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The Effects of Cognitive Behavioral Therapy as an Anti-Depressive Treatment is Falling: A Meta-Analysis

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A meta-analysis examining temporal changes (time trends) in the effects of cognitive behavioral therapy (CBT) as a treatment for unipolar depression was conducted. A comprehensive search of psychotherapy trials yielded 70 eligible studies from 1977 to 2014. Effect sizes (ES) were quantified as Hedge's g based on the Beck Depression Inventory (BDI) and the Hamilton Rating Scale for Depression (HRSD). Rates of remission were also registered. The publication year of each study was examined as a linear metaregression predictor of ES, and as part of a 2-way interaction with other moderators (Year × Moderator). The average ES of the BDI was 1.58 (95% CI [1.43, 1.74]), and 1.69 for the HRSD (95% CI [1.48, 1.89]). Subgroup analyses revealed that women profited more from therapy than did men ($p < .05$). Experienced psychologists ($g = 1.55$) achieved better results ($p < .01$) than less experienced student therapists ($g = 0.98$). The metaregressions examining the temporal trends indicated that the effects of CBT have declined linearly and steadily since its introduction, as measured by patients' self-reports (the BDI, $p < .001$), clinicians' ratings (the HRSD, $p < .01$) and rates of remission ($p < .01$). Subgroup analyses confirmed that the declining trend was present in both within-group (pre/post) designs ($p < .01$) and controlled trial designs ($p = .02$). Thus, modern CBT clinical trials seemingly provided less relief from depressive symptoms as compared with the seminal trials. Potential causes and possible implications for future studies are discussed.

Keywords: cognitive–behavioral therapy, effectiveness, depressive disorders, meta-analysis

Depressive disorders (DDs) can be highly disabling and are ranked third in terms of disease burden as defined by the World Health Organization (WHO, 2014), and first among all psychiatric disorders in terms of disability adjusted life years (Wittchen et al., 2011). In addition, DDs seem to be rising globally (Everyday Health, 2013), and a 20% annual increase in its incidence has been predicted (Healthline, 2012). Improvements in treatment methods and prevention measures, and the availability of community psychiatric services are, therefore, as important as ever before. In response, the WHO has prioritized the combating of depression by launching an action plan called "The Mental Health Gap Action Program," aimed at improving mental health services globally (WHO, 2012).

Psychotherapy is a critical asset for dealing with the future challenges associated with DDs; hence, the optimization of existing therapeutic methods and the development of new ones are important clinical research tasks. Cognitive–behavioral therapy (CBT) has represented an innovative psychotherapy approach since its introduction more than 40 years ago; it has continuously developed and overall, it has been highly successful. The CBT method refers to a class of interventions sharing the basic premise that mental disorders and psychological distress are maintained by cognitive factors or cognitive processes (Hofmann et al., 2012). As posited by Beck (1970) and Ellis (1962), maladaptive thoughts maintain emotional distress and dysfunctional behavior, for which alleviation or cure is realized by changing them. The original theory has been refined continuously by introducing new cognitive concepts (e.g., automatic thoughts, intermediate and core beliefs, and schema theory), and adapted to treat new psychiatric diagnoses. Its potential success in alleviating symptoms of schizophrenia (Rector & Beck, 2012), which was considered impervious to treatment with psychotherapy (Tarrier, 2005), is one striking example. Later variations of the method, building on the foundations of CBT, such as CBT combined with mindfulness (Segal, Williams, & Teasdale, 2002), integrated cognitive therapy with elements of interpersonal therapy (Castonguay, 1996), and metacognitive therapy (Wells, 2000), represent further innovations in CBT. These newer forms of CBT have exhibited promising efficacy in clinical trials of treatments for illnesses, such as hypochondriasis (Lovas & Barsky, 2010) and generalized anxiety disorder (Wells & King, 2006). However, few studies have demonstrated these innovations to be significantly more effective in treating DDs than classical CBT (e.g., Ashouri et al., 2013; Manicavasgar, Parker, & Perich, 2011).

A large amount of research has confirmed the efficacy of classical CBT in treating depression. Meta-analyses published in the 1980s (Dobson, 1989), the 1990s (Hollon, Shelton, & Loosen, 1991; Gloaguen et al., 1998), and after 2000 (Cuijpers et al., 2008; Wampold et al., 2002), concluded that CBT had a high treatment efficacy. Despite the large number of clinical trials and reviews of

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CBT, to the best of our knowledge, no attempts have been made to evaluate how the efficacy of CBT has evolved over time. Thus, the aim of the present meta-analysis was to study temporal changes (time trends) in the treatment effects of CBT, by posing a simple question: How have the effects of CBT changed over time? Have they improved, stayed the same, or even waned?

A hallmark of our modern society has been the rapid development in many domains, particularly in science, technology, and health. Old procedures and methods have been replaced with safer and more effective solutions. For example, in somatic health care, cruciate ligament surgery currently takes considerably less time, requires fewer resources, and has a better long-term prognosis than it did 30 years ago (Cirstoiu et al., 2011). Another example is a percutaneous coronary intervention (PCI, formerly known as coronary angioplasty), which uses a catheterization technique to insert a stent in the groin or arm to improve blood flow in the heart's arteries. The technique is quick and presently requires minimal rehabilitation (an overnight hospital stay); hence, it represents a huge improvement compared with older techniques (Knapik, 2012). Although comparable improvements in psychiatric methods and techniques are much more difficult to achieve, the purpose of this meta-analysis was to examine whether improvements in CBT, in the treatment of DDs, have taken place since its introduction.

Factors Influencing Treatment Effects

When a treatment is efficacious, psychotherapy research trials point to four sources to explain the observed improvements: (a) client factors, (b) therapist factors, (c) the so-called common factors, and finally, (d) technique-specific factors. Client factors represent the characteristics of the patient, such as personality traits, temperament, motivation for treatment, or important life events experienced by the patient during the course of therapy. Therapist factors are the characteristics of the therapist, which can include anything from gender, age, and education, to personal style and appearance. Clinical training, competency, and skills in establishing a therapeutic alliance and using therapeutic techniques are of particular importance (Crits-Christoph et al., 1991). The two latter components may also be denoted as common and technique-specific therapy factors, which influence the outcome of CBT.

The common factors represent characteristics of the treatment setting that are important and common to all therapy models. These characteristics may include the context of therapy; the client, the therapist, and their relationship (usually coined as the therapeutic alliance); how expectancies for improvement develop; a plausible rationale explaining the patient's illness; or even therapeutic techniques that are not specific to a therapy model. The technique-specific therapy factors represent those elements that are specific to a particular therapy model, and typically are described thoroughly in therapy manuals, indicating specific topics to be addressed during therapy, how they should be conveyed, the implementation of structure, the number of therapy sessions, the degree of exposure, and/or the schedule of homework tasks.

The use of experimental designs has given insight regarding which of these four variance components contribute most to the treatment effect. The major part of the treatment effect seems to be caused by the client-related and common factors, which explain between 30% and 40% and 30%–50% of the total treatment effect, respectively (e.g., Horvath & Greenberg, 1986; Luborsky et al.,

1988). The therapist-related factors have been found to explain 5%–15% of the treatment outcomes (Huppert et al., 2001; Wampold & Brown, 2005). That leaves approximately 10%–20% of the effect attributable to the specific therapy (Duncan, Miller, & Sparks, 2004; Lambert, 1992). Recent research has extended our insight into the role of the various components, as it seems that the role of specific versus nonspecific factors in CBT shift with the provision of an increasing number of therapy sessions (Honyashiki et al., 2014). This makes sense, as common factors (e.g., alliance) should be more important in the beginning of therapy, while efficient implementation of treatment-specific factors are increasingly important as therapy progresses. In addition, the role of common factors depends on the mental disorder of the patient. For example, patients with borderline personality disorder may respond much more favorably to the relationship and alliance-building skills of a therapist (Bienenfeld, 2007) compared with patients with bipolar disorders. Although the role of specific versus nonspecific factors may vary, the role of common factors in treating depression is more substantial, as one of the core issues in CBT treatment is to address distorted thoughts related to interpersonal consequences (Castonguay et al., 1996).

Because the common factors seem to be so important for attaining improvement following therapy, psychotherapy researchers have become concerned with them, and how to integrate them into the therapy (Imel & Wampold, 2008). An important line of support of the common factors model comes from meta-analyses showing that different treatment modalities produce relatively comparable treatment effects (e.g., Smith & Glass, 1977; Wampold et al., 1997); hence, the assumption that elements common to all therapies underlie the lack of marked differences among them (Lambert & Bergin, 1994; Seligman, 1995). As specific techniques dictated by a therapy model apparently represent a small part of the overall treatment effect, one would theoretically expect that refinements or improvements of CBT approaches over the past 30 years would have little impact on treatment efficacy, or reported effect sizes (ES). However, the implementation of specific treatment components is usually embedded within a common factors model approach to psychotherapy (Hoffart et al., 2009); otherwise, psychotherapy would stand out as highly decontextualized and mechanistically delivered and experienced by the patient. Therapists who use CBT are trained to establish rapport by, for example, socializing the patient to the cognitive therapy process (thus, being explicit about how the therapy will progress, which may reduce uncertainty), communicating to the patient how CBT might be helpful (instilling hope and positive expectations), and educating the patient about the disorder per se (helping patients to understand their problems). Moreover, CBT therapists set an agenda in collaboration with the patient in order to avoid spending the limited amount of time they have on irrelevant topics. They actively invite the patient to provide feedback (to ensure a mutual understanding and provide opportunities for quick adjustments). They construct and continuously refine their conceptualization of the case (further facilitating and deepening the understanding of the patient's problems). They collaborate actively with the patient in making plans for between-session tasks that may help eliminate negative personal beliefs and behaviors. The latter may help the patient to attribute positive changes to their own efforts, thereby increasing self-efficacy. For this reason, improvements in self-efficacy may be mediated by the use of specific techniques aimed at improving

self-efficacy, in addition to an effective integration of the common factors. The integration of the common factors is, thus, utterly important as they represent the chassis that enables the motor to move the vehicle forward. An important part in this context is the working alliance between the therapist and the patient, which is associated with quicker and larger treatment effects (Rector, Zuroff, & Segal, 1999), and a reduction of the number of early dropouts (Kegel & Fluckiger, 2014).

Although CBT treatments have focused less on the common factors, we believe that CBT therapists have become increasingly aware of the importance of integrating common and specific techniques to take full advantage of the therapy. Therefore, we expected that contemporary CBT treatments would show better treatment outcomes as compared with older clinical trials. If not, that would be a quite interesting and unexpected finding, which would warrant timely questions about the direction of CBT in the future, such as, "Should CBT researchers continue to improve current techniques of CBT?" and "Should they improve the integration of common factors, or should they enhance CBT via the inclusion of, for example, metacognitive (Wells, 2000) or transdiagnostic aspects (Fairburn, Cooper, & Shafran, 2003)"? In order to examine if the client, therapist, or common factors were related to treatment effects differently across time, we included all of the available data related to these components in the CBT studies in the meta-analysis.

An advantage of examining the temporal trends in treatment effects based on CBT trials is the high degree of standardization, a factor that has not changed appreciably over the years. Since the 1970s, almost all studies have utilized the Beck Depression Inventory (BDI; Beck et al., 1961). The BDI is a self-report rating inventory that measures different attitudes, symptoms, and behaviors that characterize depression. The internal consistency is generally good with high alpha coefficients (e.g., .86 and .81 in psychiatric and nonpsychiatric populations; Beck, Steer, & Carbin, 1988). The other depression measures have been more variable, with the exception of the Hamilton Rating Scale for Depression (HRSD; Hamilton, 1960), which has been utilized more frequently, in conjunction with the BDI or by itself. The HRSD is a clinician administered rating scale, measuring similar characteristics of depression as the BDI. The interrater reliability is generally high with coefficients typically exceeding .84 (Hedlund & Vieweg, 1979). The correlation between the BDI and the HRSD is in the moderate to high range, $r = .5$ to $.8$ (Beck et al., 1988; Beck, Steer, & Brown, 1996). Moreover, most clinical researchers have followed a standardized CBT treatment manual that therapists have been trained to deliver. This methodological allegiance has allowed a more empirically valid and reliable comparison of effect sizes for CBT interventions across the decades.

Moderators of Temporal Treatment Effects

In addition to the temporal (i.e., "time") factor in the present study, we examined the role of selected moderator variables that are available in most clinical studies. The following client-specific factors were included in the analysis: gender, age, degree of psychiatric comorbidity, use of psychotropic medication, severity of depression, and provision of CBT to special patient samples (such as those with diabetes). The therapist-

related factors were the type of therapist (e.g., psychologist or student) and ratings of the competence of the therapist. The treatment-specific and methodological factors included the publication year, number of therapy sessions, application of the original CBT manual (Beck et al., 1979) or not, checks of adherence to the treatment protocol (including subsequent feedback to the therapists), type of statistical analyses (intention to treat [ITT] or completers only), and ratings of the methodological quality of the study. The only available variable indicating common factors was the ratings of the therapeutic alliance; however, the number of studies reporting the alliance was disappointingly small.

Client-Related

Previous studies have typically not revealed any significant differences in treatment effects related to gender and age (Joutsenniemi et al., 2012; Wierzbicki & Pekarik, 1993). A higher degree of psychiatric comorbidity often implies a worse course of illness or treatment prognosis. The most common Axis I comorbidity is anxiety disorders (Kessler et al., 2003), which usually imply a higher degree of severity at intake (Kohler et al., 2013), as well as a poorer natural course (Penninx et al., 2011). The presence of comorbid Axis-II disorders, of which the Cluster C diagnoses, particularly, avoidant personality disorder, are the most prevalent (Friborg et al., 2014), heightens the risk of a worse outcome following treatment (Newton-Howes, Tyrer, & Johnson, 2006). The relative efficacy of psychotropic medication versus CBT has been subjected to many clinical trials; however, a meta-analysis of 21 studies found no differences between the two treatment modalities in alleviating depression (Roshanaei-Moghaddam et al., 2011). The addition of medication to CBT has been studied to a lesser degree; however, a meta-analysis consisting of seven studies found that CBT plus medication was slightly better ($d = 0.32$) than CBT alone (Cuijpers et al., 2009). The present meta-analysis is not entirely comparable with the study by Cuijpers et al., as we recorded the percentage of patients receiving simultaneous medication. With regard to the severity of depression, previous research has found that patients who were more severely depressed reported larger treatment effects than less severely depressed patients, a phenomenon also known as regression to the mean (Garfield, 1986; Lambert, 2001). Some of the CBT studies that were included in the present meta-analysis also recruited patients who had other somatic illnesses or difficulties in addition to depression, for instance, diabetes, alcoholism, or marital discord. Few previous studies (if any) have examined whether these patients respond differently to CBT treatment than purely depressed patients. However, as the effect size has tended to be lower for patients with psychiatric comorbidities, one also could expect a similar trend among patients having somatic or other ailments in addition to depression.

Therapist-Related

More therapeutic experience has been found to relate to a shorter time to remission (Okiishi et al., 2006), and hence, psychologists should do better than student therapists should. In the current study, three types of therapists were registered: psychiatrists, psychologists, and psychology students.

Treatment-Specific/Methodological

A dose-response relationship has been documented, in that additional sessions of therapy usually lead to a higher treatment efficacy (e.g., Howard et al., 1986). As adherence to a treatment manual ensures the implementation of CBT and improves the outcome (Shafran et al., 2009), we expected a similar relationship in the present meta-analysis. Studies integrating adherence or fidelity checks should demonstrate higher ESs than those without such checks. In the same vein, we expected that the use of the Beck treatment manual would yield stronger treatment effects than trials not using it, as fidelity checks are often involved with its use. As studies using stricter criteria with regard to methodological quality (Gould, Coulson, & Howard, 2012), and studies using between rather than within group designs (Pallesen et al., 2005) generally yield lower treatment effects, we expected the same results in the present analysis. A recent meta-analysis (Hans & Hiller, 2013) showed a slightly larger effect size in depression treatment trials using a statistical design requiring treatment completion ($d = 1.13$), as compared with an ITT design ($d = 1.06$); therefore, we expected the same trend in the present study.

Common Factors

Patients experiencing a stronger alliance with their therapist were expected to report better effects of their treatment (Rector et al., 1999).

Meta-Analytic Advantages and Objectives

The benefits of using meta-analytic methods to summarize clinical results are well-known (Borenstein et al., 2009). By accessing a large pool of studies and assigning the individual studies different weights according to their sample size, the potentially troublesome role of individual studies indicating weak or even contradictory results is minimized. A meta-analysis is also preferable in situations where the majority of studies are well-defined or similar in terms of patients, diagnoses, intervention procedures, and the measurement instrument used (e.g., the BDI), thus, simplifying the quantification of the effect size considerably. Moreover, metaregression approaches may be used to identify potential sources of covariation between study-related factors and treatment effects.

Objectives of the Present Study

The primary objective was to examine whether published clinical CBT trials (both uncontrolled and randomized controlled) aimed at treating unipolar DDs demonstrate a historical change in treatment effects, independent of study-related moderating variables. A more effective therapy should demonstrate larger positive changes in prepost scores, as rated by the patients (the BDI) and the therapists (the HRSD) over the years.

The secondary purpose was to examine the role of various moderators of the reported effect sizes. We predicted that diagnostic severity and type of therapist (psychologist better than student therapist), and therapist competency would be associated with better treatment effects, while the variables age and gender were not expected to covary with therapeutic outcome. Finally, we examined whether these moderators modified the regression slopes describing the time trends in the treatment effects.

Method

Data Collection, Studies, and Selection Criteria

We used the OvidSP Internet-based platform to identify relevant empirical English-language studies. All searches were conducted in January 2015 using the following databases (without publication year restrictions): PsycINFO, APA PsycNET, Embase, and Ovid Medline. In PsycINFO, the query "treatment effectiveness evaluation" returned 14,935 titles. In APA PsycNET, the search "depression *and* study" and "depression *and* treatment" returned 5,996 and 1,974 titles, respectively. A third query in all databases using "depression *and* efficacy or efficacious" returned 4,353 titles. A final query in all the databases using the phrase, "depression *and* trial *and* cognitive" returned an additional 1,793 titles. The total number of titles was 29,051. After examining the titles, 1,670 abstracts were considered relevant. Following a review of the abstracts, 489 articles were obtained via the university library. The following exclusion criteria were then applied: (a) the implemented therapy was not pure CBT. Thus, we did not include studies/study arms of CBT combined with other treatment forms, such as mindfulness based CBT. We did include one study arm consisting of integrative CT (Castonguay et al., 2004) as the published treatment protocol, in essence, indicated standard cognitive therapy, albeit, with an additional structured procedure for repairing any ruptures in the patient-therapist alliance. Among the studies comparing CBT with other treatment forms (interpersonal therapy, for instance), we included only the CBT treatment arm; (b) a unipolar DD (either mild, moderate, severe, or recurrent) was not the primary psychiatric diagnosis; (c) participants were not adults (mean age $<$ 18); (d) therapy was not implemented by a therapist trained in CBT; (e) the psychotherapeutic intervention was not intended to treat depression; (f) the outcome was not measured with the BDI or the HRSD; (g) patients had acute physical illnesses or suffered from bipolar or psychotic disorders; (h) treatment was not implemented as individual face-to-face therapy; and (i) the patients had a BDI score lower than 13.5. The last criterion is in accordance with the manual of the revised BDI, and several depression treatment researchers (Beach & O'Leary, 1992; Emanuels-Zuurven & Emmelkamp, 1997; Kendall et al., 1987; Murphy et al., 1995; Wright et al., 2005).

If a study assigned patients to different subgroups based on diagnostic severity (usually based on the pretest BDI scores), only the most severe subgroup was included to avoid inflating the number of independent studies. This procedure was relevant for three studies. For the same reason, if a study assigned patients to treatment subgroups consisting of one group with CBT, and one group with CBT plus medication, we only included the pure CBT group in our analysis. The selection procedure yielded a final study pool of 70 studies (see Figure 1).

Coding of Study Information and Moderator Variables

The following data from the studies were coded: demographic information (gender and age), year of implementation of the in-
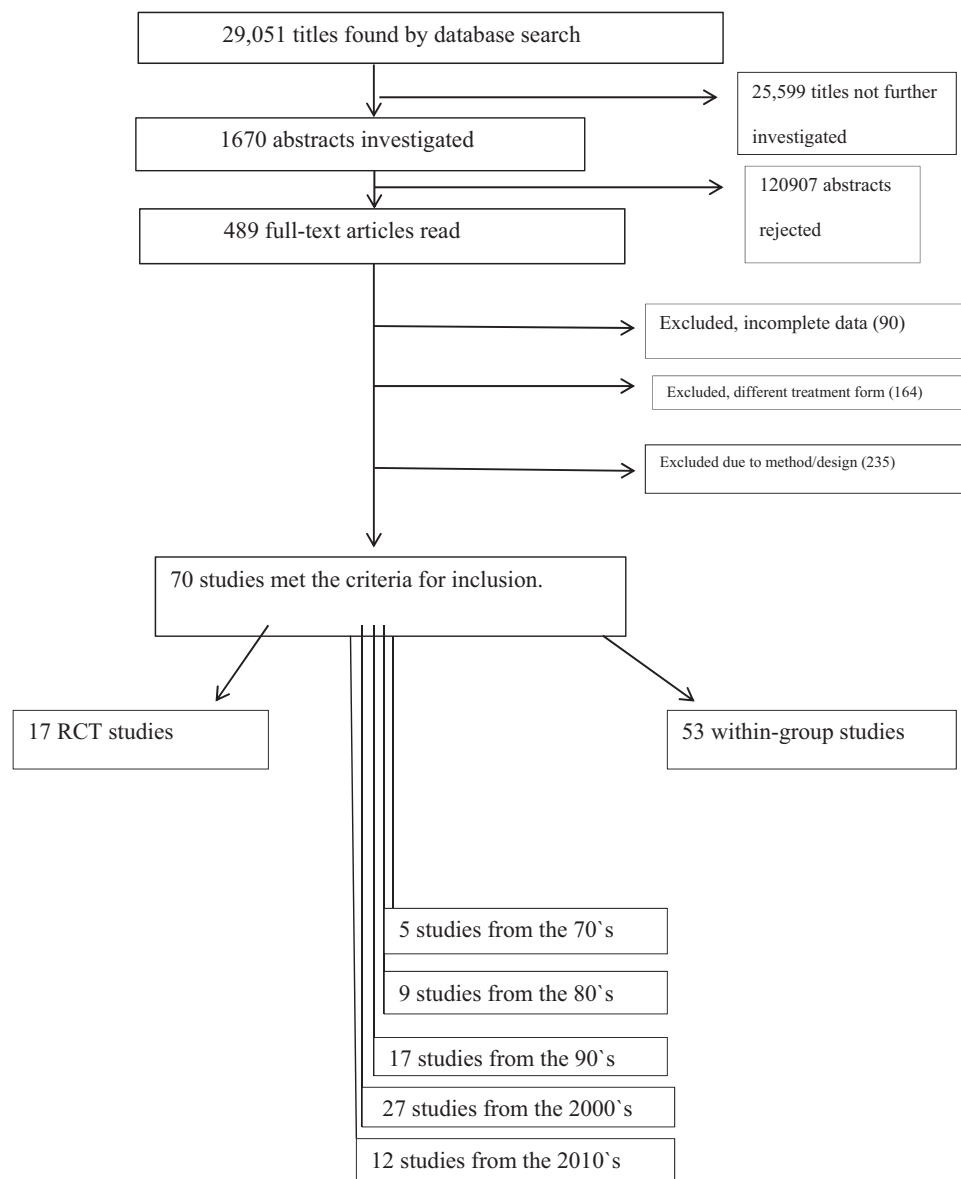
Figure 1. Flowchart of the search and selection procedure.

tervention, duration (number of sessions), type of therapist (psychologist, trained psychology-student, or other/unknown), therapist competence (as measured by the Cognitive Therapy Scale), information about the severity of the diagnosis (mild, moderate, severe, or recurrent depression) along with the proportion (%) of the sample having comorbid psychiatric diagnoses, whether the patient population had any special characteristics (marital discord, HIV, multiple sclerosis, diabetes, Parkinson's disease, alcohol abuse disorders or pregnancy), and the proportion (%) of patients using psychotropic medication. The DD diagnoses of the patients were coded according to the original authors' definitions. If unreported, we categorized the DD diagnoses based on the BDI pretest scores as mild (13–19.5), moderate (20–29.5), or severe (> 30). We coded recurrent depression as the main diagnosis if at least half of the patients previously had two or more episodes of depression.

The Randomized Controlled Trial Psychotherapy Quality Rating Scale (RCT-PQRS) was used to rate the methodological quality of the published studies (Kocsis et al., 2010). It is a comprehensive instrument consisting of 24 items measuring six study quality dimensions: (a) adequate descriptions of subjects; (b) the definition and delivery of treatment; (c) the quality of the outcome measures utilized; (d) the data analyses (e.g., description of dropouts, ITT, appropriate tests); (e) strong methods for assignment to treatment groups; and (f) an overall quality rating. Each item is assigned a score of 0 (*poor description, execution, or justification of a design element*), 1 (*brief description or either a good description or an appropriate method or criteria set, but not both*), or 2 (*well described, executed, and, where necessary, justified design element*). The scale yields a total score ranging from 0 to 48, which was used in a subsequent metaregression analysis.

Moderator Analyses

We investigated whether the effect sizes covaried with any of the following moderator variables: type of statistical analysis (ITT vs. completers analysis), gender (as % men), age, proportion of patients using medication, proportion of comorbidity, use of the Beck CBT treatment manual versus no manual, checks (and subsequent feedback) of therapist adherence to the treatment manual versus no adherence check, version of BDI (I or II), severity of the depressive disorder, diversity of the study populations (ordinary depressed patients vs. patients with co-occurring illnesses or other special characteristics, such as Parkinson's, HIV, diabetes, marital discord, alcoholism, or multiple sclerosis), number of therapy sessions, type of therapist, therapist competency, and the publication year of the CBT intervention (the moderator of most interest). We also examined whether the latter variable covaried with the effect sizes in the waiting list control groups. The competence of the therapist was, in a few studies, rated using the Cognitive Therapy Scale (CTS; Dobson et al., 1985), and it was included as a moderator. The CTS is an observer-based rating scale (usually rated by an expert in CBT) designed to measure how well the therapist applies CBT across several therapist skills dimensions, including adherence to the manual.

Effect Sizes

We used two procedures when calculating the effect sizes based on the BDI and the HRSD pre-/postintervention scores: a prepost within-study design, and a controlled trial (CT) design. For studies that did not include a no-intervention control group, a standardized mean difference (SMD, also denoted Cohen's d) was calculated for the intervention group ($M_{pre} - M_{post}$, divided by the standard deviation of the change score). A Hedges g correction was applied to the SMD, which reduced the SMD for studies having small sample sizes (Hedges & Olkin, 1985). The vast majority of these intervention studies were drawn from different randomized controlled trials, but because of methodological choices or study design issues, they could not be categorized as CTs in the present analysis. For example, some studies compared CBT with antidepressant medication or other forms of therapy, and hence, for these studies only, the intervention group receiving CBT was included as a within-group study.

For the controlled trials condition (which included 15 randomized studies with a waiting list condition as the control group, and two studies with a treatment as usual type of control group without specific interventions for depression), the effect sizes were calculated from the difference between the pre- and posttest scores on the BDI and the HRSD for the intervention group and the control group, respectively, and then standardized using the change scores. This method was preferred to standardization using post scores, because studies including a smaller number of participants might contain preintervention differences despite randomization. The change score variant is less sensitive to such differences compared to standardization using post scores. Another advantage of using the SD for change scores is that the effect sizes for CT studies are estimated similarly as studies without a control group (within-study designs). Standardization by change scores also is recommended when the objective is to assess change relative to preintervention scores (Kulinskaya et al., 2002), and it has frequently been used to quantify treatment effects in other meta-analytic

reviews of psychotherapy (e.g., Abbass et al., 2013; Kishi et al., 2012; McGuire et al., 2014; Watts et al., 2013; Zoogman et al., 2014). However, one limitation is that change scores require knowledge of the prepost correlation, and consequently, we imputed a conservative value of $r = .7$ for studies that did not report one ($k = 65$), as recommended by Rosenthal (1993).

When available, we calculated the ES based on scores from completers of an intervention (51 studies). The remaining studies only provided data from ITT samples (19 studies), and were thus coded accordingly.

The effect sizes for the treatment recovery rates (the number of patients who ended treatment with a BDI score below a predefined clinical cut-off score, <10) were coded as an event rate (rate = number of events/sample size), which the Comprehensive Meta-Analysis (CMA) program linearized by calculating the logit before estimating the metaregression coefficients. This method also is known to yield standard errors that are more accurate.

Interrater Reliability

The second author (OF), coded a random sample of 20 studies. The level of agreement between the raters (the first and second author) was determined by Kappa (κ) coefficients for dichotomously scored variables, and intraclass correlation coefficients (two-way mixed, average models) for continuously scored variables. Kappa coefficients (range: 0-no agreement, 1-perfect agreement) within the range of .41–.60 and .61–.80 were interpreted as moderate versus substantial agreement, respectively (Rigby, 2000). The ICCs (range: 0-no agreement, 1-perfect agreement) were interpreted similarly as Cronbach's alpha, with ICCs > .70 and > .80, indicating moderate and high consistency, respectively, between the raters. The coefficients were: BDI effect size calculations (ICC = .95), publication year (ICC = 1.0), study design ($\kappa = .77$), diagnosis ($\kappa = .63$), gender % (ICC = .99), therapist ($\kappa = .59$), no. of session (ICC = .97), patient's age (ICC = 1.0), remission rate (ICC = .83), type of analysis ($\kappa = .69$), comorbidity % (ICC = .96), use of the Beck manual ($\kappa = .62$), BDI version ($\kappa = 1.0$), and study quality (ICC = .89). The interrater reliability analyses thus revealed substantial agreement. The studies with disparate ratings were followed-up by the two coders, and agreement was reached by consensus following a discussion. It turned out that the first author had coded almost all of the disparate cases correctly, which was reassuring as the first author coded all of the studies.

Quantitative Data Synthesis and Statistical Calculations

The CMA software, Version 2 (Borenstein et al., 2005) was used for all statistical analyses, except for the two-way interaction analyses between the moderator variables, which had to be analyzed in SPSS 21. The random weights from the CMA program were imported into SPSS and a weighted least-squares regression analysis was conducted.

The average weighted effect sizes were estimated according to a random-effects model (in preference to a fixed effects model), as we assumed the true effect sizes would vary between studies due to the study-related factors, for example, severity of diagnosis, age, or gender. Employing a random-effects model also increases the

generalizability of the results (Field, 2003). A Q-test statistic (chi-square distributed) was calculated to examine whether the variance between studies was larger than the variance within the studies, thus, indicating predictors (or moderators) of between-study variation. Metaregression analyses were used to analyze the role of the continuous moderator variables (e.g., publication year), and were based on the unrestricted maximum-likelihood method, as it assumes an underlying random distribution of effect sizes. The moderator analyses for the categorical variables were based on a similar Q-test statistic to examine whether the variability between categories (subgroups in the study) was larger than the variability within studies. The I^2 statistic also was reported to indicate the amount of heterogeneity that was related to the true differences in effect sizes between studies, relative to sampling error. The influence of the time variable on ES was examined based on both the BDI and the HRSD, in addition to the remission rates. The associations of the other moderator variables and ES were examined based on the BDI measure, which had the largest number of studies.

Publication Bias

To measure the potential biasing effect of including studies with few participants, we visually inspected the funnel plot and used Duval and Tweedie's trim-and-fill method.

Results

Studies and Participants

The search procedure resulted in 70 eligible studies with CBT implemented as individual therapy for depression. Fifty-two studies were randomized controlled trials, five studies were controlled trials without randomization, two studies were uncontrolled and nonrandomized (pilot studies), while 11 were clinical field studies. The studies were conducted from 1977 to 2014, with 1999 as the average year. Seventeen studies were categorized as CT in the present meta-analysis (those including a waiting list control group), while 53 were categorized as within-group studies (those lacking a waiting list control group, plus the 11 field studies). The average quality rating of the studies was 28.4 ($SD = 7.5$, range 7–42).

The total number of patients was 2,426. The number varied from seven to 217 patients in the studies, with an average of 34.6 patients per study ($SD = 34.1$). Males accounted for 30.9% of the patients, and the average age was 40.5 years ($SD = 10.9$). On average, 43% of the patients had a comorbid psychiatric diagnosis ($SD = 23\%$, $k = 26$ number of studies). Thirteen studies included patient populations having other conditions or characteristics, for example, marital discord or diabetes.

The mean BDI preintervention score was 26.1 ($SD = 4.1$). Fifty-seven percent ($SD = 18.9$) of the patients had remissions from depression following treatment ($k = 43$). The patients received an average of 14.6 sessions of CBT ($SD = 5.12$, range = 6–34). Sixty-seven studies included prepost data from the BDI (48 used the BDI-I and 19 used the BDI-II), and 34 studies included depression endpoints based on the HRSD. See Table 1 for a descriptive overview of the included studies.

Effects of CBT

The average weighted effect size for the BDI ($k = 67$) was $g = 1.58$ (95% CI [1.43, 1.74]). The variance in the effect sizes and the associated confidence intervals are presented in a forest plot (see Figure 2). A Q-test indicated that the methodological design did not yield significantly different ($p = .13$) treatment effects (within-group, $g = 1.65$ vs. between-group CT, $g = 1.37$). The difference in the weighted ES between the ITT and completers was not significantly different ($p = .34$). For the HRSD ($k = 34$), the average ES was 1.69 (95% CI [1.48, 1.89]). The HRSD effect sizes and the associated CIs are presented in Figure 3. The methodological design again revealed no significant ($p = .10$) effect size differences (within-group, $g = 1.81$; between-Group CT, $g = 1.44$).

Are CBT Treatment Effects Contingent on the Year of Publication?

The CBT effect sizes (based on the BDI) had a significant negative relationship with time, that is, publication year ($p < .001$, see Table 2 for coefficients and Figure 4 for a scatterplot). According to a subgroup analysis, a similar negative relationship was evident among studies using within-group designs ($p < .001$), and CT designs ($p < .05$).

The effect sizes for the HRSD showed a comparable picture (Table 2 and Figure 5). The ES decreased with time ($p = .01$). The significant negative relationship was evident for the within-group design studies ($p < .01$). The ES in the CT studies also showed a declining trend, but it was not significant ($p = .51$).

The remission rates (percentage of patients recovering) also were negatively related with publication year ($p < .01$; see Figure 6).

Because Figure 4 indicated an apparent decline in the effect sizes for studies conducted in 1995–2002, these studies were excluded to determine if they had an undue influence on the results, but the results the same ($p < .001$, see upper part of Table 3). A similar inspection was done for the 11 clinical field studies, but excluding these studies did not change the results either ($p = .001$). We also examined how the slope of the regression line for time changed when we excluded studies consecutively, beginning with the first publication year in 1977. As seen in Figure 7, all of the coefficients for time were negative, except for those from the studies published between 1994 and 1997. However, the decline in treatment effects was again evident from 1998 and onward.

The waiting list control group condition exhibited no significant changes in effect sizes across time ($p = .48$).

Publication Bias

The funnel plots for all of the CBT studies suggested a certain degree of publication bias for ESs based on the BDI. A significant proportion of the effect sizes were plotted to the upper left of the inverted curve, which suggests that the studies with low numbers of participants had a higher ESs than the studies with more participants. This was not the case for the HRSD, which showed a more symmetrical plot (see Figures 8 and 9).

Duvall and Tweedie's trim-and-fill method also indicated a bias for the BDI, but not for the HRSD. Consequently, nine studies

Table 1

A Descriptive Overview of the 70 CBT Studies Included

Author (publication year)	Patient characteristic	Trial	N	ES BDI (HRSD)	Rec %	Sessions
Rush et al.(1977)	None	RCT	18	3.68 (4.17)	79	15
Taylor & Marshall (1977)	None	**RCT**	15	1.71	—	6
Carrington (1979)	Women	**RCT**	11	2.49	—	12
Dunn (1979)	None	RCT	10	2.16	—	8
Mclean & Hakstian (1979)	None	RCT	10	2.50	50	10
Liberman & Eckman (1981)	Suicide attempters	RCT	12	2.22	—	32
Gallagher & Thompson (1982)	Elderly	RCT	27	2.12 (1.85)	73	16
Wilson et al. (1983)	None	**RCT**	8	2.94 (2.19)	—	8
Beck et al. (1985)	None	RCT	18	2.64 (3.34)	71	14
McNamara & Horan (1986)	None	RCT	10	3.95	80	10
Thompson et al. (1987)	Elderly	**RCT**	27	1.97 (0.93)	52	16
Wierzbicki & Bartlett (1987)	None	**RCT**	11	1.97	—	12
Persons et al. (1988)	None	FS	35	1.53	80	25
Elkin et al. (1989)	None	RCT	37	1.90 (2.11)	70	12
Selmi et al. (1990)	None	**RCT**	12	1.07 (1.89)	42	6
Jacobson (1991)	Marital discord	CT	7	2.63 (2.24)	58	20
Thase et al. (1991)	None	NRT	38	2.42 (3.13)	61	20
Beach & O'Leary (1992)	Marital discord	**RCT**	15	1.36	—	16
Hollon et al. (1992)	None	RCT	16	2.44	—	15
Propst et al. (1992)	Religious	**RCT**	10	1.39 (0.82)	90	18
Gallagher-Thompson & Steffen (1994)	Elderly	RCT	36	1.32 (1.08)	77	20
Shapiro et al. (1994)	None	RCT	18	2.12	—	12
Murphy et al. (1995)	None	RCT	11	2.39 (2.85)	82	17
Teichman et al. (1995)	None	**RCT**	38	0.24	14	13
Emanuels-Zuurveen & Emmelkamp (1996)	Marital discord	RCT	14	0.38	—	16
Jacobson et al. (1996)	None	RCT	50	2.19 (2.28)	71	16
Blackburn & Moore (1997)	None	RCT	24	0.70 (1.12)	33	16
Brown et al. (1997)	Alcoholism	RCT	19	1.68 (1.47)	—	8
Emanuels-Zuurveen & Emmelkamp (1997)	None	RCT	10	0.97	—	16
Markowitz et al. (1998)	HIV	**RCT**	17	0.67 (0.95)	40	12
Persons et al. (1999)	None	CT	27	1.32	80	34
King et al. (2000)	None	RCT	63	1.63	74	12
Thase et al. (2000)	None	CT	52	1.35 (1.80)	38	16
Thompson et al. (2001)	Elder	RCT	31	0.39 (1.61)	—	16
Cahill et al. (2003)	None	FS	30	1.55	—	16
Merrill et al. (2003)	None	FS	100	1.81	55	8
Watson et al. (2003)	None	RCT	33	1.64	—	16
Castonguay et al. (2004)	None	**RCT**	11	2.44 (1.16)	100	17
Misri et al. (2004)	Postpartum	RCT	19	−(1.92)	63	12
Hardy et al. (2005)	None	FS	76	1.33	61	12
Westbrook & Kirk (2005)	None	FS	95	1.42	36	13
Wright et al. (2005)	None	**RCT**	15	1.82 (1.06)	—	9
Dimidjian et al. (2006)	None	RCT	18	1.00 (1.51)	48	16
Jarrett et al. (2007)	None	FS	126	2.21 (1.82)	63	20
McBride et al. (2006)	None	RCT	29	2.73	—	17
Persons et al. (2006)	None	FS	38	1.17	—	18
Strauman et al. (2006)	None	RCT	7	1.97 (2.04)	33	18
Dobkin et al. (2007)	Parkinson	Pilot	13	1.12	—	12
Forman et al. (2007)	None	RCT	44	0.65	61	16
Luty et al. (2007)	None	RCT	86	1.18 (1.44)	43	13
Cho et al. (2008)	Pregnant	**RCT**	12	1.74	—	18
Constantino et al. (2008)	None	RCT	11	1.53	82	16
David et al. (2008)	None	RCT	56	2.20 (2.11)	50	20
Laidlaw et al. (2008)	Elder	RCT	20	1.23 (1.35)	—	8
Quilty et al. (2008)	None	RCT	45	1.73	—	18
Craigie & Nathan (2009)	None	FS	77	1.87	53	11
Gibbons et al. (2010)	None	FS	217	0.7	36	16
Dobkin et al. (2011)	Parkinson	**RCT**	41	1.37 (2.03)	—	10
Mohr et al. (2001)	Mul. Scler.	RCT	20	1.42 (1.31)	40	16
Rieu et al. (2011)	None	RCT	11	1.07 (1.09)	—	6
Estupina & Encinas (2012)	None	FS	30	2.09	80	18
Power & Freeman (2012)	None	**RCT**	46	0.38	50	16
Ammerman et al. (2013)	Mothers	**CT**	47	1.18 (1.05)	—	11
Gibbons et al. (2013)	None	CT	18	2.21	—	19

(*table continues*)

Table 1 (*continued*)

Author (publication year)	Patient characteristic	Trial	N	ES BDI (HRSD)	Rec %	Sessions
Kohler et al. (2013)	None	FS	105	1.50 (3.02)	58	14
Parker et al. (2013)	None	RCT	11	−(0.74)	45.4	10
Wagner et al. (2014)	None	RCT	28	1.30	50	8
Lopes et al. (2014)	None	RCT	29	1.16	40	14
Tovote et al. (2014)	Diabetes	**RCT**	32	0.92	29	8
Kalapatapu et al. (2014)	**Alcoholism**	**RCT**	**53**	**−(1.84)**	**41**	**18**

Note. CT = controlled trial, not randomized; NRT = nonrandomized, noncontrolled trial; FS = field study; RCT = randomized controlled trial; *ES* = Hedge's *g*; BDI = Beck Depression Inventory; HRSD = Hamilton Rating Scale of Depression; Rec % = percentage of patients remitting; Sessions = number of treatment sessions. Studies in bold (**RCT**) are the ones with a nonintervention comparison group, hence the ones subsequently included in the CT condition in this meta-analysis.

were trimmed, which adjusted the *g* from 1.58 to 1.46. However, removal of all studies (30 in total) with small sample sizes ($n <$ 20) did not change the above findings; the slope was still negative ($p < .05$, see Table 3). The removal of these studies also excluded the two potential outliers with the highest ESs observed in Figure 2, without having a substantial influence on the outcome.

Moderators Related to Client, Therapist, Treatment-Specific/Methodologies, and Common Factors

A separate analysis for each moderator variable was conducted.

Client-related. Age was not significantly related to variation in treatment effects; however, the gender variable was ($p < .05$). Studies that included a higher percentage of women demonstrated a better treatment effect than studies consisting of more men. The proportion of comorbid psychiatric diagnoses in the studies did not significantly moderate the reported weighted ESs, nor did the proportion of psychotropic medication use or the severity of diagnosis (see Table 3). Milder depression, although not statistically significant, tended to yield lower treatment effects compared with more severe or recurrent depression. The low number of available studies in some of the subgroups (particularly the recurrent group), speaks to the need for exercising caution in interpreting these results. Diagnostic diversity in the patient group was not significantly related to ES. The 13 studies, including those with patients with special characteristics, (e.g., comorbid somatic diseases or marital discord problems), did not significantly differ from patients with depression only (see Table 4). Excluding studies with special characteristics did not change the negative temporal trend in the treatment effects. A marginally significant negative trend in ESs with time was also observed among the 13 studies with special characteristics (see Tables 3 and 4).

Therapist-related. Therapist competency did not have a significant relationship with treatment effects. However, the number of available studies was low ($k = 5$), implying low statistical power and a high vulnerability to bias of the results from single studies. Yet, the regression line was positive as expected, indicating higher ESs with higher levels of competence. The effect size differences between types of therapists were significant ($p < .01$), indicating that trained psychologists achieved better treatment effects ($g = 1.59$) than psychology students ($g = 0.98$).

Treatment-specific/methodological factors. The number of therapy sessions was not related to a better treatment effect; neither was the use of the Beck CBT manual, adherence checks, the data analysis method (ITT vs. completers), or the study quality ratings

(see Tables 3 and 4). A nonlinear weighted regression model, which examined whether shorter or longer therapy trials yielded poorer treatment results compared to a moderate amount, was not significant ($p = .99$).

Common-factors. Seven studies contained information about the patient–therapist alliance. However, five of the studies used qualitative or customized measures that were not suitable for quantification and statistical analysis. Only two studies provided quantitative data based on standardized measures of alliance. Thus, the role of common factors was not possible to analyze.

Correlations Between Time and Moderator Variables and Two-Way Interaction Tests (Time × Moderator)

The weighted correlation coefficients between *time* (publication year) and the moderator variables were as follows: (a) *client-related*: gender (male %, $r = .09$, $p = .48$), age, $r = .08$, $p = .53$, preintervention score BDI, $r = .26$, $p = .04$, comorbidity %, $r = −.14$, $p = .52$, medication %, $r = .25$, $p = .10$, patient (psychiatric vs. special) type, $r = .05$, $p = .69$, and severity (mild-moderate-severe) of depression, $r = −.04$, $p = .78$); (b) *therapist-related*: type of (student vs. psychologist) therapist, $r = .17$, $p = .26$); and (c) *study-related*: number of therapy sessions, $r = −.08$, $p = .52$, methodological quality, $r = .43$, $p < .001$, type of statistical (ITT vs. completers) analysis, $r = −.17$, $p = .17$, use of the Beck manual (no vs. yes) manual, $r = −.13$, $p = .29$, and BDI (I vs. II) version, $r = .59$, $p < .001$.

These analyses indicate that the methodological quality has improved significantly over the years. Newer studies also include more patients with higher initial BDI scores than the older studies, and employ the BDI-II rather than the original BDI-I version. Patients on medication are also more frequently included, but this coefficient was not significant.

Two-Way Interaction Tests

Finally, we examined whether the observed decline in the treatment effects depended on any of the above moderators by conducting two-way interaction tests (Time × Moderator). If the interaction coefficient was significant, or its unstandardized weight (beta$_{int}$) was positive and higher than the unstandardized time coefficient (beta$_{time}$), that would indicate the slope depended on the moderator and qualitatively changed its direction following the inclusion of the moderator. Conversely, a negative interaction
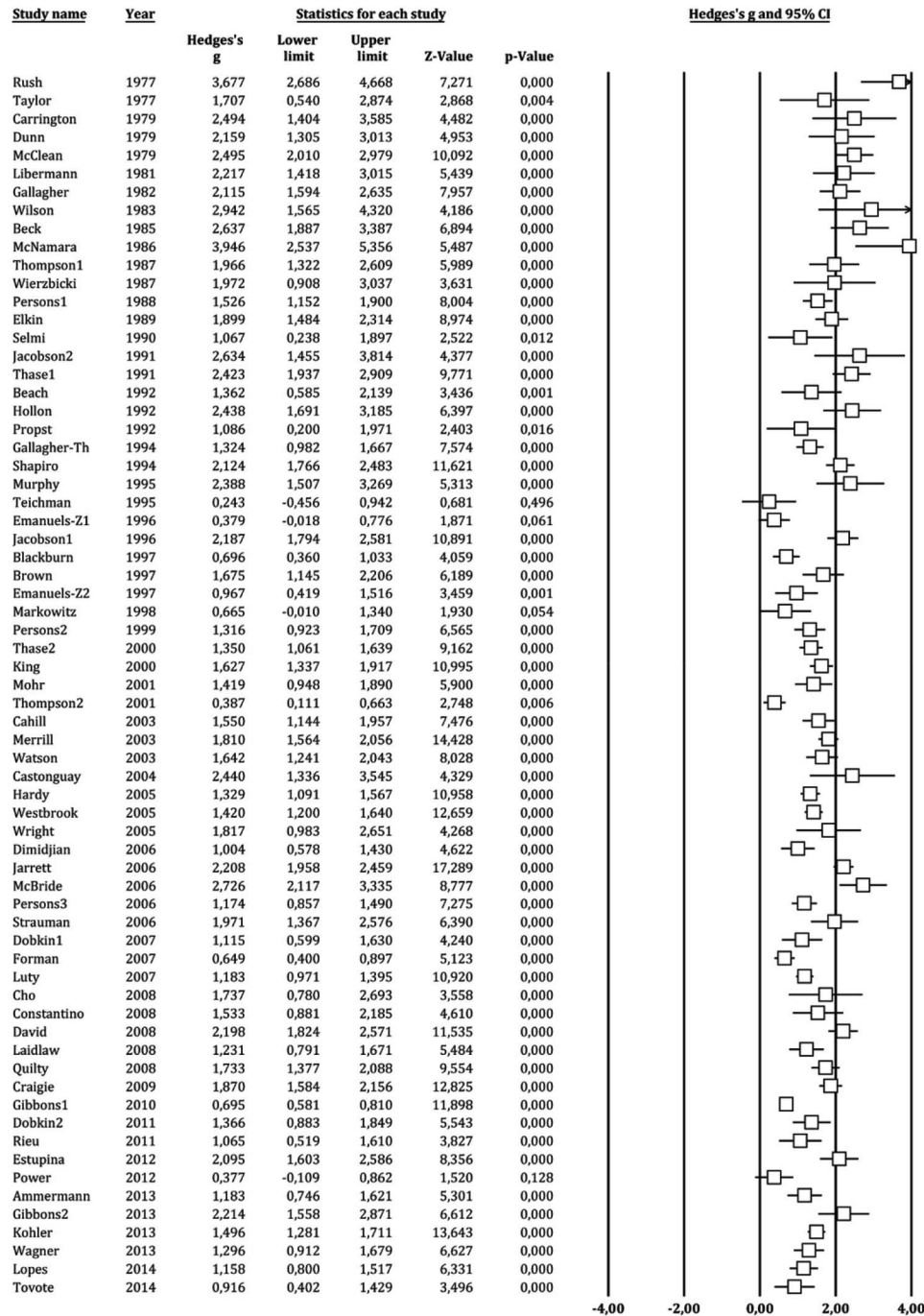
Study name	Year	Statistics for each study					Hedges's g and 95% CI
		Hedges's g	Lower limit	Upper limit	Z-Value	p-Value	
Rush	1977	3,677	2,686	4,668	7,271	0,000	
Taylor	1977	1,707	0,540	2,874	2,868	0,004	
Carrington	1979	2,494	1,404	3,585	4,482	0,000	
Dunn	1979	2,159	1,305	3,013	4,953	0,000	
McClean	1979	2,495	2,010	2,979	10,092	0,000	
Libermann	1981	2,217	1,418	3,015	5,439	0,000	
Gallagher	1982	2,115	1,594	2,635	7,957	0,000	
Wilson	1983	2,942	1,565	4,320	4,186	0,000	
Beck	1985	2,637	1,887	3,387	6,894	0,000	
McNamara	1986	3,946	2,537	5,356	5,487	0,000	
Thompson1	1987	1,966	1,322	2,609	5,989	0,000	
Wierzbicki	1987	1,972	0,908	3,037	3,631	0,000	
Persons1	1988	1,526	1,152	1,900	8,004	0,000	
Elkin	1989	1,899	1,484	2,314	8,974	0,000	
Selmi	1990	1,067	0,238	1,897	2,522	0,012	
Jacobson2	1991	2,634	1,455	3,814	4,377	0,000	
Thase1	1991	2,423	1,937	2,909	9,771	0,000	
Beach	1992	1,362	0,585	2,139	3,436	0,001	
Hollon	1992	2,438	1,691	3,185	6,397	0,000	
Propst	1992	1,086	0,200	1,971	2,403	0,016	
Gallagher-Th	1994	1,324	0,982	1,667	7,574	0,000	
Shapiro	1994	2,124	1,766	2,483	11,621	0,000	
Murphy	1995	2,388	1,507	3,269	5,313	0,000	
Teichman	1995	0,243	-0,456	0,942	0,681	0,496	
Emanuels-Z1	1996	0,379	-0,018	0,776	1,871	0,061	
Jacobson1	1996	2,187	1,794	2,581	10,891	0,000	
Blackburn	1997	0,696	0,360	1,033	4,059	0,000	
Brown	1997	1,675	1,145	2,206	6,189	0,000	
Emanuels-Z2	1997	0,967	0,419	1,516	3,459	0,001	
Markowitz	1998	0,665	-0,010	1,340	1,930	0,054	
Persons2	1999	1,316	0,923	1,709	6,565	0,000	
Thase2	2000	1,350	1,061	1,639	9,162	0,000	
King	2000	1,627	1,337	1,917	10,995	0,000	
Mohr	2001	1,419	0,948	1,890	5,900	0,000	
Thompson2	2001	0,387	0,111	0,663	2,748	0,006	
Cahill	2003	1,550	1,144	1,957	7,476	0,000	
Merrill	2003	1,810	1,564	2,056	14,428	0,000	
Watson	2003	1,642	1,241	2,043	8,028	0,000	
Castonguay	2004	2,440	1,336	3,545	4,329	0,000	
Hardy	2005	1,329	1,091	1,567	10,958	0,000	
Westbrook	2005	1,420	1,200	1,640	12,659	0,000	
Wright	2005	1,817	0,983	2,651	4,268	0,000	
Dimidjian	2006	1,004	0,578	1,430	4,622	0,000	
Jarrett	2006	2,208	1,958	2,459	17,289	0,000	
McBride	2006	2,726	2,117	3,335	8,777	0,000	
Persons3	2006	1,174	0,857	1,490	7,275	0,000	
Strauman	2006	1,971	1,367	2,576	6,390	0,000	
Dobkin1	2007	1,115	0,599	1,630	4,240	0,000	
Forman	2007	0,649	0,400	0,897	5,123	0,000	
Luty	2007	1,183	0,971	1,395	10,920	0,000	
Cho	2008	1,737	0,780	2,693	3,558	0,000	
Constantino	2008	1,533	0,881	2,185	4,610	0,000	
David	2008	2,198	1,824	2,571	11,535	0,000	
Laidlaw	2008	1,231	0,791	1,671	5,484	0,000	
Quilty	2008	1,733	1,377	2,088	9,554	0,000	
Craigie	2009	1,870	1,584	2,156	12,825	0,000	
Gibbons1	2010	0,695	0,581	0,810	11,898	0,000	
Dobkin2	2011	1,366	0,883	1,849	5,543	0,000	
Rieu	2011	1,065	0,519	1,610	3,827	0,000	
Estupina	2012	2,095	1,603	2,586	8,356	0,000	
Power	2012	0,377	-0,109	0,862	1,520	0,128	
Ammermann	2013	1,183	0,746	1,621	5,301	0,000	
Gibbons2	2013	2,214	1,558	2,871	6,612	0,000	
Kohler	2013	1,496	1,281	1,711	13,643	0,000	
Wagner	2013	1,296	0,912	1,679	6,627	0,000	
Lopes	2014	1,158	0,800	1,517	6,331	0,000	
Tovote	2014	0,916	0,402	1,429	3,496	0,000	

Figure 2.   Forest plot for the Beck Depression Inventory effect sizes.

effect indicated an even steeper decline. The size of the $beta_{time}$ coefficients varied in these analyses due to different sample sizes and correlations with the moderators.

Client-related.   None of these interaction coefficients were significant: male % ($beta_{time} = -.027$; $beta_{int} = .001$, $p = .33$), age ($beta_{time} = -.031$; $beta_{int} = .0004$, $p = .58$); preintervention score BDI ($beta_{time} = -.081$; $beta_{int} = -.003$, $p = .09$), comorbidity % ($beta_{time} = -.021$; $beta_{int} = -.0005$, $p = .54$); medica-

tion % ($beta_{time} = -.019$; $beta_{int} = -.00002$, $p = .94$); patient (normal vs. special) type ($beta_{time} = -.030$; $beta_{int} = -.007$, $p = .72$); and severity (mild-moderate-severe) of depression ($beta_{time} = -.031$; $beta_{int} = .003$, $p = .87$).

Therapist-related.   The single available variable, therapist (student vs. psychologist) type ($beta_{time} = -.021$; $beta_{int} = -.008$, $p = .79$), did not show a significant interaction with time.

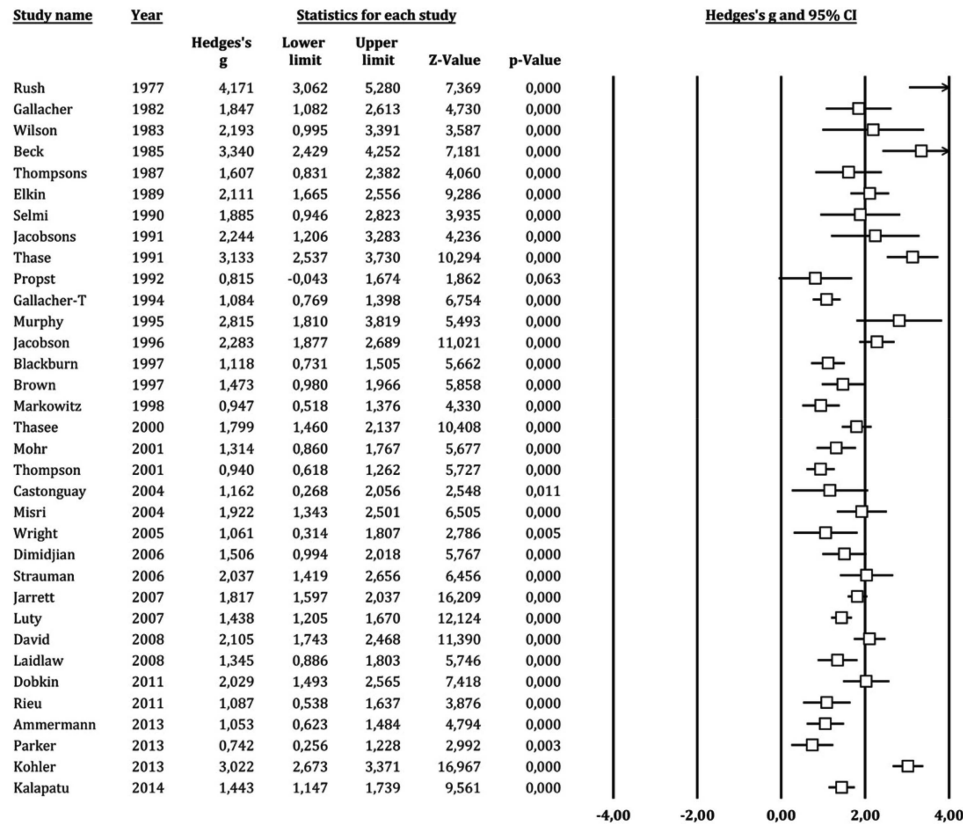| Study name	Year	Statistics for each study					Hedges's g and 95% CI
		Hedges's g	Lower limit	Upper limit	Z-Value	p-Value	
Rush	1977	4,171	3,062	5,280	7,369	0,000	
Gallacher	1982	1,847	1,082	2,613	4,730	0,000	
Wilson	1983	2,193	0,995	3,391	3,587	0,000	
Beck	1985	3,340	2,429	4,252	7,181	0,000	
Thompsons	1987	1,607	0,831	2,382	4,060	0,000	
Elkin	1989	2,111	1,665	2,556	9,286	0,000	
Selmi	1990	1,885	0,946	2,823	3,935	0,000	
Jacobsons	1991	2,244	1,206	3,283	4,236	0,000	
Thase	1991	3,133	2,537	3,730	10,294	0,000	
Propst	1992	0,815	-0,043	1,674	1,862	0,063	
Gallacher-T	1994	1,084	0,769	1,398	6,754	0,000	
Murphy	1995	2,815	1,810	3,819	5,493	0,000	
Jacobson	1996	2,283	1,877	2,689	11,021	0,000	
Blackburn	1997	1,118	0,731	1,505	5,662	0,000	
Brown	1997	1,473	0,980	1,966	5,858	0,000	
Markowitz	1998	0,947	0,518	1,376	4,330	0,000	
Thasee	2000	1,799	1,460	2,137	10,408	0,000	
Mohr	2001	1,314	0,860	1,767	5,677	0,000	
Thompson	2001	0,940	0,618	1,262	5,727	0,000	
Castonguay	2004	1,162	0,268	2,056	2,548	0,011	
Misri	2004	1,922	1,343	2,501	6,505	0,000	
Wright	2005	1,061	0,314	1,807	2,786	0,005	
Dimidjian	2006	1,506	0,994	2,018	5,767	0,000	
Strauman	2006	2,037	1,419	2,656	6,456	0,000	
Jarrett	2007	1,817	1,597	2,037	16,209	0,000	
Luty	2007	1,438	1,205	1,670	12,124	0,000	
David	2008	2,105	1,743	2,468	11,390	0,000	
Laidlaw	2008	1,345	0,886	1,803	5,746	0,000	
Dobkin	2011	2,029	1,493	2,565	7,418	0,000	
Rieu	2011	1,087	0,538	1,637	3,876	0,000	
Ammermann	2013	1,053	0,623	1,484	4,794	0,000	
Parker	2013	0,742	0,256	1,228	2,992	0,003	
Kohler	2013	3,022	2,673	3,371	16,967	0,000	
Kalapatu	2014	1,443	1,147	1,739	9,561	0,000	

Figure 3. Forest plot for the Hamilton Rating Scale of Depression effect sizes.

Study-related. The following interaction effects were not significant: The number of sessions ($beta_{time} = -.028$; $beta_{int} = .001$, $p = .37$), methodological quality ($beta_{time} = -.032$; $beta_{int} = .001$, $p = .60$), type of statistical (ITT vs. completers) analysis ($beta_{time} = -.029$; $beta_{int} = .009$, $p = .64$), and use of the Beck manual (no vs. yes; $beta_{time} = -.033$; $beta_{int} = -.023$, $p = .14$). Although the moderator, manual use, was not significant, it is interesting to note that studies using the Beck manual showed an even steeper decline than studies that did not use it. The difference in the predicted decline of ES across a 30-year period was $g = -.023 \times 30 = -.69$.

The final moderator, BDI-I versus BDI-II, was not significant ($beta_{time} = -.024$; $beta_{int} = .034$, $p = .33$). However, as the interaction coefficient was higher than, and inversely related to the time coefficient, this relationship was examined closer. A plot of the interaction (see Figure 10) indicated a significant decline in studies using the BDI-I measure, but not in studies using the BDI-II. The predicted treatment effect was equal for studies using the BDI-I and the BDI-II at about year 2006. Hence, the treatment effects that were observed when studies began employing the BDI-II started at about the same point in time as the effects of the BDI-I studies ended. The narrow range of publications for the studies using the BDI-II, however, restricted this comparison considerably. When the analyses were restricted to the years 1998–2014 (when the first study using the BDI-II was published), the interaction coefficient was not significant and slightly negative

Table 2

A Metaregression Analysis With Publication Year (or Time) as a Continuous Predictor of Effect Size

Studies (outcome)	K	b_0	b_1	95% CI	$Z (b_1)$	p Value
All studies (BDI)	67	60.17	−0.0293	[−0.044, −0.015]	−4.00	<.001
Within design	50	65.75	−0.0320	[−0.049, −0.015]	−3.70	<.001
CT design	17	51.96	−0.0253	[−0.050, −0.001]	−2.05	<.05
All studies (HRSD)	34	62.82	−0.0305	[−0.054, −0.007]	−2.53	.01
Within design	26	78.31	−0.0382	[−0.067, −0.009]	−2.61	<.01
CT design	8	22.37	−0.0105	[−0.042, 0.021]	−0.56	.51
Remission rate (%)	**42**	**54.43**	**−0.0271**	**[−0.047, −0.008]**	**−2.72**	**<.01**

Note. b_0 = intercept (year 0 A.D); b_1 = time slope (change coefficient); CI = confidence interval; BDI = Beck Depression Inventory; Within = the within-group condition; CT = controlled trials; HRSD = Hamilton Rating Scale of Depression.
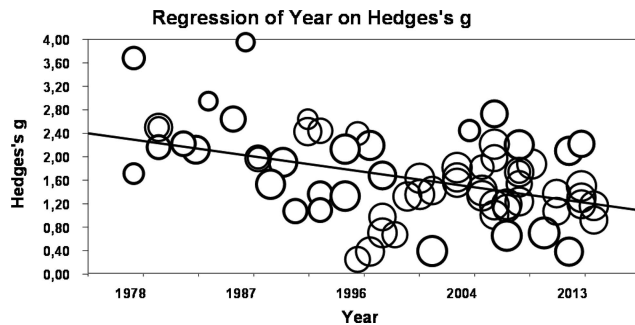
Figure 4. The plot portrays the negative change ($p < .001$) in Beck Depression Inventory effect sizes across time ($k = 61$). The size of the circles indicates the relative contribution (random weight) of each study to the analysis.

($\beta = -.015$), indicating a negative time trend that was more pronounced for the BDI-II compared with the BDI-I studies. When the analysis was restricted to 2006–2014, when use of the BDI-II really started, the negative interaction coefficient was stronger ($\beta = -.045$).

The competence of the therapists was rated in only five studies. Moreover, one of the studies (Jacobson et al., 1996) had a CTS score that deviated considerably from the remaining four with respect to time; hence, this precluded a moderator analysis due to the low number of studies and an outlier case.

Discussion

The Temporal Trends in Treatment Effects

The main objective of the present meta-analysis was to examine the temporal changes in the effects of CBT in treating unipolar DDs. Almost all of the studies utilized the BDI to quantify the treatment's effect, whereas a smaller number of studies used the HRSD by itself or in conjunction with the BDI. The main finding was that the treatment effect of CBT showed a declining trend across time and across both measures of depression (the BDI and the HRSD). Contemporary clinical treatment trials therefore, seem to be less effective than the therapies conducted decades ago.
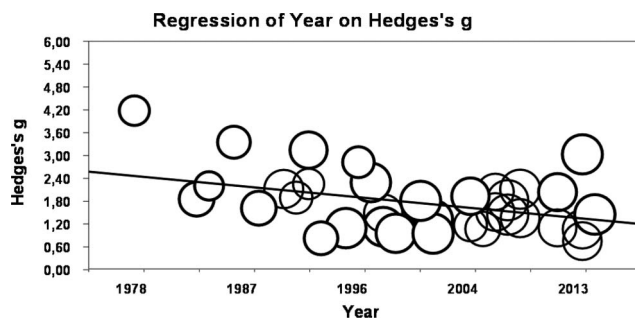
Figure 5. The plot portrays the negative Change ($p < .01$) in Hamilton Rating Scale of Depression effect sizes across time ($k = 34$). The size of the circles indicates the relative contribution (random weight) of each study to the analysis.
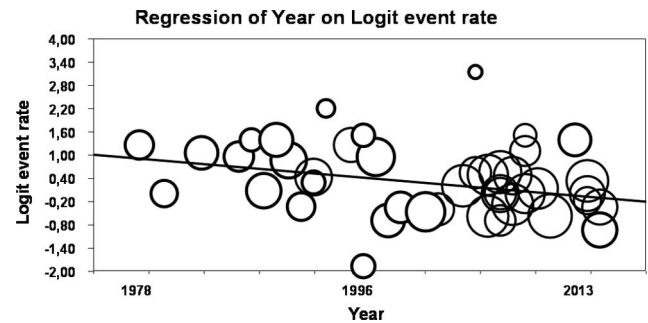
Figure 6. The plot portrays the negative change ($p = .03$) in the remission rates across time ($k = 42$). The size of the circles indicates the relative contribution (random weight) of each study to the analysis.

Moreover, most of the subgroup analyses supported this conclusion. The CBT studies employing different research designs (controlled trials vs. pre-posttest within-group study designs) showed similar declining trends. Studies based on the HRSD employing a CT design also showed a similar trend, however, the decline was not significant. The small number of studies in this group ($k = 8$) reduced the statistical power considerably. Additional subgroup analyses separating the study samples (clinical trials vs. field studies), or the number of patients in the studies (low vs. high), revealed a similar downward trend. Studies consisting of potential outliers (according to the plot diagram) were also taken into account; however, the outcome was the same.

Studies employing the BDI II did not reveal a comparable decline in treatment effects. However, as these studies were almost exclusively published after 2006, restricting the time range for the BDI-II studies considerably, this comparison is of limited value. Moreover, the treatment effects of the BDI-II studies started at approximately the same time as the BDI-I effects ended. Keeping in mind that the time trend was negative for all studies from 1998 onward (see Figure 7), and that the time trend differences from 1998 were minor for these two instruments (beta$_{int}$ = $-.015$), these findings raise no significant precautions. The timeframe of the studies using the BDI-I ranged from 1977 to 2010; hence, providing a more accurate picture of the timeframe in question. Moreover, the results of the HRSD and the remission rates for depression confirmed a significant decline in treatment effects.

The discovery of a weaker treatment effect over time cannot be explained based on a general temporal decline in patients' ability to recover from DDs, as patients on a waiting list improved in equal degrees across the entire time span. Nor can the effect be explained by lower preintervention BDI scores in the more recent studies. The correlation between the year of publication and BDI prescores was small, but positive. Two-way regression models between time (publication year) and the remaining moderators did not reveal any significant interactions, either, which would have indicated a different time trend, depending on the moderator. In summary, the declining effect of treatment over time seems robust.

Moderators of Treatment Effects

Client specific factors.   The age of the patients was not related to the treatment effects, nor did it moderate the decline in treatment effects. The role of age in treatment response has yielded mixed

Table 3

A Metaregression Analysis Examining the Association Between Continuous Moderators and Effect Sizes (BDI as the Outcome)

Moderator variable	K	b_0	b_1	95% CI	$Z (b_1)$	P Value
Time, years '95–'02 excl.	54	65.84	−0.0320	[−0.045, −0.020]	−5.00	<.001
Time, field studies excl.	56	69.56	−0.0340	[−0.050, −0.018]	−4.10	<.001
Time, low N studies excl.	37	47.76	−0.0231	[−0.043, −0.003]	−2.23	.02
Time, special patients excl.	54	64.34	−0.0314	[−0.048, −0.015]	−3.76	<.001
Time, special patients	13	47.38	−0.0230	[−0.049, 0.003]	−1.76	.08
Time, waiting list	16	−9.53	0.0050	[−0.009, 0.018]	0.71	.48
Sessions	67	1.46	0.0093	[−0.021, 0.040]	0.59	.56
Age	64	2.00	−0.0103	[−0.025, 0.004]	−1.39	.17
Gender (male %)	65	1.93	−0.0104	[−0.019, −0.001]	−2.32	.03
Medication (%)	41	1.53	−0.0070	[−0.006, 0.005]	−0.25	.81
Comorbidity (%)	25	1.69	−0.0027	[−0.012, 0.006]	−0.57	.56
Study quality (0–48)	67	1.84	−0.0085	[−0.031, 0.014]	−0.75	.45
Therapist competency (0–72)	**5**	**0.25**	**0.0253**	**[−0.026, 0.076]**	**0.97**	**.33**

Note. BDI = Beck Depression Inventory; b_0 = intercept (year 0 A.D.); b_1 = time slope (change coefficient); CI = confidence interval; Time = publication year; excl. = excluded.

findings in the clinical literature (e.g., Ammerman, Peugh, Putnam, & Van Ginkel, 2012; Lewis, Simons, & Kim, 2012), which the present analysis confirmed.

A significant gender difference was evident, indicating that women profited more from CBT for depression than did men. This was somewhat surprising, given that previous studies (Joutsenniemi et al., 2012; Wierzbicki & Pekarik, 1993) have indicated no sex differences with regard to who benefits the most from psychotherapy. We have no interpretation for this finding, but as women represent the majority of those being treated for depression, this difference means that overall, more patients improve following CBT. However, if the *p* value had been adjusted due to multiple significance testing, this difference would not have been significant.
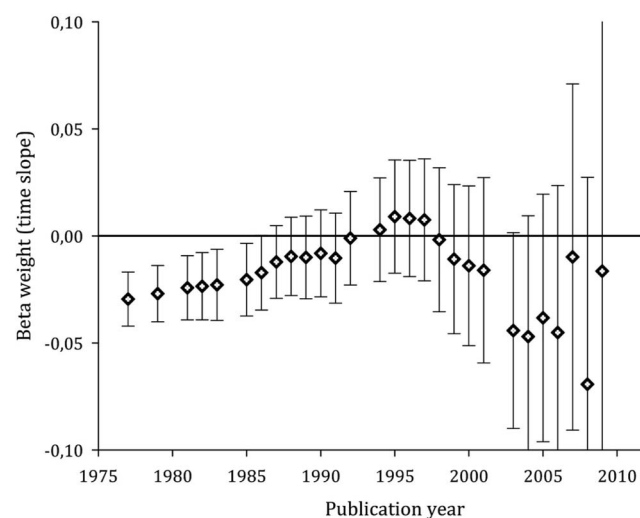
Figure 7. Temporal changes depending on the publication year start. Coefficients below 0 indicate a declining effect if estimated from the publication year as indicated on the *x* axis. The 95% error bars are increasing due to a lower number of available studies when advancing the publication year start.

The degree of comorbidity did not moderate the reported ESs, nor did it interact with time. One may thus, exclude the possibility that the declining effect of CBT is because recent studies have included patients with a higher degree of psychiatric comorbidity. An often-used strategy in clinical research is to implement new treatments on highly selected samples (comorbid conditions are excluded), that use highly trained or competent therapists who implement therapy according to a treatment manual. Such clinical trials are referred to as efficacy trials, whereas trials that are not as strict in these requirements are known as effectiveness trials. The latter include patients with varying degrees of comorbidity and/or therapist competence, which better reflect the reality of how mental health services are delivered. Therefore, one could expect that the more recent CBT trials had an overrepresentation of effectiveness trials than the previous ones. However, the situation seems to be going in the opposite direction, as the more recent studies included fewer patients with comorbidity. The declining trend in treatment effects over time was not moderated by therapist experience either. Hence, any strong objections against the present meta-analysis for not controlling for different types of implementations, efficacy versus effectiveness, seem less relevant.

The percentage of patients on stable dosages of psychotropic medication, including antidepressants, did not covary with ES. This finding is somewhat surprising, given that several studies and meta-analyses have indicated a higher treatment effect when psychotherapy was combined with antidepressants (e.g., de Maat et al., 2008; Keller et al., 2000; Pampallona et al., 2004). The meta-analysis of Cuijpers et al. (2009), comparing psychotherapy in general, with psychotherapy plus medication, and with seven trials of CBT and CBT plus medication, indicated a similar trend. The advantage of CBT plus medication was, however, small. One explanation for the lack of confirmatory findings here, may be that our study recorded a continuous percentage score of the number of patients on medication, and hence, it did not compare two distinctively defined patient groups (i.e., 100% pure CBT compared with 100% CBT+ medication), which other studies have done. This particular moderator analysis, therefore, may have been statistically underpowered. Another explanation may be related to the characteristics of the clinical samples, as most of the studies
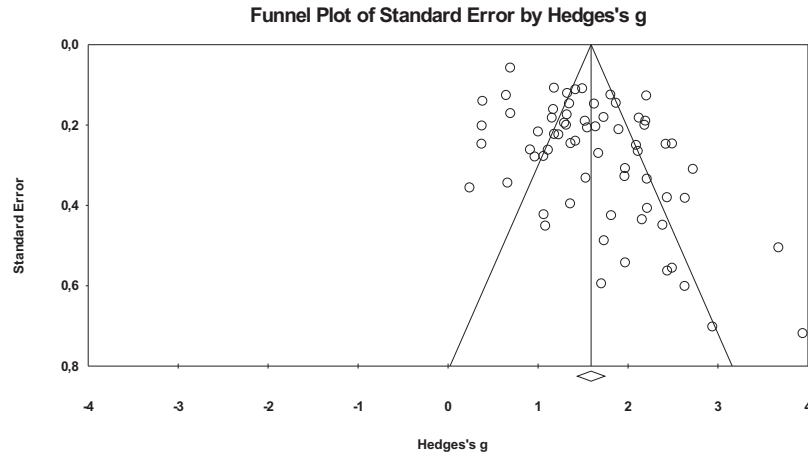
Figure 8. Funnel plot of the 67 included studies based on the Beck Depression Inventory.

sampled patients with a moderate degree of depression. It is conceivable that psychotherapy combined with medication has a higher treatment effect mainly for the severely depressed patients, as indicated by the American Psychiatric Association's guidelines for the treatment of depression (APA, 2010).

Although different diagnostic classifications of depression as mild, moderate, severe, or recurrent did not yield statistically significant effect differences (potentially due to the small number of studies), the differences were nevertheless meaningful. The highest treatment effects were seen in patients with recurrent depression. This result seems reasonable given that the diagnostic criteria for recurrent depression imply that remission is achieved between depressive episodes. These patients have a longer treatment history than those depressed for the first time; they know the rationale for CBT, and what to expect from therapy. They also are more acquainted with the methodological approaches, such as the importance of constructing a case conceptualization that the homework tasks are designed to test. These patients may also have more knowledge about how to find a skilled therapist, and thus, experience a stronger or quicker effect.

Although our study did not reveal any significant differences in ES related to samples with special characteristics, a tendency for a higher ES was found in ordinary patient populations ($g = 1.64$ vs. 1.35). This tendency is not surprising, given the fact that comorbidity, in general, is connected with poorer outcomes of therapy. However, the negative time trend was not affected by the inclusion of special patient samples. Rather, the trend was negative irrespective of the sample's patient characteristics (ordinary vs. special patient subpopulations). Restricting the time-trend analysis to the special patient group revealed a similar decline in treatment effects, albeit, not significant, probably due to the small number of studies.

Therapist-related factors. The competence of the therapist probably exerts more influence on how treatment works (Simons et al., 2010), which the present meta-analysis partly suggests: patients receiving CBT from experienced psychologists had a more pronounced reduction in depressive symptoms compared with patients receiving CBT from psychology students, with less experience doing therapy. The difference represented half of a standard deviation, which is considered a moderate effect size
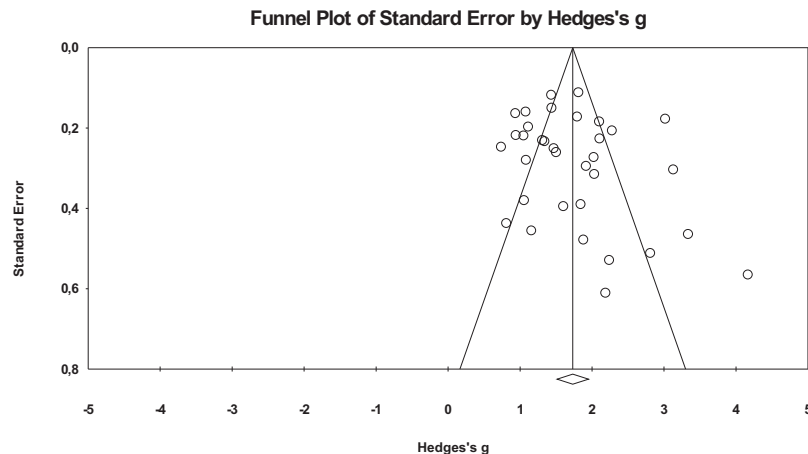
Figure 9. Funnel plot of the 34 included studies based on the Hamilton Rating Scale of Depression.

Table 4

A Subgroup Analysis of Dichotomous Variables and Effect Size Based on the BDI

Moderator	k	g	95% CI	Q_{df}	p Value	I^2
Diagnostic severity				3.10_3	.38	.89
Mild	9	1.28	[0.84, 1.71]			.90
Moderate	40	1.62	[1.42, 1.82]			.88
Severe	10	1.56	[1.18, 1.97]			.88
Recurrent	8	1.86	[1.33, 2.39]			.92
Data analysis				1.89_1	.17	.89
ITT	18	1.43	[1.18, 1.69]			.89
Completers	49	1.66	[1.46, 1.85]			.89
Beck manual				0.02_1	.89	.89
Yes	38	1.60	[1.39, 1.81]			.89
No	29	1.58	[1.36, 1.80]			.89
Adherence check				0.02_1	.89	.90
Yes	32	1.56	[1.35, 1.78]			.87
No	30	1.54	[1.32, 1.77]			.91
Patient type				2.54_1	.11	.89
Ordinary	54	1.64	[1.47, 1.81]			.90
Special	13	1.35	[1.03, 1.67]			.72
Therapist*				7.14_1	<.01	.85
Trained student	7	0.98	[0.59, 1.36]			.65
Psychologist	**37**	**1.55**	**[1.38, 1.72]**			**.83**

Note. BDI = Beck Depression Inventory; CI = confidence interval; Q_{df} = Q value for the between group difference(s); df = associated degrees of freedom; I^2 = I-squared indicates the degree of between study variance relative to total variance; ITT = intention to treat.
* Psychiatrist was not included due to few studies (k = 2). The remaining studies (k = 21) used a combination of therapists, or type of therapist was not reported. These studies were excluded from this analysis.

difference in statistical terms. Such differences may be of clinical concern as half a standard deviation on the BDI instrument typically represents a 5-point decrease in the raw score (Dworkin et al., 2008). As most CBT studies have been conducted with patients with moderate degrees of depression (BDI scores ranging between 20 and 29, and an expected mean of 25), about one third of the patients would thus, be expected to shift from the moderate to the mild diagnostic category. This represents a non-negligible difference that needs to be taken into account when assigning patients to available therapists in a clinic. The most competent therapist should be assigned to the most depressed patients. It is, of course, important that students are trained to conduct CBT, but student therapy should be offered to patients with primarily mild, or at the maximum, a moderate degree of depression.

In addition, there was a tendency (albeit a tentative one) indicating that therapist competence, as measured by the CTS, implied better treatment effects. However, this relationship was not significant. As the present meta-analysis only identified five studies reporting sufficient data, the statistical power and the possibility of generalization from these studies were low. Nevertheless, the direction of the effect concurred with the common finding that therapists who are more competent help their patients achieve remission more quickly (Stein & Lambert, 1995; Strunk, Brotman, DeRubeis, & Hollon, 2010). Yet again, variation in competence was unrelated to the reported time trend.

Specific treatment or study quality related factors. The number of therapy sessions did not reveal different treatment effects following CBT. A caveat should be noted as most of the studies consisted of interventions consisting of between 10 and 20

sessions of psychotherapy, hence, precluding any conclusions regarding further improvement (or deterioration) beyond 20 sessions of therapy. This hardly represents a limitation of the analyses, as CBT for depression is designed as a short-term therapy. The weighted mean number of therapy sessions was 14.8 (SD = 5.2). Because we did not find support for an inverse U-shaped relationship between treatment effects and number of sessions, length of therapy seems to be less important for efficacy.

We did not find evidence of significant differences in the treatment effects resulting from the use of the Beck manual (Beck et al., 1979). Contrary to expectations, the interaction analyses showed a slightly steeper decline for the CBT trials that used the manual compared to those that did not. This finding was rather surprising given that the original manual had a reputation among clinical researchers as one of the best ways to implement CBT. We cannot conceive of any sensible explanation for why clinical studies using the Beck manual fare relatively worse than those not using it. To the best of our knowledge, there have been no thorough investigations of how different ways of conducting CBT for depression may influence the outcome. Our findings indicate that further investigations regarding this matter are warranted.

This study revealed no differences in ES related to the utilization of adherence checks. This finding is at odds with the perceived importance of adhering to a treatment manual (Crits-Christoph et al., 1991; Shafran et al., 2009). One explanation may be that most therapists in the included studies were well-trained or experienced psychologists, and thus, likely to conduct CBT in a proper fashion even without checks or feedback regarding adherence to the manual. Another possibility is that adherence checks were not reported consistently.

The methodological quality of the studies was rated with the RCT-PQRS published by Kocsis et al. (2010). It is a comprehensive measure of the methodological quality of clinical trials (Gerber et al., 2011). Many of the items are derived from preexisting measures of the quality of randomized controlled trials. An advantage of the PCT-PQRS is that it was developed to fit different
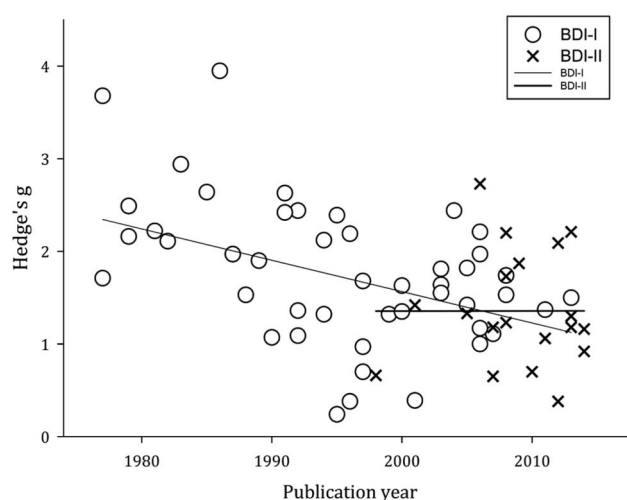

Figure 10. A plot of the interaction between publication year and type of Beck Depression Inventory (BDI) measure used.

therapy traditions (e.g., CBT, psychodynamic therapy, or pharmacology). The quality ratings have improved considerably over the years; newer studies have received much higher quality ratings than the older ones. Although the quality ratings were not significantly related with the ESs, the relationship was, nevertheless, in the expected direction, as higher quality studies yielded slightly lower therapy effects than the lower quality studies. We also observed lower effect sizes in CBT studies using CT versus within-group research designs, although they were, yet again, nonsignificant. As both methodological quality indicators pointed in the same direction, the present findings are in line with previous meta-analyses (e.g., Gould, Coulson, & Howard, 2012; Pallesen et al., 2005).

The present analysis did not reveal a significant difference in the ES between the statistical designs completers versus ITT. We did, however, replicate the tendency observed in Hans and Hiller's (2013) meta-analysis, and found a slightly larger ES for completers ($g = 1.66$) versus ITT ($g = 1.43$). This rather modest difference probably is due to the larger ratio of the early drop-outs from the ITT design, thus, preventing these patients from benefitting from all of the components of the CBT intervention.

As the number of studies reporting data related to common factors, such as the patient–therapist alliance, was extremely low, no conclusions about common factors could be drawn.

Potential Reasons for a Decline in Therapy Effect

The original manual for how to deliver and implement CBT was developed in the 1970s, and subsequently, served as the gold standard for many practitioners of psychotherapy. The reason for the declining effect is hard to explain beyond the fact that CBT for depression has not led to systematic improvements.

It is possible that the ostensibly simple treatment objective of CBT (i.e., changing maladaptive cognitions to alleviate emotional disorders), has made it particularly attractive and has created a misconception of being easy to learn. However, proper training, considerable practice, and competent supervision are very important to provide CBT in an efficacious manner. Thus, clinical researchers have warned against deviating from the evidence-based therapeutic interventions (Shafran et al., 2009), as therapists who frequently depart from the manual demonstrate poorer treatment effects than therapists who follow the manual (Luborsky et al., 1997, 1985). The lack of a stronger treatment effect among studies employing the Beck manual in the present meta-analysis does not invalidate this recommendation, as the studies that did not explicitly state that the manual was used may still have used skilled therapists that properly implemented CBT.

Another possibility is that the degree of experience or therapeutic competence may affect treatment outcomes differently, depending on whether a CBT manual is followed or not (Crits-Christoph et al., 1991). This interaction was not possible to address in our analysis. From a CBT point of view, it may be realistic to expect that the original founders of the therapy may have been more concerned with therapy fidelity (strong adherence to the manual) and with acquiring a large amount of experience with the method before examining it in a randomized clinical trial. There has been a tendency to publish clinical trials based on CBT without properly describing the contents of the treatment given,

which may indicate less concern with adherence to the manual. Although this is a possibility, the interaction effect would need to be quite strong for the declining slope to be nonsignificant, and even stronger to shift the slope to a positive direction, which is highly unlikely.

Standardization of the data collected from clinical trials may be helpful for future reviews of CBT, in order to avoid missing important moderator data, and be able to conduct more nuanced analyses in the future. Future trials should include measures of the therapeutic alliance and therapist competence, as well as an adequate description of what was done during the therapy sessions, and how it was done and when it was done. A minimum set of data related to client factors, therapist factors, as well as common and specific factors should be collected.

An interesting confounder related to the common factors should be mentioned: the placebo effect. The placebo effect is typically stronger for newer treatments, however, as time passes and experience with therapy is gained, the strong initial expectations wane. One may question whether this is the case with CBT. In the initial phase of the cognitive era, CBT was frequently portrayed as the gold standard for the treatment of many disorders. In recent times, however, an increasing number of studies (e.g., Baardseth et al., 2013; Wampold et al., 2002, 1997) have not found this method to be superior to other techniques. Coupled with the increasing availability of such information to the public, including the Internet, it is not inconceivable that patients' hope and faith in the efficacy of CBT has decreased somewhat, in recent decades. Moreover, whether widespread knowledge of the present meta-analysis results might worsen the situation, remains an open question.

If technical factors represent 10%–20% of the total treatment effect, it seems reasonable to suggest that newer psychotherapy approaches should diligently address improvements in the common factors to realize larger treatment effects. In this respect, it seems strange that CBT apparently reached a ceiling effect during its first few years.

Limitations

The present meta-analysis is not without limitations. First, this study only included depression, thus, excluding CBT trials aimed at treating other diagnosis, such as anxiety, posttraumatic stress, eating, schizophrenia, and sleep disorders. There is no reason to expect the present findings to generalize to these disorders. In particular, anxiety disorders, which include a heterogeneous group of disorders that probably yield different time trends, have been subjected to the CBT approach. The clinical presentations of, for example, panic, obsessive–compulsive, and posttraumatic stress disorders are very different, as are the CBT approaches used. A meta-analysis of five trials comparing cognitive therapy with exposure therapy to treat obsessive–compulsive disorder (Ougrin, 2011) did not indicate a decline for the newer trials. Another review examining the efficacy of 12 trials examining transdiagnostic CBT in treating common anxiety disorders, such as obsessive–compulsive, generalized, and social anxiety disorder (Reinholt & Krogh, 2014), indicated no temporal changes either. A study by Hofmann and Smits (2008), that we will finally mention, examined the efficacy of 25 clinical trials on the use of CBT for the treatment of anxiety disorders even showed a minor positive

temporal change. These examples indicate that a comprehensive meta-analysis covering other mental health disorders may yield quite different results.

The BDI has undergone some modifications during its 40-year existence. The original BDI was revised and made more user friendly in 1988, and given the acronym, BDI-Ia (Beck et al., 1988). The latest version, the BDI-II, has incorporated an item measuring hypochondriasis, changed the timeframe of symptoms from 1 week to 2 weeks, and put more emphasis on measuring all diagnostic criteria related to depression. Still, the forms are very similar to each other (Beck et al., 1996). Despite these differences, the treatment and control groups responded to the equivalent forms at any point in time. Thus, these considerations should not pose major threats to the validity of the current conclusions.

Very few studies ($k = 5$) included correlations between the BDI pre- and postintervention scores, requiring us to impute this value for the remaining 65 studies. However, the potential for this value to exert undue influence on the results does seem small for two reasons. First, the variations in correlations need to be quite high in order to change the ESs substantially. Second, and most importantly, we have no reason to expect that the prepost BDI correlations should change considerably over time. Although a shift in therapy effect over the years changed the mean of the post intervention BDI scores, the relative position between the pre- and postscores should not have changed by much.

Recovery rates were calculated according to somewhat varying criteria across the studies included in this analysis. The most stringent criterion was a cut-off score for clinical depression of 7 on the BDI, while the most liberal was 10. Although this difference might not seem substantial, it could have a confounding effect on the calculated total percentage of recovered patients, and the correlation between recovery rates and year of intervention.

A minor possible caveat relates to the time moderator. As all of the studies' years were coded based on their publication dates, it is conceivable that this date could vary somewhat from the actual year of the intervention. However, it is reasonable to assume that this discrepancy is similar to contemporary and older studies, and that the difference between the publication and actual year of intervention is not very large.

Implications

The practical significance of this study is to heighten the awareness among practitioners and clinical researchers of the trends in modern psychotherapy. If the psychotherapy of today has a lower efficacy than that conducted 30 to 40 years ago, this threatens the validity of current comparative studies. If we compare the efficacy of a new psychotherapeutic approach with the current best standard, which, for example, may be CBT, we risk concluding that the newer approach is preferable even though it may have a weaker effect than the seminal CBT trials of the 1970s. Researchers conducting randomized placebo-controlled trials today, thus, risk keeping newer treatment approaches that are relatively better than the current best CBT. Yet, what is the benefit of doing so if the absolute change is minor or even negative compared to the seminal studies?

The fact that individual cognitive therapy demonstrates a declining temporal trend implies, however, that the possibility of significant improvement exists. Treatment outcomes may be improved, not only through technical variations or new additions, but also by considering better ways of integrating common, therapist, and patient-related factors. Further research and randomized trials that include measures of the four major variance components underpinning the therapy's effects are recommended to determine the formula behind the optimal practice of CBT. All future clinical trials should be conducted according to a common standard that prescribes which information should be collected, at a minimum, in all psychotherapy studies.

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