes. If insufficient data were reported to calculate an effect size, the study was excluded (which was the case in one study, which reported no data or tests for the four conditions to which the subjects were randomized; Waring et al., 1988).

To calculate pooled mean effect sizes, we used the computer program Comprehensive Meta-Analysis (version 2.2.021). As we expected considerable heterogeneity among the studies, we decided

to calculate mean effect sizes using a random effects model. In the random effects model it is assumed that the included studies are drawn from 'populations' of studies that differ from each other systematically (heterogeneity). In this model, the effect sizes resulting from included studies not only differ because of the random error within studies (as in the fixed effects model), but also because of true variation in effect size from one study to the next.

The standardized mean difference is not easy to interpret from a clinical point of view. Therefore, we transformed the standardized mean differences into the numbers-needed-to-be-treated (NNT), using the formulae provided by Kraemer and Kupfer (2006). The NNT indicates the number of patients that have to be treated in order to generate an additional positive outcome in one of them (Smit, Ederveen, Cuijpers, Deeg, & Beekman, 2006).

As a test of homogeneity of effect sizes, we calculated the I^2-statistic which is an indicator of heterogeneity in percentages. A value of 0% indicates no observed heterogeneity, and larger values show increasing heterogeneity, with 25% as low, 50% as moderate, and 75% as high heterogeneity (Higgins, Thompson, Deeks, & Altman, 2003). We also calculated the Q-statistic, but only report whether this was significant or not.

Subgroup analyses were conducted according to the mixed effect model. In this model, studies within subgroups are pooled with the random effects model, while tests for significant differences between subgroups are conducted with the fixed effects model. For continuous variables, we used meta-regression analyses to test whether there was a significant relationship between the continuous variable and the effect size, as indicated with a Z-value and an associated p-value.

Publication bias was tested by inspecting the funnel plot on primary outcome measures, and by Duval and Tweedie's trim and fill procedure (Duval & Tweedie, 2000) which yields an estimate of the effect size after the publication bias has been taken into account (as implemented in Comprehensive Meta-analysis, version 2.2.021).

2.4. Power calculation

Because we expected a limited number of studies, we conducted a power calculation to examine how many studies should be included in order to have sufficient statistical power to identify relevant effects. We conducted a power calculation according to the procedures described by Borenstein, Hedges, Higgins, and Rothstein (2009). We aimed at a sufficient number of studies to be able to identify a small effect size of 0.3. These calculations indicated that we would need to include at least 20 studies with a mean sample size of 30 (15 participants per condition), to be able to detect an effect size of d = 0.3 (conservatively assuming a medium level of between-study variance, $\tau 2$, a statistical power of 0.80, and a significance level, alpha, of 0.05). Alternatively, we would need 15 studies with 40 participants each to detect an effect size of d = 0.30, or 14 studies with 50 participants.

3. Results

3.1. Selection and inclusion of studies

In Fig. 1, a flowchart describing the inclusion of studies is presented. A total of 10,346 abstracts were examined, of 1122 the full texts were retrieved, of which 879 were excluded. A total of 263 trials were identified and included in our database (www.evidencebasedpsychotherapies.org). Fourteen trials were aimed at inpatients, met our inclusion criteria and were included in the current meta-analysis.

3.2. Characteristics of included studies

The twelve studies included a total of 570 respondents (308 in the psychotherapy conditions and 262 in the control conditions). Selected characteristics of the studies are presented in Table 1.

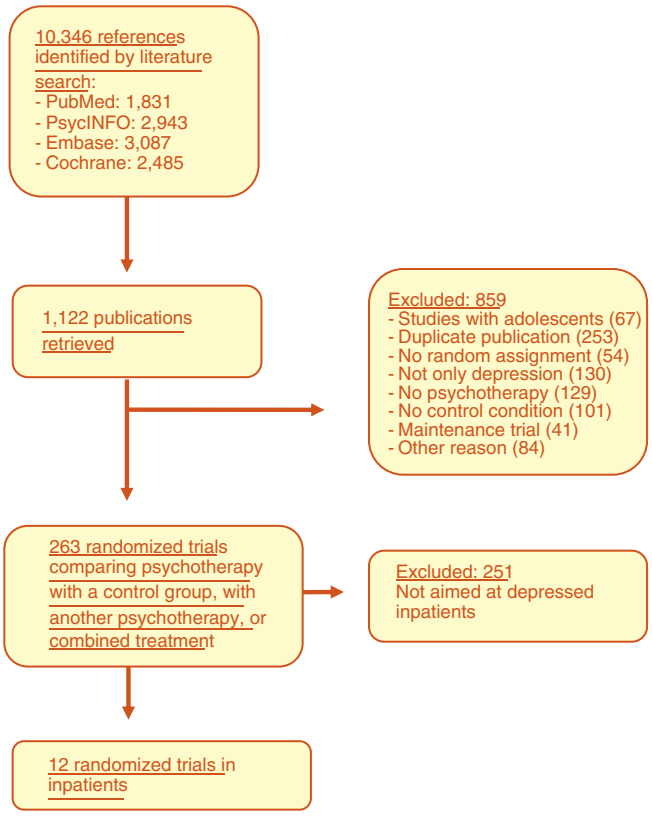

Fig. 1. Flowchart of inclusion of studies.

Two studies were aimed at patients with chronic depressive disorder, lasting for at least two years. One was specifically aimed at older adults. In nine studies one treatment was compared with a control group, while in the remaining three studies two treatments were compared with one control group. This resulted in 15 comparisons between a psychological treatment and a control group. In seven comparisons cognitive behavior therapy (CBT) was examined, behavioral activation therapy in two, and the remaining six comparisons examined other therapies (family therapy, interpersonal psychotherapy, problem-solving therapy, social skills training and a mix of different approaches). In eight therapies an individual treatment format was used, one used a group format, and the remaining six used another format (family therapy or combined format). The number of sessions ranged from six to 47 (six studies had six to nine sessions, 5 had 10 to 15 sessions, and 4 more than 15 sessions). In two studies the majority of participants did not receive pharmacotherapy in any condition (0% in De Jong et al., 1986; 9.7% in Nickel et al., 2004), while in the other studies all participants received pharmacotherapy (in one study one of the patients did not receive pharmacotherapy; Lemmens et al., 2009). The HRSD was used in 9 of the 14 studies, the BDI was used in 10 studies. Only one study used another instrument to assess depression (De Jong and colleagues used the D-scale, as well as the HRSD and the BDI; von Zerssen, 1976). Five studies were conducted in the United States, five in Germany, and two in other European countries (the United Kingdom and Belgium). Nine studies were written in English, three in German.

3.3. Quality of included studies

The quality of the studies was not optimal. Seven of the 12 studies gave insufficient information whether the allocation sequence was generated adequately. Nine studies gave insufficient information about whether the allocation was adequately concealed. We assessed whether incomplete outcome data were adequately addressed, by

Table 1
Selected characteristics of psychological treatment of depression in inpatients.

	Definition of depression	Excluded comorbidity	Conditions	N	Psychotherapy	Format	Nsess	Control interventions	Concurrent pharmaco-therapy	Follow up	Instruments	Country
Barker, Scott, & Eccleston, 1987	Chronic MDD (>2 years) (RDC) AND treatment refractory	None reported	1. CBT 2. Control	10 10	Cognitive behavior therapy	I	15	No description reported	6 weeks phenelzine, L-tryptophan + lithium; followed by another combination	Post-test	HAMD	UK
Bowers, 1990	MDD (DSM-III)	bipolar, panic, alcoholism, drug use, antisocial personality disorder, psychotic depression, schizophrenia, organic brain syndrome, mental retardation	1. CBT 2. Relaxation 3. Control	10 10 10	CBT according to Beck, Rush, Shaw and Emery, 1979	I	12	Usual attention from treatment team, including activity therapy, occupational therapy, recreational therapy	Nortriptyline	Dis-charge	HAMD; BDI	US
Bowers, Stuart, MacFarlane and Gorman, 1993	MDD (DSM-III-R) + HRSD ≥ 15 + BDI ≥ 15	Active substance abuse, other axis I disorder, axis II personality disorder,	1. CBT 2. unguided CCBT 3. Control	8 6 8	CBT according to Beck et al., 1979	I	8	Participation in the activities of the ward, including milieu therapy, occupational therapy, vocational rehabilitation.	Pharmacotherapy according to the choice of the treating physician	Post-test	HAMD; BDI	US
Brand and Clingempeel, 1992	Geriatric patients with MDD (RDC)	cognitive impairment, history of alcohol or substance abuse, psychotic features	1. BAT 2. Control	27 26	Behavioral activation therapy	G	8	Standard hospital programs, including adjunctive therapies (e.g., art, music), regular sessions with treatment team members	Not further specified	Post-test	HAMD; BDI	US
De Jong, Treiber and Henrick, 1986	Chronic MDD + dysthymia (DSM-III) + BDI > 20	endogenous or melancholic major depression; positive family history of affective disorder in any first-degree relative;	1. BAT + SST + CT 2. CBT 3. Nonspecific control	10 10 10	1. Behavioral activation therapy + social skills training + cognitive restructuring 2. Cognitive restructuring	C	33 47	Occupational/recreational therapy, relaxation training, exercise,	No pharmacotherapy during therapy in all three conditions	6 months	HAMD, BDI, D-scale	GER
De Jong-Meyer and Hautzinger, 1996	Endogeneous depression (ICD-9) + MDD (DSM-III-R)	other mental disorders (except personality disorders)	1. CBT 2. Control	36 44	CBT according to Beck et al., 1979	I	24	General supportive therapy	Amitriptyline	12 months	HAMD; BDI	GER
Hautzinger, de Jong-Meyer, Treiber, Rudolf and Thien, 1996	MDD or dysthymia (DSM-III-R) + neurotic depr (ICD-9) + HAMD/ BDI ≥ 20	other mental disorders (except personality disorders)	1. CBT 2. Control	20 22	Cognitive behavior therapy according to Beck et al., 1979	I	24	General supportive therapy	Amitriptyline	12 months	HAMD; BDI	GER
Hopko, Lejuez, Lepage, Hopko, and McNeil, 2003	MDD	psychotic disorders	1. BAT 2. Control	10 15	Behavioral activation therapy	I	6	Token economy system; no further description provided	SSRIs or TCAs (not further specified)	Post-test	BDI	US
Lemmens, Eisler, Buysse, Heene, and Demyttenaere, 2009	MDD (DSM-IV)	bipolar disorders	1. Single family therapy 2. Multi family therapy 3. Control	25 35 23	Systemic couple therapy for depression; multi-family group therapy (conceptually identical)	FG	7	Non-verbal therapies; cognitive behavioral approaches; systemic therapy; activation,	Not further specified	12 months	BDI	BEL
Miller, Norman and Keitner, 1989	MDD (DIS) + BDI > 17 + M-HRSD > 17	bipolar, alcohol or drug dependence, schizophrenia, somatization disorder, antisocial personality, organic brain syndrome	1. CBT 2. SST 3. Control	15 14 17	CBT according to Beck et al., 1979; Social skills training (Bellack, Hersen & Himmelhoch, 1981)	I	10 12	daily meetings with nursing staff, occupational therapy, social work evaluation of the family (no psychotherapy)	semi-structured medication protocol	6 and 12 months	M-HAMD, BDI	US
Nickel et al., 2004	Depression/adjustment disorder (SCID)	psychotic disorders; personality disorders	1. Couple therapy 2. Control	15 16	Couple therapy	I	6	Standard psychotherapy, including group Gestalt therapy, breathing therapy, and exercise	Only 3 of 31 had used pharmaco-therapy in past 2 years (not further specified)	Post-test	BDI	GER
Schramm et al., 2007	MDD (SCID) + HAMD > 16	bipolar I, substance abuse/ dependency, other axis I disorders, mental disorder because of organic factors, borderline/antisocial personality disorder; psychotic symptoms; severe cognitive impairment	1. IPT 2. Control	63 61	Interpersonal psychotherapy	C	11	Clinical management (psychoeducation and supportive therapy); no further description	Standardized pharmacotherapy (sertraline as first line treatment)	12 months	HAMD; BDI	GER

Abbreviations: BAT: Behavioral activation therapy; BDI: Beck Depression Inventory; BEL: Belgium; C: combination of individual and group; CAU: care-as-usual; CCBT: Computerized cognitive behavior therapy; Comb: combined individual/ group; CT: cognitive restructuring; DIS: Diagnostic Interview Schedule; D-scale: Depression scale; FG: combined family and group format;G: group; GER: Germany; Grp: group; HAMD: Hamilton Depression Rating Scale; Ind: individual; I: individual; IPT: Interpersonal psychotherapy; MDD: major depressive disorder; M-HAMD, Modified HAMD; Pharm: pharmacotherapy; RDC: Research Diagnostic Criteria; SCL-90-D: Symptom Checklist-90-depression scale; SST: Social skills training; UK: United Kingdom; US: United States.

conducting intention-to-treat analyses with all randomized subjects being included in the analyses. This was the case in 5 of the 12 studies. In 6 studies knowledge of the allocated interventions was adequately prevented by blinding of the assessors, while in three studies only self-report measures were used. Three studies met all four quality criteria (Lemmens et al., 2009; Nickel et al., 2004; Schramm et al., 2007).

3.4. Effects of psychological treatments for inpatients

The overall mean effect size indicating the difference between psychological treatments and control groups was g = 0.29 (95% CI: 0.13 ~ 0.44; p < 0.001), which corresponds with a NNT of 6.17. Heterogeneity was zero and not significant. These results are summarized in Table 2, and in Fig. 2.

A post-hoc power calculation showed that our set of studies had sufficient statistical power to detect a significant effect size of g = 0.27. This was based on the mean number of participants in the studies (which was 48), and the finding that the between-study variance (τ^2) was zero, which results in higher statistical power to detect significant effect sizes.

In this meta-analysis we included three studies in which two psychological treatments were compared with the same control group. This means that multiple comparisons from these three studies were included in the same analysis. These multiple comparisons, however, are not independent of each other, which may have resulted in an artificial reduction of heterogeneity and may have affected the pooled effect size. We examined the possible effects of this by conducting an analysis in which we included only one effect size per study. First, we included only the comparison with the largest effect size from the studies with multiple comparisons. Then we conducted another analysis in which we included only the smallest effect size. As can be seen from Table 2, the resulting effect sizes were almost the same as in the overall analyses. Heterogeneity did not increase, and remained zero in these analyses.

We also calculated the effect sizes based on the BDI (while excluding the effect sizes based on other measurement instruments), and found comparable results (g = 0.24; 95% CI: 0.08–0.40; $I^2 = 0$; NNT = 7.46). The effect size based on the HRSD was also comparable with the overall effect size (g = 0.33; 95% CI: 0.14–0.52; 5.43), and although there was some heterogeneity ($I^2 = 4.81\%$), this was very small and not significant.

Neither the funnel plot nor Duval and Tweedie's trim and fill procedure pointed at a significant publication bias. The effect size indicating the difference between the treatment and control condition was remained exactly the same after adjustment for publication bias (number of trimmed studies: 0).

At 12 months follow-up, the difference between the experimental conditions was reported by five studies (six comparisons). The overall

Table 2
Meta-analyses of studies examining the effects of psychological treatments for depressed inpatients: Hedges' g.

Study		N_{comp}	g	95% CI	Z	$I^{2\ a)}$	p [b)]	NNT
▪ All studies		15	0.29	0.13~0.44	3.57 ***	0		6.17
▪ One effect per study (highest) [c)]		12	0.30	0.13~0.47	3.41 **	0		5.95
▪ One effect per study (lowest) [c)]		12	0.27	0.10~0.45	3.12 **	0		6.58
▪ Only HRSD		11	0.33	0.14~0.52	3.39 **	4.81		5.43
▪ Only BDI		14	0.24	0.08~0.40	2.91 **	0		7.46
Subgroup analyses								
▪ Psychotherapy	– CBT	7	0.19	−0.07~0.44	1.43	0	0.367	9.43
	– BA	2	0.56	0.11~1.00	2.44 *	0		3.25
	– Other	6	0.30	0.07~0.52	2.61 **	0		5.95
▪ Format	– Individual	8	0.24	−0.02~0.50	1.82	0	0.691	7.46
	– Mixed/group/other	7	0.31	0.11~0.51	3.10 **	0		5.75
▪ Number of sessions	– 6 to 9	6	0.41	0.15~0.67	3.09 **	0	0.472	4.39
	– 10 to 15	5	0.18	−0.08~0.44	1.37	0		9.80
	– 16 or more	4	0.32	−0.06~0.69	1.64	26.42		5.56
▪ Pharmacotherapy	– Yes	12	0.26	0.09~0.42	3.02 **	0	0.314	6.85
	– No	3	0.51	0.05~0.97	2.16 *	0		3.55
▪ Region	– USA	6	0.35	0.05~0.64	2.32 *	0	0.633	5.10
	– EU	9	0.26	0.08~0.45	2.76	0		6.85
▪ Quality of study	– High	4	0.28	0.04~0.52	2.27 *	0	0.96	6.41
	– Other	11	0.29	0.08~0.49	2.76 **	0		6.17
Sensitivity analyses								
Only cognitive–behavioral therapies								
▪ All studies		9	0.28	0.06~0.50	2.45 *	0		6.41
▪ One effect per study (highest) [c)]		8	0.26	0.03~0.49	2.21 *	0		6.85
▪ One effect per study (lowest) [c)]		8	0.31	0.08~0.54	2.59 *	0		5.75
▪ Only HRSD		9	0.35	0.10~0.60	2.75 **	15.50		5.10
▪ Only BDI		9	0.26	0.04~0.49	2.34 *	0		6.85
Studies with more than 15 sessions removed								
▪ All studies		11	0.29	0.11~0.48	3.15 **	0		6.17
▪ One effect per study (highest) [c)]		9	0.32	0.12~0.53	3.17 **	0		5.56
▪ One effect per study (lowest) [c)]		9	0.31	0.11~0.52	3.04 **	0		5.75
▪ Only HRSD		7	0.37	0.14~0.59	3.18 **	0		4.85
▪ Only BDI		10	0.24	0.06~0.43	2.55 *	0		7.46
No pharmacotherapy studies removed								
▪ All studies		12	0.26	0.09~0.42	3.02 **	0		6.85
▪ One effect per study (highest) [c)]		10	0.28	0.10~0.46	3.00 **	0		6.41
▪ One effect per study (lowest) [c)]		10	0.27	0.09~0.45	2.88 **	0		6.58
▪ Only HRSD		9	0.28	0.09~0.48	2.84 **	3.37		6.41
▪ Only BDI		11	0.22	0.05~0.39	2.53 *	0		8.06

o: p < 0.10; *: p < 0.05; **: p < 0.01; ***: p < 0.001.
[a)] The Q statistic was significant in none of the analyses.
[b)] This p-value indicates whether the effect sizes between subgroups differ significantly from each other.
[c)] In these analyses only one comparison from each study was used.

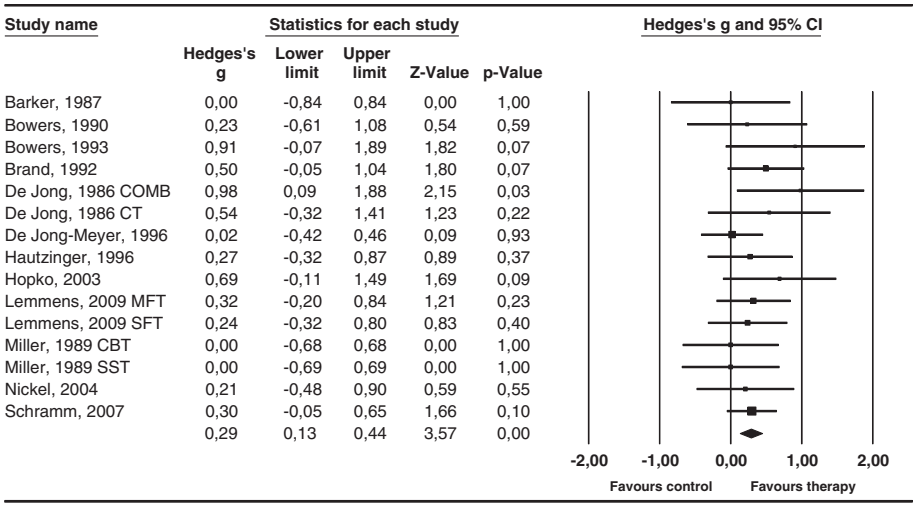

Fig. 2. Standardized effect sizes of psychological treatments for depressed inpatients at post-test: Hedges' g.

effect size was g = 0.32 (95% CI: −0.01–0.66) with a trend (p = 0.057) indicating that the psychotherapy conditions may be more effective than the control conditions. Heterogeneity was moderate to high ($I^2 = 57.28$) and significant ($p<0.05$). These results have to be considered with caution, because these were naturalistic follow-ups in which it was not clear which treatments patients received during the follow-up periods.

3.5. Subgroup and meta-regression analyses

We examined possible moderators of outcome in a series of subgroup analyses (Table 2). As can be seen, we found no indication for a significant difference between different types of psychological treatments (CBT, behavioral activation therapy; other therapies), between different treatment formats (individual; group; mixed or other); region (USA; Europe); between studies with 6 to 9 sessions, and those with 10 to 15 sessions, and 16 or more sessions; between studies in which patients received pharmacotherapy and those in which (the majority of) patients did not receive pharmacotherapy; and between high-quality and other studies (met all four quality criteria; other).

We also conducted a series of meta-regression analyses, in which we examined the association between the effect size on the one hand, and on the other hand the number of sessions, the mean age of respondents, and the percentage of women. None of these three analyses was significant.

3.6. Sensitivity analyses

Because there were several important differences between studies, we conducted a series of sensitivity analyses. In these analyses we selected the subgroup of studies examining cognitive behavioral therapies and examined the overall effect size, and the effect size based on the HRSD and the BDI. We also examined in this subset of studies whether the multiple comparisons from one and the same studies affected the overall outcome. We conducted the same analyses for the subset of studies in which the interventions had 6 to 15 sessions (studies with more sessions were removed), as well as for the subset of studies in which all participants received pharmacotherapy (the two studies in which patients received no pharmacotherapy were removed). The results of these analyses are presented in Table 2. As can be seen, the results of these analyses resulted in comparable analyses as the main analyses, and we found no indication that these subsets of studies were associated with differences in effect size, or higher levels of heterogeneity.

4. Discussion

We found clear indications that psychological treatments of depression have a small, but significant effect on depressed inpatients compared to care-as-usual or structured pharmacotherapies. The number of patients that have to be treated in order to generate one additional positive outcome compared to usual care is 6.17. This outcome was quite robust, and in almost all analyses heterogeneity was zero and not significant. We also did not find indications that the effects were related to characteristics of the patients, the interventions, or general characteristics of the studies.

Effects we found for psychological treatments for inpatients were relatively small, compared to those found in outpatients (Churchill et al., 2001; Cuijpers, van Straten, Warmerdam, & Andersson, 2008; Cuijpers, van Straten, Warmerdam, & Smits, 2008). That should not come as a surprise. In most included studies patients received many different kinds of therapy, including pharmacotherapy, occupational therapies, and unstructured support from nurses and other staff members. From this perspective, it would be better to compare the effects of psychological treatments in inpatient settings to the additional effects of psychotherapy to pharmacotherapy. In these studies, the effect sizes for psychological treatments are also relatively small (Cuijpers, van Straten, Warmerdam, & Andersson, 2009; Cuijpers, Dekker, Hollon, & Andersson, 2009), and comparable with the outcomes we found in inpatients. This suggests, that psychological treatments have small but robust effects on depression in inpatients as well as in outpatients, when these are added to other treatments.

Another possible reason for the relatively small effect may be comorbidity as hospitalization often relates to more severe symptoms and comorbidity. Comorbid disorders, such as personality disorders and anxiety, are known to be present in many cases of severe depression, and they may very well influence the outcome. Future studies on inpatients with depression should preferably report comorbidity rates better, including the role or medication and cognitive functioning which might affect outcome of psychological treatments. It may also be that outpatient studies to a greater extent include first-episode cases of depression, whereas patients who are hospitalized are more likely to have had repeated episodes. Indeed, the natural course of depression is likely to have an influence on

the likelihood of recovery (Paykel, 2008), and as we know that psychological treatments are less likely to benefit more severe and chronic cases (Cuijpers, Smit et al., 2010;Cuijpers, van Straten, Bohlmeijer et al., 2010; Cuijpers, van Straten, Schuurmans et al., 2010) smaller effects are likely to be found in hospitalized patients.

Although this study has shown that psychological treatments for depressed inpatients have a positive effect, it is not clear whether this effect is relevant from a clinical or economical point of view. On the one hand it could be said that a numbers-needed-to-treat of six is a considerable contribution to relieving the burden of disease in these patients. On the other hand it could be argued that treating six patients of which only will benefit sufficiently from the therapy does not justify such an intervention. This is even more complicated by the fact that the outcomes at the longer term are not known.

The positive effects found in this study should be an encouragement to conduct more research. High-quality studies with sufficient statistical power should focus on the longer-term effects of these treatments, and should not only focus on the clinical outcomes but also on economical cost–benefit analyses. An important question is why the psychological treatments examined in these studies have an additional effect to the therapies that are part of the usual care of most settings. Perhaps this can lead to a better understanding of what works in these therapies, and how brief interventions with larger effect sizes can be developed.

This study has several limitations. First, the number of included studies was relatively small, although we had sufficient statistical power to detect small overall effect sizes. However, the number of studies was not sufficient to examine moderators of outcome in subgroup-analyses, because each subgroup contained only a limited number of studies. On the other hand, the statistical heterogeneity was zero in virtually all analyses, suggesting that the results are quite robust in different settings and populations. Second, there were several differences between the studies we examined, including differences between the settings, the treatments, the patients, the in- and exclusion criteria, and the control groups. We found few indications, however, that the results of the studies differed between these groups of studies. A third important limitation was that the quality of many included studies was not optimal and research in psychotherapy for depressed outpatients has shown that the effect sizes are strongly related to the quality of the studies (Cuijpers, Smit et al., 2010;Cuijpers, van Straten, Bohlmeijer et al., 2010; Cuijpers, van Straten, Schuurmans et al., 2010). On the other hand, in the current study we found no evidence that the high-quality studies had significantly lower effect sizes than other studies.

Despite these limitations it seems safe to conclude that psychological treatments have a small but robust effect on depression in depressed inpatients. Offering these interventions to all depressed inpatients will result in a positive outcome in one of every six patients, compared to the usual care. Considering the huge disease burden of depression and the suffering of individual patients and their families, these positive results should be reason enough to examine these therapies better in high-quality studies and perhaps in the longer term to disseminate such treatments in routine care.

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