Efficacy of imagery rescripting in treating mental disorders associated with aversive memories - An updated meta-analysis

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Short Title: Meta-analysis of Imagery Rescripting

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Highlights

- Imagery rescripting (ImRs) addresses complaints associated with aversive memories
- ImRs is effective in treating a variety of mental health disorders
- ImRs appears similarly effective as exposure, cognitive restructuring, or EMDR
- Treatment effects of ImRs appear stable at 4-12 weeks following treatment
- Extensive cognitive preparation does not seem to enhance the efficacy of ImRs

Abstract

Imagery rescripting (ImRs) is frequently applied to treat different psychological complaints. We conducted an updated meta-analysis based on randomised controlled trials on the efficacy of ImRs for mental disorders associated with aversive memories. Medline, PsycInfo, and Web of Science were searched up to May 2023. Seventeen trials were included with a total of 908 participants (417 in the ImRs condition), suffering from posttraumatic stress disorder, anxiety disorders, depression, or eating disorders. Random effect models yielded an overall effect of g = 0.68 (95% CI 0.18-1.18; k = 7) compared to passive controls (mostly waitlist). The effect compared to (prolonged) exposure, cognitive restructuring, and EMDR was non-significant (g = -0.01; 95% CI -0.18-0.15; k = 11). Follow-up assessments indicated a long-term treatment effect. Results suggest that ImRs can effectively treat a variety of psychological disorders and produce similar treatment effects as evidence-based interventions. Limitations include the bounded number of included trials for each mental disorder. The meta-analysis was registered on PROSPERO (CRD42020220696) and received no funding.

Keywords: Imagery rescripting, aversive memories, meta-analysis

1 Introduction

Imagery rescripting (ImRs) is frequently used to treat psychological complaints related to aversive memories (Strachan et al., 2020). In ImRs, patients are first instructed to imagine the beginning of an aversive memory including sensory impressions, bodily sensations, cognitions, and emotions. Thereupon they are guided to imagine changing the course of events in a way that would satisfy current basic needs (Arntz, 2012). The main goal of ImRs is the transformation of meaning associated with strong aversive memories (Edwards, 2007). Aversive memories not only influence other memories but also decisively guide current and future behaviour (Lane et al., 2015) and play a crucial role in the development and maintenance of many mental disorders (Beckers & Kindt, 2017). Various studies have found both adverse childhood (Hughes et al., 2017; Merrick et al., 2017) and adult life experiences (Howarth et al., 2020; Kraaij et al., 2002) to be non-specific risk-factors for the development of psychopathology. Maladaptive processing and representation of aversive experiences may lead to intrusive memories, avoidant behaviour, or dysfunctional memory appraisals, thus contributing to the maintenance of psychological symptoms (Seinsche et al., 2023). While this appears apparent for posttraumatic stress disorder (PTSD), similar processes have been found for a range of disorders such as depression (Mihailova & Jobson, 2018) or social anxiety disorder (Seinsche et al., 2023).

Following the reconsolidation hypothesis (Schwabe et al., 2014), previously consolidated memories can return into an active state, which then allows for a reinforcement, reduction, or update of memory content and linked emotions. The rationale of ImRs does not involve the replacement of original memories with false memories but rather the creation of more functional meanings. Because patients recall the original event, ImRs may improve encoding of trauma information and create associations with different emotions and responses. In fact, studies have shown consistent trauma reporting before and after ImRs (Spinhoven et al., 2012) and improved memory of factual trauma details compared to a control task (Hagenaars & Arntz, 2012). A positive change in meaning and valence of memories is proposed to lead to a reduction of negative self-beliefs (Mancini & Mancini, 2018) and an increase in perceptions of mastery and self-efficacy (Kunze et al., 2019).

ImRs may either be applied as a stand-alone intervention or in combination with other treatments. While it may be integrated in different cognitive behavioural interventions (Arntz, 2012), it is a standard technique in schema therapy (Arntz & van

Genderen, 2020; Jacob & Arntz, 2013). Overall, two types of application of ImRs have been developed and tested. In one, the rescripting is prepared by using cognitive therapy techniques to challenge the dysfunctional interpretation of the (traumatic) memory. The alternative, more functional interpretation that follows from this process is then used to develop a new script that is subsequently imagined after the aversive memory has been activated in imagery. In the other approach, there is no such preparation. Rather, the patient imagines the aversive event, and the rescripting is based on the needs and action tendencies that the patient experiences during the imagery. Thus, the first approach combines imagery rescripting with Beckian cognitive therapy (Beck, 2010), whereas the second approach is more experiential and here the new script is based on needs and action tendencies that spontaneously emerge during the imagery. To our knowledge, no study has directly compared the two approaches.

Several trials have investigated the efficacy of ImRs in the treatment of posttraumatic stress disorder (e.g., Arntz, 2014), social anxiety disorder (e.g., Wild et al., 2008), personality disorders (e.g., Arntz, 2011), bulimia nervosa (e.g., Cooper et al., 2007), body dysmorphic disorder (e.g., Ritter & Stangier, 2016), obsessive-compulsive disorder (e.g., Veale et al., 2015), or major depressive disorder (e.g., Brewin et al., 2009). To the best of our knowledge, only one meta-analysis (Morina et al., 2017) has been published on the efficacy of ImRs on psychopathology related to aversive memories. Morina et al. (2017) found significant treatment effects in comparison with control conditions, however, results were limited by the small number of available controlled trials (k = 7). Since 2017, several new randomised controlled trials (RCTs) have been published on the efficacy of ImRs for different mental disorders (Boterhoven de Haan et al., 2020; Dugué et al., 2019; Hyett et al., 2018; Knutsson et al., 2020; Langkaas et al., 2017; Ma & Lo, 2022; Moritz et al., 2018; Ovanessian et al., 2019; Pile et al., 2021; Raabe et al., 2022). Given that RCTs are regarded as the highest standard of trials, we aimed at conducting an updated meta-analysis including only RCTs that investigated the efficacy of ImRs compared to passive control conditions, active control conditions, or other psychological treatments. According to recommendations by the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) group (Page et al., 2021), the main research question was outlined as "In individuals with a mental disorder (P), does imagery rescripting (I), compared to any control condition (C), reduce psychological complaints (O) in RCTs (S)?". A post-hoc aim was to explore the possible benefits of cognitive preparation before rescripting.

2 Method

The aims and methods of this meta-analysis were registered with the PROSPERO database (CRD42020220696) and were based on our previous meta-analysis (Morina et al., 2017). The current meta-analysis was conducted in accordance with the PRISMA 2020 guidelines (Page et al., 2021).

2. 1 Identification and selection of studies

Systematic database searches were conducted in Medline, PsycInfo (through EBSCOhost), and Web of Science, covering all literature from January 2016 (as earlier literature was reviewed in Morina et al., 2017) to May 21st, 2023. In line with our previous search strategy, the following terms were used for searching titles, abstracts, and key words: *"imagery rescripting" OR "updating memor*" OR "imagery modification" OR "imaginal reliving"*. Title and abstract screening was completed using Rayyan (Ouzzani et al., 2016), a web-tool for systematic reviews. After resolution of duplicates, titles and abstracts were screened for eligibility by two blinded reviewers. Afterwards, presumably eligible studies were viewed in full-text leading to in- or exclusion. Disagreements between the two reviewers were resolved through discussion with a third investigator. Furthermore, reference lists of included studies and relevant reviews were hand-searched for additional eligible studies.

Inclusion criteria were: 1) study was an RCT; 2) participants were diagnosed with a mental disorder based on ICD or DSM; 3) treatment targeted psychological complaints reported as a result of aversive memories and at least 50% of the applied treatment consisted of ImRs; 4) ImRs was compared to any passive or active control condition or to another treatment; 5) a minimum of ten participants were treated with ImRs. The last criterion displays a deviation from the protocol (see the supplementary material), which noted a minimum of five participants based on the previous meta-analysis (Morina et al., 2017). This criterion was originally motivated by the consideration of case studies in Morina et al. (2017). However, given that small samples pose a higher risk of bias (Lin, 2018) and in line with other meta-analyses (e.g., Hoppen, Kip, & Morina, 2023), a minimum of ten participants was implemented in the current meta-analysis. This decision was made before the study selection process. No restrictions were made concerning age of participants, treatment modality, publication language, or publication status. Unpublished trials were included to prevent inflated

effect sizes that tend to appear in meta-analyses that only include published data (McAuley et al., 2000). However, studies were excluded if they exclusively focused on the treatment of nightmares as this was the focus of several previous systematic reviews and meta-analyses (Casement & Swanson, 2012; Gill et al., 2023; Hansen et al., 2013; Yücel et al., 2020).

2.2 Coding of trial characteristics

Data on sample, study, and intervention characteristics were extracted by two independent reviewers. All measures of symptoms associated with aversive memories in patients were regarded as potential outcome measures. We distinguished between primary and secondary outcomes as reported in the primary studies, imagery-related outcomes (i.e., image vividness or distress), and belief outcomes (i.e., encapsulated beliefs) defined as variables that express thoughts and beliefs participants have about their images and how strong these beliefs are (Knutsson et al., 2020; Lee & Kwon, 2013; Reimer & Moscovitch, 2015). Control conditions were further coded as passive control condition (e.g., waitlist or minimal attention tasks), active control conditions (e.g., treatment as usual or supportive counseling), and active treatments (e.g., cognitive behavioural approaches or eye movement desensitization and reprocessing [EMDR]). In case of several instruments assessing one outcome, we considered the one most frequently reported (e.g., the Liebowitz Social Anxiety Scale [LSAS] for Social Anxiety Disorder; Heimberg et al., 1999). Authors were contacted twice for any missing information. For analyses on long-term treatment effects, follow-up assessments were divided into short-term follow-ups (i.e., < 6 months post-treatment) and long-term follow-ups (i.e., ≥ 6 months).

2.3 Quality Assessment

Quality coding of the included studies was conducted using a tool by Cuijpers et al. (2010) that was adapted by Smit et al. (2012). These guidelines were developed based on recommendations by Chambless and Hollon (1998) as well as on Cochrane guidelines for systematic reviews (Higgins & Green, 2009). The assessment includes nine items and is displayed in the supplementary material. Two blinded reviewers completed quality assessment based on information provided in publications and additional published material. Disagreements were resolved through discussion with a

third investigator. The overall quality of studies was determined as sum score of all ratings.

2.4 Statistical Analysis

All analyses were conducted using RStudio (version 2022.7.1.554; RStudio Team, 2022) and the metafor package (Viechtbauer, 2010; version 3.8-1). All effect sizes represent Hedges' *g*, which is more precise for small samples (Field & Gillett, 2010). Hedges' *g* was based on the differences between means at post-treatment or follow-up assessments divided by the pooled standard deviation at that assessment. Intent-to-treat data was preferred over completer data. Assuming heterogeneity between trials, we applied random effects models using the inverse-variance method for effect size calculation and the restricted maximum likelihood for estimating between-study heterogeneity (Field & Gillett, 2010; Thompson & Sharp, 1999; Viechtbauer et al., 2015). The inverse-variance method in random effects models assigns weights to studies according to their sampling variances and the estimated between-study variance. Larger studies with smaller variance are generally assigned more weight in the calculation of aggregated effects (Borenstein et al., 2010). Between-study heterogeneity was investigated using Cochran's test of heterogeneity and Higgins' I^2, and further considered by calculating 95% prediction intervals (PI; Cochran, 1954; Higgins & Thompson, 2002). These are presented for all results alongside the 95% confidence interval (CI) as they cover different relevant information regarding the application of results in clinical practice (Riley et al., 2011). While the CIs of effect sizes display the uncertainty in the estimates, PIs display the range of likely effects in individual settings. A CI not covering the null value provides confidence in the aggregated effect across studies, whereas a PI (by including an estimate of the true heterogeneity of effects between studies) not covering the null value provides confidence in beneficial treatment effects in an individual future setting. To investigate possible sources of heterogeneity, subgroup analyses and mixed-model meta-regression analyses were conducted. All analyses were calculated with a minimum of four trials to reduce risk of bias. The influence of each included trial on the overall effect size was evaluated by sensitivity analyses using the leave-one-out approach (Patsopoulos et al., 2008). Outlying studies were furthermore defined as estimates with 95% CIs not overlapping with the 95% CI of the aggregated effect

(Harrer et al., 2022). For all significant results, we tested for potential publication bias through visual assessment of funnel plots (for analyses including more than nine trials; Sterne et al., 2011). Furthermore, Egger's test and the trim- and fill procedure (Duval & Tweedie, 2000; Egger et al., 1997) were applied.

3 Results

The systematic database search revealed 229 unique studies of which eleven appeared eligible (see Figure 1 for a flow chart of the study selection process and the supplementary material for a list of excluded studies after full-text review with reasons for exclusion). Six RCTs included in Morina et al. (2017) were also included in the current meta-analysis, resulting in a total of 17 included trials. Introducing a minimum number of individuals being treated with ImRs led to the exclusion of one trial that was included in the previous meta-analysis (Nilsson et al., 2012). Unpublished raw data were used from Boterhoven de Haan et al. (2020) for all analyses because of the skewed distribution of data.

[FIGURE 1]

3.1 Characteristics of included studies

A total of 17 RCTs were included with 908 patients (417 in the ImRs condition). One study was published in German (Alliger-Horn et al., 2015), the remaining were published in English. The mean age of patients was 34.9 years and 70.8% were female. Most trials focussed on individuals suffering from PTSD (6 trials) or social anxiety (5 trials), followed by major depressive disorder (3 trials), eating disorders (2 trials), and generalised anxiety disorder (1 trial). Only one RCT reported no comorbidities, the remaining trials reported mainly comorbid depressive and anxiety disorders. On average, 31.7% of patients received concurrent psychopharmacological medication. One trial applied ImRs in a group setting (Hyett et al., 2018), one trial in a self-help format (Moritz et al., 2018), the remaining trials were conducted in individual settings (of which one trial applied individual writing sessions; Ovanessian et al., 2019). The trial by Moritz et al. (2018) included a brief as well as a long ImRs trial arm. To take account of the dependency of data, only the long ImRs trial arm was included in the meta-analysis. The average number of sessions was 4.3 (*SD* = 4.6; range 1-16) with a

mean duration of 79.6 minutes (*SD* = 20.7; range 37.5-105). Nine RCTs assessed the long-term efficacy of ImRs compared to control conditions with a mean follow-up duration of almost 9 weeks (range 4-12). One RCT conducted a second follow-up assessment after 52 weeks (Boterhoven de Haan et al., 2020). All but three trials applied ImRs as a stand-alone interventions, the remaining applied ImRs as enhancement for written exposure (Ovanessian et al., 2019), imaginal exposure (Øktedalen et al., 2015), and memory specificity training (Pile et al., 2021). Extensive cognitive preparation before the rescripting phase was applied in 29.4% of studies. It should be noted, however, that cognitive restructuring before the rescripting phase could be regarded as both independent intervention or cognitive preparation for rescripting. Details of included trials are presented in Table 1. The majority of publications (70.6%) reported on their funding sources, if applicable.

[TABLE 1]

3.2 Short-term treatment effects of ImRs on primary outcomes

Seven trials compared ImRs with a passive control condition yielding an overall effect of g = 0.68 (95% CI 0.18-1.18; 95% PI -0.57-1.93). Six of these trials included symptom severity of the mental disorder in focus as primary outcome, indicating that a greater symptom reduction was achieved in the ImRs condition. The effect was non-significant, however, when focusing only on anxiety disorders (k = 4; g = 0.82; 95% CI -0.08-1.72; 95% PI -1.07-2.71). A forest plot presenting the results is displayed in Figure 2. Two trials compared ImRs with an active control condition, namely non-directive supportive therapy (Pile et al., 2021) and supportive counselling (Lee & Kwon, 2013). Both trials found significant treatment effects of g = 0.88 for depressive symptoms (95% CI 0.33-1.43) and g = 1.36 for social anxiety symptoms (95% CI 0.45-2.28), respectively.

[FIGURE 2]

Eleven trials compared ImRs with another active psychological treatment (i.e., cognitive or exposure-based approaches and EMDR), yielding an overall effect of g = -0.01 (95% CI -0.18-0.15; 95% PI -0.18-0.15, see Figure 3). Ten of these trials included

symptom severity of the mental disorder in focus as primary outcome. Trim and fill analyses suggested two missing studies on the left side with an adjusted effect size of g = -0.09 (95% CI -0.25-0.08). Egger's test, however, did not indicate funnel plot asymmetry (p = .151). Considering only PTSD symptoms as primary outcome (k = 4), analysis revealed a non-significant effect of g = -0.15 (95% CI -0.38-0.08; 95% PI -0.38-0.08) compared to EMDR and exposure. Similar findings were obtained for anxiety outcomes compared to cognitive restructuring and exposure with g = 0.12 (95% CI -0.19-0.43; 95% PI -0.19-0.43). No significant differences in treatment efficacy were found for secondary depressive symptoms compared to other treatments (k = 5) with g = -0.20 (95% CI -0.43-0.02; 95% PI -0.43-0.02).

[FIGURE 3]

3.3 Long-term treatment effects on primary outcomes

Trials that compared ImRs with a passive or active control condition at follow-up were limited (k = 2 and k = 1, respectively). Whereas Jung and Steil (2013) found a significant treatment effect compared to a waitlist condition of g = 1.74 (95% CI 0.87-2.61) regarding contamination fear, Ovanessian et al. (2019) found no significant difference between written exposure with rescripting and a neutral control writing task regarding symptoms of generalised anxiety disorder (g = -0.25; 95% CI -0.79-0.30). Pile et al. (2021) found a significant effect of ImRs compared to non-directive supportive therapy for depressive symptoms (g = 0.57; 95% CI 0.03-1.10). The effect when comparing ImRs to other treatments remained non-significant after a mean follow-up duration of eight weeks following treatment (range 4-12 weeks; k = 6; g = 0.07; 95% CI -0.14- 0.28; 95% PI -0.14-0.28). Primary outcomes included symptoms of PTSD, depression, social anxiety disorder as well as generalised anxiety disorder (see Figure 4). In the only trial that included a later follow-up (Boterhoven de Haan et al., 2020), reduction in PTSD symptoms did not differ between ImRs and EMDR after one year post-treatment (g = -0.04; 95% CI -0.42-0.35).

[FIGURE 4]

3.4 Treatment effects on imagery-related and belief outcomes

Only two studies with different control conditions reported controlled effects for imagery-related outcomes. Lee and Kwon (2013) found a superiority of ImRs compared to supportive counselling regarding imagery distress (g = 3.74; 95% CI 2.38-5.10), imagery vividness (g = 1.81; 95% CI 0.83-2.78), and memory distress (g = 2.40; 95% CI 1.32-3.47). Norton and Abbott (2016) found no significant effects of ImRs compared to cognitive restructuring regarding imagery distress (g = 0.61; 95% CI -0.03-1.24) or imagery vividness (g = 0.49; 95% CI -0.14-1.12). Similarly, the authors found no significant effects of ImRs on imagery distress and vividness compared to a puzzle task (g = 0.58; 95% CI -0.06-1.21 and g = 0.52; 95% CI -0.11-1.15, respectively).

Four studies with different control conditions reported controlled effects for encapsulated belief outcomes. Both Reimer and Moscovitch (2015) and Lee and Kwon (2013) found a large treatment effect of ImRs compared to a waitlist condition (g = 0.92; 95% CI 0.10-1.75) or supportive counselling (g = 2.01; 95% CI 1.00-3.02), respectively. Dugué et al. (2019) differentiated between ratings of rational core beliefs (i.e., how much patients intellectually believed the meaning of their mental image was true) and emotional core beliefs (i.e., how much patients felt the meaning was true). Results at post-treatment indicate a non-significant effect of ImRs compared to cognitive restructuring regarding rational core beliefs (g = -0.58; 95% CI -1.24-0.09) and an inferiority of ImRs regarding emotional core beliefs (g = -0.82; 95% CI -1.50-0.14). Yet, individuals in the ImRs condition reported higher scores on the emotional core belief outcome at baseline, which is why ImRs displayed a larger uncontrolled effect size compared to cognitive restructuring. Finally, Cooper et al. (2007) included negative self-beliefs as their primary outcome and reported a similar efficacy of imagery rescripting compared to discussion of cognitions (g = 0.20; 95% CI -0.60-1.01).

3.5 Sensitivity and moderator analyses

Sensitivity analyses were conducted to examine whether effect sizes were driven by individual studies. Regarding controlled effect sizes, heterogeneity was substantial (I^2 = 77%). The aggregated effect size for ImRs vs. passive control conditions was mainly driven by one study (Hyett et al., 2018), whose elimination from analyses reduced the heterogeneity to I^2 = 15% and the aggregated effect size to g = 0.39 (95% CI 0.12-0.66). No outliers were detected for comparisons against other treatments or comparisons at follow-up. One RCT reported violation of the randomisation procedure

(Alliger-Horn et al., 2015), which is why the influence of this particular study on results was investigated. Results revealed no significant changes in overall results.

Moderator analyses were conducted for comparisons with passive controls and other treatments and for demographic as well as treatment-related moderators. Effect sizes were not influenced by the percentage of female participants in trials (p = .603 and p = .556 for passive controls and other treatment controls, respectively) or by the age of participants (p = .819 and p = .610). Results further revealed that the number of applied ImRs sessions did not explain heterogeneity among effect sizes (p = .703 and p = .097). Only a minority of trials applied extensive cognitive preparation before the start of ImRs. The application of extensive cognitive preparation, however, did not affect effect sizes (p = .763 and p = .521). There was moreover no significant difference between effect sizes from trials applying ImRs as a stand-alone intervention compared to those applying combined interventions (p = .558 and p = .924). Finally, effect sizes were not influenced by the type of the treated mental disorder (p = .817 and p = .369).

3.6 Study quality

The intraclass correlation coefficient (ICC) for all studies combined among the two raters of study quality was 0.96, 95% CI = 0.95-0.97, indicating excellent inter-rater reliability. Overall, study quality was mixed (see Table 2). The majority of included studies used (semi-)structured interviews to obtain a diagnosis for study inclusion (82%). Information on the independence of randomisation procedures were not provided in 59% of the studies. Treatments were manualised in the majority of studies (88%) and provided by trained therapists (80%, whereby this criterion did not apply for self-help interventions). No information was provided in 27% of studies on treatment integrity checks (excluding self-help interventions). Study and treatment dropouts were adequately reported in most studies (82%) and 63% of the studies provided data for intent-to-treat analyses. Finally, most trials applied self-report assessments to measure primary outcomes (65%) while the remaining employed blinded interviews.

Sensitivity analyses revealed that study quality had no impact on results (p = .374 and p = .376 for comparisons with passive controls and other treatments, respectively). To estimate whether missing data in primary trials had an influence on results, analyses were calculated to compare studies that provided intent-to-treat data

with those that only provided completer data. Results showed no significant difference between the two forms of trial (p = .311 and p = .692).

[TABLE 2]

4 Discussion

We conducted a meta-analysis of 17 trials that investigated the efficacy of ImRs compared to control conditions for individuals suffering from mental disorders associated with aversive memories. Results indicate that ImRs can significantly reduce different mental health complaints and that both short- and long-term treatment effects are comparable to those of other evidence-based psychological interventions. Most trials were conducted with individuals suffering from PTSD and social anxiety disorder, yet the overall number of RCTs remains limited.

Established psychological interventions that were compared to ImRs included (prolonged) exposure, cognitive restructuring, and EMDR. Notably, (prolonged) exposure and cognitive restructuring represent psychological interventions with a strong evidence base, whereas the evidence-base for long-term effects of EMDR is still thin (Hoppen, Jehn, et al., 2023). Results from our meta-analysis suggest no differences in the treatment efficacy between ImRs and the treatment controls. While this suggests that ImRs is as effective as other established treatments, several putative advantages of ImRs may be taken into account. Considering that some PTSD patients refuse to (extensively) relive the most aversive parts of traumatic events (as often applied in imaginal exposure), the necessity to elaborate the whole traumatic event has been debated. While research on this is scarce, existing results indicate that ImRs may be favoured by some patients. In a study by Dibbets and Arntz (2016), individuals who received ImRs reported less distress during the intervention compared to individuals who received imaginal exposure. In line with this, Arntz et al. (2007) reported a lower drop-out rate for ImRs compared to imaginal exposure, suggesting that ImRs might be more acceptable for some patients. However, while several studies and meta-analyses have investigated the drop-out rates for different psychological interventions and for different groups of patients (Fernandez et al., 2015; Hoppen et al., 2022; Varker et al., 2021), treatment attrition from ImRs has not yet been systematically investigated. Previous studies have reported rates of 4.2% (Reiss et al., 2017), 8.1% (Boterhoven de Haan et al., 2020), 19% (Raabe et al., 2022), or 25% (Arntz et al., 2007; note however

that here the majority of the treatment consisted of imaginal exposure). Further research is needed to directly compare attrition rates between ImRs and other interventions. An additional hypothesis included the superiority of ImRs regarding the reduction of secondary emotions such as anger, guilt, or disgust. Until now, findings on this are both limited as well as mixed and additional studies are required (Boterhoven de Haan et al., 2020; Dibbets & Arntz, 2016; Dugué et al., 2019; Kuck et al., 2023; Langkaas et al., 2017). Finally, considering the heterogeneity of psychological complaints in our meta-analysis, ImRs appears to be a promising transdiagnostic approach for a variety of mental disorders that are associated with aversive memories.

Secondary or comorbid depressive disorders are common among individuals with anxiety disorders (Lamers et al., 2011), PTSD (Stander et al., 2014), or eating disorders (Keski-Rahkonen & Mustelin, 2016) and are associated with chronicity of symptoms and increased symptom severity. Treatment efficacy beyond the primary diagnosis, but also regarding comorbid depressive symptoms is therefore of high relevance. Our results suggest that the effectiveness of ImRs in treating comorbid depressive symptoms is equivalent to that of other treatments, namely EMDR, cognitive restructuring, or prolonged exposure. This is a promising finding considering the significant treatment effects that were found in meta-analyses for the mentioned interventions in patients with PTSD (Ronconi et al., 2015) or social anxiety disorder (Kindred et al., 2022).

Trials on ImRs differ in terms of the degree to which extensive cognitive preparation is applied. It has been argued that the use of cognitive restructuring before the rescripting phase might be helpful, given that patients might not be aware of their dysfunctional interpretation of key memories (Arntz, 2012). Our moderator analysis found no influence of extensive cognitive preparation on the treatment efficacy, suggesting that ImRs does not require a preceding cognitive preparation. In line with this, a recent trial comparing cognitive restructuring and imagery rescripting with a therapist attention placebo and imagery rescripting found no differences in the reduction of believability in encapsulated beliefs (Voncken et al., 2023). Similarly, treatment effects did not appear to differ between applications of ImRs as a stand-alone intervention or in combination with other treatment approaches.

Previous research suggests that the working mechanisms of ImRs compared to imagery exposure differ (Kunze et al., 2019). Changing the meaning of aversive memories is at the core of ImRs, suggesting that changes in memory-derived erroneous

beliefs might be one possible underlying process. Encapsulated beliefs about the self, others, and the world may lead to perceived threat and thus contribute to the maintenance of mental disorders. Pre-existing results from RCTs indicate large effects of ImRs on changes in encapsulated beliefs compared to passive or active control conditions and similar effects compared to cognitive restructuring (Dugué et al., 2019; Knutsson et al., 2020; Lee & Kwon, 2013; Reimer & Moscovitch, 2015; Romano et al., 2020). Yet, opposing results exist that yield null findings for an effect of ImRs on core beliefs (Kadriu et al., 2023). While most theories on working mechanisms underlying ImRs focus on changes in meaning and encapsulated beliefs, other possible mechanisms include changes in memory vividness or distress that might underly treatment effects. These theories are supported by different studies showing that ImRs effectively influences these memory aspects (Lee & Kwon, 2013; Lloyd & Marczak, 2022). However, due to the limited number of studies including encapsulated belief or imagery-related outcomes, we were not able to run meta-analyses on possible working mechanisms. Thus, future research needs to further investigate the distinct working mechanisms of ImRs.

4.1 Strengths and limitations

By including only RCTs, this update meta-analysis provides reliable estimates on the efficacy of ImRs for different psychological complaints. Nonetheless, we note several limitations. Most importantly, the number of RCTs investigating the efficacy of ImRs is still limited. This applies especially to trials on ImRs as a stand-alone intervention. This results in an increased impact of individual studies on the aggregated effect sizes. Excluding one outlying study from the analysis on the efficacy of ImRs compared to passive control conditions at post-treatment resulted in a decrease of the effect size. Yet, the remaining heterogeneity between studies diminished to a negligible size, underlining the consensus of the remaining studies on the efficacy of ImRs. Furthermore, the significance of results did not appear to be grounded on studies that reported completer data only (i.e., reporting data only on patients who completed treatment and the assessments). Overall, however, studies were very heterogeneous in terms of implementation of ImRs (i.e., group vs. individual settings vs. self-help, single-session vs. multiple sessions, stand-alone vs. combined), choice of control conditions (i.e., passive control conditions, active control conditions, or other treatments), or

targeted psychological complaints or mental disorders. That way, only subgroup analyses on primary PTSD symptoms, overall anxiety symptoms, and secondary depressive symptoms were possible. The quality of included RCTs was mixed whereby major shortcomings were noted regarding independent randomisation, treatment integrity checks, and the use of intent-to-treat data for analyses. Given the limited number of studies, insufficient analyses on potential risk of bias (e.g., publication bias) could be calculated, which limits the certainty in results.

4.2 Implications for clinical practice and research

From a clinical perspective, the equivalence of treatment efficacy of ImRs relative to other established interventions displays the most important finding. Findings from the calculated PIs suggest that there may be just as many settings in which ImRs is slightly more beneficial than other interventions as there are settings in which other established interventions are slightly more beneficial than ImRs. As such, ImRs appears to be a suitable alternative for patients with aversive memories who are not willing to undergo exposure or do not benefit enough from exposure. Future research should investigate which approach is most effective for whom, whereby our results indicate that gender and age are no significant moderators of treatment efficacy. People moreover differ in their general tendency and ability to recall or create vivid images. Previous studies on phobias (Hunt & Fenton, 2007) or social anxiety (Lee & Kwon, 2013) did not find mental imagery ability to be a predictor of treatment efficacy. However, in these studies imagery ability was only assessed using self-report measures. Future studies should incorporate more objective measures of imagery ability and capacity to further investigate the relationship between imagery ability and efficacy of ImRs.

The mean number of ImRs sessions in the included studies was only 4.3, suggesting that clinically relevant improvements of symptoms can be reached after only a few sessions. In light of common comorbidities among mental disorders, the efficacy of ImRs across different psychological complaints appears moreover beneficial. However, many mental disorders were only covered by a very limited number of studies, which highlights the need for future research. A non-significant effect was found for anxiety symptoms when compared to passive controls. The smallest effect was thereby contributed by the only study investigating generalised anxiety disorder. More trials are needed to draw conclusions on the efficacy of ImRs for distinct anxiety

disorders. Additional trials are further needed to investigate the efficacy of ImRs as a stand-alone intervention for different psychological complaints. Future trials may furthermore investigate the processes underlying ImRs. Given that ImRs appears as effective as exposure and previous results on differences in the treatment of non-fear-related emotions are both limited and inconclusive, the added value of ImRs should be the focus of future research. Related to this, further study of extensive cognitive preparation of the rescripting is necessary. Direct comparisons are needed to investigate what the most effective approach is and whether similar mechanisms of change are involved.

4.3 Conclusions

The results of our meta-analysis suggest that ImRs is a promising transdiagnostic treatment approach for psychological complaints associated with aversive memories. Significant symptom reduction was achieved despite a limited number of applied sessions and effects appeared to be stable over time. Results further suggest that ImRs achieves comparable treatment effects as other established interventions such as (prolonged) exposure, cognitive restructuring, and EMDR and appears to be a suitable alternative to these interventions. Future research needs to further examine treatment efficacy and mechanisms of ImRs.

This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

Declarations of interest: None.

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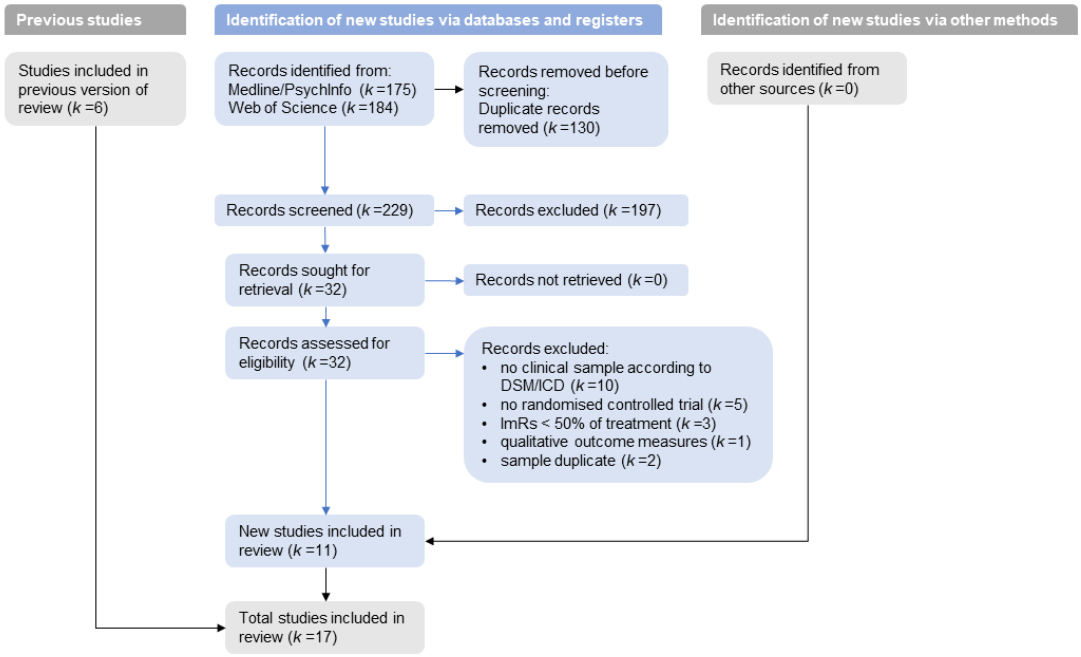
Figure 1

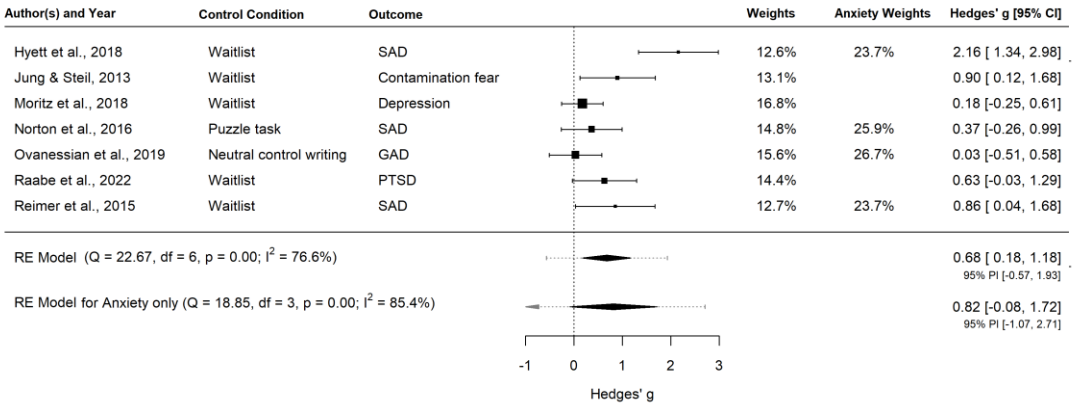
PRISMA flow chart of the study selection process



Note. DSM = Diagnostic and Statistical Manual of Mental Disorders; ICD = International Classification of Diseases; ImRs = Imagery rescripting.

Figure 2

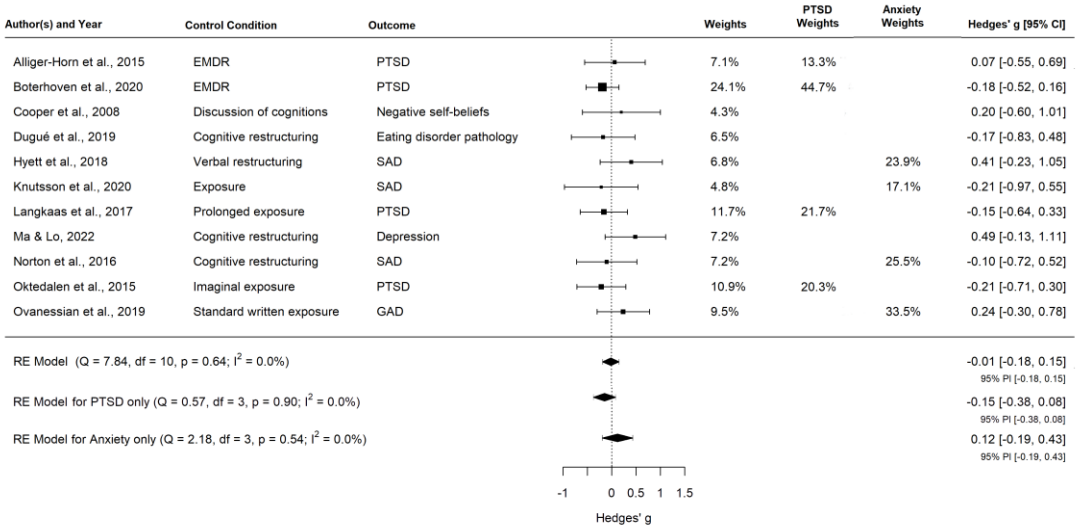
Forest plot of treatment effects compared to passive control conditions at post-treatment



Note. CI = confidence interval; GAD = generalised anxiety disorder; PI = prediction interval; PTSD = posttraumatic stress disorder; SAD = social anxiety disorder.

Figure 3

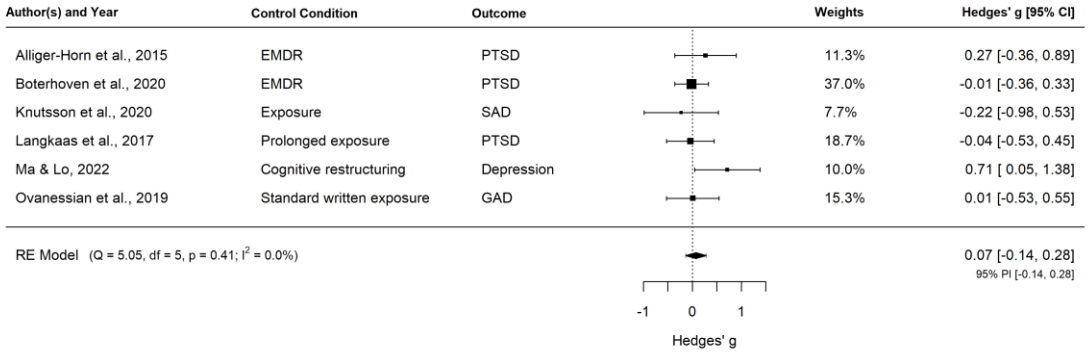
Forest plot of treatment effects compared to other active psychological treatments at post-treatment



Note. CI = confidence interval; EMDR = eye movement desensitization and reprocessing; GAD = generalised anxiety disorder; PI = prediction interval; PTSD = posttraumatic stress disorder; SAD = social anxiety disorder.

Figure 4

Forest plot of treatment effects compared to other active psychological treatments at follow-up



Note. CI = confidence interval; EMDR = eye movement desensitization and reprocessing; GAD = generalised anxiety disorder; PI = prediction interval; PTSD = posttraumatic stress disorder; SAD = social anxiety disorder.

Table 1

Overview of the included studies

Disorder	Study, type of treatment (# of sessions, duration)	N*	Primary outcome	Secondary outcome	Imagery-related and belief outcome	Follow-up	Extensive cognitive preparation?	ImRs as stand-alone intervention?	Mean age (SD)	% female	ITT vs. Comp
PTSD	Alliger-Horn et al., 2015										
	ImRs (3, 90-100min)°	18	PDS	BDI	n.a.	3 months	No	Yes	38.1	9.1	ITT
	EMDR (3, 90-100min)°	22							(8.0)		
	Boterhoven de Haan et al., 2020										
	ImRs (12, 90min)	66	CAPS	BDI	n.a.	2 months,	No	Yes	38.5	76.8	ITT
	EMDR (12, 90min)	68				1 year			(11.2)		
	Jung & Steil, 2013										
	ImRs (2, 90min)	14	FBC	BDI	n.a.	1 month	Yes	Yes	37.2	100	ITT
	Waitlist (n.a.)	14		PDS					(10.9)		
	Langkaas et al., 2017										
	ImRs (10, 90-120min)	34	PSS-I	BDI	n.a.	3 months	Yes	Yes	45.2	58.0	ITT
	Prolonged exposure (10, 90-120min)	31							(9.7)		
	Øktedalen et al., 2015										
	ImRs (7 + 3 IE, 40-60min)	32	PSS-I	n.a.	n.a.	n.a.	Yes	No	45.2	56.9	Comp
	Imaginal exposure (10, 40-60min)	29							(9.7)		

	Raabe et al., 2022										
	ImRs (16, 90min)	17	CAPS	BDI	n.a.	n.a.	No	Yes	35.5	88.0	Comp
	Waitlist (n.a.)	20							(n.r.)		
SAD	Hyett et al., 2018										
	ImRs (1, 90min, group)	17	SIPS	n.a.	n.a.	n.a.	No	Yes	35.2	67.0	ITT
	Verbal restructuring (1, 90min, group)	22							(15.0)		
	Waitlist (n.a.)	19									
	Knutsson et al., 2020										
	ImRs (1, 90min)	14	LSAS-SR	n.a.	r.b.i.	1 month	No	Yes	25.0	59.3	ITT
	Exposure (1)	13							(3.8)		
	Lee & Kwon, 2013										
	ImRs (3, 60-120min)	13	K-BFNE		encapsulated beliefs, imagery distress, imagery vividness, memory distress	3 months	Yes	Yes	23.9	61.5	ITT
	Supportive counselling (3, 60min)	10							(3.4)		
	Norton & Abbott, 2016										
	ImRs (1, 30-45min)	20§	SIAS	DASS-21-D	imagery distress,	n.a.	No	Yes	20.8	85.0	n.r.
	Cognitive restructuring (1, 30-45min)	20§							(4.0)		
		20§									

	Puzzle task (1, 30-45min)				imagery vividness						
	Reimer & Moscovitch, 2015										
	ImRs (1, 90min)	13	LSAS-SR	n.a.	encapsulated beliefs	n.a.	No	Yes	19.5 (1.3)	70.0	ITT
	Waitlist (n.a.)	12									
MDD	Ma & Lo, 2022										
	ImRs (3, 60-90min)	20	BDI	n.a.	n.a.	2 months	No	Yes	44.6 (12.0)	90.2	Comp
	Cognitive restructuring (3, n.r.)	21									
	Moritz et al., 2018										
	ImRs – long (self-help)	42	BDI	GAD-7	n.a.	n.a.	No	Yes	43.1 (n.r.)	67.7	ITT
	Waitlist (n.a.)	42									
	Pile et al., 2021										
	ImRs (4, 90min)	29	MFQ	SCARED	n.a.	4 months	Yes	No	17.0 (0.6)	60.7	ITT
	Non-directive supportive therapy (4, 90min)	27									
BN / BED	Cooper et al., 2007										
	ImRs (1, n.r.)	12	Negative self-beliefs†	Depression, Anxiety Urge to binge/restrict †	n.a.	n.a.	n.r.	Yes	25.6 (n.r.)	100	n.r.
	Discussion of cognitions (1, n.r.)	12									
	Dugué et al., 2019										
	ImRs (1, 41min)	18	EDE-Q	n.a.		n.a.	No	Yes	43.1 (10.6)	75.0	ITT
		18									

	Cognitive restructuring (1, 41min)				rational and emotional core beliefs						
GAD	Ovanessian et al., 2019										
	Written exposure + ImRs (3, 30min)	26 27	GAD-Q-IV	n.a.	n.a.	1 month	No	No	27.3 (7.6)	82.3	Comp
	Written exposure (3, 30min)	26									
	Neutral control writing (3, 30min)										

Note. BDI= Beck Depression Inventory; BN= Bulimia nervosa; CAPS= Clinician Administered PTSD Scale; Comp = completer data; DASS-21-D= Depression, Anxiety, and Stress Scale; EDE-Q= Eating Disorder Examination Questionnaire; EMDR= Eye movement desensitization and reprocessing; FBC= Feeling of being contaminated; GAD= Generalised anxiety disorder; K-BFNE= Brief Fear of Negative Evaluation Scale; lmRs= Imagery rescripting; ITT = intent-to-treat data; LSAS-SR= Liebowitz Social Anxiety Scale and Self Report; MDD= Major depressive disorder; MFQ= Mood and Feelings Questionnaire; n.a.= not applicable; n.r.= not reported; PDS= Posttraumatic Diagnostic Scale; PSS-I= The PTSD Symptom Scale Interview; PTSD= Posttraumatic stress disorder; r.b.i. = reported, but irrelevant; SAD= Social anxiety disorder; SCARED= Screen for Child Anxiety Related Disorders; SIAS= Social Interaction Anxiety Scale; SIPS= Social Interaction Phobia Scale; SPS= Social Phobia Scale.
* as used for post-treatment analyses for their primary outcome
§ no information provided in publication on how many participants were randomised to each condition (information could not be obtained from authors).
† Likert scales 0-100 developed for the purpose of the study
° both groups received an additional three sessions (50min each) of stabilisation

Table 2

Quality Ratings of the included studies

Study	1. Diagnosis	2. Manual	3. Therapist Training	4. Treatment Integrity	5. ITT Analyses	6. Randomisation	7. Independent Randomisation	8. Blinding*	9. Drop-Outs	Overall Score§
Alliger-Horn et al., 2015	2	3	3	2	3	2	0	n.a.	3	**18**
Boterhoven de Haan et al., 2020	2	3	3	3	1	3	3	3	3	**24**
Cooper et al., 2007	2	3	0	0	0	3	0	n.a.	0	**8**
Dugué et al., 2019	2	3	0	0	3	3	0	n.a.	1	**9**
Hyett et al., 2018	2	3	3	2	3	3	3	3	2	**24**
Jung & Steil, 2013	3	1	3	2	2	3	0	3	3	**20**
Knutsson et al., 2020	1	3	3	2	3	3	1	n.a.	3	**19**
Langkaas et al., 2017	3	3	3	2	3	3	3	3	3	**26**
Lee & Kwon, 2013	2	3	3	2	3	3	3	n.a.	3	**22**
Ma & Lo, 2022	2	3	3	0	1	3	3	n.a.	3	**18**
Moritz et al., 2018	0	2	n.a.	n.a.	3	3	3	n.a.	3	**14**
Norton & Abbott, 2016	3	3	3	3	0	3	3	n.a.	3	**21**
Øktedalen et al., 2015	3	3	3	3	1	3	3	n.a.	3	**24**

Ovanessian et al., 2019	2	1	n.a.	n.a.	1	3	0	n.a.	3	**10**
Pile et al., 2021	1	2	3	3	3	3	3	3	3	**24**
Raabe et al., 2022	2	3	3	3	1	3	3	3	3	**26**
Reimer & Moscovitch, 2015	2	3	0	0	3	3	0	n.a.	3	**14**

Note. ITT = intent-to-treat; n.a. = not applicable.

* n.a.'s were given if self-report data was used

§ for moderator analyses, n.a.'s were treated as 3 given that no shortage of quality can be assumed if items are not applicable

Supplementary Table S1

Deviations from the protocol

PROSPERO Protocol	Deviation from Protocol	Reason for deviation
Minimum of 5 patients treated with imagery rescripting in each trial (as applied in Morina et al., 2017)	A minimum of ten patients were treated with ImRs	The original criterion was motivated by the limited number of studies. However, including primary studies with small sample sizes increases the risk of biases in overall estimates and confidence intervals (Lin, 2018)

This deviation led to the exclusion of one randomised controlled trial that was originally included in Morina et al., 2017:

Nilsson, J. E., Lundh, L. G., & Viborg, G. (2012). Imagery rescripting of early memories in social anxiety disorder: An experimental study. *Behaviour Research and Therapy, 50*(6), 387-392.

Supplementary Table S2

Quality coding tool

	Items for quality rating	Answers
1.	Diagnosis with semi-structured diagnostic interview (SCID-2, etc.)	3. YES, with ≥ adequate ICC/kappas are reported 2. YES, without adequate ICC/kappas 1. NO 0. Unknown
2.	Use of treatment manual (in case a specific treatment was investigated)	3. YES, manual published / online 2. YES, but manual not published 1. NO 0. Unknown -1 NA (e.g., TAU)
3.	Therapists were trained either specifically for the study or in a general training	3. YES 2. Unclear, but clearly experts 1. NO 0. Unknown -1 NA (e.g., TAU)
4.	Treatment integrity was checked (by supervision and/or recordings and/or standardized instruments)	3. YES, by independent raters 2. YES by supervision 1. NO 0. Unknown -1 NA (e.g., TAU)
5.	Data analyzed with intent-to-treat analysis	3. YES 2. YES, but partially violated by inappropriate exclusion of some individuals (e.g., exclusion of (very) early dropouts) 1. NO 0. Unknown
6.	Randomized study	3. YES 2. YES, but randomization violated (e.g., crossover to other condition) 1. NO

		0. Unknown
7.	If randomized, by independent 3rd person (or computer or sealed envelopes)	3. YES 1. NO 0. Unknown -1 NA
8.	Blinded assessors for interviews	3. YES, blinded assessors 2. independent but not (100%) blind 1. NO 0. Unknown -1. NA
9.	Dropouts adequately reported	3. YES (distinguishing treatment & study dropouts) 2. YES, but not distinguishing type of dropouts 1. NO 0. Unknown -1. NA (e.g., retrospective study)

Supplementary Table S3

List of excluded studies after full-text review with reason for exclusion

Authors	Title	Year	Reason for exclusion
Ahn & Kwon	Modifying negative self-imagery increases the effectiveness of cognitive behavior therapy for social anxiety disorder: A benchmarking study	2018	No RCT
Assmann et al.	Differential Effects of Comorbid Psychiatric Disorders on Treatment Outcome in Posttraumatic Stress Disorder from Childhood Trauma	2021	Sample duplicate (Boterhoven de Haan et al., 2020)
Clarke et al.	A randomised multiple baseline case series of a novel imagery rescripting protocol for intrusive trauma memories in people with psychosis.	2022	No RCT
Cooper et al.	A novel experimental investigation of online imagery rescripting for obsessive-compulsive prospective imagery	2023	No clinical sample according to DSM or ICD
Fink et al.	Changing disgust through imagery rescripting and cognitive reappraisal in contamination-based obsessive-compulsive disorder	2018	Less than 50% of treatment is ImRs
Fink-Lamotte et al.	Mechanisms and effectiveness of imagery strategies in reducing disgust in contamination-related obsessive-compulsive disorder: comparing imagery rescripting, imagery self-compassion and mood-focused imagery	2021	No RCT
Ghaderi et al.	Imagery rescripting for reducing body image dissatisfaction: A randomized controlled trial	2022	No clinical sample according to DSM or ICD
Kadriu et al.	Imagery rescripting of autobiographical memories versus intrusive images in individuals with disordered eating	2021	No clinical sample according to DSM or ICD
Kuck et al.	Intraindividual variability and emotional change as predictors of sudden gains in imagery rescripting and EMDR for PTSD	2023	Sample duplicate (Boterhoven de Haan et al., 2020)

	in adult survivors of childhood abuse.		
Landkroon et al.	The effect of imagery rescripting on prospective mental imagery of a feared social situation.	2022	No clinical sample according to DSM or ICD
Lee et al.	Online guided imagery in traumatic memory processing for at-risk complex ptsd adults	2020	No clinical sample according to DSM or ICD
Nilsson et al. (RCT from previous meta-analysis)	Imagery rescripting of early memories in social anxiety disorder: An experimental study	2012	Less than 10 participants receiving ImRs
Rahnama et al.	Effectiveness of Imagery Rescripting and Reprocessing Therapy on Insomnia, Nightmare and Suicide Ideation in Depressed Persons with Suicide Attempt History	2016	No clinical sample according to DSM or ICD
Reiss et al.	Effects of cognitive behavioral therapy with relaxation vs. imagery rescripting on test anxiety: A randomized controlled trial	2017	Less than 50% of treatment is ImRs
Reiss et al.	Effects of cognitive-behavioral therapy with relaxation vs. imagery rescripting on psychophysiological stress responses of students with test anxiety in a randomized controlled trial.	2019	Less than 50% of treatment is ImRs
Romano et al.	The effects of imagery rescripting on memory outcomes in social anxiety disorder	2020	qualitative outcome measures
Strohm et al.	Imagery rescripting of aversive autobiographical memories: Effects on memory distress, emotions, and feelings of mastery	2019	No clinical sample according to DSM or ICD
Strohm et al.	Psychological and physiological effects of imagery rescripting for aversive autobiographical memories	2019	No clinical sample according to DSM or ICD
Taylor et al.	IMAgery focused psychological therapy for persecutory delusions in PSychosis (iMAPS): a multiple baseline experimental case series.	2020	No RCT

Twardawski et al.	Imagery rescripting helps victims cope with experienced injustice	2021	No clinical sample according to DSM or ICD
Uhl et al.	Interpersonal processes during imagery rescripting Association between physiological synchrony and emotional processing	2022	No RCT
Woelk et al.	Imagery rescripting versus extinction: Distinct and combined effects on expectancy and revaluation learning	2022	No clinical sample according to DSM or ICD
Full-text review of studies identified through other sources			
Asarian et al.	The effectiveness of rescripting of pain mental imagery on pain intensity in patients with chronic pain	2018	No clinical sample according to DSM or ICD
Gehring	Single-session imagery rescripting for social anxiety disorder: Efficacy and mechanisms.	2014	Sample duplicate (Reimer & Moscovitch, 2015)
Giacobbi et al.	Guided imagery for arthritis and other rheumatic diseases: A systematic review of randomized controlled trials	2015	No clinical sample according to DSM or ICD
Hunt & Fenton	Imagery rescripting versus in vivo exposure in the treatment of snake fear	2007	No clinical sample according to DSM or ICD
Marsden et al.	Memory-focused cognitive therapy for cocaine use disorder: Theory, procedures and preliminary evidence from an external pilot randomised controlled trial	2018	Less than 50% of treatment is ImRs
Strohm et al.	Imagery Rescripting Versus Cognitive Restructuring for Social Anxiety: Treatment Effects and Working Mechanisms	2021	No clinical sample according to DSM or ICD