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Review

The efficacy of psychotherapy for social anxiety disorder, a systematic review and meta-analysis

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ABSTRACT

Background: Given the growth in research examining the effects of psychotherapy on social anxiety disorder (SAD), an up-to-date comprehensive meta-analysis in this field is needed.
Methods: We selected studies from a database of randomized trials (RCTs) on psychotherapies for anxiety disorders (last updated search of PubMed, PsycINFO, Embase, and Cochrane (CENTRAL): 1 January 2024) We included RCTs comparing psychotherapy to a control condition for adults with SAD and conducted random effects meta-analyses to examine the efficacy of psychotherapy compared to control conditions at post-treatment.
Results: Sixty-six RCTs were included with 5560 participants and 98 comparisons between psychotherapy and control groups. Psychotherapy was effective in reducing SAD symptoms, with a large effect size ($g = 0.88$; 95 % CI: 0.76 to 1.0; $I^2 = 74$ %; 95 % CI: 69 to 79, NNT = 3.8). Effects remained robust across sensitivity analyses. However, there was evidence for significant risk of bias in the included trials. The multivariable meta-regression indicated significant differences in treatment delivery formats, type of recruitment strategy, target group, and number of sessions.
Conclusion: Psychotherapy is an effective treatment for SAD, with moderate to large effect sizes across all treatment types and formats. Future research is needed to determine the long-term effects.

1. Introduction

Social anxiety disorder (SAD) is one of the most common lifetime mental disorders characterized by a significant and persistent fear or anxiety that arises in response to potential scrutiny by others in one or more social situations (American Psychiatric Association, 2013; Bandelow & Michaelis, 2015; Kessler et al., 2012). The lifetime prevalence of SAD ranges from four to 15.4 % worldwide, with higher prevalence rates in high-income countries (e.g., 12.1 % in the United States; Koyuncu et al., 2019; Stein et al., 2017). Moreover, SAD is the earliest emerging anxiety disorder, with a mean age of onset ranging from 10.6 to 13 years, whilst often leading to long-lasting symptoms, indicating a chronic course (Grant et al., 2005a; Lijster et al., 2017; Stein & Stein, 2008; Steinert et al., 2013).

SAD is an impairing disorder associated with poor physical health, high societal costs, impairments in social and work-related functioning, and high comorbidity with other mental disorders (Barrera & Norton, 2009; Kessler, 2003; National Collaborating Centre for Mental Health NCCMH, 2013Ruscio et al., 2008). The profound negative impact of SAD on individuals and society clearly emphasize the need for treatment. However, despite these negative impact, the rates of seeking and receiving treatment are strikingly low (Dalrymple & Zimmerman, 2011; Wang et al., 2005). The rate of treatment seeking among individuals with SAD has been estimated to be low, with delays between SAD onset

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and first treatment of 12 years (Dalrymple & Zimmerman, 2011; Grant et al., 2005b). Similarly, less than 40 % of people with SAD receive treatment of any kind, with higher treatment rates in high-income countries (44.2 %), compared to only 18 % in low or lower-middle-income countries (Stein et al., 2017). Counting that treatment of any kind also includes alternative medicine and interventions delivered outside the conventional healthcare system framework, only one in ten individuals with SAD receive adequate treatment (Alonso et al., 2018; Stein et al., 2017). Such low treatment rates result from persistent barriers and challenges in accessing appropriate care for individuals with SAD.

People have indicated a plethora of personal treatment barriers, including stigma, shame, and a preference to handle problems on their own. Higher symptom severity and being young at the time of diagnosis are associated with higher treatment barriers perception (Eisenberg et al., 2007; Goetter et al., 2020). Furthermore, there are general treatment barriers which prevent most of the world's population from accessing psychotherapy treatment, such as the cost of treatment and the limited availability of trained clinicians (Patel et al., 2018). Given the significant number of people not receiving treatment for SAD, it is essential to consider psychotherapy treatment types and formats that potentially decrease these barriers and are more appealing to individuals.

Over the past 20 years, there has been a significant technological revolution in the field of psychotherapy resulting in various treatment delivery formats that can address many of the aforementioned barriers (Andersson et al., 2019; Goetter et al., 2020; Webb et al., 2017). Treatment delivery formats such as guided and self-guided e-health interventions have emerged as potential alternatives to face-to-face treatment for SAD (Clark et al., 2023; Titov et al., 2008c). E-health interventions enable individuals with SAD to conveniently receive psychotherapy from the comfort of their own homes while increasing the availability of treatment in settings where trained psychotherapists are scarce (Kählke et al., 2019; Patel et al., 2018; Stolz et al., 2018; Titov et al., 2008c).

In addition to the evolution of psychotherapy delivery formats, there have been notable trends and developments in the types of psychotherapies (Norcross et al., 2013; Soares et al., 2020; Wiltsey Stirman et al., 2010). Cognitive behavioural therapy (CBT) is by far the most extensively studied psychotherapy for SAD and has paved the way for the development of new therapies like third-wave cognitive behavioural therapies, including for example mindfulness-based cognitive therapy (MBCT) and acceptance and commitment therapy (ACT; Hayes & Hofmann, 2017; Ivanova et al., 2016; Kocovski et al., 2013; Liu et al., 2021; Papola et al., 2023a). Furthermore, several other types of psychotherapy have been examined for the treatment of SAD, such as psychodynamic therapy, interpersonal therapy, and imagery rescripting (Nilsson et al., 2012; Rahmani et al., 2020; Reimer & Moscovitch, 2015; Stangier et al., 2011). Given these developments, it is crucial to examine the effectiveness of all available psychotherapies thoroughly to provide the best evidence-based treatments for SAD.

Recently, several meta-analyses have been published on the effects of specific types of psychotherapy for SAD, such as CBT, mindfulness-based interventions, and psychodynamic therapy. These analyses consistently showed positive outcomes for each of these specific types of psychotherapy (Cuijpers et al., 2016a; Cuijpers et al., 2016b; Kindred et al., 2022; Liu et al., 2021; Zhang et al., 2022). However, while giving valuable conclusions, these recent studies provide only a partial perspective on the overall effects of psychotherapy for SAD as they do not synthesise all available evidence of psychotherapies. Such a comprehensive synthesis has been made in the past by several meta-analyses that demonstrated positive outcomes for all psychotherapies (Acarturk et al., 2009; Bandelow et al., 2015; Mayo-Wilson et al., 2014; Powers et al., 2008). Unfortunately, these findings are now outdated, as these meta-analyses were conducted almost a decade ago. Given the exponential growth in research during the last years, our understanding of the current developments in psychotherapy research in this field remains limited.

Therefore, it is imperative to update and supplement the existing meta-analytic research on the efficacy of psychotherapy in treating SAD (Acarturk et al., 2009; Bandelow et al., 2015; Mayo-Wilson et al., 2014; Powers et al., 2008). In the present meta-analysis, our objective was to examine the efficacy of psychotherapy compared to control conditions in reducing symptom severity in adults with SAD. We aimed to provide an up-to-date, comprehensive synthesis and overview of the field, with the most robust estimations of treatment efficacy and moderators that may influence the studies' effects.

2. Methods

This systematic review and meta-analysis was reported according to the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines (Supplement A; Page et al., 2021). The protocol for this current meta-analysis has been published at the Open Science Framework, prior to the analyses of the data (https://osf.io/wr945; de Ponti et al., 2023).

2.1. Identification and selection of studies

We searched four bibliographic databases (PubMed, PsycINFO, Embase, and the Cochrane Register of Controlled Trials (CENTRAL)) from the databases inception to January 1st, 2024, to identify RCTs examining the effects of psychotherapy for any anxiety disorder. This endeavour has been termed the anxiety Meta-analytical Research Domain (MARD) and has served as a the basis for selection of studies for other publications on anxiety disorders (Cuijpers et al., 2022; Papola et al., 2022; Papola et al., 2023a; Papola et al., 2023b; Papola et al., 2024). In the anxiety MARD all RCTs are included which examine a psychotherapy condition with any other condition, which includes another psychotherapy, pharmacotherapy, combined psychotherapy and pharmacotherapy, or a control condition, for adults with (elevated symptoms of) any anxiety disorder (Papola et al., 2023a). Index and free terms of psychotherapy and anxiety disorders were combined and filtered for RCTs. In addition, we supplemented the anxiety MARD with studies found through reference tracking of several previously published systematic reviews and meta-analyses for all anxiety disorders. The search strings are provided in the supplementary material (Supplement B). Every study found through this search was first screened based on title and abstract by two researchers independently (NdP, DP, CM, PF). When at least one researcher identified a study as potentially eligible, the full texts were retrieved. Afterwards, full-text articles were again assessed for inclusion by two of the same authors independently, any disagreements were solved through discussion.

For the current meta-analysis, we selected RCTs from the anxiety MARD, in which a (a) psychological intervention; (b) was compared to a control group such as waitlist, care-as-usual, or other inactive control (suggesting that all direct comparisons of active interventions were excluded); (c) for adults (18 years and older); (d) with a diagnosis of current SAD according to an operationalized diagnostic manual (e.g., DSM, ICD, Research diagnostic criteria). We excluded studies that are not comprehensively understood by one of the authors (i.e. English, Dutch, German, Spanish, Italian, Greek, Persian, and Turkish).

2.2. Risk of bias assessment

To evaluate the potential risk of bias in the included studies, we used the Cochrane risk of bias tool 2 (ROB 2; Sterne et al., 2019). This tool systematically examines the risk of bias in randomized trials across five distinct domains: (1) risk of bias arising from the randomization process; (2) risk of bias due to deviations from the intended interventions; (3) risk of bias due to missing outcome data; (4) risk of bias in the measurement of the outcome; (5) risk of bias in the selection of the reported results.

Each domain can be rated as low risk, some concerns, or high risk. The overall risk of bias is determined by the combined rating across all domains. A study is deemed to have a low risk of bias when all domains receive a low-risk rating. Conversely, a study is considered high risk of bias if at least one domain is high risk or if at least three domains are rated as some concerns. When one or two domains are deemed as having some concerns, the study is classified as having some concerns in terms of risk of bias. Assessment of the risk of bias in the individual studies was done by two researchers (NdP and MM), and disagreements were solved through discussion involving another author (CM).

2.3. Data extraction

The data from each study were extracted by two independent researchers. To ensure the former, the first author (NdP) was paired with a second reviewer (AS, MM, PC, PF). Discrepancies were solved through discussion with another author (CM, DP, PC). We extracted the following data from the studies: (a) post-test and baseline data (i.e., means, standard deviations, and number of participants) of validated SAD severity questionnaires; (b) number of participants randomized; (c) attrition of the participants (i.e., number of participants who dropped out from the study or did not complete post-test measures). Additionally, we extracted the characteristics of the participants, the interventions, and the study.

Participant characteristics included mean age, and percentage of women in the trial. Intervention characteristics included the type of treatment (e.g., CBT, exposure, third-wave therapies) and the number of sessions. Study characteristics included: (a) year of publication; (b) country location by continent (i.e., North America, Europe, Asia, Australia); (c) type of recruitment (i.e., community, clinic, or other); (d) target group (i.e., adults, students, or other); (e) type of control condition (i.e., waitlist, care-as-usual, or other). Intervention characteristics were extracted on a comparison level, whilst study and participant characteristics were extracted on a study level.

2.4. Outcomes

For each comparison between a psychological intervention and a control group, we calculated the small-sample bias corrected standardized mean difference (Hedges' *g*) at post-test, which indicates the difference between the two groups (Hedges & Olkin, 1985). The calculation of Hedges' *g* involves subtracting the post-test mean score of the control group from the post-test mean score of the intervention group and then dividing this difference by the pooled standard deviation. We selected Hedges' *g* as the effect size measure because some studies had a small sample size, and Hedges' *g* corrects for the potential bias associated with small samples (Hedges & Olkin, 1985). When means and standard deviations were unavailable, we used other statistics, such as dichotomous outcomes or the F-value, to calculate the effect size. Interpretations of Hedges' *g* are as follows: scores of.2 are classified as small, scores of.5 are categorized as moderate, and scores of.8 are regarded as large (Cohen, 1988).

For each comparison within a study, we calculated the effect size, which indicates the effects of the psychological intervention on SAD severity. For the latter, we used all validated outcome measures reported in a study, including clinician-rated instruments and self-report questionnaires (e.g., Social Phobia Scale, Social Interaction Anxiety Scale, Liebowitz Social Anxiety Scale (Self-Report), and (Brief) Fear of Negative Evaluation Scale (Fresco et al., 2001; Leary, 1983; Liebowitz, 1987; Mattick & Clarke, 1998; Watson & Friend, 1969).

2.5. Meta-analyses

All analyses were conducted in R (version 4.1.3) and RStudio (version RStudio 2022.02.1 +461 for macOS) using the 'metapsyTools' package (Harrer et al., 2022). This package imports the functionality of the 'meta', 'metafor', and 'dmetar' packages (Balduzzi et al., 2019; Harrer et al., 2019; Viechtbauer, 2010).

We utilized various methods available in the metapsyTools package to calculate the pooled effect sizes so that we could investigate potential variations based on different pooling methods. In our main analysis, we initially aggregated all available effect size data for a specific comparison within a study. These combined effects were subsequently pooled across studies and comparisons. To aggregate effects within comparisons, we assumed an intra-study correlation coefficient of $\rho = 0.5$.

To ensure the robustness of our main outcomes, we performed several additional analyses. First, we employed the same methodology as the main model but adjusted the first aggregation step to be conducted at the study level instead of the comparison level. This means that multiple treatment arms within a study were pooled together within that study. Second, we used a hierarchical three-level meta-analytic model to estimate the overall effect. This model estimates two heterogeneity variance components, assuming that effect sizes are nested within studies (Hedges et al., 2010). To ensure unbiased estimates even in cases where the model itself is not perfectly specified, we employed robust variance estimation (RVE). Third, we estimated the pooled effects using the 'correlated and hierarchical effects' (CHE) model (Pustejovsky & Tipton, 2022). This three-level model additionally accounts for correlated effects within studies, for which we assumed a constant sampling correlation of $\rho = 0.5$. Cluster-robust variance estimation was also employed for this model. Fourth, we employed a cross-classified three-level hierarchical model (CCREM) in which the instrument used for outcome measurement was added as a crossed random effect, thus incorporating effect heterogeneity resulting from different instruments being used across studies (again employing RVE; Fernández-Castilla et al., 2019). Fifth, we computed the effect size by considering only the smallest or largest effect within each study. Sixth, we utilized the 'non-overlapping confidence intervals' approach to pool effects while excluding outliers, in which a comparison of a study is identified as an outlier when the 95 % confidence interval (CI) of its effect size does not overlap with the 95 % CI of the pooled effect size (Harrer et al., 2021).

Seventh, in the influence analysis we pooled effects while excluding comparisons identified as influential. This involves re-running the pooling model after excluding studies flagged as outliers or influential defined by the "rules of thumb" of the more sophisticated diagnostics from Viechtbauer and Cheung (2010). Eighth, as an additional sensitivity analysis, we estimated the pooled effect size under the assumption of independence among effect sizes. Ninth, we estimated the pooled effect using only the comparisons with a low risk of bias.

Lastly, we used three different methods to assess and adjust for potential publication bias: Duval and Tweedie's trim and fill procedure, Rücker's 'Limit meta-analysis method', and a step function selection model (selection model; Carter et al., 2019; Duval & Tweedie, 2000; Harrer et al., 2021; Maier et al., 2022; McShane et al., 2016; Rücker et al., 2011). The trim and fill procedure operates under the assumption that funnel plot asymmetry is indicative of publication bias. It employs an algorithm to fill in missing studies to restore symmetry to the plot and then recalculates the results based on this adjusted data (Duval & Tweedie, 2000). Rücker's 'Limit meta-analysis method' is based on the 'small study effects' assumption, which indicates that studies with smaller sample sizes and larger standard errors are more susceptible to publication bias (Rücker et al., 2011). While accounting for between-study heterogeneity, it calculates the adjusted or 'shrunken' pooled effect when small-study effects are controlled for. Lastly, the selection model allows to estimate the relative likelihood of publication based on the specific *p* value of the results (McShane et al., 2016; Carter et al., 2019). The selection cut-point was set to $p = 0.1$, which allows to assess if effects below this threshold had a higher probability of being selected for publication that those above that threshold. This setup is equivalent to a "three-parameter" selection model (3PSM; Carter et al., 2019). This method also allows to correct the pooled effect estimate for differential selection probabilities.

A random-effects model was assumed for all analyses. We estimated the between-study heterogeneity variance (components) using restricted maximum likelihood. In models where RVE was not used for model fitting, we applied the Knapp-Hartung method to obtain robust confidence intervals and significance tests for the overall effect (IntHout et al., 2014). To assess the homogeneity of effect sizes, we calculated the I^2-statistic and its corresponding 95 % confidence interval. I^2-statistic represents the percentage of heterogeneity observed among the studies, where a value of 0 % indicates no observed heterogeneity, and larger values indicate increasing levels of heterogeneity. The thresholds for interpretation are 25 % for low heterogeneity, 50 % for moderate heterogeneity, and 75 % for high heterogeneity (Higgins et al., 2003). For the three-level models, we calculated a multilevel extension of the I^2-statistic, which characterizes the proportion of total variability attributable to heterogeneity within studies (level 2) and heterogeneity between studies (level 3; Cheung, 2014; Harrer et al., 2021). As the I^2-statistic cannot be directly interpreted as an absolute measure of between-study heterogeneity, we supplemented our analysis by incorporating the prediction interval (PI). The PI provides an estimation of the range within which the true effect size for 95 % of all populations is expected to lie, and is a direct reflection of the estimated between-study heterogeneity variance (Borenstein et al., 2009; Borenstein et al., 2017). In addition to calculating Hedges' *g*, we also calculated the Numbers-needed-to-treat (NNT) using the formulas presented by Furukawa (1999). In these calculations, we assumed a control group event rate of 12 % (Cuijpers et al., 2023a, in press).

We conducted a series of pre-planned subgroup analyses, four meta-regressions with one predictor, and a multivariable meta-regression, with some deviations from the protocol, which are explained in Supplement C. First, in the subgroup analyses we examined the effects of the interventions according to the type of treatment, type of control group, type of recruitment, treatment delivery format, location where the trial was conducted (continent), and the target group. For type of treatment, we added the subgroups with less than five comparisons to the 'other psychotherapy' subgroup. Second, we estimated the association between the continuous variables (publication year, mean age of, percentage of women, and number of sessions) with the pooled effect size from our main model using bivariate meta-regressions. Lastly, we performed a multivariable meta-regression, in which all characteristics were included to control for potential covariance. In addition to calculating the effect size after adjustment for publication bias using the three formerly mentioned approaches, we visually inspected the funnel plot on combined outcomes of comparisons and conducted Egger's test on the intercept. These steps were taken to visually inspect the bias captured by the funnel plot and determine its statistical significance.

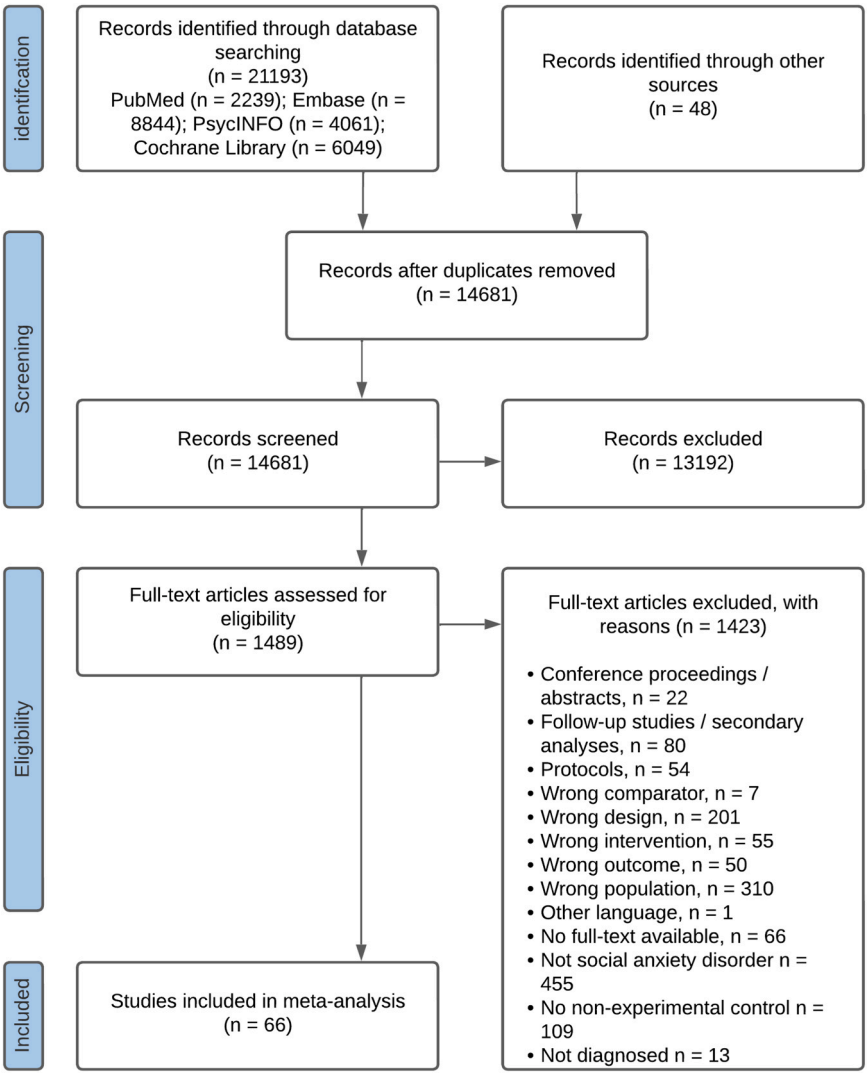

Fig. 1. PRISMA flowchart for the inclusion of studies.

3. Results

3.1. Selection and inclusion of studies

After examining a total of 14682 records after the removal of duplicates, we excluded 13192 records based on title and abstract screening. For the remaining 1490, we retrieved the full texts for further consideration. During the full-text examination, we excluded 1424 additional papers. The PRISMA flowchart is presented in Fig. 1, which contains an overview of the inclusion process, including reasons for exclusion based on full text. For this meta-analysis, 66 randomized controlled trials (with 98 comparisons between a psychotherapy and a control group) met the inclusion criteria.

3.2. Characteristics of included studies

A summary of the key characteristics of the 66 studies and the 98 comparisons is presented in Table 1. A total of 5560 participants participated in the trials, with 3573 in the intervention groups and 1987 in the control groups. The population mean age of all participants was 33.3 ($SD = 6.15$), ranging from 19.4 to 46.6. Additionally, women were slightly more represented in the trials, with a mean proportion of 56.7 % ($SD = 11.45$).

Most studies recruited participants through the community (52; 78.8 %), eight through clinical referrals, and six through other recruitment methods. Most studies (57; 86.4 %) were aimed at adults in general, seven at students, and two had other more specific target groups (i.e., unemployed homeless and symptomatic participants after antidepressant treatment). Nearly all studies employed a waitlist control group (55; 83.3 %), while four used care-as-usual, and seven utilized other control groups (i.e., pill placebo, reading task, self monitoring). Geographically, most trials were conducted in the continents of Europe (27; 40.9 %) and North America (24; 36.4 %), nine studies were conducted in Asia, and six in Australia. In terms of publication date, nearly half (28; 42.4 %) of the studies were published in the last decade.

The 66 studies included 98 psychotherapy arms, which were compared to a control group. Of these intervention arms, the majority examined CBT (62; 63.3 %), 13 examined exposure therapy, 11 third-wave therapies, four psychodynamic, and 8 other forms of psychotherapy. In terms of treatment delivery formats, 30 interventions were individual face-to-face in person, 29 were group face-to-face in person, 17 remote guided self-help, 11 remote unguided self-help formats, and 11 had other formats such as mixed or virtual reality. The number of sessions ranged from one to 26 ($M = 11.2$, $SD = 5.07$), with the majority (81 interventions; 82.7 %) being delivered between six and 16 sessions.

3.3. Risk of bias of the included studies

In domain 1, bias arising from the randomization process, most studies (42/66; 63.6 %) had some concerns primarily due to no information about allocation concealment. Most studies (42/66; 63.6 %) had low risk regarding bias due to deviations from intended interventions (domain 2). In domain 3, bias due to missing outcome data, approximately a quarter of studies (18/66; 27.3 %) had a high risk of bias derived mainly from no information about analysis or inappropriate analytical methods, and more than half (36/66; 54.5 %) had some concerns. Almost all trials had low risk regarding the bias in the measurement of the outcome (domain 4; 62/66; 93.9 %) because they either used self-report measures and/or measures were administered by a blinded clinician. In domain 5, bias in the selection of the reported result, most studies (51/66; 77.3 %) had some concerns due to no information about a protocol or registration (44/51 studies; 86.3 %), while the other seven studies were registered retrospectively. The overall risk of bias was substantial, with most studies (41/66; 62.1 %) having a high risk of bias, more than a third of studies (21/66; 31.8 %) having some concerns, while only a small number of studies (4/66; 6.1 %) demonstrated a low risk of bias.

A summary of the proportion of studies rated as low, having some concerns, or high risk in each domain can be found in Supplement D. A detailed overview of the risk of bias for every study in each domain is provided in Supplement E.

3.4. Effects of psychotherapy on SAD symptoms

The results of the main analyses are presented in Table 2, and the forest plot is presented in Supplement F. The main effect size indicating the overall difference between psychotherapy and control conditions at post-treatment was $g = 0.88$ (95 % CI: 0.76 to 1.0), corresponding with an NNT of 3.8. The heterogeneity was high ($I^2 = 74$ %, 95 % CI: 69 to 79), with broad a prediction interval ranging from − 0.1 to 1.85.

Most sensitivity analyses yielded similar results (Table 2), and all effect sizes were significant. Firstly, most analyses had a comparable effect size with the main model, with less than 0.25 difference (range: $g = 0.76$ to 1.00, NNT range: 3.22 to 4.57), and moderate to high heterogeneity (range: $I^2 = 62$ to 81). Moreover, prediction intervals remained broad, except for the influence analysis, which had a narrower significant interval (PI = 0.14 to 1.46). Secondly, the analyses of the smallest or the largest effect size per study showed that, when selecting the smallest effect size within a trial, the pooled effect was $g = 0.65$ (95 % CI: 0.49 to 0.81, NNT = 5.6), whilst it was $g = 1.29$ (95 % CI: 1.09 to 1.48, NNT = 2.36) when selecting the largest effect size. Heterogeneity was comparable to the main model, and the prediction intervals remained broad. Thirdly, the outlier analysis, which removed 19 outliers, decreased the effect size only a little ($g = 0.76$, 95 % CI: 0.71 to 0.81), whilst the heterogeneity was reduced to no observed heterogeneity with 95 % CI ranging from zero to low heterogeneity ($I^2 = 0$ %, 95 % CI: 0 to 27). Moreover, the PI was considerably narrower ($PI =$ 0.71 to 0.87). Lastly, the analysis in which only comparisons with low risk of bias were included (five), resulted in a large effect size, with a very broad PI (g = 1.58, 95 % CI: 0.59 to 2.56, $PI = -1.05$ to 4.2).

There was significant and considerable publication bias (Egger's test, $p < 0.001$), the funnel plot is presented in Supplement G. After publication bias adjustment, the estimated effect size was reduced considerably for two of the three methods (Table 2). Firstly, the trim and fill procedure imputed 26 comparisons to enhance the symmetry of the funnel plot, which caused the estimated effect size to drop to $g = 0.63$ (95 % CI: 0.49 to 0.78). Secondly, Rücker's 'Limit meta-analysis method' indicated a considerable publication bias, resulting in an even greater decrease in the estimated effect size ($g = 0.3$, 95 % CI: 0.15 to 0.6). Lastly, the selection model indicated a comparable effect size to the main model ($g = 0.86$, 95 % CI: 0.74 to 0.98). Furthermore, prediction intervals were broad and crossed zero in all publication bias adjustments, and heterogeneity remained high.

3.5. Subgroup analyses and meta-regressions

The results of the subgroup analyses are presented in Table 3. We found no evidence of a significant difference in the effect size between any of the moderator variables, including different psychotherapy types ($p = 0.709$), type of control condition ($p = 0.275$), (recruitment strategy ($p = 0.067$), type of treatment delivery format ($p = 0.5$), continent where the trial was conducted ($p = 0.347$), or target group ($p = 0.058$).

The results of the bivariate meta-regressions are presented in Supplement H and the bubble plots in Supplement I through L. Study publication year ($\beta = 0.18$, 95 % CI: 0.06 to 0.30, $p = 0.004$) and mean age of the participants ($\beta = -0.24$, 95 % CI: −0.35 to −0.12, $p < 0.001$) were significantly associated with treatment effect with a positive and negative association respectively, indicating that an increase in publication year, and a decrease in mean age were associated with larger treatment effect sizes. Conversely, the number of sessions ($p = 0.067$) and percentage of women in the trial ($p = 0.964$) were not significantly associated with the treatment effect size. Heterogeneity remained high

Table 1
Selected characteristics of the included studies.

Study	conditions	N	Format	N sess.	Mean age	% women	Recr.	Target	Location
Abramowitz et al. (2009)	cbt	11	gsh	8	43.4	76	com	adul	NAM
	wlc	10							
Anderson et al. (2013)	cbt (group)	39	grp	8	39	62	com	adul	NAM
	cbt (vret)	30	mix	8					
	wlc	28							
Andersson et al. (2006)	cbt	32	mix	11	37.3	52	com	adul	EU
	wlc	32							
Andersson et al. (2012)	cbt	102	gsh	9	38.3	60	com	adul	EU
	wlc	102							
Beidel et al. (2014)	exposure + sst	46	mix	24	36.4	52	oth	adul	NAM
	exposure	41	ind	24					
	wlc	19							
Berger et al. (2009)	cbt	31	gsh	5	28.9	56	com	adul	EU
	wlc	21							
Blanco et al. (2010)	cbt	40	grp	12	31.9	46	com	adul	NAM
	oth	39							
Botella et al. (2010)	cbt (ush)	62	ush	NR	24.4	79	com	adul	EU
	cbt (ind)	36	ind	NR					
	wlc	29							
Bouchard et al. (2017)	cbt (vr)	17	mix	14	34.5	73	com	adul	NAM
	cbt (in vivo)	22	ind	14					
	wlc	20							
Carlbring et al. (2007)	cbt	30	gsh	9	32.6	65	com	adul	EU
	wlc	30							
Clark et al. (2006)	cbt	21	ind	14	32	44	com	adul	EU
	exposure + ar	21	ind	14					
	wlc	20							
Clark et al. (2023)	cbt(ind)	34	ind	14	32.2	52	clin	adul	EU
	cbt(gsh)	34	gsh	8					
	wlc	34							
Craske et al. (2014)	cbt	33	ind	12	28.4	46	com	adul	NAM
	3rd	29	ind	12					
	wlc	25							
Davidson et al. (2004)	cbt	60	grp	14	36.8	50	com	adul	NAM
	oth	60							
Furmark et al. (2009)	cbt (biblio)	40	ush	9	36.1	68	com	adul	EU
	cbt (internet)	40	gsh	9					
	wlc	40							
Gallego et al. (2011)	cbt	24	ush	NR	39.3	68	com	adul	EU
	wlc	17							
Goldin et al. (2012)	cbt	38	ind	16	33.5	46	com	adul	NAM
	wlc	37							
Goldin et al. (2016)	cbt	36	grp	12	32.7	56	com	adul	NAM
	3rd	36	grp	12					
	wlc	36							
Gruber et al. (2001)	cbt (cbgt)	18	grp	8	41.7	52	com	adul	NAM
	cbt (cacbgt)	18	grp	12					
	wlc	18							
He et al. (2021)	3rd	45	grp	12	26.6	55	com	adul	Asia
	wlc	45							
Heimberg et al. (1998)	cbt	36	grp	12	34.9	50	oth	adul	NAM
	oth	33							
Himle et al. (2014)	cbt	29	grp	12	43.6	33	oth	oth	NAM
	cau	29							
Hope et al. (1995)	cbt	18	grp	12	34.3	50	clin	adul	NAM
	exposure	11	grp	12					
	wlc	11							
(Ivanova et al., 2016)	3rd (gsh)	37	gsh	8	35.3	65	com	adul	EU
	3rd (ush)	37	ush	8					
	wlc	39							
Johansson et al. (2017)	dyn	36	gsh	9	42.9	61	com	adul	EU
	wlc	36							
Kählke et al. (2019)	cbt	100	ush	9	26.7	62	com	stud	EU
	wlc	100							
Kampmann et al. (2016)	exposure (in vivo)	20	ind	10	36.9	63	com	adul	EU
	exposure (vr)	20	mix	10					
	wlc	20							
Khoramnia et al. (2020)	3rd	12	ind	12	22.1	71	com	stud	Asia
	wlc	12							
Kim et al. (2022)	exposure	28	vr	8	23.4	39	com	adul	Asia
	wlc	24							
Kocovski et al. (2013)	cbt	53	grp	12	34.4	54	com	adul	NAM
	3rd	53	grp	12					
	wlc	31							

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Table 1 (*continued*)

Study	conditions	N	Format	N sess.	Mean age	% women	Recr.	Target	Location
Koszycki et al. (2016)	3rd	21	grp	12	39.8	79	com	adul	NAM
	wlc	18							
Ledley et al. (2009)	cbt	16	ind	16	34.9	58	clin	adul	NAM
	wlc	22							
Leichsenring et al. (2013)	cbt	209	ind	25	35.2	55	com	adul	EU
	dyn	207	ind	25					
	wlc	79							
Mattick et al. (1989)	cbt	11	grp	6	41.6	54	com	adul	AU
	exposure	11	grp	6					
	ct	11	grp	6					
	wlc	10							
Mersch (1995)	other psy	24	ind	14	35.6	32	com	adul	EU
	wlc	12							
(Mortberg et al., 2006)	cbt	13	grp	NR	33.4	65	clin	adul	EU
	wlc	13							
(Mortberg et al., 2007)	cbt (ind)	32	ind	16	34.6	63	com	adul	EU
	cbt (grp)	35	grp	16					
	cau	33							
Mulkens et al. (2001)	other psy	14	ind	6	NR	77	com	adul	EU
	wlc	17							
Newman et al. (1994)	exposure	18	grp	8	46.6	50	com	adul	NAM
	wlc	18							
Nilsson et al. (2012)	other psy	7	ind	1	33.5	43	com	adul	EU
	oth	7							
Olivares-Olivares et al. (2016)	cbt	30	grp	12	19.9	63	com	stud	EU
	other psy	31	mix	26					
	wlc	30							
Oosterbaan et al., (2001, pp. 291)	cbt	28	ind	12	37	42	clin	adul	EU
	oth	27							
Pishyar et al. (2008)	cbt	16	grp	8	30.5	44	com	adul	AU
	wlc	16							
Price and Anderson (2011)	cbt (in vivo)	40	grp	8	39.1	61	com	adul	NAM
	cbt (vr)	29	vr	8					
	wlc	29							
Rahmani et al. (2020)	dyn (istdp)	14	ind	10	23.1	54	com	stud	Asia
	dyn (ib-istdp)	14	ind	10					
	wlc	14							
Rapee et al. (2007)	cbt (gsh + grp)	57	mix	5	35.5	50	com	adul	AU
	cbt (grp)	59	grp	10					
	cbt (ush)	56	ush	NA					
	wlc	52							
Reimer and Moscovitch (2015)	other psy	13	ind	1	19.5	70	oth	stud	NAM
	wlc	12							
Robillard et al. (2010)	cbt (vr)	14	mix	16	34.9	71	NR	adul	NAM
	cbt (ind)	16	ind	16					
	wlc	15							
(Salaberria and Echeburua, 1998)	exposure	24	grp	8	31	48	com	adul	EU
	cbt	24	grp	8					
	wlc	23							
Schulz et al. (2016)	cbt (ind)	60	gsh	8	35.4	53	com	adul	EU
	cbt (group)	60	gsh	8					
	wlc	29							
Schweden et al. (2016)	cbt	29	ind	25	25.3	40	clin	adul	EU
	wlc	27							
Schwob and Newman (2023)	exposure	39	ush	NA	19.4	54	com	stud	NAM
	oth	43							
Soltani et al. (2023)	3rd	15	ind	12	27	40	clin	adul	Asia
	wlc	15							
Stangier et al. (2003)	cbt (ind)	26	ind	15	38.8	49	com	adul	EU
	cbt (grp)	32	grp	15					
	wlc	37							
Stangier et al. (2011)	cbt	38	ind	16	35.6	56	com	adul	EU
	ipt	38	ind	16					
	wlc	41							
Stolz et al. (2018)	cbt (computer)	60	gsh	8	34.8	63	com	adul	EU
	cbt (app)	60	gsh	8					
	wlc	30							
Teale Sapach & Carleton (2023)	3rd	40	ush	6	34.3	68	com	adul	NAM
	ar	21	ush	6					
	wlc	21							
Thew et al. (2022)	cbt	22	gsh	14	33.1	70	com	adul	Asia
	wlc	22							
Titov et al. (2008a)	cbt	50	gsh	6	38.1	59	com	adul	AU
	wlc	55							
Titov et al. (2008b)	cbt	43	gsh	6	36.8	63	com	adul	AU
	wlc	45							

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Table 1 (*continued*)

Study	conditions	N	Format	N sess.	Mean age	% women	Recr.	Target	Location
Titov et al. (2008c)	cbt (gsh)	32	gsh	6	38	61	com	adul	AU
	cbt (ush)	31	ush	6					
	wlc	35							
Turner et al. (1994)	exposure	26	ind	20	35.4	61	clin	adul	NAM
	oth	21							
Wang et al. (2020)	cbt (gsh)	70	gsh	8	24.9	70	com	adul	Asia
	cbt (ush)	70	ush	8					
	wlc	70							
Ye (2017)	3rd	14	grp	8	25.1	37	NR	stud	Asia
	cau	13							
Yoshinaga et al. (2016)	cbt	21	ind	16	32	41	com	oth	Asia
	cau	21							
Zainal et al. (2021)	exposure	26	vr	4	23.3	77	com	adul	NAM
	wlc	18							

Note. N = number of participants in the intervention or control group, N sess. = number of sessions of the intervention group, % women = percentage of women in the trial, Recr. = recruitment strategy, cbt = cognitive behavioural group therapy, vret = virtual reality exposure therapy, sst = social skills training, vr = virtual reality, ar = applied relaxation, cbgt = cognitive behavioural group treatment, cacbgt = computer assisted cognitive behavioural group treatment, 3rd = third wave cognitive behavioural therapy, dyn = psychodynamic therapy, ct = cognitive therapy, other psy = other forms of psychotherapy, istdp = Intensive short-term dynamic psychotherapy, ib- istdp = interpretation-based intensive short-term dynamic psychotherapy, wlc = waitlist control, cau = care-as-usual, oth = other, gsh = guided self-help format, grp = group therapy format, mix = mixed format, ind = individual format, ush = unguided self-help format, NA = not applicable, com = recruitment trough the community, clin = recruitment trough clinical referrals, NR = not reported, adul = adults, stud = students, NAM = North America, EU = Europe, AU = Australia.

Table 2
Effects of psychotherapy on social anxiety symptoms.

Model	*N*	*k*	*g*	CI	I^2	CI	PI	NNT
Main: all comparisons (effect sizes combined)	5560	98	0.88	0.76-1	74	69-79	-0.1 to 1.85	3.8
All studies: (effect sizes combined)	5560	66	0.83	0.68-0.98	76	69-81	-0.18 to 1.84	4.07
Three-Level Model	5560	315	0.96	0.81-1.11	81		-0.21 to 2.13	3.4
Three-Level Model (CHE)	5560	315	0.95	0.81-1.09	81		-0.22 to 2.12	3.44
Three-Level Model (CCREM)[a]	5560	315	1.00	0.83-1.17	-		-0.22 to 2.23	3.22
One ES/study (lowest)	5335	66	0.65	0.49-0.81	73	66-79	-0.42 to 1.71	5.6
One ES/study (highest)	5392	66	1.29	1.09-1.48	77	70-81	0.04-2.53	2.36
Outliers removed[b]	4332	79	0.76	0.71-0.81	0	0-27	0.71-0.81	4.57
Influence Analysis[c]	5435	95	0.8	0.71-0.89	62	53-70	0.14-1.46	4.27
Only low risk of bias	257	5	1.58	0.59-2.56	88	74-94	-1.05 to 4.2	1.87
All effect sizes (assuming independence)	5560	315	0.87	0.8-0.94	70	67-74	-0.11 to 1.85	3.83
Publication bias correction[d]								
- Trim and fill method[e]	-	124	0.63	0.49-0.78	83	81-86	-0.84 to 2.11	5.73
- Limit meta-analysis[f]	5560	98	0.37	0.15-0.6	83		-0.62 to 1.37	11
- Selection model[g]	5560	98	0.86	0.74-0.98	82	74-88	-0.16 to 1.88	3.9

Note. N = Number of participants, k = number of comparisons/studies, g = Hedges' g, CI = 95 % confidence interval, I^2 = heterogeneity, PI = prediction interval, NNT = numbers-needed-to treat. [a] *τ^2 (between studies) = 0.31, τ^2 (within studies) = 0.01, τ^2 (instruments) = 0.07* [b] Removed as outliers: Beidel, 2014 Exposure + SST; Blanco, 2010; Clark, 2006 cbt; Clark, 2023 cbt(ind); Clark, 2023 cbt(gsh); He, 2021; Heimberg, 1998; Kampmann, 2016 Exposure (VR); Koszycki, 2016; Leichsenring, 2013 dyn; Mörtberg, 2007 cbt (grp); Olivares-Olivares, 2016 cbt; Olivares-Olivares, 2016 other psy; Oosterbaan, 2001; Pishyar, 2008; Rapee, 2007 cbt (ush); Schwob, 2023, Soltani, 2023, Stangier, 2003 cbt (grp) [c] Removed as influential cases: Clark, 2023 cbt(ind); Olivares-Olivares, 2016 cbt; Olivares-Olivares, 2016 other psy [d] Corrections were applied to the 'Main' model. [e] 26 studies added. [f] For the limit meta-analysis, the value under I-squared refers to the G-squared heterogeneity statistic. [g] Step-function selection model with cutpoints $p = 0.1$. The selection model parameter test was not significant: $\chi^2 = 2.1$ (p = 0.147). The model was fitted using maximum likelihood estimation.

in all meta-regressions with one predictor ($I^2 > 77$) with only the mean age of the participants ($R^2 = 0.19$) and the study publication year ($R^2 = 0.14$) explaining a small part of the total heterogeneity.

The multivariable meta-regression indicated several variables having a significant association with the effect size (Table 4). Firstly, recruitment through the community ($\beta = -0.67$, 95 % CI: −1.11 to −0.24, $p = 0.003$) and other types of recruitment strategies ($\beta = -0.8$, 95 % CI: −1.4 to −0.2, $p = 0.010$) were associated with smaller effect sizes as compared with the reference category of clinical recruitment. Secondly, treatment delivery formats, individual ($\beta = -0.68$, 95 % CI: −1.11 to −0.24, $p = 0.003$) and unguided self-help ($\beta = -0.54$, 95 % CI: −1.06 to −0.02, $p = 0.043$) treatment delivery formats were significantly associated with smaller effect sizes as compared to the reference category of group treatment delivery format. Thirdly, other types of target groups (such as students) were associated with a larger effect size ($\beta = 0.62$, 95 % CI: 0.11 to 1.13, $p = 0.019$) compared to adults. Lastly, the number of sessions showed a positive association with effect size ($\beta = 0.17$, 95 % CI: 0.02 to 0.31, $p = 0.023$), indicating a higher effect size when the number of sessions increases. Whilst this multivariable meta-regression model accounted for a notable amount of heterogeneity ($R^2 = 0.4$), heterogeneity remained moderate to large ($I^2 = 71$).

4. Discussion

We conducted a systematic review and meta-analysis to examine the efficacy of psychotherapy compared to control conditions in reducing

Table 3
Subgroup analyses.

Variable	Level	*N*	*Ncomp*	*g*	CI	I^2	CI	NNT	*p*
Treatment type	cbt	3994	62	0.84	0.69-0.98	74	67-80	4.02	0.709
	exposure	494	13	0.87	0.57-1.17	68	43-82	3.84	
	3rd	594	11	1.05	0.64-1.47	71	46-84	3.03	
	other psy	528	12	0.98	0.43-1.53	82	70-89	3.30	
Control type	wlc	4867	86	0.91	0.78-1.04	72	65-78	3.63	0.275
	other ctr	466	7	0.58	-0.04 to 1.2	86	74-93	6.41	
	cau	227	5	0.67	0.07-1.27	70	25-88	5.35	
Recruitment	com	4836	80	0.83	0.7-0.95	71	63-77	4.08	0.067
	oth	330	8	0.81	0.34-1.28	82	66-91	4.21	
	clin	394	10	1.38	0.86-1.9	80	64-89	2.17	
Format	gsh	1336	17	0.87	0.71-1.04	46	5-69	3.84	0.5
	grp	1421	29	0.91	0.61-1.21	82	75-87	3.63	
	ind	1606	30	0.92	0.72-1.11	67	52-78	3.58	
	ush	793	11	0.69	0.46-0.92	56	13-77	5.16	
	mix/oth	404	11	0.94	0.30-1.58	86	77-92	3.48	
Location	North America	1646	35	0.84	0.7-0.99	61	44-73	4.02	0.347
	Europe	2763	41	0.89	0.64-1.13	81	75-86	3.73	
	Asia	561	11	1.14	0.74-1.55	70	45-84	2.73	
	Australia	590	11	0.74	0.43-1.05	65	33-81	4.72	
Target group	adults	4969	87	0.8	0.7-0.9	66	57-73	4.28	0.058
	stud/oth	591	11	1.51	0.69-2.34	91	86-94	1.96	

Note. *N* = Number of participants, *Ncomp* = Number of comparisons, *g* = Hedges' g, CI = 95 % confidence interval, I^2 = heterogeneity, NNT = numbers-needed-to-treat, cbt = cognitive behavioural therapy, 3rd = third wave cognitive behavioural therapy, other psy = other types of psychotherapy, wlc = waitlist control, other ctr = other types of control conditions, cau = care as usual, com = community, oth = other, clin = clinics, gsh = guided self-help, grp = group, ind = individual, ush = unguided self-help, mix/oth = mixed and other formats, stud/oth = students or other target groups.

Table 4
Multivariable meta-regression.

Variable	Level	*β*	95 % CI	*t*	se	*p*
Treatment	3rd	Ref.				
	Cbt	-0.051	-0.44 to 0.34	-0.26	0.19	0.793
	Exposure	-0.067	-0.57 to 0.44	-0.26	0.25	0.793
	Other psy	-0.077	-0.59 to 0.44	-0.3	0.26	0.440
Control	Cau	Ref.				
	Other ctr	-0.054	-0.78 to 0.68	-0.15	0.37	0.676
	Wlc	0.506	-0.07 to 1.08	1.76	0.29	0.083
Recruitment	Clin	Ref.				
	Com	-0.675	-1.11 to − 0.24	-3.06	0.23	**0.003**
	Other	-0.802	-1.40 to − 0.20	-2.67	0.30	**0.010**
Format	Grp	Ref.				
	Gsh	-0.117	-0.58 to 0.35	-0.50	0.23	0.615
	Ind	-0.373	-0.72 to − 0.03	-2.14	0.17	**0.035**
	Ush	-0.539	-1.06 to − 0.02	-2.06	0.26	**0.043**
	Mixed/other	-0.257	-0.69 to 0.18	-1.18	0.22	0.243
Location	Asia	Ref.				
	Australia	0.464	-0.15 to 1.08	1.51	0.31	0.136
	Europe	0.173	-0.28 to 0.62	0.77	0.22	0.443
	North America	0.180	-0.32 to 0.68	0.72	0.25	0.472
Target group: adults versus other		0.619	0.11 to 1.13	2.41	0.26	**0.019**
Publication year (continuous)		0.128	-0.05 to 0.31	1.42	0.09	0.159
Mean age (continuous)		-0.166	-0.34 to 0.01	-1.90	0.09	0.061
Prop. women (continuous)		0.028	-0.12 to 0.18	0.38	0.07	0.175
Number of sessions (continuous)		0.168	0.02 to 0.31	2.32	0.07	**0.023**

Note. *β* = standardized regression coefficient, CI = confidence interval, *t* = t-value, se = standard error, Ref. = reference group, 3rd = third wave cognitive behavioral therapy, Cbt = cognitive behavioral therapy, Other psy = other types of psychotherapy, Cau = care as usual, Other ctr = other control condition, Wlc = waitlist control, Clin = clinic, com = community, Grp = group, Gsh = guided self-help, Ind = individual, Ush = unguided self-help, Prop. = proportion.

symptom severity in adults with SAD. We aimed to offer a comprehensive synthesis and overview of the field, providing the most robust and up-to-date estimations of treatment efficacy. We found a significant treatment effect of psychotherapy on SAD symptoms, with a large effect size of $g = 0.88$ and an NNT of 3.8, suggesting that roughly four individuals would need to be treated with psychotherapy to observe a positive outcome in one person. Similar results were found across an extensive series of sensitivity analyses, which further endorsed our hypothesis. However, across most analyses, heterogeneity was high, and the prediction intervals were broad. Furthermore, only four studies had a low risk of bias, and there was a significant indication of publication bias, which suggests that the present outcomes should be interpreted cautiously. Additionally, we conducted an extensive series of moderator analyses, and after controlling for the shared influence of all moderators, recruitment strategy, treatment delivery format, target group, mean age of the participants, and number of sessions were significantly associated with the studies' outcomes.

The findings of the present main analysis are in line with previous literature on this topic, which estimated the pooled treatment effect of all psychotherapies (Acarturk et al., 2009; Powers et al., 2008). Our estimated effect size was a little larger than the effect size (d = 0.70) in the meta-analysis of Acarturk et al. (2009) and almost identical to the effect size (d = 0.86) found by Powers et al. (2008) with similar heterogeneity. Nevertheless, in our analysis, we had double the number of studies of the previous meta-analyses, which supports the overall conclusions more robustly. In addition to the effect size, we calculated the NNT, which makes it easier to interpret the results in the clinical context. The increased number of studies allowed us to compare our results to more recent meta-analyses conducted on specific types of therapy for SAD (Cuijpers et al., 2016a; Cuijpers et al., 2016b; Kindred et al., 2022; Liu et al., 2021; Zhang et al., 2022). In addition to confirming previous meta-analytic findings on the efficacy of specific psychotherapy types separately, our moderator analyses demonstrated that there was no evidence for a significant difference between the effect sizes of specific types of psychotherapy in reducing symptoms of SAD. However, it must be noted that we categorized CBT and exposure as distinct treatments even though there is a significant overlap between the two. This could have resulted in decreased power to detect differences among subgroups, and this finding should therefore be interpretated cautiously (Cuijpers et al., 2021).

Several significant moderators were identified in the multivariable meta-regression analysis, suggesting possible associations between the

examined variables and the treatment effects. More specifically, the finding that individual psychotherapy is less effective than group psychotherapy contradicts previous literature findings indicating that individual treatment has the best effects on SAD (Mayo-Wilson et al., 2014). A possible reason for this discrepancy is that Mayo-Wilson et al. (2014) only examined CBT formats, whereas we examined all types of psychotherapy, suggesting that there could be an association between psychotherapy treatment types and formats. Another possible explanation to consider is the willingness of participants to be randomized to a group treatment, as participating a group treatment can trigger anxiety in individuals with SAD. This could mean that the participants in trials examining group formats could be systematically different from those in trials examining individual formats. The underlying (un)observable characteristics of these participants could be related to the treatment outcomes. Next, we found that unguided self-help had a smaller effect compared to a group treatment delivery format, which is in line with previous meta-analytic research that has consistently shown that unguided treatment has smaller effects in reducing symptoms of mental disorders compared to other formats or controls (Cuijpers et al., 2019; Cuijpers et al., 2023b; Karyotaki et al., 2021; Papola et al., 2023b; Pauley et al., 2023).

The finding that recruitment through clinical samples was associated with larger effects compared to recruitment through community or other means could be related to previous findings on symptom severity (Low et al., 2008; Scholten et al., 2023). Individuals recruited from clinics often present with a higher baseline symptom severity, which may be associated with larger treatment effects (Low et al., 2008; Scholten et al., 2023). Furthermore, there was an indication that the number of sessions positively associated with the treatment effect, indicating a larger effect size when the number of sessions increased. This finding is not in line with previous meta-analytic research about psychotherapy for SAD, in which no significant associations were found (Cuijpers et al., 2016b; Kindred et al., 2022) Moreover, other target groups, which in our analysis was for the majority student samples compared with adults, showed a similar association with larger effect sizes. This finding may be related to the early age of onset of SAD, with older participants potentially experiencing a more chronic course of the disorder, influencing the treatment effects (Grant et al., 2005a; Lijster et al., 2017; Stein & Stein, 2008; Steinert et al., 2013). Nevertheless, we should acknowledge that the present results of the multivariable meta-regression analysis must be interpreted with caution since they are correlational and typically severely underpowered (Cuijpers et al., 2021).

This study has several strengths. To the best of our knowledge, this is the largest meta-analysis ever conducted on psychotherapies for SAD, with a comprehensive selection of studies being identified through rigorous, up-to-date searches. Furthermore, we performed state-of-the-art analyses to identify moderators of the treatment effects in existing studies. However, there are also several limitations that we should consider in the interpretation of these findings. First, despite our proficiency in multiple languages, we excluded one study that was written in a language not comprehensively understood by one of the authors (D'El Rey et al., 2008). Furthermore, there is possibility that other relevant studies were inadvertently overlooked during screening due to language barriers. Second, the included studies were at a considerable risk of bias, with only four trials rated as low risk, whereas more than 60 % of the studies had a high risk of bias. Therefore, methodological weaknesses in the included RCTs may have inflated effect sizes, as previously suggested in the literature (Cuijpers et al., 2019). Third, there were indications of publication bias, resulting in decreased adjusted treatment effect estimates across most analyses. The publication bias seemed to have derived mostly from small sample studies with large standard errors confirming the 'small study effects' assumption (Rücker et al., 2011). Fourth, the heterogeneity was substantial in most analyses, and the prediction intervals crossed zero, indicating that the true effect size might differ across studies and that future studies could find positive or negative effects. Moreover, subgroup analyses and meta-regressions only explained a small portion of the total variance, leaving the present heterogeneity largely unexplained. However, in a sensitivity analysis, in which we removed 19 outliers, the heterogeneity was reduced to low, and the prediction interval narrowed, suggesting that the studies with very large or small effect sizes had an influence on the high heterogeneity. Notably, in this sensitivity analysis, the effect size dropped slightly, but supported the overall conclusions of the present meta-analysis. Fifth, since almost all psychotherapies were compared to a waitlist control group, we were not able to estimate the long-term treatment efficacy of psychotherapy for SAD since most of the time participants in the waitlist control group received the treatment directly after the post-test assessment. Moreover, the use of waitlists itself is a limitation, as the use of waitlist control groups is known to inflate effect sizes (Furukawa et al., 2014). Finally, the present studies were mostly conducted in high-income countries, limiting the generalisability of the current findings to low- and middle-income countries.

Our findings have several important implications for clinical practice and policymakers. Whilst SAD is a very impairing disorder, many individuals do not seek and/or receive treatment (Alonso et al., 2018; Dalrymple & Zimmerman, 2011; Goetter et al., 2020; Lijster et al., 2017; NCCMH, 2013; Stein et al., 2017; Steinert et al., 2013). Our study found comparable, large effect sizes for all types of psychotherapy and most treatment delivery formats, suggesting that various psychotherapy types and formats are adequate treatment options for SAD. Such findings support the use of various psychotherapy options, which could be more appealing to some individuals, thereby increasing the treatment uptake (Katzman et al., 2014; NCCMH, 2013). Next, the group- and guided self-help formats may increase treatment scalability because clinicians can treat more people at once in a group and could spend less time providing feedback to a person in the guided self-help treatment delivery format. From a global perspective, it is important to note that guided self-help treatments can be effective alternatives to face-to-face treatment delivery formats. Furthermore, although the effect of unguided self-help was smaller than other treatment delivery formats, it was still moderate to large compared to control conditions. Since unguided self-help is the most scalable and possibly cost-effective treatment delivery format, it may be a viable treatment option in settings where mental healthcare facilities are scarce or inaccessible due to high costs (Patel et al., 2018).

There are several implications for future research. First, whilst the current conventional meta-analysis is foundational to establish the current absolute efficacy of psychotherapy for SAD compared to control conditions, network meta-analyses (NMAs) of the most recent evidence are needed to establish the relative effectiveness of psychotherapy relative effectiveness of treatment types and formats. In an NMA all comparisons can be examined against each other regardless of whether the existing studies have compared them in a head-to-head fashion. Second, given possible individual participant differences in response to treatment, large-scale multicentre RCTs and individual patient data meta-analyses are needed to examine subgroups of individuals who benefit from a specific psychotherapy modality. These methods typically have more statistical power that can be used to determine which treatment works for whom, paving the way towards personalized psychotherapy. Third, studies should consider including more reliable comparisons than waitlist (e.g., care-as-usual) to lead to more precise estimates and determine the long-term efficacy of psychotherapy for SAD. Fourth, the methodological quality of studies and their reporting must be improved to provide a more valid estimate of the treatment efficacy. Lastly, there is a need for new RCTs in low- and middle-income countries to improve our understanding of the efficacy of psychotherapy for SAD globally.

In conclusion, we demonstrated that psychotherapy is probably an effective treatment for SAD, with moderate to large effect sizes across all treatment types and formats. Thus, the current findings encourage the wide dissemination of psychotherapy to scale up and increase the

availability of SAD treatment. Nevertheless, more high-quality studies with long-term outcomes and studies conducted in low-resourced settings are needed to draw conclusions regarding the short and long-term efficacy of psychotherapy for SAD.

CRediT authorship contribution statement

Nino de Ponti: Writing – review & editing, Writing – original draft, Visualization, Investigation, Formal analysis, Resources, Conceptualization. **Minoo Matbouriahi:** Writing – review & editing, Investigation, Resources. **Pamela Franco:** Writing – review & editing, Investigation, Resources. **Mathias Harrer:** Writing – review & editing, Software, Methodology, Formal analysis. **Clara Miguel:** Writing – review & editing, Investigation, Resources. **Davide Papola:** Writing – review & editing, Methodology, Investigation, Resources, Conceptualization. **Ayşesu Sicimoğlu:** Writing – review & editing, Investigation, Resources. **Pim Cuijpers:** Writing – review & editing, Supervision, Methodology, Investigation, Resources, Conceptualization. **Eirini Karyotaki:** Writing – review & editing, Supervision, Methodology, Conceptualization.

Declaration of Competing Interest

none.

Data availability

Explore the dataset at https://www.metapsy.org. With Metapsy, anyone has the opportunity to conduct their own analyses using the data from the current meta-analysis.

Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.janxdis.2024.102881.

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