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A meta-analysis

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Imagery rescripting as a clinical intervention for aversive memories: A meta-analysis

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ABSTRACT

Background and objectives: Literature suggests that imagery rescripting (ImRs) is an effective psychological intervention.
Methods: We conducted a meta-analysis of ImRs for psychological complaints that are associated with aversive memories. Relevant publications were collected from the databases Medline, PsychInfo, and Web of Science.
Results: The search identified 19 trials (including seven randomized controlled trials) with 363 adult patients with posttraumatic stress disorder (eight trials), social anxiety disorder (six trials), body dysmorphic disorder (two trials), major depression (one trial), bulimia nervosa (one trial), or obsessive compulsive disorder (one trial). ImRs was administered over a mean of 4.5 sessions (range, 1–16). Effect size estimates suggest that ImRs is largely effective in reducing symptoms from pretreatment to posttreatment and follow-up in the overall sample (Hedges' $g = 1.22$ and 1.79, respectively). The comparison of ImRs to passive treatment conditions resulted in a large effect size ($g = 0.90$) at posttreatment. Finally, the effects of ImRs on comorbid depression, aversive imagery, and encapsulated beliefs were also large.
Limitations: Most of the analyses involved pre-post comparisons and the findings are limited by the small number of randomized controlled trials.
Conclusions: Our findings indicate that ImRs is a promising intervention for psychological complaints related to aversive memories, with large effects obtained in a small number of session.
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1. Introduction

Imagery rescripting (ImRs) has been used as a therapeutic intervention either in combination with other treatments (particularly within cognitive-behavior therapy) or alone for a variety of psychological complaints that are associated with aversive memories (Edwards, 2007). This form of intervention consists of a set of therapeutic procedures applied to modify the content of preexisting unpleasant memories into more benign images or to use new positive images to rescript negative schematic beliefs (Holmes, Arntz, & Smucker, 2007). Herein, aversive memories are activated and thereupon emotional and cognitive features of the mental representation of aversive stimuli are changed and potentially reconsolidated. For example, a client with symptoms of posttraumatic stress disorder (PTSD) following sexual assault might rescript her aversive memory into an image that portrays her as successfully defending herself against the assailant. A rather puzzling finding among clients undergoing ImRs is that although clients are aware of the fact that the rescripted image is not the accurate representation of the original event, they still report that the new image better meets their current emotional needs. It has been suggested that the underlying working mechanism of ImRs might be the change in meaning of the representation of the negative valence of aversive stimuli (Arntz, 2012). Accordingly, instead of weakening the association between the conditional stimulus (CS) and unconditional stimulus (US) as often done in exposure therapy, ImRs is proposed to devalue or reevaluate US memories directly and thus reduce CS-elicited affect. This notion is in line with accumulating findings that memories can be changed after storage during a process labeled as reconsolidation (Schwabe, Nader, & Pruessner, 2014). Although there is some preliminary evidence to support this notion of the change in meaning of the representation of the negative valence of aversive stimuli (Dibbets, Poort, & Arntz, 2012; Hagenaars & Arntz, 2012), a thorough investigation of the working mechanisms of ImRs remains open.

Several trials have reported that ImRs can effectively reduce symptoms associated with aversive memories. Arntz (2012) published a narrative review of intervention studies applying ImRs either as part of an another treatment package (12 trials) or as a stand-alone intervention (seven trials). Arntz concluded that the existing publications provide promising results regarding the efficacy of ImRs. Several clinical trials on ImRs have been published following the systematic and narrative review completed by Arntz in 2011. Therefore, we aimed at conducting an updated systematic review and meta-analysis of clinical trials on ImRs to assess its efficacy in reducing levels of psychopathology related to aversive memories.

2. Method

2.1. Identification and selection of studies

The aims and methods of this meta-analysis were registered with the PROSPERO database (CRD42016032451, http://www.crd.york.ac.uk/prospero). We defined the main structured research question describing the Population, Intervention, Comparison, Outcome, and Study design (PICOS) in accordance with the recommendations by the Preferred Reporting Items for Systematic Reviews and Meta-analysis (PRISMA) group (Moher, Liberati, Tetzlaff, Altman, & PRISMA Grp, 2009). The question was "In patients with psychological symptoms associated with aversive memories (P), does imagery rescripting (I), in within-group and between-group comparisons (C), improve symptoms (O) in clinical trials (S)?" The criteria for including trials into the current meta-analysis were: 1) ImRs consisted of at least 50% of the applied treatment, 2) treatment targeted psychological complaints reported as a result of aversive memories in patients with a mental disorder; and 3) at least five clients were treated with ImRs. If a publication did not provide enough data to calculate effect-sizes, its authors were contacted by e-mail to retrieve the data. We excluded publications on the efficacy of imagery rehearsal for nightmares because its efficacy has been reported in two recent meta-analyses (Casement & Swanson, 2012; Hansen, Hofling, KronerBorowik, Stangier, & Steil, 2013). An additional reason for excluding trials on imagery rehearsal for nightmares was related to the aim of our meta-analysis to include trials that apply imagery rescripting to treat symptoms associated with memories of real aversive experiences. Whereas nightmares may develop following exposure to aversive experiences, the content of the nightmares might not represent memories of real aversive experiences. No restrictions were made upon publication language, year of publication, length of reported follow-up, or age of participants.

We searched the databases MEDLINE, PsycINFO, and Web of Science for relevant publications. The last search was conducted on March 24th, 2016 and included the following search terms: "*imagery rescripting or updating memory or imagery modification or imaginal reliving*". Following the search in the bibliographic databases, reference lists from articles that met inclusion criteria for the meta-analysis were examined. Finally, the following registers of controlled trials were searched: Australian New Zealand Clinical Trials Registry, Chinese Clinical Trial Register, Clinical Trials, Clinical Trials Registry- India, German Clinical Trials Register, ISRCTN Register, Netherlands Trial Register, and UMIN Clinical Trials Registry.

2.2. Quality assessment

Coding for the quality of studies was based on the quality analysis constructed by Cuijpers, van Straten, Bohlmeijer, Hollon, and Andersson (2010) and adjusted by Smit et al. (2012). The quality of the studies was coded based on the following questions: *Was the diagnosis determined using a semi-structured interview?, Was a treatment manual used?, Were therapists trained either specifically for the study or in a general training?, Was treatment integrity checked by supervision and/or recordings and/or standardized instruments?, Was data analyzed with intent-to-treat analysis?, Was it a randomized study?, Was randomization done by an independent third person (or computer or sealed envelopes)?, Were blinded assessors used for interviews?*, and *Were dropouts adequately reported?* Items

were scored on a four-point scale, where 3 indicates high quality (e.g., a published treatment manual was used), 2 indicates limited quality (e.g., an unpublished treatment manual was used), 1 indicates lack of required quality (e.g., no treatment manual was used), and 0 indicates unknown. Furthermore, if a given item was not applicable, than the score −1 was used. In the current meta-analysis, a score of −1 was used while rating the item "blinded assessment" if treatment outcome was assessed by self-reports and "independent randomization" if the trial was not randomized. The first and second authors independently rated all studies based on what was reported in the included publications.

2.3. Coding of treatment characteristics and effect size calculation

Using a self-developed codebook, the following information was extracted from each study included in the meta-analysis: sample size, mean age, mental disorder, type of intervention(s), number of sessions, intervention format (individual or group), control condition, statistical analysis (completer or intent to treat), number of drop-outs, and measurements. We first coded all treatment conditions as either ImRs or control conditions. Then, control conditions were further specified as active or inactive. Finally, we coded assessment characteristics as primary and secondary outcome measures. We coded a measurement as primary outcome if it corresponded with the assessment of what the authors had reported as main treatment target. If, however, more than one instrument was used to assess the main treatment target (e.g., symptoms of social anxiety disorder), we aimed at using the data from the instrument that was most often reported across the included trials. With regard to secondary outcome, we coded the assessment of comorbid depressive symptoms as well as factors assumed to be related to aversive images, which would be image or memory related variables (e.g., image or memory distress). Finally, if a publication reported as treatment target the change in some other variable than symptoms of the mental disorder that represented the sample, we coded the assessment of symptoms of the mental disorder in question as secondary outcome. One example for this would be the study by Jung and Steil (2013) that primarily aimed at reducing feeling of being contaminated among patients with PTSD. With regard to this trial, we coded feeling of being contaminated as primary outcome measure and symptoms of PTSD as secondary outcome measure.

We first computed uncontrolled effect sizes (e.g., change from pre-to posttreatment; change from pretreatment to follow-up) for ImRs conditions. It should be noted, however, that this approach need to be considered as an inferior type of evidence as potential changes from pre-to posttreatment might result not only from the applied treatment but also from factors not specifically related to the treatment in question or passage of time alone. Second, controlled effect sizes were computed for all trials that compared ImRs with active or inactive treatment groups. Within- and between-group effect sizes were computed using a modified version of Hedge's *g* that provides a better estimate of the effect size based on small samples than Cohen's *d* (Field & Gillett, 2010). This was obtained by first subtracting the pretreatment mean from the posttreatment mean or follow-up mean (uncontrolled effect size) or the control group mean from the treatment group mean at post-treatment (controlled effect size) respectively and dividing the outcome by the pretreatment standard deviation. We chose to substitute the standard deviations at post-treatment and follow-up with the pretreatment standard deviation as literature suggests that baseline standard deviations provide the least biased estimate of a clinical population (Feingold, 2009; Morris, 2008). The outcome was then multiplied by a sample size correction factor $J = 1\text{-}(3/(4df - 1))$ to obtain the effect size Hedges's *g* (Lipsey & Wilson, 2001). Effect size *g* can conservatively be interpreted using suggestions by Cohen (1988), with 0.2 indicating a small, 0.5 a medium, and 0.8 a large effect, respectively. Furthermore, we used random effects model to calculate effect sizes given the heterogeneity of the studies (Field & Gillett, 2010). Potential publication bias was assessed for the primary outcome measures through visual inspection of the funnel plot (for analyses including more than nine trials, see Sterne et al., 2011) by examining the relation between effect- and standard error with relatively higher effect sizes of smaller studies being an indicator for publication bias (Sterne et al., 2011). Additionally, we calculated the likely number of missing studies using the trim-and fill procedure (Duval & Tweedie, 2000), which yields an estimate of the effect size after publication bias has been taken into account. We further conducted meta-regression to examine whether the observed heterogeneity could be explained

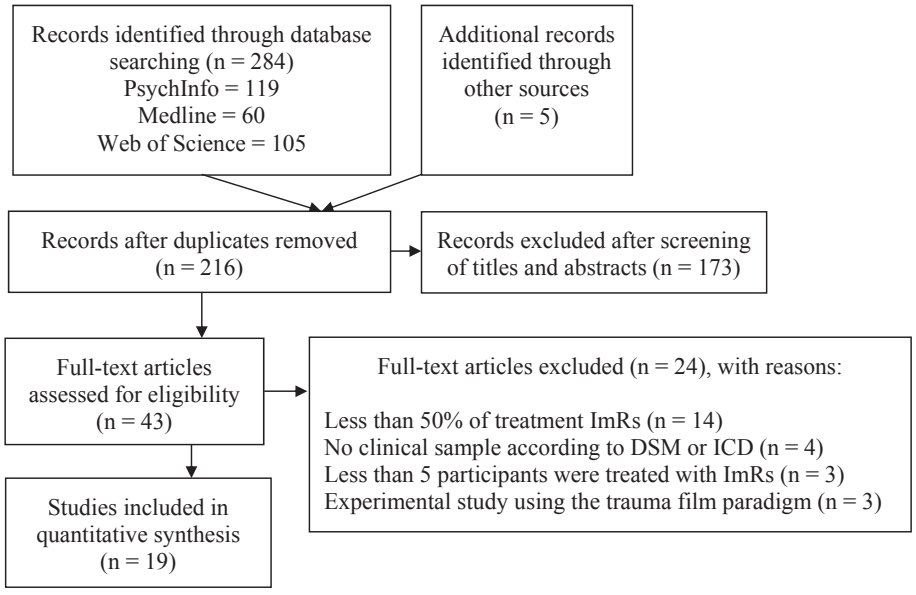

Fig. 1. Flow diagram of study selection process.

Table 1
Overview of the included studies.

Disorder	Study and type of treatment	# of sessions	N*	Age Mean (SD)	Primary outcome	Secondary outcome	Follow-up	Extensive cognitive prep-aration?	ImRs as stand-alone intervention?	Design
PTSD	Alliger-Horn et al. (2015)									
	ImRs	3	18	38.1 (8.0)	PDS	BDI	6 months	No	Yes	RCT
	EMDR	3	22							
	Arntz, Sofi, & van Breukelen (2013)									
	ImRs	10	10	39.9 (12.2)	PSS-I	BDI	3 months	No	Yes	Case series
	Grunert, Weis, Smucker, & Christianson, 2007									
	ImRs	1 to 3	23	Range 20-47	IES	BDI	6 months	No	Yes#	Open trial
	Jung & Steil, 2013									
	ImRs	2	14	37.1 (10.9)		BDI		Yes	Yes	RCT
	Waitlist	n.a.	14		FBC	PDS	1 month			
	Kindt et al., 2007									
	ImRs	10	25	33.0 (10.1)	PSS-SR	n.a.	1 month	No	ImRs + exposure	Open trial
	Oktedalen et al., 2015									
	ImRs	10	33	45.2 (9.7)	PSS-I	n.a.	n.a.	Yes	Yes	RCT
	Imaginal exposure	10	32							
	Raabe et al., 2015									
	ImRs	16	8	34.4 (8.2)	CAPS	BDI	3 months	No	Yes	Case series
	Steil et al., 2011									
	ImRs	2	9	43.8 (9.0)	FBC	PDS	2 months	Yes	Yes	Open trial
SAD	Frets, Kevenaar, & van der Heiden, 2014									
	ImRs	11	6	32.8 (9.2)	FNE	n.a.	6 months	No	Yes	Case series
	Lee & Kwon, 2013									
	ImRs	3	13	23.9 (3.4)	FNE	Encapsulated beliefs Image distress Image vividness Memory distress	3 months	Yes	Yes	RCT
	Supportive Counselling	3	10							
	Nilsson et al., 2012									
	ImRs	1	7	33.5 (12.9)	FNE	Image distress Image vividness Memory distress	##	No	Yes	RCT
	Reading task	1	7							
	Reimer & Moscovitch, 2015									
	ImRs	1	13	19.5 (1.3)	LSAS-SR	Encapsulated beliefs	n.a.	No	Yes	RCT
	Waitlist	n.a.	12							
	Wild et al., 2007									
	ImRs	1	8	28.6 (3.8)	SPWSS	Encapsulated beliefs Image distress Image vividness Memory distress	##	Yes	Yes	Open trial
	Wild et al., 2008									
	ImRs	1	11	35.2 (9.4)	FNE	Encapsulated beliefs Memory distress Image distress Image vividness	##	Yes	Yes	Open trial
BDD	Ritter & Stangier, 2016									
	ImRs	2	6	28.2 (7.0)	FSK	BDI	2 weeks	Yes	Yes	Case series
	Willson et al., 2016									
	ImRs	1	6	25.7 (5.9)	BDD-YBOCS	BDI	3 months	Yes	Yes	Case series
Bulima Nervosa	Cooper et al., 2007									
				24.9 (6.2)	Negative self- beliefs	BDI; Urge to binge/restrict	n.a.	n.r.	Yes	RCT

(*continued on next page*)

Table 1 (*continued*)

Disorder	Study and type of treatment	# of sessions	N*	Age Mean (SD)	Primary outcome	Secondary outcome	Follow-up	Extensive cognitive prep-aration?	ImRs as stand-alone intervention?	Design
	ImRs	1	12							
	Discussion of cognitions	1	12							
MDD	Brewin et al., 2009									
	ImRs	8	10	41.3 (n.r.)	BDI	Intrusive memory	12 months	No	Yes	Case series
OCD	Veale, Page, Woodward, & Salkovskis, 2015									
	ImRs	1	12	40.2 (11.2)	Y-BOCS	BDI	3 months	No	Yes	Case series

Note: * as used in pre-post analyses per condition; # patients in this study had a recent history of unsuccessful prolonged exposure treatment; ## follow-up assessment was conducted one week after a single session of ImRs, therefore this assessment was treated as post-treatment; BDD: Body Dysmorphic Disorder; BDD-YBOCS: Yale–Brown Obsessive Compulsive Scale modified for BDD; BDI: Beck Depression Inventory; BSS: Beck Scale for Suicidal Ideation; CAPS: Clinician Administered PTSD Scale; CRIM: Cognitive Restructuring and Image Modification; EMDR = Eye movement desensitization and reprocessing; FBC: Feeling of Being Contaminated; FNE: Fear of Negative Evaluation Scale; FSK: Fragebogen köorperdysmorpher Symptome (Engl.: Body Dysmorphic Symptoms Inventory); ImRs: Imagery Rescripting; LSAS-SR: Liebowitz Social Anxiety Scale e Self Report; MDD: Major Depressive Disorder; n.a.: not applicable; n.r.: not reported; PDS: Posttraumatic Diagnostic Scale; PSS-I: The PTSD Symptom Scale Interview; PSS-SR: The PTSD Symptom Scale Self-Report; PTSD: Posttraumatic Stress Disorder; RCT: randomized controlled trial; SAD: Social Anxiety Disorder; SPWSS: Social Phobia Weekly Summary Scale; Y-BOCS: Yale-Brown Obsessive-Compulsive Scale.

by variables of interest. Borenstein, Hedges, Higgins, and Rothstein (2009a) recommended a ratio involving at least ten studies for each moderator. Meta-regressions were conducted separately for number of treatment sessions and assessment of methodological quality of the included publications. All analyses were completed with comprehensive meta-analysis (CMA, version 3; Borenstein, Hedges, Higgins, & Rothstein, 2009b). We conducted the meta-analysis separately for a specific group of interventions consisted of at least four trials.

3. Results

3.1. Selection and characteristics of included studies

The bibliographic search identified 284 hits. Reference list and register search identified five additional publications, bringing the total number of identified hits to 289 (see Fig. 1). When duplicate publications were removed, 216 publications remained. An examination of abstracts led to the exclusion of 173 publications that were evaluated as not meeting the inclusion criteria. The evaluation of the full text of the remaining 43 publications led to the exclusion of 24 publications (see Fig. 1 for more information).

Table 1 presents descriptive characteristics for each study included in the meta-analysis. We included a total of 19 trials. Of these, eight trials were conducted with patients with PTSD, six with patients with social anxiety disorder (SAD), two with patients with body dysmorphic disorder, one with patients with depression, one with patients with bulimia nervosa, and one with patients with obsessive compulsive disorder. Sixteen of the trials reported reduction of symptoms of the specific disorder as the main treatment target. In two trials with patients with PTSD, the authors (Jung & Steil, 2013; Steil, Jung, & Stangier, 2011) reported reduction of feeling of being contaminated as main treatment target and in one trial with patients with bulimia nervosa, the authors (Cooper, Todd, & Turner, 2007) reported reduction of negative self-beliefs as main treatment outcome. Trials examining the efficacy of ImRs for SAD mostly used more than one instrument for assessing symptoms of SAD. Among those trials, the most common used instrument was the Fear of Negative Evaluation (Watson & Friend, 1969) that was reported in four trials. Accordingly, we used data resulting from this instrument to calculate the effect size in the four trials in question. The two trials on body dysmorphic disorder (Ritter & Stangier, 2016; Willson, Veale, & Freeston, 2016) had both applied the Yale–Brown Obsessive Compulsive Scale modified for body dysmorphic disorder (Phillips et al., 1997). However, in one of them, the authors (Ritter & Stangier, 2016) had applied this scale at pretreatment and follow-up only (and thus not at post-treatment) and therefore we chose another instrument as the primary outcome measure that the authors had used at all assessment points (see Table 1). With regard to the secondary outcome measures, all trials that assessed comorbid depression had used the Beck Depression Inventory (Beck, Steer, & Brown, 1996). Table 1 presents information on all outcome measures that were reported in the included trials and were used to calculate the effect sizes for this meta-analysis.

In 17 of the 19 trials, analyses were conducted with all participants who started treatment, which either included intent to treat or lack of drop out (the latter applied to all seven trials that consisted of one session only). Brewin et al. (2009) and Kindt, Buck, Arntz, and Soeter (2007) reported completer analyses only. Study sample sizes ranged from six to 33 and the total number of participants included in the meta-analysis was 363. All of the included studies were limited to adult patients and ImRs was applied in an individual format in all trials. The number of ImRs sessions administered ranged from one to 16 and had an average of 4.5

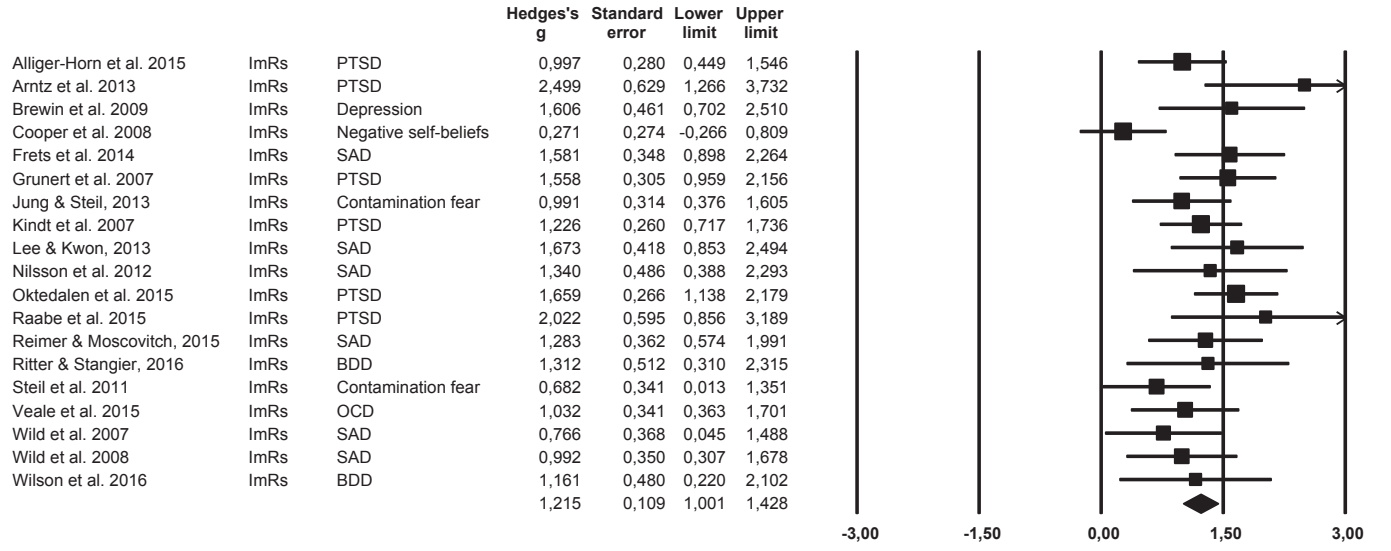

Fig. 2. Uncontrolled effect size estimates (pre-vs. posttreatment) for the efficacy of IR on primary outcome measures. *Note*: PTSD: Posttraumatic Stress Disorder; SAD: Social Anxiety Disorder.

sessions (SD = 4.67). Seven trials were conducted with one session only, four trials with two sessions only, two trials with three sessions only, and the remaining six trials with eight or more sessions.

Sixteen publications reported follow-up assessments that ranged between one week and 12 months (see Table 1). However, with regard to the three studies that reported a follow-up of one week (Nilsson, Lundh, & Viborg, 2012; Wild, Hackmann, & Clark, 2007; Wild, Hackmann, & Clark, 2008), the follow-up assessment was treated as post-treatment assessment as ImRs consisted of only one session. Seven publications reported on randomized controlled trials.

3.2. Effect of ImRs on primary outcome measures

3.2.1. Uncontrolled effect sizes

We first computed effect sizes for the impact of ImRs on main outcome variables from pre-to posttreatment. Across all 19 treatments, a large pre-post effect size was found, $g = 1.22$; 95% CI = [1.00; 1.43] (see also Fig. 2 for a forest plot). Thirteen publications reported on the effect of ImRs on main outcome variables from pretreatment to follow-up. Results showed large pre-follow-up-effect sizes, $g = 1.79$, 95% CI = [1.54; 2.03].

3.2.2. Controlled effect sizes

Seven trials compared ImRs to an active or passive control group on primary outcome measures. Five of these trials compared ImRs to a passive control condition and the effect at post-treatment was large in favor of ImRs, $g = 0.90$; 95% CI = [0.46; 1.35] (see also Fig. 3 for a forest plot). Of these five trials, only two reported a follow-up measurement conducted later than one week after treatment (Jung & Steil, 2012; Lee & Kwon, 2013). Furthermore, only two studies compared ImRs to an active control condition (Alliger-Horn, Zimmermann, & Mitte, 2015; Oktedalen, Hoffart, & Langkaas, 2015). In both cases, we judged the number of trials to be too small to conduct a meta-analysis.

3.2.3. Heterogeneity

Heterogeneity was moderate for within-group effect sizes at posttreatment ($I^2 = 41.31$; $Q = 30.67$, $df = 18$, $p = 0.03$), indicating moderate variability in the intervention effects between the studies. However, heterogeneity was not significant neither for within-group effect sizes at follow-up ($I^2 = 0$; $Q = 8.83$, $df = 12$, $p = 0.72$) nor for between-group effect sizes at posttreatment ($I^2 = 26.87$; $Q = 5.47$, $df = 4$, $p = 0.24$).

3.3. Effect of ImRs on secondary outcome measures

3.3.1. Uncontrolled effect sizes

Sixteen trials assessed the efficacy of ImRs from pre-to post-assessment on secondary outcome measures (see Table 1). The effect on secondary outcome measures was large, $g = 1.03$; 95% CI = [0.67; 1.38]. The analysis among eleven trials on the effect of ImRs on secondary outcome variables from pretreatment to follow-up showed also a large aggregated pre-follow-up effect size, $g = 1.39$, 95% CI = [0.77; 2.02].

3.3.2. Controlled effect sizes

Six trials compared ImRs to an active or passive control group on secondary outcome measures. Five of these trials compared ImRs to a passive control condition and our computation of the controlled (i.e., between-group) effect size at post-treatment on secondary outcome variables revealed a large aggregated effect size in favor of ImRs, $g = 1.00$; 95% CI = [0.27; 1.74]. Of these five trials, only one reported a follow-up measurement where ImRs was compared to a passive control condition (Jung & Steil, 2012). Furthermore, only one trial compared ImRs to an active control condition on secondary outcome measures (Alliger-Horn et al., 2015). Accordingly, the number of trials was too small to conduct a meta-analysis.

3.4. Quality assessment

The first and second authors independently rated all studies. The Intraclass Correlation Coefficient (ICC) of the total score for all studies combined was 0.95, 95% CI [0.93, 0.96], indicating very good inter-rater reliability. The items “blinded assessment” and “independent randomization” were mostly not applicable for the included trials. Blinded assessment could be rated only if a structured interview, rather than a self-report, was used to assess treatment efficacy and this applied to two studies only (Jung & Steil, 2013; Raabe, Ehring, Marquenie, Olff, & Kindt, 2015) and both reported blinded assessment. Independent randomization could be assessed in RCTs only and of the seven included RCTs (see Table 1),

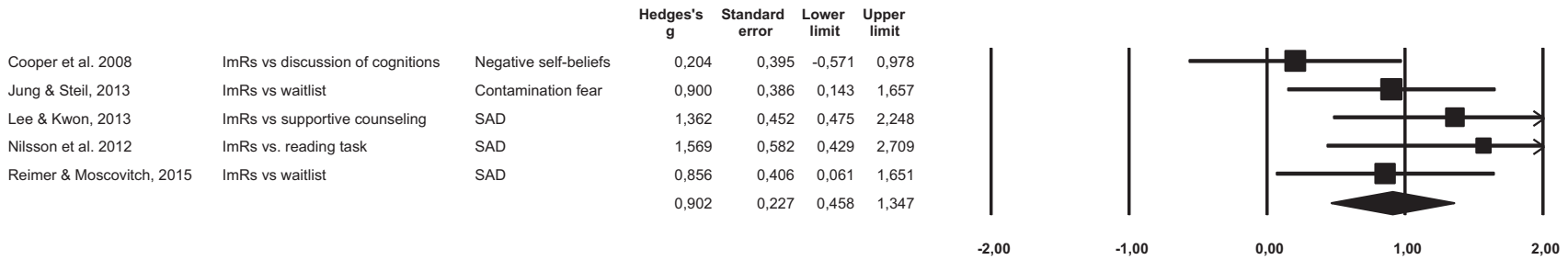

Fig. 3. Controlled effect size estimates (posttreatment) for the efficacy of ImRs as compared to passive control conditions. *Note*: SAD: Social Anxiety Disorder.

Table 2
Quality assessment according to criteria used by Cuijpers et al. (2010).

	Alliger-Horn et al., 2015	Arntz et al., 2013	Brewin et al., 2009	Cooper, Todd, & Turner, 2007	Frets et al., 2014	Grunert et al., 2007	Jung & Steil, 2013	Kindt et al., 2007	Lee & Kwon, 2013	Nilsson et al., 2012	Oktedalen et al., 2015	Raabe et al., 2015	Reimer & Moscovitch, 2015	Ritter & Stangier, 2016	Steil et al., 2011	Veale et al., 2015	Wild et al., 2008	Wild et al., 2007	Willson et al., 2016
Semi-structured diagnostic interview	3	3	3	3	3	0	3	3	3	0	3	3	3	3	3	3	3	3	3
Treatment manual	3	3	3	3	3	3	1	3	3	3	3	3	3	3	1	3	3	3	3
Therapist training	0	3	2	0	2	0	3	3	3	0	3	3	0	3	2	2	3	2	0
Treatment integrity	2	1	2	0	0	0	2	2	2	0	3	2	0	0	2	2	0	0	0
Intent-to-treat analysis	3	3	1	0	3	3	2	1	3	3	3	3	3	3	3	3	3	3	3
Randomization	2	1	1	3	1	1	3	0	3	3	3	1	3	1	1	1	1	1	1
Independent randomization	0	n.a.	n.a.	0	n.a.	n.a.	0	n.a.	3	0	3	n.a.	0	n.a.	n.a.	n.a.	n.a.	n.a.	3
Blinded assessments	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	3	n.a.	n.a.	n.a.	n.a.	3	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Reporting of dropout	3	3	3	0	3	3	3	3	3	1	3	3	3	3	3	3	1	1	3
Mean	2.0	2.4	2.1	1.3	2.1	1.4	2.0	2.1	2.9	1.3	3.0	2.6	1.9	2	2.1	2.4	2	1.9	1.9

Note: 3: high quality; 2: limited quality; 1: lack of required quality; 0: unknown; n.a.: not applicable (see explanation in the text).

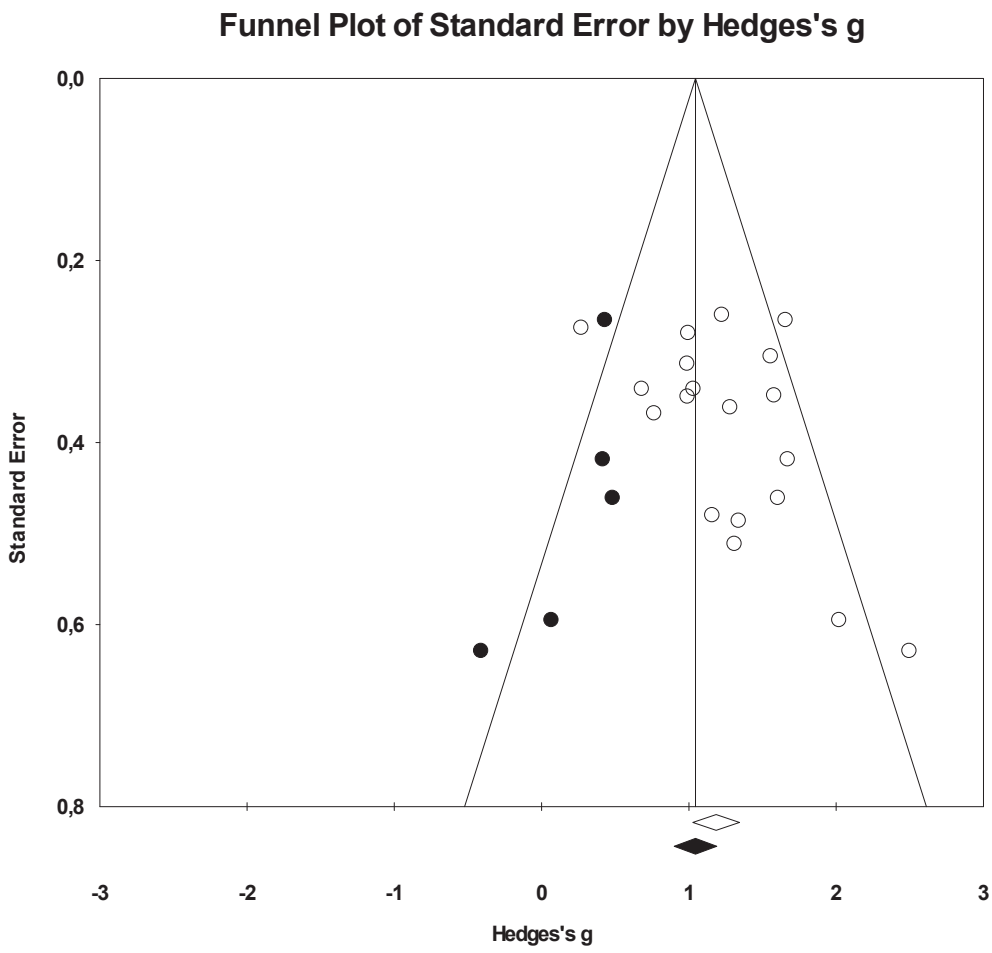

Fig. 4. Funnel plot by Hedge's g for pre-to postassessment on preprimary outcome measures. *Note*: open circles represent observed effect sizes and dark circles represent imputed effect sizes.

independent randomization was reported in two of the trials only. In total, 13 publications (68.4%) reported a score of two or higher. On the item level, trials scored on average good in the items use of a semi-structured interview to screen study participants (M = 2.7), treatment manual (M = 2.8), intent to treat analysis (M = 2.6), and reporting of drop-out (M = 2.5). See Table 2 for further information.

3.5. Publication bias

For the within analyses at posttreatment, visual inspection of the funnel plots indicated potential publication bias. Fig. 4 shows a plot of effect sizes in relation to their standard error. The trim and fill procedure introduced five studies to the left side and this reduced the estimated effect from $g = 1.22$ to $g = 1.06$ (95%CI: 0.83 to 1.28). Accordingly, the estimated effect size by the trim and fill procedure is still large. For the within analyses at follow-up, visual inspection of the funnel plots suggested no publication bias. Trim and fill analysis similarly suggested that no studies are missing. With regard to other comparisons, the number of trials included in the analyses was smaller than the minimum of the recommended ten trials (Sterne et al., 2011).

3.6. Additional analyses

Given the moderate heterogeneity regarding the uncontrolled effect sizes (pre-vs. posttreatment, see above), we conducted the following two meta-analyses that complied with our pre-specified criterion of minimum of four trials.

First, we separately conducted analyses for trials on PTSD and SAD. Note that for the separate analysis with PTSD trials, we included only the six trials that reported reduction of PTSD symptoms as main treatment target (see Table 1). Results yielded a large pre-post effect size among these trials, $g = 1.48$; 95% CI = [1.14; 1.82]. Similarly, the aggregated effect sizes for the six trials on the efficacy of ImRs for SAD was also large, $g = 1.25$; 95% CI = [0.95; 1.56].

Second, we conducted separate analyses for trials assessing the efficacy of ImRs on secondary outcome measures if the given subgroup consisted of at least four trials. This applied to comorbid depression (k = 9), aversive imagery (k = 5), and encapsulated beliefs (k = 4). The effect of ImRs on comorbid depression was medium to large, $g = 0.61$; 95% CI = [0.30; 0.92], the effect on aversive imagery was large, $g = 1.80$; 95% CI = [0.93; 2.67], and the effect on encapsulated beliefs was also large, $g = 1.81$; 95% CI = [1.15; 2.51].

Finally, the meta-regression analyses revealed that the within effect size of ImRs on primary outcome variables was positively moderated by number of treatment sessions ($\beta = 0.07$, $SE = 0.02$, 95% CI = [0.03 .11], $p = 0.001$) and study quality score ($\beta = 0.47$, $SE = 0.20$, 95% CI = [0.08; 0.87], $p = 0.02$).

4. Discussion

In this meta-analysis, we evaluated the efficacy of 19 clinical trials on ImRs as a clinical intervention for psychological

complaints that are associated with aversive memories. We conducted both within (i.e., pre-vs. posttreatment and follow-up) and between (i.e., post-treatment) analyses. Our results indicate that ImRs can significantly reduce psychological complaints among individuals with different mental disorders. Extending the narrative review by Arntz (2012), our updated meta-analytic review showed large treatment effects of ImRs in both primary and secondary treatment outcome measures.

ImRs has been applied to treat a variety of psychological complains related to aversive memories. In our meta-analysis only PTSD ($k = 8$) and SAD ($k = 6$) were relatively well represented, whereas the remaining five trials covered body dysmorphic disorder, bulimia nervosa, major depression, and obsessive compulsive disorder. Accordingly, our results first and foremost indicate that ImRs can effectively reduce levels of PTSD and SAD. Separate analyses regarding these two disorders resulted in large pre-vs posttreatment effect sizes ($g = 1.48$ for PTSD and $g = 1.25$ for SAD). Yet, the existing literature suggests that ImRs is also a promising treatment for depression as well. First, in the trial that directly targeted symptoms of major depressive disorder (Brewin et al., 2009) ImRs led to a significant reduction of symptoms of depression. More importantly, however, in the context of our meta-analysis is the finding that the nine trials evaluating the efficacy of ImRs on comorbid depression reported a medium to large effect size ($g = 0.61$) in this regard.

The findings of our meta-analysis further show that ImRs can produce large treatment effects for aversive imagery and encapsulated beliefs. This finding is very promising for at least two reasons. First, aversive memories play an essential role not only with regard to PTSD (Brewin, 2015) but to many types of psychopathology (Brewin, Gregory, Lipton, & Burgess, 2010; Holmes & Mathews, 2010; McTeague et al., 2009). Second, research suggests that many patients not only may struggle with aversive imagery of past events, they might also have inflated subjective probabilities and greater anticipation that negative events will occur as well as increased vividness for images of negative prospective events (MacLeod, Tata, Kentish, & Jacobsen, 1997; Morina, Deeprose, Pusowski, Schmid, & Holmes, 2011). Future research needs to investigate the efficacy of ImRs beyond treating PTSD and anxiety disorders and perhaps also target prospective imagery. This applies to both patients with mental disorders covered in our meta-analysis as well as to patients with other disorders who report intrusive imagery in general, such as individuals with bipolar disorder (Holmes et al., 2011).

The potential efficacy of ImRs must also been seen in light of the low number of sessions applied in the included trials with a mean of 4.5 sessions only. In fact, in half of the included trials, the authors reported only one or two sessions. The finding that in our meta-analysis number of sessions with associated with a larger effect size might indicate that the efficacy of ImRs can even be enhanced if more sessions than the average of 4.5 reported here are applied. The notion that ImRs can be effective after a rather small number of sessions indicates that ImRs might have both a clinical as well as an economical advantage compared to treatments that require a higher number of sessions. On a related note, preliminary results suggests that therapists might perceive ImRs as a less stressful treatment for both the client and themselves as compared to exposure without any component of ImRs (Arntz, Tiesema, & Kindt, 2007). However, both points mentioned here need to be examined in future randomized controlled trials.

There are several limitations associated with this meta-analysis. First, the number of included trials was small and this applies in particular to the number of trials evaluating the efficacy of ImRs in relation to a control condition. Second, the number of participants in several trials was rather small. For example, seven trials were based on samples with less than ten participants in the ImRs condition. Third, PTSD and SAD were the only conditions that were examined in more than two trials. Fourth, the methodological quality of several included studies had their limitations, especially because many were uncontrolled. In line with these limitations, the findings of our meta-analysis need to be seen as rather preliminary. Accordingly, more clinical research needs to investigate ImRs as a stand-alone treatment, while examining its mechanisms of change and comparing it to other effective treatments. Furthermore, future research needs to evaluate the efficacy of ImRs among children and adolescents suffering from complaints related to aversive memories. Basic science research can further increase the knowledge about underlying mechanisms of ImRs and ways to further improve its efficacy.

In conclusion, the results of our meta-analysis suggest that ImRs is a promising psychological intervention for psychological complaints related to aversive memories. In the included trials, patients with a variety of mental disorders reported significant reduction of their complaints after undergoing an average of 4.5 sessions of ImRs. Yet, the results on ImRs need to be seen as preliminary until the findings have been replicated by larger and more rigorously designed clinical trials.

Ethical approval

As our manuscript involves a meta-analysis, we do not have ethical approval. The governance organization would be the Ethics Review Board, Faculty of Social and Behavioral Sciences, University of Amsterdam, https://www.lab.uva.nl/lab/ethics.

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