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Review

How much psychotherapy is needed to treat depression? A metaregression analysis

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ABSTRACT

Background: Although psychotherapies are effective in the treatment of adult depression it is not clear how this treatment effect is related to amount, frequency and intensity of therapy.

Methods: To fill this gap in knowledge, the present metaregression analysis examined the association between the effects of psychotherapy for adult depression and several indicators of amount, frequency and intensity of therapy. The analysis included 70 studies (92 comparisons) with 5403 patients, in which individual psychotherapy was compared with a control group (e.g. waiting list, care-as-usual).

Results: There was only a small association between number of therapy sessions and effect size, and this association was no longer significant when the analysis adjusted for other characteristics of the studies. The multivariable analyses also found no significant association with the total contact time or duration of the therapy. However, there was a strong association between number of sessions per week and effect size. An increase from one to two sessions per week increased the effect size with $g=0.45$, while keeping the total number of treatment sessions constant.

Discussion: More research is needed to establish the robustness of this finding. Based on these findings, it may be advisable to concentrate psychotherapy sessions within a brief time frame.

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1. Introduction

It is well-established that psychological therapies, such as cognitive behavior therapy (Butler et al., 2006), interpersonal psychotherapy (Cuijpers et al., 2011), behavioral activation therapy (Dimidjian et al., 2011), problem-solving therapy (Cuijpers et al., 2007; Malouff et al., 2007), and possibly psychodynamic therapy (Shedler, 2010) are effective in the treatment of adult depression. It is not clear, however, how long such a therapy should last, how many sessions are optimal and what the best intensity of psychotherapy is.

Determining the amount, frequency and intensity of therapy is of great practical and scientific significance. In practical terms, we may draw an analogy here with pharmacological dose-response studies, which seek to discover the optimal dose of a medical substance. If the optimal dose is unknown, chances are that patients will receive either too little or too much medication. Likewise, there probably exists an optimal dose of psychotherapy. If the optimal dose of psychotherapy is low, then a few brief sessions may suffice to combat depression. Such brief treatments may greatly reduce the personal and societal burdens of depression (Kazdin and Blase, 2011). On the other hand, if the optimal dose of psychotherapy is high, then it becomes sensible for society to invest in extended treatments, because these translate directly into greater health benefits. At a scientific level, gaining insight into the optimal number of treatment sessions can illuminate the change process in patients and improve the theoretical understanding of how psychotherapies support this process.

Early research in psychotherapy has suggested that the improvement in patients increases with a larger number of sessions (Howard et al., 1986; Kopta et al., 1994). A systematic review found that about 60% of patients had improved after about 13 sessions (Hansen et al., 2002). In this research, it is usually assumed that the effect of therapy is greater in earlier sessions and levels off as the number of sessions increases (Kopta et al., 1994). There is, however, also evidence suggesting that the effects of increasing the number of sessions differ depending on the characteristics of the problem and the therapy (Barkham et al., 2006; Reynolds et al., 1996). Pooling the outcomes for several groups of patients and therapies may artificially create the impression of diminishing returns for later sessions, which in actuality are based on a set of multiple linear improvements. Most research on the influence of number of sessions on outcome, however, is based on open, uncontrolled studies (Hansen et al., 2002; Hansen and Lambert, 2003). These studies do not account for natural recovery rates and consequently these studies can only show that patients get better during treatment. Whether this improvement can be attributed to the treatment cannot be established with uncontrolled studies.

Trials in which longer and shorter therapies are directly compared with each other in randomized trials, can much better answer the question whether longer therapies are more effective than brief therapies. In the field of psychotherapy for adult depression, three such trials have been conducted (Shapiro et al., 1994; Barkham et al., 1996; Dekker et al., 2005). The results of these studies have been mixed. Some studies found larger effects for longer therapies, especially in more severe depression

(Shapiro et al., 1994; Barkham et al., 1996). However, other studies found no or limited differential effects of longer versus shorter therapies (Dekker et al., 2005; Molenaar et al., 2011).

It is further important to note that not only the number of sessions is relevant when examining the association between amount, frequency and intensity of therapy on the one hand and outcome on the other. The duration of a session typically varies from half an hour to 2 h, resulting in considerable differences in total time of contact between client and therapist. Moreover, the frequency of therapy session can also vary considerably, with some therapies having two sessions per week, while others have only one session per two weeks. This results in considerable differences in the total duration and intensity of psychotherapy. There is very little knowledge about these indicators and their association with the effects of psychotherapies for depression.

There is some evidence from the field of anxiety disorders on these issues. In one randomized trial it was found that 12-weekly sessions of cognitive behavior therapy resulted in better outcomes and less drop-out than the same number of sessions extended over 18 weeks (Herbert et al., 2004). Another trial found that a massed three-week cognitive behavioral therapy for panic disorder was equally effective as a traditional spaced 13-week cognitive behavioral therapy schedule (Bohni et al., 2009). Abramowitz et al. (2003) examined whether 15 sessions of therapy delivered daily over 3 weeks was more effective than the same 15 sessions delivered twice weekly over 8 weeks, and found a trend toward more improvement in the intensive group. We found no trial of this kind in the field of psychotherapy for depression, however.

In view of these important gaps in the literature, we decided to conduct a meta-analytic study that systematically examined the association between effectiveness of psychotherapies for adult depression on the one hand and the number of treatment sessions, duration and frequency on the other. Our meta-analysis sought to improve prior research in this domain in two main ways. First, unlike previous work, we focused on randomized controlled trials, which afford greater confidence in the causal effects of psychotherapy. Second, we examined the association between the effects of psychotherapies for adult depression and several indicators, namely: numbers of treatment sessions, duration of treatment, total contact time with the therapist, and the number of sessions per week. To the best of our knowledge, no meta-analysis has examined these indicators before in the field of psychotherapy for adult depression.

2. Methods

2.1. Identification and selection of studies

We used a database of 1344 papers on the psychological treatment of depression that has been described in detail elsewhere (Cuijpers et al., 2008b), and that has been used in a series of earlier published meta-analyses (www.evidencebasedpsychotherapies.org). This database is continuously updated through comprehensive literature searches (from 1966 to January 2012). In these searches we examined 13,407 abstracts in Pubmed (3320 abstracts), PsycInfo (2710), Embase (4389) and the Cochrane

Central Register of Controlled Trials (2988). These abstracts were identified by combining terms indicative of psychological treatment and depression (both MeSH terms and text words). For this database, we also checked the primary studies from 42 meta-analyses of psychological treatment for depression to ensure that no published studies were missed (www.evidencebasedpsychotherapies.org). From the 13,407 abstracts (9860 after removal of duplicates) 1344 full-text papers were retrieved for possible inclusion in the database.

For this meta-analysis, we included trials that (a) were randomized, (b) compared the effects of an individual, face-to-face psychological treatment (c) with a control group (waiting list, care-as-usual, pill placebo, other) (d) in adult patients (e) with depression (either established with a diagnostic interview or as scoring above a cut-off on a self-report depression measure). Studies on group therapies, telephone-administered therapies, guided self-help, as well computer-assisted and Internet-based therapies were excluded, because we wanted to focus on amount, frequency and intensity of individual face-to-face therapies. We did include marital therapies, because these sessions are comparable with individual therapies in the sense that they have one therapist and one depressed patient. Studies in which the effect size could not be calculated exactly (typically because only a general *p*-value was given for the comparison between treatment and control group at post-test, and no other information could be used to calculate the effect size) were excluded. Secondary, comorbid general medical or psychiatric disorders were not used as an exclusion criterion.

2.2. Quality assessment

We assessed the validity of included studies using four criteria of the 'Risk of bias' assessment tool, developed by the Cochrane Collaboration (Higgins and 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 (masking of assessors); and dealing with incomplete outcome data (this was assessed as positive when intention-to-treat analyses were conducted, meaning that all randomized patients were included in the analyses). The 'risk of bias' assessment tool includes two other criteria, namely, suggestions of selective outcome reporting; and other problems that could put it at a high risk of bias. The latter two criteria were not used in the present research, because we found no indication in any of the studies that these had influenced the validity of the study.

2.3. Indicators of amount and intensity of treatments, and other characteristics of the studies

For each study we rated the following indicators of the amount, frequency and intensity of therapy (if reported): (a) the number of treatment sessions; (b) duration of therapy: the time period during which the therapy was given (in weeks); (c) total contact time: the total time spent with the therapist (number of treatment sessions multiplied by the time each session lasted; this was rated in hours); and (d) the number of sessions per week (the total number of sessions divided by the duration of the therapy).

Besides indicators of study quality and amount of treatment, we coded several aspects of the included studies, including the following participant characteristics: recruitment method (community, from clinical samples, or other), definition of depression (assessment with a diagnostic interview or not), and target group (adults in general, or more specific target groups such as older

adults). We also assessed the type of psychotherapy (i.e., cognitive behavior therapy, interpersonal psychotherapy, or other; for definitions, see Cuijpers et al., 2008a), baseline severity of depression according to the Beck Depression Inventory (BDI; Beck et al., 1961), and the Hamilton Rating Scale for Depression (HAM-D; Hamilton, 1960). Baseline depression severity in the study samples was categorized according to the BDI as mild to moderate (≤ 18), moderate to severe (19–29) or severe (30–63) (Beck et al., 1988), and according to the HAM-D as mild (≤ 17), moderate (18–24) or severe (≥ 25) (Katz et al., 1995).

2.4. Meta-analyses

We first calculated the effect sizes indicating the difference between the psychotherapy group and the control group at post-test (Hedges's *g* or standardized mean difference). Effect sizes were calculated by subtracting (at post-test) the average score of the psychotherapy group from the average score of the control group, and dividing the result by the pooled standard deviations of the two groups. Because several studies had relatively 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 BDI, or the HAM-D. If more than one depression measure was used, the mean of the effect sizes was calculated, so that each comparison yielded only one effect. 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, or other statistics that were available for calculating effect sizes.

To calculate pooled mean effect sizes, we used the computer program Comprehensive Meta-Analysis (version 2.2.021). Because we expected considerable heterogeneity among the studies, we pooled the studies according to the random effects model.

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 indicate increasing heterogeneity, with 25% as low, 50% as moderate, and 75% as high heterogeneity (Higgins et al., 2003). We calculated 95% confidence intervals around I^2 (Ioannidis et al., 2007), using the non-central chi-squared-based approach within the heterogi module for Stata (Orsini et al., 2005). We also calculated the *Q*-statistic, but only report whether this was significant.

We tested for publication bias by inspecting the funnel plot on primary outcome measures and by Duval and Tweedie (2000) trim and fill procedure, 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). We also conducted Egger's test of the intercept to quantify the bias captured by the funnel plot and test whether it was significant (Egger et al., 1997).

Because we were mainly interested in the association between outcome on the one hand and amount, frequency and intensity of the therapy on the other, we conducted four univariable meta-regression analyses, examining the association between the effect size and (1) the total number of sessions; (2) the duration of treatment (in weeks); (3) the total time spent with the therapist (in minutes); and (4) the number of sessions per week.

Finally, we conducted a multivariable meta-regression analysis, in which the indicators of amount, frequency and intensity of therapy were entered as predictors of the effect size, while adjusting for all the other characteristics of the patients (recruitment method; definition of depression; target group; baseline

severity according to BDI and HAM-D), the therapy (type of therapy), and general characteristics of the studies (type of control group; and study quality).

3. Results

3.1. Selection and inclusion of studies

After examining a total of 13,407 abstracts (9860 after removal of duplicates), we retrieved 1344 full-text papers for further consideration. We excluded 1274 of the retrieved papers. The reasons for excluding studies are given in Fig. 1. This resulted in a total of 315 randomized psychotherapy trials on adult depression. Seventy trials examined the effects of individual psychotherapy and met all other inclusion criteria. Fig. 1 presents a flowchart describing the inclusion process.

3.2. Characteristics of included studies

Seventy studies included 5403 patients (2968 in the psychotherapy conditions, and 2435 in the control conditions). Selected characteristics of the included studies appear in Table 1.

In 28 of 70 studies patients were recruited from the community, 22 studies recruited patients from clinical samples, 20 studies used another method of recruitment. Thirty-three studies were aimed at adults in general, 10 were aimed at older adults, nine at women with postpartum depression, seven at patients with a comorbid general medical disorder, and 11 at other target groups. In 49 studies a diagnostic interview was used to establish the presence of a depressive disorder, while the remaining 21 studies used a cut-off on a self-report scale to establish the presence of depression.

In 70 studies, a total of 92 psychotherapy conditions were compared with a control group. Forty-seven comparisons examined CBT, 11 examined interpersonal psychotherapy, six examined

problem-solving therapy, eight non-directive supportive counseling, five behavioral activation treatment, five psychodynamic therapy, and in eight another type of psychotherapy was examined.

All studies reported the number of treatment sessions for all 92 psychotherapy conditions. The number of sessions ranged from 3 to 24 ($M=11.79$; $SD=5.51$; see also Table 2). Twenty-three psychotherapies had three to six sessions, 27 had 7 to 10 sessions, 22 had 12 to 16 sessions (none had 11 sessions), and 20 had 18 to 24 sessions (none had 17). The duration ranged from 3 to 36 weeks ($M=11.23$; $SD=5.71$; see also Table 2). For 61 of the 92 of the psychotherapies, it was possible to calculate the exact contact time between therapist and client. The total contact time ranged from 2.5 to 20 h ($M=10.05$; $SD=4.72$). The number of sessions per week ranged from 0.44 (less than one session per two weeks) to 2 (2 sessions per week) ($M=1.08$; $SD=0.32$). The majority of comparisons (46 of the 78 for which these data were available) reported one session per week.

A total of 36 of the 70 studies were conducted in the US, 25 in Europe, and 11 in other countries. In 19 studies a waiting list was used as control group, 32 used a care-as-usual control group, and another 19 used another control group (placebo, other).

3.3. Quality assessment

The quality of the studies varied. A total of 30 studies reported an adequate sequence generation, while the other 40 did not. Twenty-four studies reported allocation to conditions by an independent (third) party. Sixty-one studies reported blinding of outcome assessors or used a self-report measure, whereas nine did not. Forty-two studies conducted intention-to-treat analyses (a post-treatment score was analyzed for every patient even if the last observation prior to attrition had to be carried forward or that score was estimated from earlier response trajectories). Twenty-one studies met all four quality criteria, while 21 met two or three criteria, and 28 met none or only one criterion. We also calculated

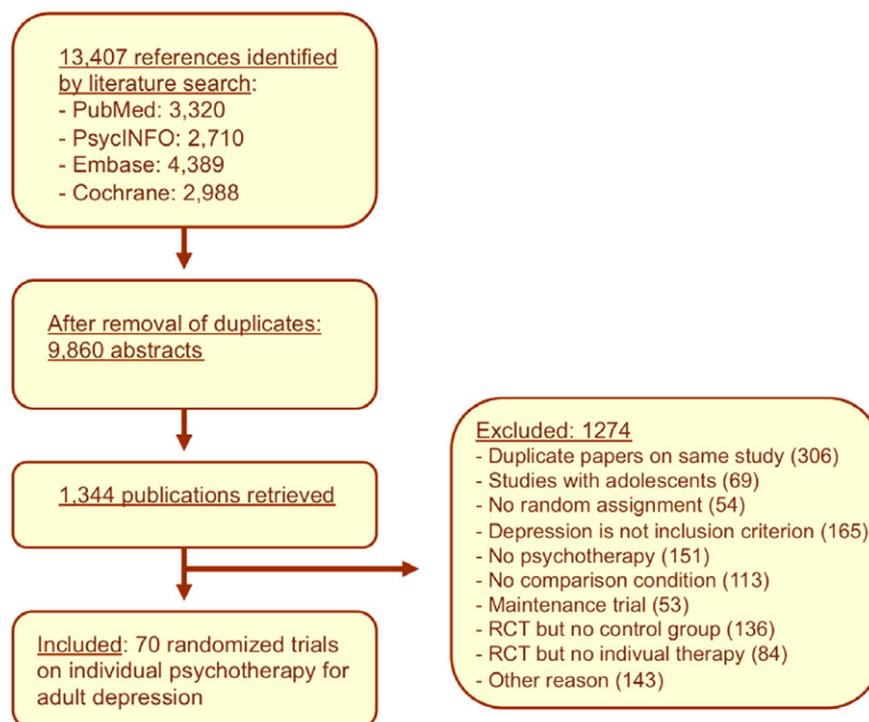


Fig. 1. Flowchart of inclusion of studies.

Table 1
Selected characteristics of studies examining individual psychotherapy for adult depression.

	Recr	DD	Targ Grp	Cond	N _{psy}	Contr	N _{ctr}	N _{sess}	Ther time	Period	S/wk	SG	IR	BA	IT
Baker et al. (2010)	Comm	SR	Other	CBT	71	Oth	70	10	10.5	10	1	+	+	+	+
Barber et al. (2011)	Comm	DD	Adults	Other	51	Oth	50	20	15	16	1.25	+	-	+	+
Barrett et al. (2001)	Clin	DD	Adults	PST	80	Oth	81	6	3.5	11	0.55	+	+	+	+
Beach and O'Leary (1992)	Comm	DD	Adults	Other	15	WL	15	18		15	1.2	-	-	+	-
				CBT	15	WL	15	18		15	1.2	-	-	+	-
Beeber et al. (2010)	Oth	SR	Other	IPT	34	CAU	37	16				+	-	+	-
Brown and Lewinsohn (1984)	Comm	DD	Adults	CBT	13	WL	11	12	10	8	1.5	-	-	+	-
Burns et al. (2007)	Oth	SR	Elderly	Other	60	CAU	61	6		6	1	+	+	+	+
Carpenter et al. (2008)	Clin	DD	Other	BAT	18	Oth	20	24		24	1	-	-	-	+
Castonguay et al. (2004)	Comm	DD	Adults	CBT	11	Oth	10	16		19	0.84	-	-	+	-
Cho et al. (2008)	Oth	DD	PPD	CBT	12	CAU	10	9	9	18	0.5	-	-	+	-
Cohen et al. (2010)	Comm	DD	Adults	Other	16	WL	14	5	10	5	1	-	-	+	+
Cooper et al. (2003)	Oth	DD	PPD	SUP	47	CAU	50	10		10	1	+	+	+	+
				DYN	45	CAU	50	10		10	1	+	+	+	+
				CBT	42	CAU	50	10		10	1	+	+	+	+
DeRubeis et al. (2005)	Comm	DD	Adults	CBT	60	Oth	60	24	20	16	1.5	-	-	+	+
Dimidjian et al. (2006)	Comm	DD	Adults	CBT	38	Oth	41	24	20	16	1.5	+	-	+	+
				BAT	37	Oth	41	24	20	16	1.5	+	-	+	+
Dobkin et al. (2011)	Comm	DD	Somatic	CBT	41	CAU	39	10	11.25	10	1	+	+	+	+
Dowrick et al. (2000)	Comm	DD	Adults	PST	98	CAU	139	6				+	+	+	+
Ekers et al. (2011)	Clin	DD	Adults	BAT	16	CAU	22	12	12	12	1	+	+	+	+
Elkin et al. (1989)	Clin	DD	Adults	CBT	59	Oth	62	18	15	16	1.13	+	+	+	+
				IPT	61	Oth	62	18	15	16	1.13	+	+	+	+
Floyd et al. (2004)	Comm	DD	Elderly	CBT	8	WL	14	16		12	1.33	-	-	-	-
Freedland et al. (2009)	Oth	DD	Somatic	CBT	41	CAU	40	12	11	12	1	+	+	+	+
				Oth	42	CAU	40	12	11	12	1	+	+	+	+
Holden et al. (1989)	Oth	DD	PPD	SUP	26	CAU	24	8	4	8	1	+	+	+	-
Jarrett et al. (1999)	Clin	DD	Adults	CBT	36	Oth	36	20		10	2	+	+	+	+
Kay-Lambkin et al. (2009)	Comm	SR	Other	CBT	21	Oth	24	10		10	1	+	+	+	+
King et al. (2000)	Clin	SR	Adults	CBT	63	CAU	67	6	5	6	1	+	+	+	+
				SUP	67	CAU	67	6	5	6	1	+	+	+	+
Krampen (1997)	Clin	DD	Adults	CBT	15	WL	14	20		10	2	-	-	+	-
Laidlaw et al. (2008)	Clin	DD	Elderly	CBT	20	CAU	20	8				+	+	+	-
Lamers et al. (2010)	Oth	DD	Elderly	CBT	111	CAU	125	6	6	12	0.5	+	+	+	+
Lexis et al. (2011)	Oth	SR	Adults	PST	69	CAU	70	10	7.5			+	+	+	+
Maina et al. (2005)	Clin	DD	Adults	DYN	10	WL	10	20	17.25	23	0.85	-	-	+	+
				SUP	10	WL	10	20	17.25	23	0.85	-	-	+	+
McLean and Hakstian (1979)	Comm	DD	Adults	DYN	51	Oth	48	10	10	10	1	-	-	+	-
				BT	44	Oth	48	10	10	10	1	-	-	+	-
Milgrom et al. (2011)	Oth	SR	PPD	CBT a	22	CAU	23	6		6	1	+	+	+	+
				CBT	23	CAU	23	6		6	1	+	+	+	+
Milgrom et al. (2005)	Oth	DD	PPD	SUP	66	CAU	10	9	13.5	9	1	+	+	+	+
Mossey et al. (1996)	Oth	SR	Elderly	IPT	31	CAU	13	10	10			-	-	+	-
Murphy et al. (1995)	Comm	DD	Adults	Other	11	Oth	13	18	15	16	1.13	+	-	-	-
Mynors-Wallis et al. (1995)	Clin	DD	Adults	PST	29	Oth	26	6	3.5	12	0.5	-	-	+	+
O'Hara et al. (2000)	Oth	DD	PPD	IPT	48	WL	51	12	12	12	1	+	-	-	+
Pace and Dixon (1993)	Oth	SR	Students	CBT	31	WL	43	7	5.25	6	1.27	-	-	+	-
Pecheur and Edwards (1984)	Comm	DD	Students	CBT b	7	WL	7	8	6.67	4	2	-	-	-	+
				CBT c	7	WL	7	8	6.67	4	2	-	-	-	+
Prendergast and Austin (2001)	Oth	DD	PPD	CBT	17	Oth	20	6	6	6	1	+	-	-	-
Propst et al. (1992)	Comm	SR	Adults	Other	10	WL	11	19	15			-	-	+	-
				CBT d	9		11	19	15			-	-	+	-
				CBT e	10		11	19	15			-	-	+	-
				CBT f	9		11	19	15			-	-	+	-
				CBT g	10		11	19	15			-	-	+	-

Table 1 (continued)

	Recr	DD	Targ Grp	Cond	N _{psy}	Contr	N _{ctr}	N _{sess}	Ther time	Period	S/wk	SG	IR	BA	IT
Safren et al. (2009)	Comm	DD	Somatic	CBT	23	WL	22	12	10	12	1	–	+	+	+
Savard et al. (2006)	Comm	SR	Somatic	CBT	21	WL	16	8	10	8	1	+	+	+	–
Schmidt and Miller (1983)	Comm	SR	Adults	CBT	12	WL	10	8	12	8	1	–	–	+	–
Schulberg et al. (1996)	Clin	DD	Adults	IPT	93	CAU	92	16		16	1	–	–	+	+
Scott and Freeman (1992)	Clin	DD	Adults	CBT	29	CAU	29	16	13.33	16	1	+	+	+	+
				SUP	29	CAU	29	16	13.33	16	1	+	+	+	+
Scott et al. (1997)	Clin	DD	Adults	CBT	18	CAU	16	6	3	6	1	–	–	+	–
Selmi et al. (1990)	Comm	DD	Adults	CBT	12	WL	12	6		6	1	–	–	+	+
Serfaty et al. (2009)	Comm	DD	Elderly	CBT	64	CAU	55	12	10			+	+	+	+
Serrano et al. (2004)	Oth	SR	Elderly	Other	20	CAU	23	4		4	1	–	–	+	–
Simpson et al. (2003)	Clin	SR	Adults	DYN	83	CAU	80	9	7.5			+	+	+	+
Simson et al. (2008)	Comm	SR	Somatic	SUP	15	CAU	15	5	2.5	5	1	–	–	+	+
Sloane et al. (1985)	Oth	DD	Elderly	IPT	19	Oth	14	6		6	1	–	–	+	–
Smit et al. (2006)	Clin	DD	Adults	CBT	36	CAU	62	14	14	14	1	+	+	+	+
Spinelli and Endicott (2003)	Clin	DD	Other	IPT	21	Oth	17	16	12	16	1	–	–	–	+
Swartz et al. (2008)	Comm	DD	Other	IPT	24	CAU	18	8				–	–	+	+
Talbot et al. (2011)	Clin	DD	Other	IPT	34	CAU	24	16		36	0.44	–	–	–	+
Taylor and Marshall (1977)	Comm	SR	Students	CT	7	WL	7	6	4	4	1.5	–	–	+	–
				BAT	7	WL	7	6	4	4	1.5	–	–	+	–
				CBT	7	WL	7	6	4	4	1.5	–	–	+	–
Taylor et al. (2009)	Oth	SR	Somatic	CBT	19	CAU	22	15				–	–	+	+
Teasdale et al. (1984)	Clin	DD	Adults	CBT	17	CAU	17	20		15	1.33	–	–	+	+
Teichman et al. (1995)	Clin	DD	Adults	Other	15	WL	15	15		15	1	–	–	+	–
				CBT	15	WL	15	15		15	1	–	–	+	–
Teri et al. (1997)	Oth	DD	Somatic	BAT	23	WL	20	9	9	9	1	–	–	+	–
				PST	19	WL	20	9	9	9	1	–	–	+	–
Turner et al. (1979)	Comm	SR	Adults	BAT	17	Oth	16	5	4.17	4	1.25	–	–	+	–
Van Schaik et al. (2006)	Clin	DD	Elderly	IPT	69	CAU	74	10		20	0.5	+	+	+	+
Vitriol et al. (2009)	Clin	DD	Other	DYN	44	CAU	43	12		12	1	–	–	+	+
Weissman et al. (1979)	Clin	DD	Adults	IPT	25	Oth	23	16	13.33	16	1	–	–	+	–
Wickberg and Hwang (1996)	Oth	DD	PPD	SUP	15	CAU	16	6	6	6	1	–	–	+	–
Wiklund et al. (2010)	Oth	SR	PPD	CBT	33	CAU	34	3	3	3	1	–	–	+	–
Williams et al. (2000)	Comm	DD	Elderly	PST	113	Oth	119	6	3.5	11	0.55	–	–	+	+
Wilson et al. (1983)	Comm	SR	Adults	BT	8	WL	9	8		8	1	–	–	–	–
				CBT	8	WL	9	8		8	1	–	–	–	–
Wright et al. (2005)	Comm	SR	Adults	CBT	13	WL	14	9	7.5	8	1.13	–	–	+	+

Abbreviations: BA: blinded assessment of outcome; BAT: behavioral activation therapy; BT: behavior therapy; CAU: care-as-usual; CBT a: CBT by a nurse; CBT b: secular CBT; CBT c: religious CBT; CBT d: religious CBT with non-religious therapist; CBT e: religious CBT with religious therapist; CBT f: non-religious CBT with non-religious therapist; CBT g: non-religious CBT with religious therapist; CBT: cognitive behavior therapy; Clin: recruitment only from clinical samples; Comm: recruitment from the community; Cond.: psychotherapy condition; Contr: type of control group; CT: cognitive therapy; DD: diagnosis of depression; DD: meeting criteria for a depressive disorder according to a diagnostic interview; DYN: psychodynamic therapy; IPT: interpersonal psychotherapy; IR: allocation to conditions by an independent party; IT: intention-to-treat analyses; N sess: number of sessions; Nctr: N in the control condition; Npsy: N in the psychotherapy condition; Oth: other; Period: total period of the therapy (in weeks); PPD: postpartum depression; PST: problem-solving therapy; Recr: Recruitment; S/wk: sessions per week; SG: sequence generation; SR: scoring above a cut-off on a self-report measure; SUP: non-directive supportive counseling; Targ Grp: target group; Ther time: contact time with therapist (in hours); WL: waiting list.

a total score by summing up the scores of the four items ($M=2.24$; $SD=1.31$).

3.4. Overall effects

The overall mean effect of the 92 treatments compared to the control groups was $g=0.59$ (95% CI: 0.50–0.67), with moderate to high heterogeneity ($I^2=58$; 95% CI: 47–67). The results are summarized in Table 2. When we limited the outcomes to the Beck Depression Inventory (BDI), the effect of psychotherapy compared to control groups was $g=0.75$ (95% CI: 0.60–0.89; $I^2=65$; 95% CI: 52–74), which corresponds with a difference of 6.48 points on the BDI (95% CI: 5.29–7.66) between treatment and control groups at post-test. The effect size according to the HAM-D was 0.79 (95% CI: 0.64–0.93); this corresponds with a difference of 4.76 points (95% CI: 3.95–5.57) between treatment and control groups.

In this meta-analysis we included 15 studies in which more than one psychological treatment was compared with the same control group (in 12 studies two therapies were compared with one control group; in two studies, three therapies were compared with one control group; and in one study, five therapies were compared with one control group). This means that multiple comparisons from these study were included in the same analysis, that are not independent of each other, which may have

resulted in an artificial reduction of heterogeneity and may have affected the pooled effect size. In sensitivity analyses, 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 sizes from these studies and then we conducted another analysis in which we included only the smallest effect sizes. As can be seen from Table 2, the resulting effect sizes as well as the levels of heterogeneity were comparable with the overall analyses.

In line with earlier research (Cuijpers et al., 2010c), we found strong indications for publication bias. After adjustment for publication bias according to Duval and Tweedie's trim and fill procedure, the effect size was reduced from 0.59 to 0.40 (95% CI: 0.30–0.50; number of imputed studies: 28).

3.5. Association of effect size with amount, frequency and intensity of therapy

To examine the association between the effects and the amount, frequency and intensity of therapy, we conducted a series of bivariate metaregression analyses. First, we conducted a metaregression analysis with the effect size as the dependent variable and the number of treatment sessions as independent variable. For illustrative purposes, we also divided the studies into four categories: those with four to six session, those with 7 to 10

Table 2

Effects of psychotherapy for adult depression and association with amount, frequency and intensity of therapy.

		N	g	95% CI	I^2 ^a	95% CI	Slope	95% CI	p
All comparisons		92	0.59	0.50–0.67	58***	47–67			
One effect size per study (highest)		70	0.60	0.50–0.70	63***	52–71			
One effect size per study (lowest)		70	0.51	0.42–0.61	58***	45–68			
BDI only		50	0.75	0.60–0.89	65***	52–74			
HAM-D only		41	0.79	0.64–0.93	54***	35–68			
Number of sessions ^b							0.010	0.000–0.020	0.04
	4–6	23	0.47	0.30–0.65	45*	9–66			
	7–10	27	0.58	0.42–0.74	69***	55–79			
	12–16	22	0.68	0.50–0.85	57***	30–73			
	18–24	20	0.61	0.41–0.81	42*	1–66			
Contact time (h) ^c							0.013	–0.000–0.027	0.06
	≤ 5	13	0.52	0.29–0.76	53*	9–75			
	5–10	21	0.49	0.30–0.67	44**	25–72			
	11–15	22	0.60	0.42–0.77	63***	42–77			
	15–20	5	0.70	0.33–1.08	21	0–66			
Duration (weeks) ^c							–0.13	–0.024––0.001	0.03
	4–6	21	0.65	0.45–0.86	45*	8–67			
	8–10	20	0.63	0.44–0.83	62***	39–77			
	11–15	18	0.65	0.45–0.84	70***	52–82			
	16–24	19	0.48	0.28–0.67	46*	8–69			
Number of sessions per week							0.45	0.25–0.64	0.000
	< 1	10	0.44	0.19–0.69	64**	29–82			
	1	46	0.58	0.46–0.70	53***	35–67			
	> 1	22	0.71	0.52–0.91	53**	24–71			
Sensitivity analyses: CBT only									
Number of sessions		47					0.005	–0.010–0.192	0.51
Contact time (h)		31					–0.000	–0.000–0.000	0.51
Duration (weeks)		39					–0.016	–0.040–0.006	0.15
Number of sessions per week		39					0.334	0.080–0.59	0.01
Sensitivity analyses: only diagnosed depressive disorders									
Number of sessions		62					0.011	–0.000–0.023	0.05
Contact time (h)		40					0.000	–0.000–0.000	0.05
Duration (weeks)		40					–0.11	–0.257–0.002	0.11
Number of sessions per week		58					0.40	0.12–0.60	0.000

^dThis metaregression analysis was based on the 78 comparisons that reported duration of therapy.

* $p < 0.05$.

** $p < 0.01$.

*** $p < 0.001$.

^a The p -value in this column indicates whether the Q -statistic was significant.

^b No study had 11 or 17 sessions.

^c This metaregression analysis was based on the 60 comparisons that reported therapist time.

^e No study lasted for 7 weeks.

sessions, those with 12 to 16 sessions, and those with 18 to 24 sessions (none of the therapies had 11 or 17 sessions). For each of these categories, the effect size and the level of heterogeneity are displayed in Table 2. The metaregression showed a small but significant association between number of treatment sessions and effect size (slope: 0.010; 95% CI: 0.000–0.020; $p < 0.05$). This means that the effect size increases with 0.01 with each additional session, or 10 sessions extra will result in an increase of the effect size of 0.1. For illustrative purposes we have also graphically represented the association between number of sessions and the effect sizes in Fig. 2.

Second, we examined the association between the effect size and the total contact time between therapist and patient. As can be seen in Table 2, there was a trend ($p < 0.1$) indicating that more hours of contact were associated with a greater effect size (slope: 0.013; 95% CI: -0.000 – 0.027). This means that 10 h of extra contact would result in an increase of the effect size with 0.13.

Third, we examined the association between therapy duration (in weeks) and effect size. Unexpectedly, we found that a longer duration resulted in a lower effect size (slope: -0.13 ; 95% CI: -0.024 – 0.001 ; $p < 0.05$). Every extra week of therapy resulted in a decrease of the effect size with 0.13. A graphical presentation of this association is given in Fig. 2.

Finally, we focused on the association between the number of treatment sessions per week. Here, we found that more sessions per week resulted in a considerably larger effect size than less sessions per week (slope: 0.45; 95% CI: 0.25–0.64; $p < 0.001$). This indicates that having two sessions per week instead of one, would result in an increase of the effect size of 0.45 (see also Fig. 2).

Because the majority of studies had one session per week, we repeated this analysis with the studies in which the studies with one session per week were removed, and only those with less or more than one session per week were examined. This metaregression found almost the same results (slope: 0.44, 95% CI: 0.25–0.64, $p < 0.0001$). In another sensitivity analysis, we left out the

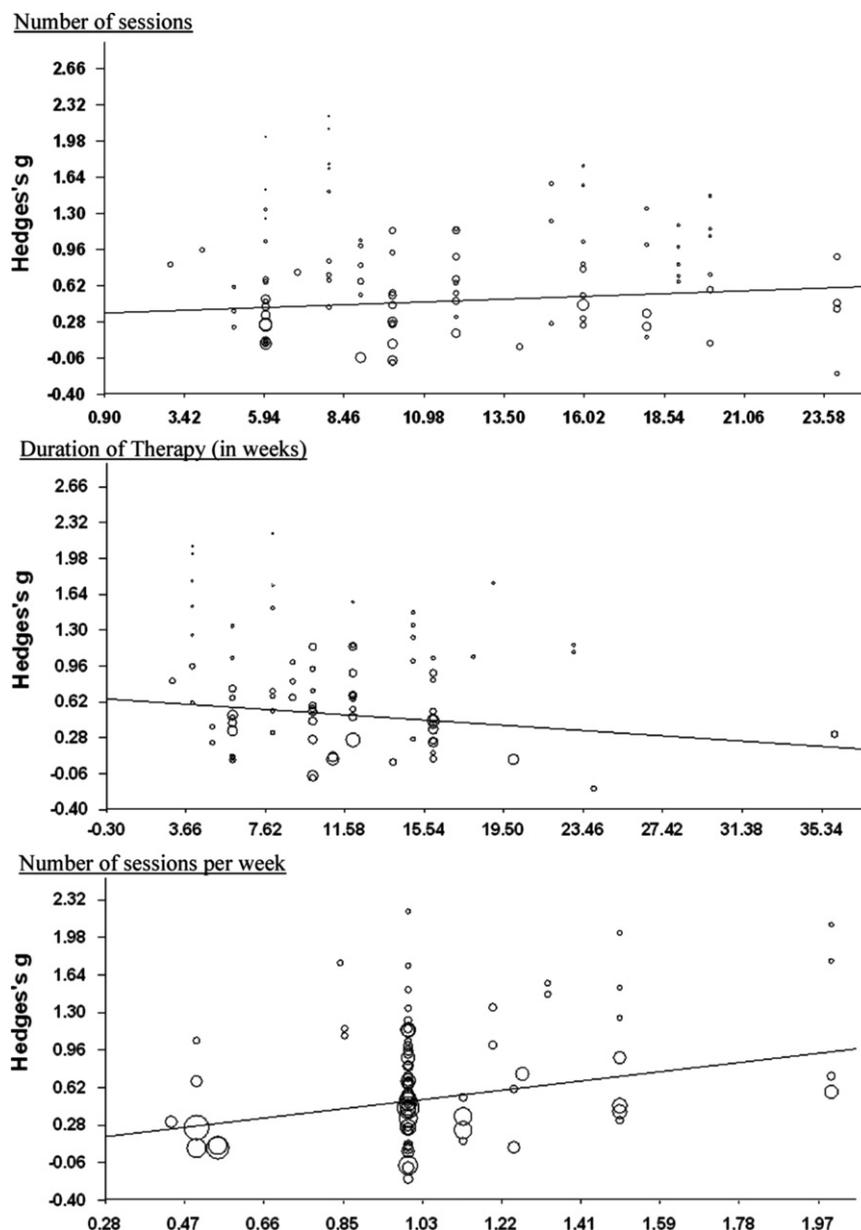


Fig. 2. Association between effect size and number of treatment sessions, duration of therapy, and number of sessions per week in psychotherapy for adult depression: metaregression analyses.

studies in which psychotherapy was delivered less often than once per week. The results of these analyses were still significant (slope: 0.32; 95% CI: 0.02–0.61; $p < 0.05$). When we left out the studies with more than one session per week, the results were also significant (slope: 0.64; 95% CI: 0.32–0.96; $p < 0.001$).

3.6. Sensitivity analyses

Because there were important differences between the studies, and because we found considerable heterogeneity in the sets of studies, we repeated the main analyses in two subsets of studies. In one subset, we included only studies examining CBT, and in the other subset we excluded studies in which depression was not established with a diagnostic interview (but with a cut-off score on a self-report questionnaire). The results of these analyses are reported in Table 2. As can be seen, only the association between the effect size and the number of sessions per week remained significant in the two subsets of studies.

3.7. Multivariable metaregression analysis

We wanted to examine whether the association between the effect size and the indicators of amount, frequency and intensity of therapy remained significant in a multivariable metaregression analysis, in which we controlled for other major characteristics of the studies. First, we calculated the correlation between the four indicators (numbers of treatment sessions, duration of treatment, total contact time, and the number of sessions per week). In order to avoid collinearity in multivariable metaregression analyses, it is important that the correlation between included variables is not too high. As expected, we found that the total number of treatment sessions was highly correlated with the total duration of therapy (in weeks; $r=0.74$, $p < 0.01$), and with total contact time ($r=0.93$, $p < 0.01$), but less highly with the number of sessions per week ($r=0.27$, $p < 0.05$). Total contact time was also highly correlated with the duration of therapy ($r=0.78$, $p < 0.01$), but not with the number of sessions per week ($r=0.13$, n.s.); and therapy period was significantly correlated with the number of sessions per week ($r=-0.34$, $p < 0.01$).

Because of the high correlation between the number of treatment sessions, total therapy time and therapy period, we

decided to include only one of these three variables in the multivariable metaregression analyses. Because all studies reported the total number of treatment sessions, we decided to use this variable as predictor in the main metaregression analysis. Because the correlation between number of sessions per week and the other three indicators of treatment intensity was moderate ($r < 0.40$), we decided this could be included as the predictor in the main multivariable analyses as well.

In the multivariable metaregression analysis, we used the effect size as the dependent variable and as predictors we included the total number of sessions, the number of sessions per week, and the basic characteristics of the studies, participants, and interventions as predictors. The results of these analyses are presented in Table 3. As can be seen, the total number of sessions was no longer significantly associated with the effect size. However, there was a trend ($p < 0.1$) indicating that the number of sessions per week was significantly associated with the effect size. Other characteristics that were significantly associated with the effect size were the type of control group (placebo and other control groups), and the total quality of the study.

We also conducted a (manual) back-step metaregression analysis. In this analysis, we dropped the least significant variable in each step, until only significant predictors were retained in the model (Table 3). The results of this parsimonious model indicated that the number of sessions per week was significantly associated with the effect size ($p < 0.01$), as well as type of control group and study quality.

Only a limited number of studies reported data on the severity of depression at baseline. Therefore, we did not add baseline severity as a predictor in the main analyses, but we did conduct separate metaregression analyses in which we also added baseline severity according to the BDI as predictor, and another analysis with the baseline HAM-D as predictor. None of these indicated that baseline severity was significantly associated with the effect size, nor that this had an influence of the significance of other predictors.

Finally, we conducted additional metaregression analyses in which we removed the total number of sessions as predictor, and added the total contact time (in hours) as predictor. As expected, contact time was not found to be significantly associated with the effect size. We also added duration of therapy (in weeks) as

Table 3
Standardized regression coefficients of characteristics of studies on psychotherapies for adult depression: multivariable metaregression analyses.

		Full model			Parsimonious model		
		Coef.	95% CI	<i>p</i>	Coef.	95% CI	<i>p</i>
Number of sessions		0.00	–0.23–0.02				
Number of sessions per week	0.35	–0.04–0.74	°		0.43	0.16–0.71	**
Aimed at adults in general (dummy)	0.03	–0.21–0.27					
Recruitment		Ref.					
	Community						
	Clinical	–0.12	–0.36–0.13				
	Other	–0.06	–0.34–0.22				
Diagnostic interview (dummy)		–0.02	–0.28–0.24				
CBT as psychotherapy (dummy)		–0.06	–0.25–0.12				
Control group		Ref.					
	Waiting list						
	Care-as-usual	–0.22	–0.50–0.06				
	Other	–0.53	–0.79––0.28	***	–0.36	–0.54––0.19	***
Quality of study		–0.08	–0.15––0.01	*	–0.12	–0.18––0.05	***
Baseline severity (HAM-D) ^a	–0.02	–0.05–0.02					
Baseline severity (BDI) ^b		–0.01	–0.06–0.04				
Constant		0.77	0.30–1.24	**			

° $p < 0.1$.

* $p < 0.05$.

** $p < 0.01$.

*** $p < 0.001$.

^a The outcomes for baseline severity on the HAM-D were based on a separate analysis of the 47 studies in which the baseline HAM-D-score was reported.

^b The outcomes for baseline severity on the BDI were based on a separate analysis of the 50 studies in which the baseline BDI-score was reported.

predictor (after removing number of sessions) and did not find that this was associated with the effect size either (results not reported in Table 3).

4. Discussion

In the present study, we examined the association of the effects of psychotherapy for adult depression and multiple indicators of amount, frequency and intensity of therapy. We found that the effects increased somewhat with a higher number of treatment sessions. However, this association was not very strong, with ten more session resulting in an increase of the effect size with 0.1. This is a very small effect (Cohen, 1988) and it can be questioned whether this has any clinical relevance. Furthermore, after adjusting for other characteristics (including the quality of the studies) this association was no longer significant.

As could have been expected, we found that the total contact time between therapist and client was highly correlated with the total number of sessions. Here we also found a trend ($p < 0.1$) indicating that contact time was associated with the effects of the therapies, but only in the bivariate metaregression analyses. After adjustment for other characteristics of the studies, this association was not significant anymore.

We did find, however, that there was a strong relationship between the number of treatment sessions per week and the effect size. When two instead of one treatment sessions are given per week, without increasing the total number of sessions, the effect size increases with 0.45, which is a moderate effect. Furthermore, this association remained significant after adjustment for the other study characteristics. The overall quantity of psychotherapy thus seems less important in determining its effectiveness than the quantity of psychotherapy within a weekly time frame. It may thus be the intensity rather than the quantity of psychotherapy that determines therapeutic effectiveness.

Why might two weekly sessions of psychotherapy be more effective than one weekly session? One tentative explanation might be found within the neurobiology literature, in which animal models have demonstrated that it is the continued survival of neurons “born” within the last five days that is necessary for learning to take place (Henn and Vollmayr, 2004). Another explanation could be that the relationship between client and counselor may develop more rapidly when the contact is more intense.

Unexpectedly, we found a small negative association between the duration of the therapy and the effect size, indicating smaller effects when the therapy lasts longer. This did not remain significant in the multivariable analyses, however. We think this finding is related to the finding that a lower frequency of sessions is associated with smaller effect sizes. When the frequency is lower, the time of the therapy lasts longer.

It has often been suggested that time-limited therapies lead to more improvement than therapies in which time-limiting is less of an issue (Beck, 1995). Our findings suggest that this may be even the case within the category of time-limited therapies, as the maximum number of sessions in our sample of studies was ‘only’ 24.

When interpreting the present findings, we have to remember that significant associations between a predictor and the effect size in a metaregression analysis are not necessarily indicators of a causal association. It is very well possible that these significant associations are caused by other factors that have not been accounted for. It is also possible, that there are subgroups of patients in which higher numbers of sessions lead to better outcomes. For example in a meta-analysis of psychotherapy for

chronic depression, we found a clear association between the improvement in patients and the number of treatment sessions (Cuijpers et al., 2010b). These findings, therefore, can only be seen as indirect evidence, and randomized trials are needed to confirm the findings. To confirm the causal significance of session frequency, one would need to conduct controlled trials in which a treatment with two sessions per week is compared with the same treatment but with one session per week or one per two weeks (while keeping the total number of sessions constant) is needed to confirm the importance of the treatment intensity. As far as we know, such a trial has not yet been conducted in the field of depression.

This omission in the literature is hard to understand as the early depression manual by Beck et al. (1979) suggested that the treatment should start with two sessions per week, but this recommendation seems to have been lost in many subsequent studies on cognitive therapy. From a clinical point of view, it is commonly the case that more intense therapy is called for in the early stages of therapy, and in particular when the patient is suicidal and very depressed. It is highly likely that some patients need more intensive contact early on in the treatment and this could be one reason why guided self-help via the internet, in which the patient often can contact the therapist within 24 h, has been found to be as effective as face-to-face therapies, with large effect sizes (Andersson and Cuijpers, 2009; Richards and Richardson, 2012).

If the findings of this study are confirmed in primary research, it would seem best to give brief therapies (as longer therapies are not more effective), with brief sessions (as contact time in itself is also not associated with the effect size), but with a high frequency. For example, six sessions of three quarters of an hour, given within three weeks, would probably give not only very good results, but would also be highly efficient. But, once again, this is highly speculative, as our findings should first be confirmed in randomized trials, in which also for example the longterm effects are examined.

We want to stress once more that the results of this study are only observational, and should not be considered as conclusive evidence that the number of treatment sessions is not related to outcome. Studies such as this one are in grave danger of being cited by policy makers as justification of limiting resources. No such interpretations are justified by the weight of the current evidence.

This study has several strengths and limitations. One of the strengths involves the relatively large number of studies. But there are also several limitations. One important limitation is that we could not account for the actual treatment sessions that the patients actually received in each of the trials. We worked with indicators of amount, frequency and intensity of therapy as were planned by the researchers, but we could not examine the actual number of sessions received from the patients and account for drop-out and no-show. Furthermore, the actual time to recovery or improvement in the patient is not measured in these studies and this may vary between patients. In this meta-analysis, we can only look at the differences between patients in treatment and control groups at the end of the treatment. But the relevant question here is how much additional therapy those patients need who did not recover at this moment. But this cannot be examined in a meta-analysis.

The present meta-analysis also did not account for the quality of the therapy being delivered within the trials. Although we did not find that indicators of therapy quality were strongly related to the effect sizes in an earlier meta-analysis (Cuijpers et al., 2010a), it can make a big difference how therapy is conducted. Another limitation is that we included a wide variety of therapies, although it is possible that the number of sessions are associated

with better outcome in one therapy, but not in another. Furthermore, we did examine the association between baseline severity of depression, but this was only at the study level. In order to examine this association between baseline severity and number of sessions needed for recovery, a large-sample experimental design is necessary. Another important limitation is that all treatments that were included in our meta-analysis were relatively brief, with none having more than 24 sessions. In longer-term treatments an association between number of sessions and outcomes may still be possible.

Another limitation is that the quality of the included studies was not optimal (see also Cuijpers et al., 2010a). Because of this negative association between study quality and effect sizes, the results, especially those from the lower quality studies, have to be interpreted with caution. Furthermore, we found strong indications for significant publication bias, which is also in agreement with earlier research (Cuijpers et al., 2010c). It is very well possible that small, short studies with nonsignificant results were not published, while those with large effects are published. This may have skewed the relationship between number of sessions and outcome. Moreover, this study only considered depression outcome immediately after treatment and did not consider the role of booster sessions after completed therapy (therapist effects), effects on the therapeutic relationship, or other outcomes than depression such as quality of life.

Despite these limitations, however, this meta-analysis points towards promising new directions in depression research. While currently a lot of attention in depression research is going towards evaluating the efficacy of new interventions (e.g., third wave techniques), less attention is given to evaluate ways to improve efficacy of established methods, for example by experimentally manipulating ways of treatment delivery such as session frequency, number of sessions and length of the sessions. In fact, psychotherapy has been around for a long time, yet there are very few studies in which the number of sessions, including their length and spacing has been experimentally manipulated. No one would probably consider treating depression in one week, but there is a need for more research on the role of session frequency. And findings from this study implicate that such primary research could have a major impact. We conclude that not such much the number of sessions seems to be relevant for the effects of therapy, but the number of sessions per week.

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Conflict of interest

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