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RESEARCH ARTICLE

The efficacy of individual humanistic-experiential therapies for the treatment of depression: A systematic review and meta-analysis of randomized controlled trials

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ABSTRACT

Objective: Conduct a systematic review and meta-analysis of randomized controlled trials (RCTs) evaluating the efficacy of individual humanistic-experiential therapies (HEPs) for depression.
Method: Database searches (Scopus, Medline, and PsycINFO) identified RCTs comparing any HEP intervention with a treatment-as-usual (TAU) control or active alternative intervention for the treatment of depression. Included studies were assessed using the Risk of Bias 2 tool and narratively synthesized. Post-treatment and follow-up effect sizes were aggregated using random-effects meta-analysis and moderators of treatment effect were explored (PROSPERO: CRD42021240485).
Results: Seventeen RCTs, synthesized across four meta-analyzes, indicated HEP depression outcomes were significantly better than TAU controls at post-treatment ($g = 0.41$, 95% CI [0.18, 0.65], $n = 735$), but not significantly different at follow-up ($g = 0.14$, 95% CI [−0.30, 0.58], $n = 631$). HEP depression outcomes were comparable to active treatments at post-treatment ($g = -0.09$, 95% CI [−0.26, 0.08], $n = 2131$), but significantly favored non-HEP alternative interventions at follow-up ($g = -0.21$, 95% CI [−0.35, −0.07], $n = 1196$).
Conclusion: Relative to usual care, HEPs are effective in the short-term and comparable to non-HEP alternative interventions at post-treatment, but not at follow-up. However, imprecision, inconsistency, and risk of bias concerns were identified as limitations of the evidence included. Future large-scale trials of HEPs with equipoise between comparator conditions are required.

Keywords: humanistic-experiential therapies; depression; randomized controlled trial; systematic review; meta-analysis; process-guiding

Clinical or methodological significance of this article: This meta-analytic review of the efficacy of humanistic-experiential therapies (HEPs) in randomized controlled trials found that for the treatment of depression, HEPs have benefits over usual care and are comparable to active treatment at post-treatment, but not in the long-term. Findings support HEPs as an additional treatment choice for patients. Future research should conduct high quality, large randomized controlled trials in HEPs and enhance the longer-term impact of this form of therapy.

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Depression is a major public health concern globally (Liu et al., 2020) and a considerable body of evidence exists regarding the current range of psychological therapies for the treatment of depression (see Barkham & Lambert, 2021). A large meta-analysis comparing 15 psychological therapies for depression with discrete control conditions (i.e., wait list, treatment as usual, and placebo) found all to be effective, as indicated by an overall Hedges' effect size (ES) of $g = 0.72$ (95% CIs [0.67, 0.78]; Cuijpers et al., 2020). However, the meta-analysis also showed non-directive counseling to yield numerically less favorable outcomes than other therapies. A similar finding was obtained in a larger meta-analysis where non-directive supportive counseling was less effective than other therapies, although not when considering only studies rated as low risk of bias (Cuijpers et al., 2021). A reason why recent meta-analytic studies have found non-directive supportive counseling to be less effective than other therapies has been the relatively poor definitions of this form of therapy whereby trials have often used counseling as a control condition in an investigation of a preferred candidate treatment (e.g., Koszycki et al., 2012). This argument is supported by Cuijpers et al. (2012). In such trials, the quality of the counseling intervention is likely compromised and the position of equipoise between the comparative treatments in the trial debatable.

However, non-directive supportive counseling is one form of therapy within the super-ordinate label of *humanistic-experiential psychotherapies (HEPs)*, an umbrella term encompassing differing active therapy formats specifically focusing on depression (e.g., non-directive counseling or person-centered therapy, supportive counseling, process-experiential, emotion focused therapy; Saunders, 2012). These therapies place an emphasis on (1) the therapeutic relationship, which is viewed as empathic and potentially curative; (2) promoting client experiencing (and hence, emotions) in therapy, and (3) are fundamentally person-centered, having a holistic view of each client (Elliott et al., 2021, p. 422). Given such a coherent underpinning to this *class* of psychological therapies, we sought to conduct a systematic review and meta-analysis focusing on HEPs and specifically focusing on depression.

In a wide-ranging review of the HEP literature, and in contrast to the targeted approach focusing on a single therapy brand, Elliott et al. (2021) reported a comparative ES for depression of −.19 (95% CI [−.30, −.07]) favoring alternate treatments. The review also showed a less favorable outcome for HEPs compared with an earlier ES of −.02 (Elliott et al., 2013). Moreover, Elliott et al. (2021) reported a main effect for client centered and supportive psychotherapy in the treatment of depression, indicating potentially differential levels of effectiveness between brand names within HEP types, although they were all less effective than comparator treatments. Indeed, a feature of the reviews by Elliott and colleagues has been the adoption of an all-inclusive approach to incorporating, for example, group and individual therapy, medical settings, and both observational and trial designs as well as focusing on a range of presenting conditions (e.g., depression, interpersonal problems). However, these reviews did not report data on assessments of the risk of bias.

The current systematic review and meta-analysis targeted the efficacy of individual therapy, as determined by randomized controlled trials, including assessments of bias, identified within the super-ordinate label of HEPs in the treatment of depression where it was identified as the *primary* presenting condition within the trial. Trials that targeted depression related to the onset of other identified conditions, psychological or physical, were not the focus of this review. Such a restriction did not preclude, for example, studies reporting on comorbid conditions (e.g., anxiety), but the interventions were directed to the treatment of depression as the target condition. We considered named brands and major hallmarks of treatments for depression as identified in recent summaries of the humanistic-experiential literature (e.g., Elliott et al., 2021; Lambert et al., 2016; Pascual-Leone et al., 2016) that included the centrality of the relationship between therapist and patient as a vehicle for change, the core role of therapist empathy, and a focus on depression.

Notwithstanding their shared hallmarks, one factor in which HEPs may differ from each other is on the spectrum of *process guiding* (Elliott et al., 2021). This feature captures the extent to which therapists utilize principles and strategies that make them more active (e.g., process experiential; emotion focused) as opposed to more traditional non-directive formats (e.g., non-directive; person-centered). Adopting a dichotomous approach as to whether a form of HEP was lower or higher on process guiding, Elliott reported a non-significant ES of 0.18 (95% CI [−.12, .48]) favoring therapies utilizing greater process-guiding based on a sample of six diverse studies (i.e., not restricted to RCT designs or to depression). The present study incorporated a research focus on the spectrum of process guiding across HEP therapies.

Irrespective of the type or format of intervention, determining the efficacy of therapy immediately at post-treatment has been a cornerstone of outcome research. However, it is as important to determine whether any such effects are maintained at longer-term follow-up. Previous research has reported that, in the 12 months following receipt of HEPs,

participants tend to maintain gains made at post-treatment (Elliott et al., 2021). Meta-analyzes by Cuijpers and colleagues show that at both short-term follow-up (i.e., 3–6 months) and longer-term follow-up (i.e., 9–12 months), the effect of non-directive supportive therapy (NDST) versus care-as-usual controls on depression outcomes are typically small or very small, non-significant, and in favor of NDST (Cuijpers et al., 2012, 2021). When comparing NDST to other active psychotherapies or CBT specifically, again the effects are very small but fall in the direction of favoring the active psychotherapies or CBT rather than NDST (Cuijpers et al., 2012, 2021). The present review evaluated outcomes at both post-treatment and also follow-up.

Regardless of the timing of assessment for the active treatment, equal consideration needs to be given to the specificity of the control condition as it has been shown that the effect of treatment is heavily influenced by the type of control condition, with reduced effects being reported when TAU was used as compared with a wait-list condition (Barkham & Lambert, 2021). Indeed, the effect size for psychological therapies appears to reduce from approximately 0.8 to 0.5 when using TAU as compared with wait-list controls (Cuijpers et al., 2020). The crucial point is that different control conditions yield different treatment effects. Hence, the present review grouped any control conditions as discrete clusters for comparative purposes rather than pooling all control conditions into a single comparison group.

In sum, to complement the long tradition of accounts in successive editions of *Bergin and Garfield's Handbook of Psychotherapy and Behavior Change* (see Elliott et al., 2004, 2013, 2021; Greenberg et al., 1994), but to align with attention on trials methodology, the present study comprised a focused, systematic review and meta-analysis of individual HEPs derived from RCTs and targeting depression as the primary presenting condition in the absence of other psychological or physical presenting conditions as a primary focus of treatment. Four questions were addressed: (1) are individual HEPs superior to controls?; (2) are individual HEPs less effective than alternative active treatments?; (3) are the effects of HEPs maintained at follow-up in relation to both controls and other active treatments?; and (4) do types of HEPs yield differential outcomes favoring more process-guiding treatments?

Method

This review was written in accordance with the 27-item PRISMA 2020 checklist (See Supplemental Material).

Protocol and Registration

The review protocol was registered prospectively in the PROSPERO database on the 4th March 2021 and subsequently amended to include control condition comparators. The update was published on 20th April 2021; See https://www.crd.york.ac.uk/PROSPERO/display_record.php?RecordID = 240485.

Search Strategy and Study Selection

Three electronic databases—Scopus, Medline, and PsycINFO—were searched using terms adapted from those used by Elliott et al. (2021) on the basis of titles, abstracts, and keywords on March 4th, 2021 (see Supplemental Material; Appendix A) according to prespecified inclusion and exclusion criteria (see Table I). The search was re-run on 20th November 2021 as an update and the PRISMA diagram (Figure 1) was revised accordingly. The search was restricted to peer reviewed articles published in the English language with no date limitation applied and did not include gray literature. The criteria used to determine whether the treatment or intervention of interest met the threshold of being humanistic-experiential in nature required one of the following two elements to be explicitly documented in an article: (1) that the therapy either had to refer to or draw directly on the work of Carl Rogers (e.g., 1951); or (2) make explicit reference to factors of therapy indicative of a common factors approach (e.g., empathy, listening, relationship, etc.), which are viewed as central components to all HEPs (Elliott et al., 2021). Once identified, all records were imported into Endnote to remove duplicates and the revised references and abstracts were uploaded to Rayyan (Ouzzani et al., 2016).

An initial pool of 8779 records were identified from which 402 duplicates were removed, yielding 8377 records to be classified via a two-stage screening process. Stage 1 comprised independent screening of titles and abstracts by the lead author and 8277 records were excluded. The remaining 100 articles were accessed and reviewed according to the inclusion and exclusion criteria. A total of 83 full-text articles were excluded and the final process yielded 17 records available for narrative review and potential meta-analysis. Two psychology students external to the review team conducted an independent audit of a random 50% sample of full-text articles (25 articles each), indicating 90% agreement with the primary screening decisions overall (88% for rater 1 and 92% for rater 2). Figure 1 presents a PRISMA diagram summarizing the study selection process (see Supplemental Material Appendix B for full list of excluded studies with reasons). All

Table I. Inclusion and exclusion criteria for included studies.

	Review question	
	How effective are humanistic-experiential therapies for depression in the short-term, and how durable are the effects after the end of treatment?	
	Inclusion criteria	Exclusion criteria
Population	Adult patients (18 and above years of age) accessing humanistic-experiential therapies for depression (e.g., supportive counseling, person-centered counseling, focused expressive therapy) Patients meeting criteria for depression on any recognized diagnostic procedure or a pre-specified threshold on a depression measure.	Studies containing children and/or adolescents under 18 years. Studies focussed on treatment resistant depression and bipolar depression.
Intervention	Any humanistic-experiential therapy intervention with the objective of treating depression as the primary presenting problem. Individual modality only.	Studies that do not contain humanistic-experiential therapies for depression or where the primary presenting condition was not depression (e.g., depression arising from medical conditions).
Comparator	Any active intervention aimed at treating depression as the primary presenting condition which is used as a comparator against humanistic-experiential therapy inclusive of other psychological therapies (e.g., CBT). Control comparators e.g., waitlist or care-as-usual.	Studies that contain psychopharmacology interventions as the only comparator to humanistic-experiential therapy.
Outcomes	Between groups post-treatment depression outcomes and, where reported, follow-up data of any duration. Studies must report a clinical measure of depression either self-report or clinician/observer rated.	Studies which do not measure or report depression outcomes.
Setting	Any routine therapy setting where humanistic-experiential therapies are delivered, inclusive of in-person, telephone, and online formats.	
Study design	Randomized controlled trials.	Gray literature and articles published in non-English language.

corresponding authors of the identified articles were contacted with a list of included studies asking whether any further eligible studies had been conducted. Four authors responded but yielded no additional studies.

Data Extraction

Data were extracted independently by the lead author using a standardized form with pre-defined criteria as set out by the protocol. A subset of 25% of ES data extraction was independently checked and verified by a second reviewer. The criteria according to which data were extracted comprised: participant demographics (mean age, gender [% female], total randomized *N*, primary disorder); intervention and comparator details (specifying intervention and comparator conditions employed); design features (i.e., type of randomized control trial [RCT] design, study setting, depression outcome measures); and outcomes (post-treatment means and standard deviations [SDs] for TAU controls and other active treatments at the end of treatment and follow-up).

Quality and Risk of Bias Assessment

Quality and risk of bias were determined by the Risk of Bias 2 (RoB2; Sterne et al., 2019) tool yielding ratings of *low*, of *some concern*, or *high* risk of bias for five components: (1) randomization process, (2) deviations from intended interventions, (3) missing outcome data, (4) measurement of the outcome, and (5) selection of the reported results. Ratings of the 17 articles were carried out independently by KEMD and RH such that all articles were doubled rated. Agreement was determined by calculating kappa statistic. Disagreements were resolved by discussion between the two raters.

Data Synthesis and Analysis

As well as providing a narrative account of the reviewed studies, a random effects meta-analysis was conducted using a DerSimonian and Laird estimator (1986). This was achieved using Meta-Essentials workbooks (Suurmond et al., 2017) and plots were produced using the *metafor* and *forestplot* R packages (Viechtbauer, 2010). Two treatment-based comparisons were made: first, comparing

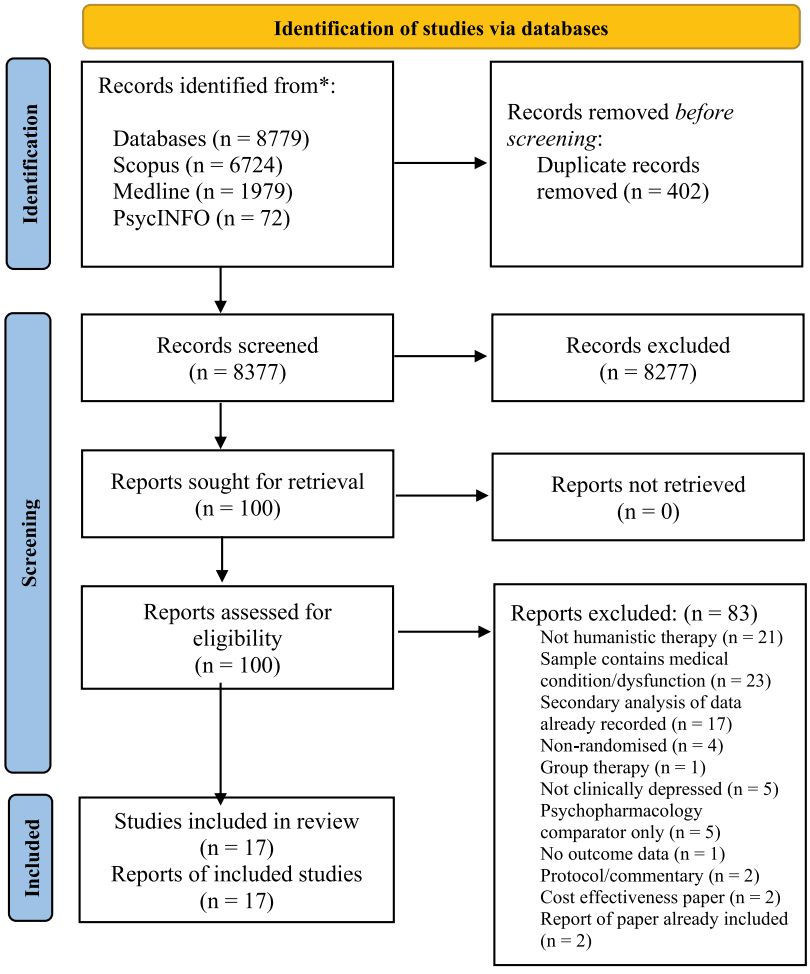


Figure 1. PRISMA flow diagram of the systematic study selection.

HEPs with TAU controls; and secondly, with other active treatments. Within each of these comparisons, two time points were assessed: (1) post-treatment depression scores and, if sufficient data was recorded, (2) follow-up depression scores. Studies that reported the required statistical information to calculate effect sizes (ES) were included. For studies that reported both clinician-rated and self-report measures of depression, priority was given to self-report measures for reasons of consistency. In cases where studies used multiple self-report measures of depression, the primary outcome measure was taken. Given all data were continuous, in the first instance standardized mean difference ESs (Hedges' *g*) were calculated utilizing post-treatment (and if reported, follow-up) means, SDs, and sample sizes for both HEPs and comparator groups. If these data were unreported, Hedges' *g* was calculated alternatively by using *t* values or *z* scores from Mann–Whitney U-test. Due to differences in the study samples regarding the time point at which post-treatment outcome measures, and follow-up outcomes, were assessed, selection for the post-treatment meta-analysis was based on the earliest possible time-point measurement at end of treatment, while for follow-up studies where multiple time-points were reported, the first follow-up assessment was used. This was due to greater collective homogeneity within the first follow-up timepoints (range = 6–18 months) as opposed to the last time-points (range = 6–60 months).

Individual study ESs were aggregated to produce an overall pooled ES and 95% confidence intervals were calculated and visualized in forest plots. Positive Hedges' *g* ESs indicated effects favoring HEPs interventions over the comparator, whereas negative Hedges' *g* ESs indicated effects favoring the comparator intervention over HEPs. ESs were interpreted according to the rubric of 0.2, 0.5 and 0.8 indicating small, medium and large ES, respectively (Cohen, 1992). To aid interpretation of meaningful treatment differences, the equivalent absolute percentage differences in HEP vs. comparator treatment effects are reported. A positive percentage represents

an increased probability of success if receiving HEP and a negative percentage represents a decreased probability of success if receiving HEP.

Heterogeneity was assessed using the Q, I^2 and Tau (τ) statistics. The I^2 statistic was used to indicate the percentage of overall variability attributable to between-study heterogeneity, categorized into low (25%), moderate (50%), and substantial (75%) groupings according to guidelines reported by Higgins et al. (2003). Tau was reported to provide a robust estimate of the variance in true effect sizes (*SD*), which is not susceptible to influences from number and precision of included studies (as can be the case for Q and I^2).

Publication bias. The likelihood of publication bias was evaluated using four methods to prevent overreliance on one approach. These comprised the following: (1) Funnel plots of standardized mean differences plotted against standard errors which were visually observed to detect possible asymmetry; (2) Duval and Tweedie's (2000) Trim and Fill imputation was used to predict the adjusted combined ES taking account of publication bias; (3) Egger's regression was utilized as a formal statistical assessment of potential publication bias by regressing standardized effect estimates onto a measure of precision (Egger et al., 1997); and (4) Rosenthal fail-safe N calculation which estimates the number of additional studies with an ES of zero required to turn the overall effect insignificant (Rosenthal, 1991).

GRADE Analysis

In the meta-analysis, the quality of the evidence was assessed for each comparison using the Grading of Recommendations Assessment, Development, and Evaluation (GRADE) tool (Dijkers, 2013). Meta-analytic comparisons were graded by four of the authors to reach a consensus quality rating (high, moderate, low, or very low quality) based on five domains; (1) risk of bias in the individual included studies, (2) inconsistency, (3) indirectness of treatment estimate effects, (4) imprecision, and (5) publication bias.

Moderator and Sensitivity Analyzes

Heterogeneity between studies was inspected using moderator analyzes. Moderator variables were specified a priori and pre-registered in the review protocol. However, two were investigated post-hoc to evaluate the impact of researcher allegiance and HEP comparator status. Six variables were evaluated in the subgroup analysis: depression type (depression; postpartum/postnatal depression), risk of bias (low; some concerns), depression measure (Beck Depression Inventory-II; Edinburgh Postnatal Depression Scale; Patient Heath Questionnaire-9; Other), HEP type (Brief Supportive Psychotherapy; Experiential; Person-centred), active comparator type (CBT; CBASP; Other), and HEP comparator status (specified control condition; candidate treatment). HEP treatments were deemed to be the control condition when they were used as a non-specific control comparator to which a candidate intervention was tested against and hypothesized to be more beneficial than the HEP treatment. In addition, therapy types were categorized into three groups according to the extent to which process guiding was viewed as a central component of the model: brief supportive psychotherapy, person centered, and experiential component (see Supplemental Materials, Appendix J for categorization of studies). Three moderator variables were explored in the meta-regressions: gender (% female), mean age, and researcher allegiance (determined by a reprint method; Gaffan et al., 1995). The reprint method comprises rating on a scale with anchor points of 0 (no allegiance) to 3 (strong allegiance) on the basis of study characteristics as reported by the author from the introduction to the article. The reprint method used the differential score between the absolute allegiance rating for each arm of the trial. A minimum of 10 studies were required to investigate moderators of ES in each meta-analytic comparison (Cochrane Collaboration, 2011). Due to multiple testing, the alpha level for significance was adjusted to $p < .01$ (alpha = .05/5) for subgroup analyzes and $p < .017$ (alpha = .05/3) for meta-regressions.

A post-hoc sensitivity analysis was conducted on post-treatment depression scores in the HEPs vs. TAU comparison to investigate the impact of using a non-parametric test as the source for deriving the ES of the meta-analysis. This involved removing Holden et al. (1989) from the meta-analysis to test the robustness of the findings.

Results

Study Characteristics

A total of 17 randomized controlled trials met the required inclusion criteria (see Supplemental Materials Appendix C for study characteristics). Studies were published between 1989 and 2021 and were conducted in five countries: almost half of the studies were carried out in the United Kingdom, ($k = 8$), with the remaining studies conducted in Canada ($k = 4$), Germany ($k = 2$), the United States ($k = 2$), and Sweden ($k = 1$). While

all studies used a controlled trial design (RCT), as set out in the inclusion criteria, the specific type of RCT differed. Forms of RCTs were varied and comprised pragmatic non-inferiority, two-arm parallel group feasibility, controlled random order, open parallel, pragmatic cluster, patient preference, multicentre, and two-, three-, or four-arm trials.

The mean (*SD*) sample size was 207 (*SD* = 213.09), (*Median* = 130), ranging from 34 to 755 participants. The grand mean age was 38.42 (*SD* = 6.30) and all studies comprised more females than males, ranging from 57% to 100%. Ten (58.8%) studies reported ethnicity showing samples to be predominantly White/European (*Median* = 89.6%; Interquartile Range = 3.9%). The primary disorder included different forms of depression; general depression (*k* = 4), major depressive disorder (*k* = 4), early-onset chronic depression (*k* = 2), postpartum/postnatal depression (*k* = 3), chronic depression (*k* = 1), early-onset dysthymic disorder (*k* = 1), mild depression (*k* = 1), and moderate/severe depression (*k* = 1).

The study setting comprised participants' homes (*k* = 4), university research clinics (*k* = 4), general practices (*k* = 3), outpatient clinics (*k* = 3), academic centers (*k* = 1), a single service within the English National Health Service (*k* = 1), and primary care facilities (*k* = 1). Of the 17 studies, six provided control comparisons comprising routine GP care whereby patients were treated in line with usual practice and asked to refrain from using psychological interventions unless absolutely necessary. Of these six studies, all were amenable to post-treatment depression outcomes and four also contained data on follow-up depression outcomes.

The mean follow-up duration for HEPs vs. TAU controls was 9 months (*SD* = 2.12). Twelve studies provided active treatment comparisons with post-treatment outcomes (excluding controls) taken at varying assessment points: 3 months (*k* = 3), 4 months (*k* = 3), 6 months (*k* = 2), 4.5 months (*k* = 1), and 5 months (*k* = 1) after randomization. Two studies did not specify the exact time point of post-treatment outcomes but stated that treatments lasted between 16 and 20 weeks (Goldman et al., 2006; Greenberg & Watson, 1998). Eight of the active treatment comparison samples provided follow-up outcomes with a range of durations: 6 months (*k* = 3), 12 months (*k* = 3), 9 months (*k* = 1), and 18 months (*k* = 1). The mean follow-up duration was 10 months (*SD* = 3.95).

Measures

A variety of self-report measures were employed to assess the level of depressive symptoms in the meta-analysis. The most frequently used measure of depression was the Beck Depression Inventory (BDI-II; Beck et al., 1996; *k* = 7: Ellison et al., 2009; Friedli et al., 1997; Goldman et al., 2006; Greenberg & Watson, 1998; King et al., 2014; Markowitz et al., 2005; Watson et al., 2003). Three recent UK studies also reported Patient Health Questionnaire-9 (PHQ-9; Kroenke et al., 2001) depression outcomes (Barkham et al., 2021; Freire et al., 2015; MacPherson et al., 2013). Three postpartum/postnatal depression studies used the Edinburgh Postnatal Depression Scale (EPDS; Cox et al., 1987; Cooper et al., 2003; Holden et al., 1989; Morrell et al., 2009) while Wickberg and Hwang (1996) used the Montgomery and Åsberg Depression Rating Scale (Montgomery & Åsberg, 1979). Additional self-report measures included the Inventory of Depressive Symptomatology Self Report (IDS-SR; Rush et al., 1996) (*k* = 2: Schramm et al., 2017, 2019) and the Quick Inventory of Depressive Symptomatology (QIDS; Rush et al., 2003; Kocsis et al., 2009).

Interventions

All of the studies in the control comparison (*k* = 6) used routine treatment as the control condition with three of these studies also yielding ESs for the active treatment comparisons (*k* = 12), and with the most common comparator intervention being cognitive behavioral therapy (CBT) (*k* = 6). Other psychological treatments included cognitive behavioral analysis system of psychotherapy (CBASP) (*k* = 2), and interpersonal therapy (IPT) (*k* = 1). One study combined the use of CBASP with anti-depressants as part of a multi-phase trial (Kocsis et al., 2009). Two studies used client-centered therapy as the comparator intervention for examining the effects of forms of experiential therapies that were predicted to be superior to the control format (Goldman et al., 2006; Greenberg & Watson, 1998). The only non-psychologically informed intervention was acupuncture (MacPherson et al., 2013). At follow-up, CBT remained the most common comparator condition (*k* = 4). Other comparator conditions at follow up comprised client centered therapy (*k* = 2), CBASP (*k* = 1), and acupuncture (*k* = 1)

While all active treatments met the criterion of being HEPs, there were differences in the names that were indicative of a difference in theoretical or clinical emphasis. The most common form of HEP was non-directive counseling (*k* = 5). Other HEPs comprised brief supportive psychotherapy (*k* = 2), emotion focused (*k* = 2), person-centered counseling (*k* = 2), process-experiential (*k* = 2), supportive

psychotherapy ($k = 2$), counseling ($k = 1$), and person-centered experiential therapy ($k = 1$). Summaries of findings from individual studies are presented in Supplemental Materials Appendix D.

Risk of Bias Assessment

Cohen's kappa (k) statistic showed substantial agreement between raters, $k = .70$ (95% CI [.55, .86]), $p < .001$. See Supplemental Material Appendix E for quality ratings summary. The most frequent source of bias stemmed from selective reporting of results (e.g., absence of a pre-determined plan of analysis) and the account of missing outcome data (i.e., not reported). Post hoc analyzes revealed the year of study publication and overall risk of bias outcome to be significantly negatively correlated, $r(15) = -.70$, $p = .002$, indicating that more recently published studies are associated with a lower risk of bias in this sample of psychotherapy randomized controlled trials. The sample size and overall risk of bias outcome were also significantly negatively correlated, $r(15) = -.62$, $p = .008$, suggesting that studies with larger samples are associated with less risk of bias.

Meta-Analysis

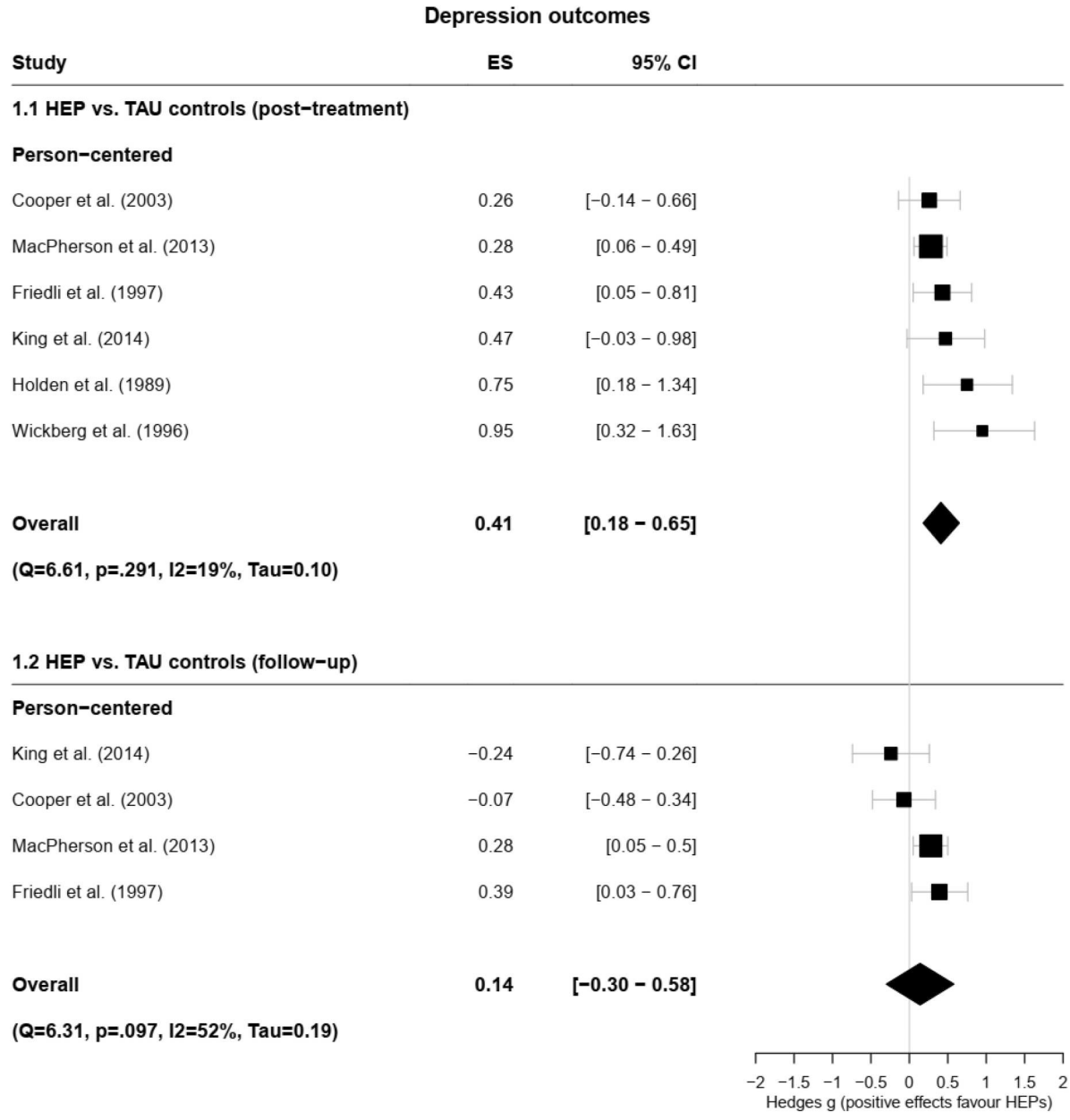
Four meta-analyzes were conducted: (1) TAU/post-treatment, (2) TAU/follow-up, (3) active/post-treatment, and (4) active/follow-up. The quality of evidence that contributed to each meta-analysis was indicated by the GRADE ratings. Each comparison started as high quality evidence as treatment effects were obtained from randomized controlled trials. The TAU post-treatment comparison had limited issues with inconsistency and indirectness of treatment effects, but there were concerns around study limitations (specifically for handling of missing data and selective reporting), imprecise estimates from small samples and some evidence of publication bias, resulting in quality of evidence downgraded by one level to moderate. The TAU follow-up comparison was downgraded to low quality evidence due to additional issues with inconsistency (wide variation in study estimates) and imprecision (wide CI and small number of studies). For both post-treatment and follow-up active treatment comparisons, issues were found with study limitations and inconsistency due to levels of heterogeneity, so quality of evidence was downgraded one level to moderate.

Treatment-as-usual (TAU) control comparisons (post-treatment). A meta-analysis was conducted using ES data from six studies ($n = 735$; HEP arm $n = 438$; TAU control arm $n = 297$) to investigate differences in post-treatment outcomes for depression in HEPs vs. TAU control groups. The weighted mean Hedges' ES was $g = 0.41$ (95% CI [0.18, 0.65], $p < .001$), indicating a small to moderate, significant difference between HEP and TAU for depression outcomes in favor of HEPs and equivalent to ≅ 23% difference (GRADE = moderate). The results are presented in Figure 2(1.1) and ordered according to ascending ES. The degree of overall variability due to between-study heterogeneity was low and not significant ($Q = 6.16$, $p = .291$, $I^2 = 18.82\%$, $\tau = 0.10$). As a result of minimal heterogeneity and insufficient studies (i.e., below 10), moderator analyzes were not viable with this analysis grouping.

Visual observation of the funnel plot (see Supplemental Material Appendix F) and Egger's regression test indicated significant asymmetry in the distribution of studies reporting post-treatment vs. controls depression outcomes, $B = -0.19$, $t(5) = 3.39$, $p = .028$. Using a random-effects model, Duval and Tweedie's Trim and Fill method identified and imputed three missing studies resulting in a smaller estimated ES than the original analysis ($d = 0.29$, 95% CI [0.07, 0.52]). Interpretation of the adjusted ES is similar to the unadjusted ES in that the ES is positive, and significantly different from zero, albeit it more conservative. Therefore, the impact of publication bias in the current meta-analysis is minimal. The fail-safe N statistic indicated that 61 studies with non-significant results would be needed to determine that the effect of HEPs versus control for depression is not significant.

A sensitivity analysis was conducted to investigate the impact of using the p value from a Mann–Whitney between groups test for Holden et al. (1989). The meta-analysis was re-run after removing Holden et al. (1989) from the analysis, which had little impact on the result. The overall ES slightly reduced from 0.41 (95% CI [0.18, 0.65]) to 0.37 (95% CI [0.12, 0.62]) but the effect was still statistically significant and small to moderate in magnitude.

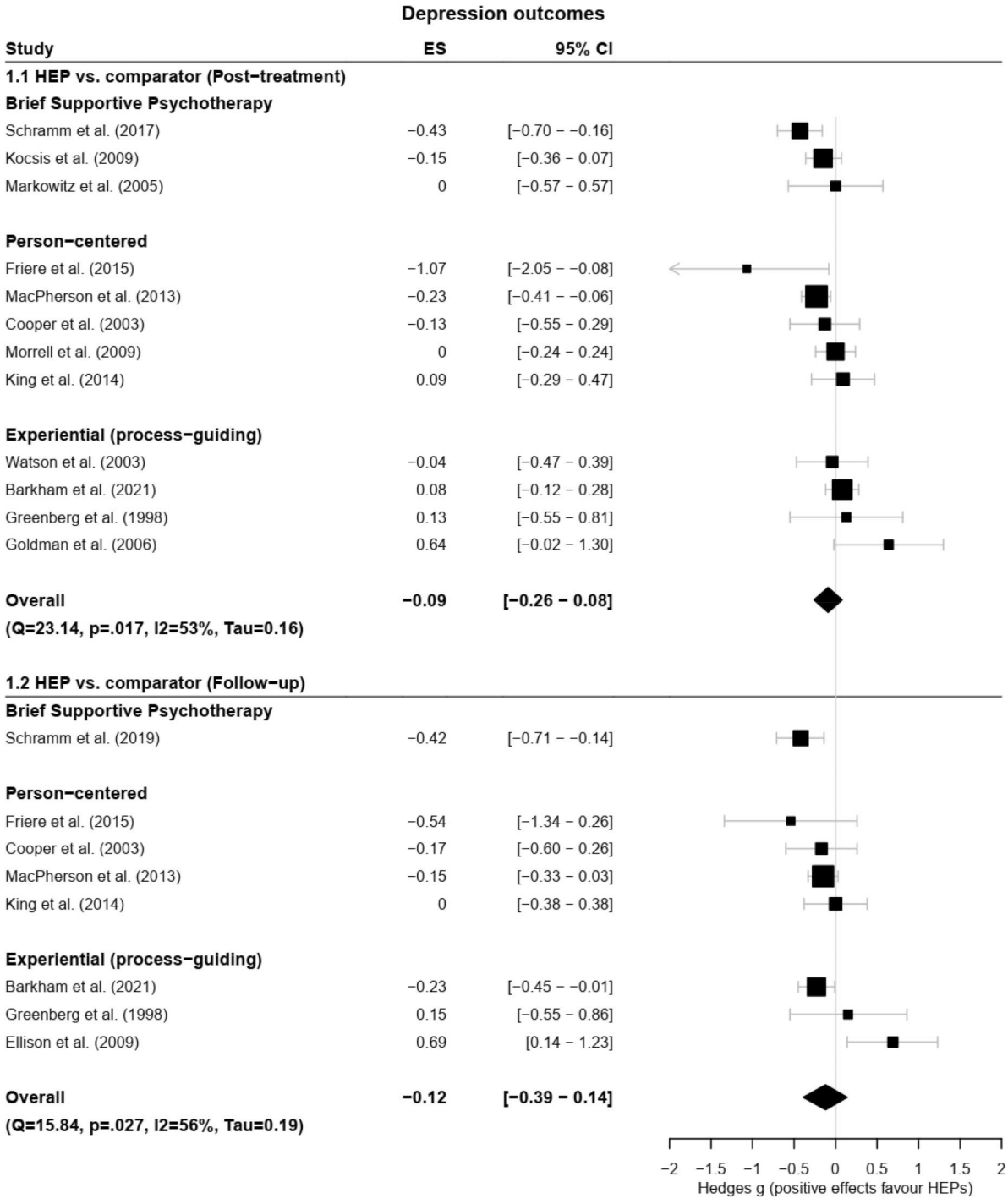
Treatment-as-usual (TAU) control comparisons (follow-up). At follow-up, ES data from four studies ($n = 631$; HEP arm $n = 385$; TAU control arm $n = 246$) were used to determine differences in follow-up outcomes for depression in HEPs vs. TAU controls. The weighted mean Hedges' ES was $g = 0.14$ (95% CI = [−0.30, 0.58], $p = .317$), suggesting there was a non-significant, small difference in follow-up outcomes favoring HEPs compared to TAU controls equivalent to ≅ 8% difference (GRADE = low). Results are displayed in Figure 2



Figures 2. (1.1 and 1.2) Forest Plot of Humanistic-Experiential Psychotherapy (HEP) for Depression Effect Sizes (ES) and 95% confidence intervals (CI) against Treatment-as-Usual (TAU) Control Groups at Post-Treatment and Follow-up Grouped by HEP Type (Ordered According to Ascending ES).

(1.2) and ordered according to increasing ES. The degree of overall variability due to between-study heterogeneity was classified as moderate ($Q = 6.31$, $p = .097$, $I^2 = 52.45\%$, $\tau = 0.19$). Cochrane's Q test was not significant but should be interpreted with caution due to low power from the small number of included studies. Visual inspection of the funnel plot (see Supplemental Material Appendix G) suggested an even distribution of studies and Egger's regression test revealed no significant asymmetry, $B = 1.49$, $t(3) = -1.58$, $p = .256$. Duval and Tweedie's Trim and Fill method identified no missing studies and the failsafe N was zero. Taken together, the evidence suggests minimal impact of publication bias.

Active treatment comparisons (post-treatment). At post-treatment, ES data from 12 studies ($n = 2131$; HEP arm $n = 1043$; active treatment arm $n = 1088$) were used to investigate differences in post-treatment outcomes for depression in HEPs vs. alternative active treatments. Meta-analysis with random-effects exhibited a mean-weighted



Figures 3. (1.1 and 1.2) Forest Plots of Humanistic-Experiential Psychotherapy (HEP) Versus Other Active Treatments for Depression Effect Sizes (ES) and 95% confidence intervals (CI) at Post-treatment and Follow-up Grouped by HEP Type (Ordered According to Ascending ES).

Hedges' ES of $g = -0.09$ (95% CI = [−0.26, 0.08], $p = .259$), indicating no significant difference between HEPs and the comparators in post-treatment outcomes for depression (equivalent to ≅ −5% difference; GRADE = moderate). The results are displayed in Figure 3(1.1) and ordered according to ascending ES. The degree of overall variability due to between-study heterogeneity was moderate and significant ($Q = 23.14$, $p = .017$, $I^2 = 52.47\%$, $\tau = 0.16$) and warranted further investigation via moderator analyzes.

Visual observation of the funnel plot (see Supplemental Material Appendix H) suggested some asymmetry in the study distribution but Egger's

regression test did not reveal statistically significant asymmetry, $B = -0.14$, $t(11) = 0.17$, $p = .869$. Duval and Tweedie's Trim and Fill method identified no missing studies. The fail-safe N statistic indicated that four studies with significant results would be needed to determine that the effect of HEPs versus active treatment comparisons for depression is significant.

A sensitivity analysis omitting the two studies in which comparisons were made between two active forms of HEPs also yielded a non-significant ES of Hedges' $g = -0.12$ (95% CI [−0.28, 0.03]) with values of $Q = 17.35$, $I^2 = 48.13\%$ and $\tau = 0.14$.

Active treatment comparisons (follow-up). At follow-up, ES data from eight studies ($n = 1284$; HEP arm $n = 641$; active treatment arm $n = 643$) were used to investigate the differences in follow-up outcomes for depression in HEPs versus alternative treatments. The weighted mean Hedges' ES was $g = -0.12$ (95% CI [−0.39, 0.14]), $p = .270$, indicating a non-significant difference in follow-up depression outcomes in favor of other interventions relative to HEPs (equivalent to ≅ −7% difference; GRADE = moderate). The results are presented in Figure 3 (1.2). The degree of overall variability due to between-study heterogeneity was moderate and significant ($Q = 15.84$, $p = .027$, $I^2 = 55.82\%$, $\tau = 0.19$). Visual inspection of the funnel plot (see Supplemental Material Appendix I) suggested some asymmetry in the study distribution for the reporting of follow-up depression outcomes. However, Egger's regression test revealed no significant asymmetry, $B = -0.51$, $t(7) = 0.79$, $p = .460$, indicating little impact of publication bias. Duval and Tweedie's Trim and Fill method identified no missing studies. The failsafe N statistic indicated that five studies with significant results would be needed to determine that the effect of HEPs versus active treatment comparisons for depression is significant.

A sensitivity analysis omitting studies making a comparison between two active forms of HEPs yielded a significant, small Hedges' ES of $g = -0.21$ in favor of alternative active interventions (95% CI [−0.35, −0.07]) with values of $Q = 4.53$, $I^2 = 0.0\%$ and $\tau = 0.00$.

Subgroup Analyzes and Meta-Regressions

Subgroup analyzes and meta-regressions were performed to investigate whether the moderator variables were able to explain variations in the HEPs vs. active treatments post-treatment depression outcomes (there were insufficient studies to carry out similar analyzes for TAU or for follow-up). ESs for the subgroup analyzes and meta-regressions are shown in Supplemental Materials Appendix J and K respectively. Examination of ESs for subgroup categories revealed no significant differences as a result of outcome measure, risk of bias, type of depression, or active comparator type. Interpretation of the subgroup ESs suggested HEPs were broadly comparable to CBT and other active comparators, but not as effective as CBASP interventions, equivalent to a 16% difference. Subgroup analysis showed that type of HEP delivered approached significance ($p = .054$). Studies that included an experiential component to the treatment—namely, were more process-guiding—produced a better ES ($g = 0.11$, equivalent to 6% difference) in favor of HEPs as opposed to person-centered (−7% difference) and BSP (−13% difference) groupings, both of which are less process-guiding. When HEP interventions were used as the control condition, outcomes significantly favored active alternative interventions ($g = -0.23$, 95% CI [−0.46, −0.01]), in contrast to the comparable effect of HEP and active interventions when the HEP condition was considered a candidate or equitable intervention ($g = -0.03$, 95% CI [−0.21, 0.15]). However, the aggregated ESs for the two subgroups were not significantly different from each other ($p = .152$).

A meta-regression denoted a non-significant negative effect of gender (% females) on the magnitude of post-treatment depression scores for HEPs vs. active treatments ($B = -0.00$, 95% CI [−0.01, 0.01], $p = .699$). The R^2 value showed that 1.24% of the heterogeneity was accounted for by this moderator. Mean age as a moderator also indicated a non-significant negative effect on post-treatment depression scores ($B = -0.02$, 95% CI [−0.05, 0.01], $p = .153$). The calculation of R^2 suggested that 14.38% of the heterogeneity was accounted for by mean age. Depression outcomes were associated with variations in ESs for different levels of researcher allegiance ($B = 0.15$, 95% CI [0.03, 0.27]), explaining 39.54% of the variance. As a moderator, researcher allegiance was significant ($p = .008$) at the Bonferroni-adjusted significance level of $p < .017$. ESs were larger for the allegiant therapy under investigation.

Discussion

This systematic review and meta-analysis found a small to moderate and statistically significant effect, indicating individual HEPs to be superior to TAU control groups in alleviating depressive symptoms at post-treatment. However, the difference reduced to a non-significant effect at an average nine-month

follow-up, although still favoring HEPs, a result that still pertained when excluding the studies comparing active HEPs within the same study. By contrast, when HEPs were compared to alternative active treatments, primarily CBT, a non-significant advantage to the comparator interventions obtained at posttreatment, which, although not significant at 10-month follow-up, became significant when excluding the single study in which both the active and comparator treatments were HEPs.

The present results are broadly consistent with those reported in Elliott et al.'s (2021) wide-ranging review showing a significant advantage to non-HEPs in which approximately half the studies were RCTs and half of these made comparisons with CBT. In a further analysis, based on 56 RCTs with no restrictions on the presenting condition, Elliott and colleagues reported a non-significant ES of −0.07 (95% CI [−0.21, 0.07]) favoring non-HEPs but did not report comparisons with TAU controls. Hence, the evidence from both the current review and that of Elliott et al. (2021) shows the outcomes of HEPs to be less favorable than alternative treatments but not significantly so at posttreatment, with the ESs from RCTs being < 0.10 from both meta-analytic studies.

Similarly, the finding of the current meta-analysis, which yielded non-significant results in favor of alternative therapies, is also consistent with Cuijpers et al.'s (2012) meta-analysis after they took account of researcher allegiance. And in terms of comparisons with control conditions, the current finding of an ES of 0.41 in comparison with TAU is virtually identical with Cuijpers et al.'s (2020) finding of 0.42 comparing non-directive counseling to care-as-usual controls.

The current meta-analysis is a timely update to the Cuijpers et al.'s. (2012) review although the current study focuses on a broader class of therapies (i.e., HEPs) as opposed to a specific form (i.e., non-directive supportive therapy). It also includes studies published since the Cuijpers et al. meta-analysis comprising a number of larger trials (e.g., Barkham et al., 2021; MacPherson et al., 2013) as well as the effects of therapies that would be identified as experiential (e.g., Goldman et al., 2006; Greenberg & Watson, 1998). And in relation to the meta-analytic review by Elliott et al. (2021), the current findings provide a sharper focus on the effects specifically for depression from RCTs.

One procedure for translating the effect from the current meta-analysis is to convert to a common language effect size (CLES; McGraw & Wong, 1992). This procedure translates an ES of .41 (the ES for HEPs vs. TAU) into 61.4% successful outcomes for patients receiving HEPs vs. 38.6% for the treatment-as-usual, while an ES of 0.24, Cuijpers et al.' s (2014) minimal important difference, would yield 56.7% successful outcomes. At follow-up, successful cases reduce to 53.9%. For comparative active treatments, an ES of 0.09 (the advantage of active comparator treatments over HEPs) yields 52.5% successful outcomes vs. 47.5%.

In addition, a comparison between trial data and routine practice can be made utilizing data from Barkham et al. (2021) comparing person-centered experiential therapy (PCET) with CBT in which the trial component was embedded in routine NHS practice, thereby yielding trial measures (at 6 and 12-months post-randomization) as well as routine practice data (first to last session). The PHQ-9 treatment difference for the routine data was 0.1 point (in favor of PCET) and at 6-months the ES advantage was 0.03 to PCET, suggesting that both trial and routine data taken at a time more proximal to the administration of therapy yield similar results.

Hence, the differences between *active* treatments are relatively small and proponents of HEPs and of common factors would likely argue that such differences are synonymous with broad equivalence (e.g., Wampold & Imel, 2015). However, others have raised the specter that such smaller differences may be important for specific groups of patients occurring within large national initiatives such as the English Improving Access to Psychological Therapies program (e.g., Barkham et al., 2021).

The finding from the subgroup analysis of a significant moderating effect for researcher allegiance is consistent with a review of reviews of the literature (Munder et al., 2013). The effect transcended active or comparator treatments such that associations were evident in both treatment arms. The result of no significant effect of process-guiding is consistent with the finding of Elliott et al. (2021) even though based on a larger sample of studies. However, results were in the direction of favoring more process-guiding therapies and so this still remains an area of promise although it is noteworthy that this effect is weighted by a single study yielding the largest effect in both posttreatment (Goldman et al., 2006) and follow-up (Ellison et al., 2009) comparisons derived from the York University (Toronto) research group (see Greenberg & Watson, 2022).

In addition, although the subgroup analysis showed no significant effect of risk of bias on depression outcomes, the individual ESs of the two groups (low; $g = -0.11$, and some concerns; $g = -0.06$) showed that lower risk of bias within studies was associated with decreased ESs and, therefore, less favorable to HEPs. This is consistent with the literature in terms of risk of bias influencing findings (e.g., Cuijpers et al., 2021).

Limitations

Although an independent audit of a random sample of articles showed 90% agreement in screening decisions, full independent double screening of the literature or data extraction was not conducted, thereby increasing the risk of potential bias. In addition, the search was limited to published English-language articles and accordingly language bias cannot be ruled out although evidence suggests any such impact is likely to be minimal (Morrison et al., 2012). Also, the gray literature was not searched and hence any unpublished trials would not have been included. However, unpublished trials are more likely to be small (i.e., underpowered) or at high risk of bias. It is well documented that smaller trials are more likely to yield larger effects. Although one function of meta-analysis is to aggregate the effects of smaller studies, in the current study almost half (eight studies) comprised total sample sizes <100 with four having a sample size <50. In addition, while there was little impact of publication bias, the quality of the included evidence as assessed using the GRADE system was low-moderate. Limitations of the evidence were due to most of the included studies being deemed to have some concerns regarding risk of bias, mainly a product of no pre-determined plan of analysis and absence of reporting dealing with missing data and concerns around imprecise and inconsistent effect estimates. With regards to active treatment comparators, non-CBT based interventions were scarce, limiting meaningful comparisons to other types of therapy.

Future Research

Future research efforts would benefit from conducting further larger-scale RCTs comparing HEPs for depression which are adequately powered to detect the small effects which are consistently reported (Barkham, 2023). Large-scale trials are evident in the field of investigating CBT efficacy, but the same level high-quality trials do not exist to the same extent in the HEPs research field. Indeed, the call made by Hollon and Ponniah (2010) for advocates of such therapeutic approaches to take ownership of their research agenda still holds true. The suggestion of encouraging future research to invest in larger trials is reflected in recommendations reported in a 25-year review of humanistic psychotherapy which stressed the importance of large *N* trials to continue and improve the evidence base which informs treatment guidelines (Angus et al., 2015). In addition to ensuring sufficient power, trials should strive for equipoise between comparator conditions to make sure both interventions are fairly represented. This would help mitigate the issue whereby previous studies have presented HEPs as a control and therefore implemented a diluted version of the therapy (e.g., Koszycki et al., 2012; Markowitz et al., 1998, 2008). Finally, a further step to enhancing the quality of future studies is to embed trials within routine practice such as in the English NHS Talking Therapies for Anxiety and Depression program (previously known as the Improving Access to Psychological Therapies [IAPT] program) in which all patients complete a mandatory set of outcome measures at every therapy session as part of routine practice (e.g., Barkham et al., 2021). This step would improve the ecological validity of the results because it reflects how psychological interventions are delivered in primary care settings (e.g., Delgadillo & Gonzalez Salas Duhne, 2020). Taken together, these methodological improvements would create a higher standard of evidence to inform clinical practice.

In conclusion, the findings provide support regarding the benefits of HEPs over TAU for the treatment of depression and confirmation that it does not yield significantly poorer outcomes than comparator active treatments (primarily CBT) at post-treatment, a position that does not hold at follow-up. While these conclusions provide support regarding choice of treatments for patients and thereby provide sufficient therapy resources (i.e., practitioners) to meet demand, they suggest the need for further theoretical and clinical work to develop strategies for enhancing the longer-term efficacy of HEPs.

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Supplemental Data

Supplemental data for this article can be accessed at https://doi.org/10.1080/10503307.2023.2227757.

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