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When does imagery rescripting become a double-edged sword? - Investigating the risk of memory distortion through imagery rescripting in an online Trauma film study

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ABSTRACT

Imagery Rescripting (ImRs) has proven effective in reducing involuntary emotional memories. However, it is unclear whether and when it may lead to reduced accuracy of voluntary memory. Although previous analogue studies suggest that ImRs does not pose a general risk regarding memory distortion, it can not be ruled out that ImRs could cause memory impairment under certain risk conditions. In our three-day online trauma film study we investigated in a healthy sample (N = 267) whether specific instructions during ImRs as typically provided in clinical practice (i.e., detailed imagery with a sensory focus) increase the risk of memory distortions. Additionally, we examined whether the completeness of the original memory moderates these instruction effects. Contrary to our expectations, a sensory focus during ImRs was associated with *higher* memory accuracy in a recognition task, independently of the quality of the original memory. These results extend previous findings by suggesting that ImRs does not even impair memory performance when the quality of the original memory is poor and when the production of sensory-rich images is specifically encouraged. Our results question current practices employed to assess witness statement credibility, which are partly based on concerns that trauma-focused interventions like ImRs undermine memory accuracy.

Open practices and data sharing

All data, codes and materials have been made publicly available via the Open Science Framework and can be accessed at https://osf.io/j9f85/.

1. Introduction

Intrusive, distressing memories are a core feature of various emotional disorders (Brewin et al., 2010). Recent clinical approaches, such as Imagery Rescripting (ImRs), specifically target these memories in order to reduce associated symptoms. In ImRs, an aversive memory is first reactivated and then modified in the patient's imagination so that the outcome is perceived as being less distressing (e.g., the perpetrator is disempowered or the victim's needs are taken care of, e.g., they are comforted and brought to safety; e.g., Arntz & Weertman, 1999; Holmes et al., 2007; Smucker et al., 1995).

While ImRs aims to reduce the involuntary and incontrollable recall of aversive memories and the associated distress, it is intended to preserve voluntary memory of factual information about an event. This is important considering the adaptive function of remembering (e.g., for future danger assessment), but also in terms of the role of trauma memory recall in legal contexts (e.g., in witness statements and the assessment of their credibility in court).

Theoretical approaches to the underlying mechanisms assume that ImRs does indeed selectively modify the *meaning* of emotional memory, but *not* the memory of *factual event details* as such (i.e., Arntz, 2012; Arntz & Weertman, 1999). Regarding the first part of this assumption, there is increasing evidence that ImRs reduces the involuntary occurrence of aversive memories and the associated emotional distress (e.g., Arntz, 2012; Morina et al., 2017). Regarding the second part of the assumption, the question of whether ImRs might unintentionally also cause distortions of memories of factual event information or even induce false memories of events that did not happen has recently gained increasing attention (e.g., Ganslmeier et al., 2022, 2023; Otgaar et al., 2021).

Current discussions about the potential of imagery-based trauma-focused interventions, such as ImRs, to cause memory distortions (e.g.,

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Bublitz, 2020; Ganslmeier et al., 2022, 2023; Otgaar et al., 2021) point to evidence from two influential lines of memory research showing that: (1) After reactivation, consolidated memories can enter a destabilized state. In this plastic state, they are vulnerable to modification through the integration and reconsolidation of either correcting or distorting information (Beckers & Kindt, 2017; Lee, 2009; Nader et al., 2000); and (2) memories are fallible to the extent that – under certain conditions – not only can a memory of actually experienced event details be manipulated, but people can develop rich autobiographical memories for entire events that never actually happened (i.e., "false memories"; for an overview see Davis & Loftus, 2020).

For instance, research on the so-called "misinformation effect" has shown that exposure to counterfactual information after an event can reduce memory accuracy for the event (Tousignant et al., 1986; see Loftus & Klemfuss, 2023 for an overview). These studies have mainly used a three-stage experimental procedure. First, a memory was induced, for example, by showing participants a video of an event (e.g., crime scene). Then, participants were exposed to misinformation about the event. This misleading information was typically subtly integrated in post-event questions about the film content or in narrative accounts of the event (Blank & Launay, 2014). Afterwards, participants' memories of correct details about the original event and/or their endorsement of misleading details were assessed using a memory test (e.g., Brewin & Andrews, 2017; Loftus, 1975; Loftus et al., 1978).

According to the Discrepancy Detection Principle, such memory distortions are more likely to occur when an individual does not immediately detect discrepancies between a memory of the original event and post-event misinformation, and then falsely incorporates the misinformation into their memory (Tousignant et al., 1986). The ability to detect discrepancies can be influenced by various factors, such as the strength of memory of the original event, the time interval between the original event and the memory test, the subtlety of the misinformation introduced, and the presence of warnings regarding misinformation (e.g., Brewin & Andrews, 2017; Leding & Antonio, 2019; Loftus, 1992).

In addition, studies on the "imagination inflation effect" (Garry et al., 1996; Goff & Roediger, 1998) provide evidence that imagining an event increases confidence that the event has actually occurred and, in some cases, can produce a false memory of the event (e.g., Goff & Roediger, 1998; Seamon et al., 2006; Thomas & Loftus, 2002). It has also been shown that imagination can alter the stored representation of actually experienced events (Goff & Roediger, 1998) or actually seen objects (Lyle & Johnson, 2007). Imagination can influence both recent (e.g., Seamon et al., 2006) and old (e.g., Garry et al., 1996) memories. Moreover, this is the case not only if the imagined event is plausible but also if it is implausible (e.g., Sharman & Scoboria, 2009).

Such memory distortions can be explained by the Source Monitoring Framework (Johnson et al., 1993; Lindsay & Johnson, 2000), according to which inaccurate memory reports occur when a memory of an imagined event (= internal source) is falsely attributed to an actually experienced event (= external source). The more perceptual and conceptual detail the imagined event and the actually experienced event share, the higher a person's susceptibility to such memory errors (Lyle & Johnson, 2007; Thomas et al., 2003). Accordingly, imagination instructions that include more elaboration of perceptual information and sensory detail increase susceptibility to memory distortion (e.g., Johnson et al., 1993; Thomas et al., 2003). This could be explained by the fact that people rely on the amount of sensory detail to determine the source of their memory, because actually experienced events typically contain more sensory detail than imagined events (Johnson et al., 1993; Özbek et al., 2017).

In light of these findings, it is not surprising that some authors have raised concerns that ImRs might have distorting effects on factual memory (e.g., Otgaar et al., 2021). In fact, ImRs shares some important characteristics with the experimental procedures used to demonstrate the process of memory distortion: It involves both imagination and exposure to counterfactual information during the rescripting phase.

However, the results of the few studies to date that have examined the effects of ImRs on memory accuracy did not find that the intervention distorts memories of factual event details. On the contrary, previous trauma analogue studies even found that ImRs led to improved memory performance as assessed by a free recall task (Ganslmeier et al., 2022), and did not impair (Ganslmeier et al., 2023; Siegesleitner et al., 2019) – or even improved (Hagenaars & Arntz, 2012) – memory accuracy as assessed with recognition and cued recall tasks.

Although none of the previous studies have been able to show the suspected negative impact of ImRs on the accuracy of an original event memory, the number of studies is still very small. In addition, the informative value of previous study results is limited regarding two important aspects. First, some of these studies (Hagenaars & Arntz, 2012; Siegesleitner et al., 2019) were not primarily designed to assess intervention effects on memory accuracy. Instead, memory accuracy was only investigated exploratively and/or as a secondary outcome. Hence, these studies used a relatively small number of items, that, to our knowledge, were not explicitly designed to assess memory for information that might be relevant in practical contexts, such as the legal field.

Second, the generalizability of these studies is limited by the fact that they did not take into account the specific conditions under which ImRs is typically delivered in clinical practice and that these conditions might in fact elevate the risk of memory distortions through ImRs: All studies mentioned so far assessed the effects of ImRs on relatively recent memories that were either induced minutes before the intervention took place (Hagenaars & Arntz, 2012) or up to one day before ImRs was applied (Ganslmeier et al., 2022, 2023; Siegesleitner et al., 2019). However, in clinical practice, most patients start psychological treatment months or even years after a traumatic or distressing life event has taken place; therefore, memory of certain event details may already be weak or vague in clinical populations. Moreover, recent evidence encourages the use of ImRs in the treatment of post-traumatic stress disorder (e.g., Boterhoven De Haan et al., 2020; Raabe et al., 2015), a disorder that is in part defined by "the inability to recall key features of the trauma" (criterion D 1., Diagnostic and Statistical Manual of Mental Disorders, 5th edition [DSM-5]; American Psychiatric Association, 2013). Critically, in cases where the original memory is vague, detecting discrepancies and/or monitoring the memory source can become more difficult (Johnson et al., 1993). As a result, it may become more likely that memory sources will be confused or that memory gaps will potentially be filled with false information (Loftus, 1997). Such false information can be introduced by the acceptance of misinformation (Loftus, 2005), via confabulation and autosuggestion (e.g., Ackil & Zaragoza, 1998), or via associative memory processes (Howe et al., 2009).

In addition to the quality of the original memory, it is also important to account for specific therapeutic instructions that could inadvertently have an influence on the probability of memory distortion when assessing potential unwanted effects of ImRs. For example, in clinical practice, the production of vivid, sensory-rich images during rescripting is often encouraged as this is considered necessary for therapeutic change (e.g., Arntz & Weertman, 1999). However, based on the source monitoring framework, a vivid and detailed imagination could reduce a patient's ability to correctly discriminate the sources of memory (actually experienced vs. imagined only), making patients more vulnerable to memory errors. Patient subgroups that have weak memories of their distressing or traumatic life events may be particularly vulnerable as they might have greater difficulty detecting discrepancies between actually experienced and (spontaneously) imagined information, including counterfactual information. It could be particularly risky to ask these patients to elaborate on and vividly imagine details they do not recollect during memory reactivation and rescripting.

To summarize, results from previous experimental studies suggest that ImRs does not pose a *general* risk of memory distortion (i.e., always and inevitably lead to memory impairment). However, these studies have not taken into account some factors that are typically present in the

clinical use of ImRs and which, based on findings from the false memory literature, could increase the potential of ImRs to impair memory.

1.1. Aim of the current study

The main goal of the present study was to take a first step towards a systematic investigation of potential risk conditions under which ImRs could lead to memory distortions. More specifically, we focused on the impact of therapeutic instructions commonly used in clinical practice, which encourage patients to focus on sensory-perceptual information while reactivating and changing their distressing memories in their imagination. We also examined whether the effects of these instructions on memory depended on the completeness and clarity of the memory of the original event.

We conducted a three-day online trauma film study, which allowed us to examine the effects of the intervention on consolidated memories (see Ganslmeier et al., 2022, 2023; James et al., 2015; Siegesleitner et al., 2019). On the first day an aversive memory was induced using an aversive film clip, the intervention took place on the second day, and the memory test was applied on the third day.

We developed two ImRs intervention protocols, which contained specific instructions to either focus on sensory-perceptual details during memory reactivation and rescripting or *not* to focus on sensory-perceptual details during memory reactivation and rescripting. Additionally, a no-intervention control condition (NIC) was introduced to account for the effects of normal forgetting.

To manipulate the quality of the original memory, participants were presented either with a modified version of the film in order to induce an unclear and incomplete memory, or with the original version of the film to create a clearer and more complete memory.

1.2. Hypotheses

Our predictions about the effects of ImRs on memory accuracy were based on the source monitoring framework (Johnson et al., 1993) and the discrepancy detection principle (Tousignant et al., 1986). First, we expected ImRs with a sensory focus, but not ImRs without a sensory focus (or NIC) to impair memory accuracy. This should be reflected in lower memory accuracy in ImRs with a sensory-perceptual focus compared to NIC and ImRs without a sensory-perceptual focus.

Second, we hypothesized that the differential intervention effects would be moderated by the completeness and clarity of the original memory (pre-intervention): Participants with an unclear and incomplete memory of the film clip should have less accurate memories after receiving ImRs with a sensory-perceptual focus than participants with a clear and complete memory of the film clip.

Third, in line with literature on the effectiveness of ImRs for reducing psychopathology (see Morina et al., 2017) and emotional distress (e.g., Strohm et al., 2019) associated with aversive memories, we hypothesized that the two versions of ImRs intervention would each be more effective at reducing intrusions from the film clip, as well as distress and arousal associated with the film clip, as compared to the NIC.

Fourth, and finally, we expected ImRs with a sensory-perceptual focus to be more effective than ImRs without a sensory-perceptual focus and NIC. This should be reflected both 1) in a higher reduction of intrusions and 2) a greater reduction in subjective arousal and distress associated with voluntary memory recall.

We based this prediction on the fact that ImRs intervention protocols emphasize the need for vivid, sensory-rich imagery during rescripting for emotional reactivation, which is considered crucial to achieve the best intervention effects (e.g., Arntz & Weertman, 1999). In addition, we explored the effects of memory clarity and its interaction with the factor intervention in our analyses.

2. Methods

2.1. Participants

An a priori power analysis was conducted to calculate the appropriate sample size with regard to the proposed hypotheses on the primary outcome measure (i.e., memory accuracy). Due to the lack of similar studies from which effect sizes could be derived, a differential effect size of $f = 0.20$ (small to medium effect) was assumed. Calculations using G*Power software (ANOVA: fixed effects, special, main effects and interactions) resulted in a total sample size of $N = 244$ (41 participants per condition) with an $\alpha = 0.05$ and a statistical power of $1-\beta = 0.80$.

1289 participants were recruited through advertisements in online social networks (i.e., Facebook, Instagram, student WhatsApp groups) and the online panel PsyWeb (https://psyweb.uni-muenster.de/). Exclusion criteria were (a) age below 18 or above 55 (based on findings on a decline in episodic memory performance above the age of 55, eg., Rönnlund et al., 2005; Toppala et al., 2021), (b) current suicidality, (c) self-reported current psychological or neurological disorder, (d) history of psychosis or self-injurious behavior, (e) use of beta-blockers or other anti-hypertensive medication, (f) experience of one or more traumatic events in the past, similar to the content of the film, and (g) drug intake up to 72 h before testing or more than three alcohol beverages within 24 h before testing.

Based on these criteria, 301 participants were excluded. Another 106 participants did not finish the screening questionnaire. 330 participants who completed screening did not continue with the first session. 107 participants dropped out during Session 1, 52 dropped out after Session 1, and 21 dropped out after Session 2. We had to exclude an additional 94 participants from the analyses based on failure to comply with the protocol procedure. For outcomes on the memory recognition task, we conducted an outlier analysis using a 1.5 interquartile range criterion to identify outliers within each condition (memory_unclear/ImRs_Sensory: 4 outliers, memory_clear/ImRs_Sensory: 3 outliers, memory_clear/ImRs_NotSensory: 2 outliers, memory_clear/NIC: 3 outliers). All 11 identified outliers have been excluded from the analyses, resulting in a total sample of 267 participants (153 females, 113 males, 1 non-binary, mean age = 29.80, SD = 8.92, range = 18 to 55; 91,76 % of German nationality).[1]

Participants were randomly allocated to one of six experimental conditions that resulted from the 2 (*memory*: memory_clear; memory_unclear) x3 (*intervention:* ImRs_Sensory; ImRs_NotSensory; NIC) factorial design.

Participants received partial course credit or a small monetary reimbursement (10 € for complete study participation).

2.2. Materials

All materials are available at the Open Science Framework at https://osf.io/j9f85.

2.2.1. Trauma Film

2.2.1.1. Content. A 7-min aversive film clip from the movie "Picco" (Koch, 2010, 1:18:25–1:26:25) was used to induce an aversive memory. The film clip shows a group of three prisoners torturing another inmate through both physical (e.g., beatings) and psychological violence (e.g., verbal humiliations and attempts to convince the victim to commit suicide).

[1] Results of the analyses before outlier exclusion can be found in table S3 the supplemental material on the OSF (https://doi.org/10.17605/OSF.IO/S67NC).

2.2.1.2. Memory manipulation. The completeness and clarity of the memory of the film prior to the intervention was experimentally manipulated by either showing the film clip in its original form (memory_clear) or by using a visual blur filter that covered the whole picture and a blur audio filter masking parts of the dialogues so that certain visual and auditory information was no longer clearly identifiable (memory_unclear).

The film clips used were piloted in order to ensure that 1) the film clips induce an equal amount of distress and 2) memory accuracy and confidence are higher for the film clip without blur filters vs. the film clip with blur filter, as assessed by the memory recognition task (see Table S1 in the supplemental material on the OSF). The instructions for film viewing were based on a previous online trauma film study by Espinosa et al. (2023). The exact wording of the instructions for film viewing used in our study can be found on the OSF (osf.io/pqnh5, Materials, General_Instructions_Control_Questions).

2.2.2. Interventions

Both ImRs interventions were standardized and delivered via audio. The ImRs procedure was adapted from Arntz and Weertman (1999) and consisted of a brief imagery exercise for memory reactivation and a rescripting phase (see Kunze et al., 2017). Participants were first instructed to close their eyes and to reactivate the beginning of the scene as told. After the short reactivation (4.5 min in ImRs_NotSensory; 6 min in ImRs_Sensory), they rated their subjective distress and memory vividness. They were then asked to close their eyes again and to imagine the rescripted course of events as instructed (5.5 min in ImRs_NotSensory; 8 min in ImRs_Sensory). During rescripting, participants were instructed to imagine how the violent attacks towards the victim are stopped by prison guards who confront and disempower the perpetrators, remove them from the scene, and then take care of the victim.

The exact wording and audio files of the instructions for memory reactivation and rescripting can be found on the OSF (osf.io/pqnh5, Materials).

2.2.2.1. Imagery rescripting with sensory-perceptual focus (ImRs_Sensory). Subjects in the ImRs_Sensory condition were instructed to imagine the scene and all changes as vividly and in as much detail as possible and to pay attention to all sensory channels throughout both the reactivation and the rescripting phase (e.g., "Now the perpetrators are being taken away in handcuffs by additional prison guards who have just arrived. *Watch closely as they leave the room. How do the perpetrators look to you now? What do you observe in their body language and their facial expressions?"*).

2.2.2.2. Imagery rescripting without sensory-perceptual focus (ImRs_NotSensory). Subjects in the ImRs_NotSensory condition were instructed to focus on the same conceptual changes to the action as in the ImRs_Sensory condition, but without the explicit instruction to imagine everything in as much detail and as vividly as possible, and without the instruction to focus on sensory-perceptual details while doing so (e.g., "Now the perpetrators are being taken away in handcuffs by additional prison guards who have just arrived. [*no further instructions*]").

2.2.2.3. No-intervention control (NIC). Participants in the NIC group did not receive any intervention.

2.3. Measures

2.3.1. Baseline measures

Baseline measures were assessed for depressive symptoms using the Patient Health Questionnaire-9 (PHQ-9; Kroenke et al., 2002; German translation by Gräfe et al., 2004) and for trait anxiety using the trait form State-Trait-Anxiety-Inventory (STAI-T, Spielberger et al., 1970; German translation by Laux et al., 1981).

2.3.2. Manipulation checks

Manipulation checks for stress induction and memory reactivation were performed in line with previous work (e.g., James et al., 2015, 2016). The Positive and Negative Affect Schedule (PANAS; German version: Krohne et al., 1996) was used to assess mood immediately before and after watching the film. Additionally, subjective distress (SUD) and memory vividness were assessed by visual analogue scales on a scale ranging from 0 to 100. Arousal was assessed using Self-Assessment Manikins (SAM; Bradley & Lang, 1994). SUD, SAM, and memory vividness were assessed at different timepoints (i.e., pre- and post-film viewing, pre- and post-memory reactivation and pre- and post-rescripting, see Fig. 1).

2.3.3. Control variables

Control questions about the film and audio content, as well as about compliance with the experimental requirements (e.g., being alone and undisturbed, whether and for how long participants looked away from the screen, etc.), were administered as compliance checks. We assessed runtime variables for the duration of film viewing and the time delay between each time of assessment for further compliance checks. Further details on these measures as well as the exact items we used to assess protocol compliance can be found in the document "General_instructions_control_questions.pdf" in the materials section on the OSF (https://osf.io/j9f85).

2.3.4. Memory recognition task

Memory accuracy was assessed by means of a memory recognition task that contained 39 questions with one true and two false answer options as well as the option "I don't know" (e.g., "What was used to hit the victim in the back of the head?"; true answer: iron bar, false answers: baseball bat, broomstick). Following Ganslmeier et al. (2022), questions were chosen based on a guideline for police examinations (Hermanutz & Schröder, 2015) and focused on the place of action (e.g., "How many windows were in the room?"), the persons involved (e.g., "Who put the plastic bag over the victim's head?") and the events taking place in the film (e.g., "How many cuts was the victim injured with on the forearm?"). The total number of correct answers, the total number of wrong answers, and the total number of "I don't know" answers constituted the primary outcome measures for memory accuracy. The items used in the memory recognition task were piloted in order to ensure appropriate difficulty of the items (i.e., we aimed for an approximately balanced number of items across different levels of difficulty ranging from very difficult to very easy, and replaced items where necessary to meet this criterion). Item difficulties for our pilot data can be found in Table S4 in the supplemental material on the OSF.

2.3.5. Intrusion diary

The quantity (total number) and quality (type of memory as defined below; content of the memory; trigger situation; distress and vividness, each scored on a scale from 0 to 10) of intrusive memories in response to the film clip was assessed pre- and post-intervention at t2 and t3 by means of a retrospective summary of the total number of intrusions since the last study appointment (e.g., Hackmann et al., 2004; Rattel et al., 2019).

Intrusive memories were defined as spontaneously occurring involuntary memories of the film clip, which could be mental images, sounds, verbal thoughts, emotions, bodily sensations, or a combination. The reduction of the total number of intrusions from pre-to post-intervention was assessed as a measure of intervention effectiveness.

2.4. Procedure

See Fig. 1 for a schematic overview of the study procedure.

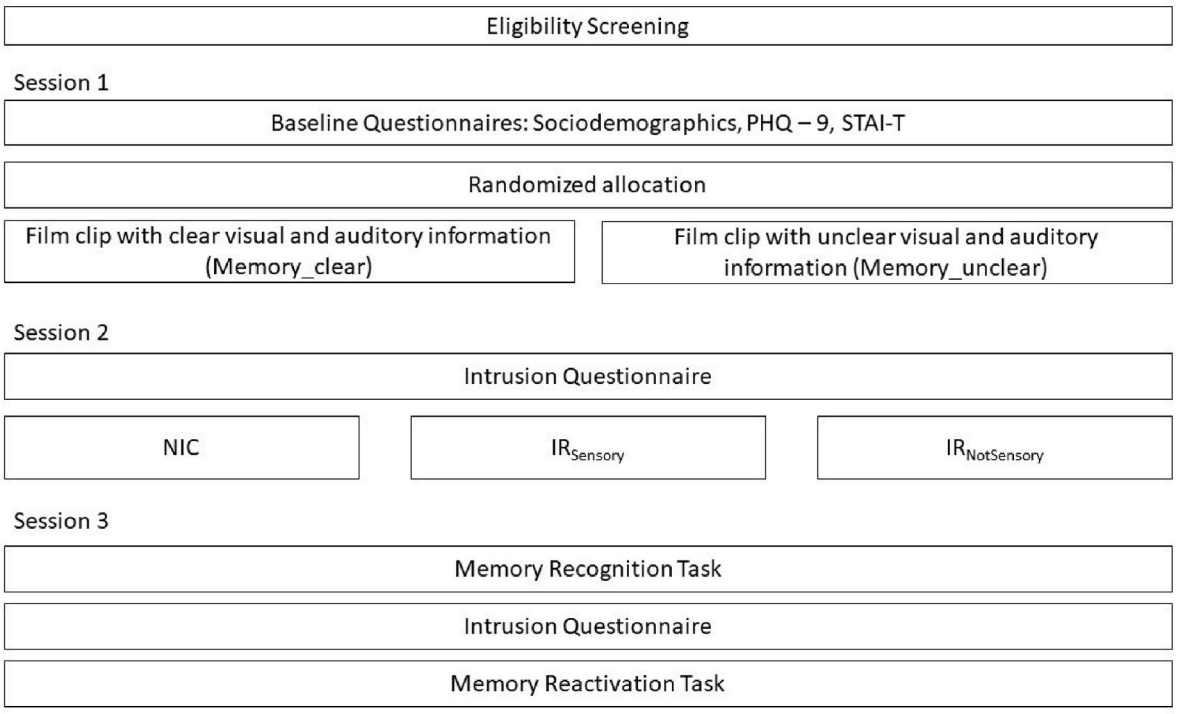

Fig. 1. Schematic overview of the study procedure.

2.4.1. Online screening

In a brief online screening, the participants were given an overview of the study procedure and the requirements for study participation. Participants were informed of the distressing nature of the film and that they could withdraw from the study at any point. After providing informed consent, the eligibility criteria were assessed. Those who met the inclusion criteria received additional information regarding the continuation of the study. Participants who did not meet the inclusion criteria were not invited to attend future appointments.

At the start of each session, participants were reminded of the requirements of the experiment (e.g., being in a quiet and undisturbed environment and using a laptop or PC instead of a smartphone or tablet). Those who were unable to meet these requirements were asked to reschedule the respective session for a later time when they could meet the conditions.

2.4.2. Session 1

At the beginning of the first session, participants completed a questionnaire on sociodemographic data (age, gender, education, nationality) and baseline questionnaires. A short health questionnaire was administered to gather information about participants' sleep quality and duration, drug and alcohol consumption in the days prior to the study, neurological disorders, and presence of uncorrected visual impairments. Participants were then randomly allocated to one of the six conditions and watched the trauma film clip. SUD, SAM, and PANAS were assessed pre- and post-film viewing.

2.4.3. Session 2

The second session began with the completion of the health questionnaire, followed by the intrusion questionnaire. For participants in the NIC condition, the second session ended after they filled out these questionnaires. Participants in the intervention conditions continued with pre-memory reactivation assessments of SUD, SAM, and memory vividness, followed by a brief imagery exercise to reactivate their memories. After the imagery exercise, participants completed post-assessments of SUD, SAM, and memory vividness. The session then proceeded with the imagery rescripting phase, followed by post-rescripting-assessments of SUD, SAM, and vividness.

2.4.4. Session 3

Session 3 started with the health questionnaire, followed by the memory recognition task and the second administration of the intrusion questionnaire. Subsequently, the memory of the film was again reactivated in a short imagery exercise, preceded and followed by SUD, SAM, and memory vividness assessments. The session ended with a debriefing of the participants.

2.5. Statistical analyses

2.5.1. Baseline differences

To identify possible covariates, three univariate ANOVAs on PHQ – 9, STAI-T, and PANAS (pre-film clip) were conducted in order to assess differences between the six groups (*memory:* memory_unclear, memory_clear x i*ntervention:* IR_Sensory, IRNot_Sensory, NIC) in baseline responding.

2.5.2. Manipulation checks

2.5.2.1. Emotional distress caused by the trauma film. Four mixed 2 (*memory*: memory_unclear vs. memory_clear) x 3 (*intervention*: ImRs_Sensory vs. ImRs_NotSensory vs. NIC) x 2 (*time*: pre-film vs. post-film) ANOVAs were conducted to check whether both film clips were equally successful in inducing distress (SUD), arousal (SAM), and negative affect, and in reducing positive affect (PANAS) in all experimental groups in Session 2 (the intervention took place only in Session 2).

2.5.2.2. Memory reactivation pre-intervention in session 2. To assess memory reactivation effects in the intervention groups in Session 2 (prior to the interventions) on SUD, SAM, and memory vividness, three mixed 2 (*memory*: memory_unclear vs. memory_clear) x 2 (*intervention*: ImRs_Sensory vs. ImRs_NotSensory) x 2 (*time*: pre-reactivation vs. post-reactivation) ANOVAs were performed.

2.5.3. Analyses of primary hypotheses

2.5.3.1. Hypothesis 1 (group differences in memory accuracy) & hypothesis 2 (moderating effect of memory completeness and clarity). To assess the effect of intervention on memory accuracy and to assess the potential moderating effects of the completeness and clarity of the original memory (pre-intervention) on memory accuracy, we conducted three 2 (*memory:* memory_unclear, memory_clear) x 3 (*intervention:* IR_Sensory, IRNot_Sensory, NIC) ANOVAs on the number of 1) correct answers, 2) incorrect answers, and 3) "I don't know" answers in the memory recognition task.

2.5.3.2. Hypothesis 3 & hypothesis 4 (group differences in intrusions, distress, arousal; session 3). Group differences reflecting the number of participants who did not develop any intrusive memories within the first 24 h after the film viewing were explored using chi-square tests (Siegesleitner et al., 2019).

Due to zero inflation in the data, a 2-level Poisson regression model was conducted instead of the pre-registered mixed ANOVA in order to assess group differences in the reduction of intrusions between Session 2 and Session 3, with the variable *time* on level 1 and the variables *memory* and *intervention* on level 2 to predict the number of intrusions. NIC (*intervention*), memory_unclear (*memory*), and intrusions measured at Session 2 (*time*) were used as reference levels.

To analyze intervention effects on memory distress (*SUD)* and arousal (*SAM*) in response to memory reactivation in Session 3 of study participation, two mixed 2 (*memory*: memory_unclear vs. memory_clear) x 3 (*intervention*: ImRs_Sensory vs. ImRs_NotSensory vs. NIC) x 2 (*time*: pre-reactivation vs. post-reactivation) ANOVAs were conducted. Bonferroni corrections were applied for post-hoc analyses.

2.5.4. Exploratory analyses

To explore group differences in memory confidence ratings, a 2 (memory: memory_unclear vs. memory_clear) x 3 (intervention: ImRs_Sensory vs. ImRs_NotSensory vs. NIC) multivariate analysis of variance (MANOVA) was carried out on mean confidence ratings for correct answers and for wrong answers as dependent variables.

All analyses described above were conducted in R (R Development Core Team, 2023) using the following packages: 'dplyr' (Wickham, François, et al., 2023) and 'car' (Fox & Weisberg, 2019) for data wrangling, 'psych' (Revelle, 2023) for data screening and calculating descriptive statistics, 'ggplot2' (Wickham et al., 2023), 'ggpubr' (Kassambara, 2023a) and 'cowplot' (Wilke, 2023) for visualizing data, 'rstatix' (Kassambara, 2023b) for basic statistical tests, 'glmmTMB' (Brooks et al., 2023) for computing mixed-effects models and 'MBESS' (Kelley, 2023) for calculating confidence intervals.

3. Results

For effect sizes, 90% confidence intervals were computed (Steiger, 2004). Bonferroni corrections were conducted for post-hoc tests.

3.1. Baseline and control variable differences between conditions

As illustrated in Table 1, there were no significant differences between the six groups (*memory* x *intervention*) in terms of sociodemographic or control variables.

3.2. Manipulation checks

Descriptive statistics of all manipulation check scores for SUD, SAM, vividness, and PANAS are displayed in Tables S1 and S2 in the Supplemental Material.

3.2.1. Emotional distress caused by the trauma film

In all ANOVAs, a significant main effect of time was found (SUD: $F(1, 261) = 370.13$, $p < .001$, $\eta_p^2 = 0.59$, 90% *CI* [0.53, 0.63]; SAM: $F(1, 261) = 685.90$, $p < .001$, $\eta_p^2 = 0.72$, 90% *CI* [0.68, 0.76]; negative affect: $F(1,$

Table 1
Means (M) and standard deviations (SD) of sociodemographic and control variables.

Variables	Condition						Statistics	p
	ImRs_Sensory		ImRs_NotSensory		NIC			
	Clear (n = 49)	Unclear (n = 39)	Clear (n = 48)	Unclear (n = 41)	Clear (n = 45)	Unclear (n = 45)		
Sociodemographic variables	*M(SD)*	*M(SD)*	*M(SD)*	*M(SD)*	*M(SD)*	*M(SD)*		
Age	30.12(8.96)	31.15(10.60)	29.17(7.53)	28.59(8.81)	30.89(9.77)	28.96(8.03)	$F(5, 261) = 0.60$	.698
Number of years of education	16.06(4.95)	14.17(4.54)	15.44(4.13)	15.96(5.81)	16.27(3.60)	16.20(2.77)	$F(5, 254) = 1.31$	.259
	%	%	%	%	%	%		
Gender (female)	55.10	56.41	56.25	60.98	60.00	55.55	$\chi2(5) = 0.55$	.99
Student (yes)	33.33	48.98	48.78	53.66	46.67	51.11	$\chi2(5) = 3.30$	.653
Control variables	*M(SD)*	*M(SD)*	*M(SD)*	*M(SD)*	*M(SD)*	*M(SD)*		
Sleep before Session 1	7.83(1.01)	7.68(1.02)	7.71(1.34)	7.76(1.44)	7.55(1.24)	7.70(1.05)	$F(5, 261) = 0.27$	.932
Sleep before Session 2	7.73(1.19)	7.45(0.97)	7.31(1.32)	7.60(1.15)	7.51(0.98)	7.52(1.15)	$F(5, 261) = 0.76$	.580
Sleep before Session 3	7.57(1.26)	7.55(1.13)	7.48(1.58)	7.50(1.53)	7.21(1.07)	7.71(1.06)	$F(5, 261) = 0.73$	.599
PHQ	3.49(3.35)	3.92(3.35)	4.25(3.64)	4.98(4.17)	4.56(4.05)	4.64(3.50)	$F(5, 261) = 0.95$	.447
STAI-T	37.06(9.73)	35.85(8.95)	37.21(9.32)	38.61(9.54)	38.00 (11.08)	34.96(7.34)	$F(5, 261) = 0.89$	.489
Compliance check variables								
Missed content while looking away from film (%)	0.43(1.71)	0.67(1.74)	0.92(2.28)	0.41(1.47)	1.07(2.58)	0.36(1.25)	$F(5, 261) = 1.09$	.364
Time between Session 1 and Session 2 (ks)	91.93 (11.96)	94.17(13.38)	92.36 (10.89)	89.33(7.15)	96.30 (14.13)	91.70(11.53)	$F(5, 261) = 1.75$	.123
Time between Session 2 and Session 3 (ks)	93,842 (13.34)	91.68(10.88)	91.59(9.00)	90.40(8.39)	92.76 (11.97)	91.74(10.78)	$F(5, 261) = 0.52$	.759

Note. ImRs_Sensory Imagery Rescripting with sensory-perceptual focus, *ImRs_NotSensory* Imagery Rescripting without sensory-perceptual focus, *NIC* no-intervention control group, *PHQ* Patient Health Questionnaire-9, *STAI-T* trait form of the State-Trait-Anxiety-Inventory, *ks* 1000 s.

261) = 662.11, $p < .001$, $\eta_p^2 = 0.72$, 90% *CI* [0.67, 0.75]; positive affect: $F(1, 261) = 576.59$, $p < .001$, $\eta_p^2 = 0.69$, 90% *CI* [0.64, 0.73]), indicating an increase of subjective distress (SUD), subjective arousal (SAM), and negative affect, as well as a decrease of positive affect from pre-to post-film viewing. No main effects emerged for memory (all *Fs* < 0.71, all *ps* > .399, all $\eta_p^2 = 0.00$), or intervention (all *Fs* < 2.61, all *ps* > .075, all $\eta_p^2 < 0.02$), nor did we find interaction effects (all *Fs* < 2.43, all *ps* > .090, all $\eta_p^2 < 0.02$) in these analyses.

3.2.2. Memory reactivation pre-intervention in session 2

3.2.2.1. Subjective distress. Regarding memory reactivation in the intervention groups in Session 2 (prior to the interventions), a large main effect of time showed higher post-than pre-memory reactivation SUD scores, $F(1, 173) = 192.32$, $p < .001$, $\eta_p^2 = 0.53$, 90% *CI* [0.44, 0.59]. There was also a small main effect of *intervention*, $F(1, 173) = 5.12$, $p = .025$, $\eta_p^2 = 0.03$, 90% *CI* [0.00, 0.08], indicating higher *SUD* in the ImRs_Sensory condition than in the ImRs_NotSensory condition. Neither the main effect of *memory*, $F(1, 173) = 0.16$, $p = .690$, $\eta_p^2 = 0.00$, 90% *CI* [0.00, 0.02], nor any interaction effects were significant (all *Fs* < 0.39, all *ps* > .536, all $\eta_p^2 < 0.02$).

3.2.2.2. Subjective arousal. For SAM, we found a significant main effect of *time*, $F(1, 173) = 210.39$, $p < .001$, $\eta_p^2 = 0.55$, 90% *CI* [0.47, 0.61] indicating an increase in subjective arousal over time, and a significant main effect of *intervention*, $F(1, 173) = 5.38$, $p = .022$, $\eta_p^2 = 0.03$, 90% *CI* [0.00, 0.08], with higher scores in the ImRs_Sensory condition than in the ImRs_NotSensory condition. We found no significant main effect of *memory*, $F(1, 173) = 0.45$, $p = .502$, $\eta_p^2 = 0.00$, 90% *CI* [0.00, 0.03], nor any interaction effects (all *Fs* < 0.60, all *ps* > .471, all $\eta_p^2 < 0.01$).

3.2.2.3. Memory vividness. For memory vividness, a main effect of *time*, $F(1, 173) = 44.45$, $p < .001$, $\eta_p^2 = 0.20$, 90% *CI* [0.12, 0.29], indicated a more vivid memory representation after memory reactivation. There were no main effects of *memory* or *intervention*, both *Fs* < 2.33, both *ps* > .129, both $\eta_p^2 < 0.01$. We found a significant interaction between *memory* and *time*, $F(1, 173) = 6.13$, $p = .014$, $\eta_p^2 = 0.03$, 90% *CI* [0.00, 0.09] with a higher increase of memory vividness from pre-to post memory reactivation in the memory_unclear than in the memory_clear condition. No other significant interaction effects were observed (all *Fs* < 1.50, *ps* > .223, all $\eta_p^2 < 0.01$).

3.3. Main analyses

Descriptive statistics for the results of the main analyses can be found in Table 2.

3.3.1. Memory accuracy

3.3.1.1. Number of correct answers. Descriptive statistics for memory recognition task responses are presented in Table 2.

Regarding correct answers, a main effect of *memory*, $F(1, 261) = 120.52$, $p < .001$, $\eta_p^2 = 0.32$, 90% *CI* [0.24, 0.38], showed significantly more correct answers in the memory_clear condition than in the memory_unclear condition. There was also a main effect of *intervention*, $F(2, 261) = 4{,}38$, $p = .032$, $\eta_p^2 = 0.03$, 90% *CI* [0.00, 0.07]. Bonferroni-corrected pairwise testing revealed that the mean number of correct answers was significantly higher in the ImRs_Sensory condition than in the NIC condition ($p_{adj} = .016$). No differences were found between ImRs_Sensory and ImRs_NotSensory ($p_{adj} = .289$) or between ImRs_NotSensory and NIC ($p_{adj} = .760$). In addition, there was no significant interaction effect between *memory* and *intervention*, $F(2, 261) = 0.28$, $p = .754$, $\eta_p^2 = 0.00$, 90% *CI* [0.00, 0.01] (See Fig. 2A).

3.3.1.2. Number of wrong answers. For wrong answers, the ANOVA yielded a significant main effect of *memory*, $F(1, 261) = 11.70$, $p = .001$, $\eta_p^2 = 0.04$, 90% *CI* [0.01, 0.08], with more wrong answers in the memory_unclear condition. However, neither the main effect of *intervention* ($F(2, 261) = 0.78$, $p = .461$, $\eta_p^2 = 0.01$, 90% *CI* [0.00, 0.02]) nor the interaction effect ($F(2, 261) = 0.41$, $p = .665$, $\eta_p^2 = 0.00$, 90% *CI* [0.00, 0.02]) reached significance (See Fig. 2B).

3.3.1.3. Number of "I don't know" answers. Looking at the "I don't know" answers, significantly higher scores were obtained in the memory_unclear condition, $F(1, 261) = 39.22$, $p < .001$, $\eta_p^2 = 0.13$, 90% *CI* [0.07, 0.19]. Additionally, a main effect of *intervention* was found, $F(2, 261) = 4{,}36$, $p = .032$, $\eta_p^2 = 0.03$, 90% *CI* [0.00, 0.07]. Bonferroni-corrected pairwise testing revealed that the mean number of "I don't know" answers was significantly lower in the ImRs_Sensory condition than in the NIC condition ($p_{adj} = .014$), but did not differ between ImRs_Sensory and ImRs_NotSensory ($p_{adj} = .101$) or between ImRs_NotSensory and NIC ($p_{adj} = 1.000$). There was no interaction effect between *memory* and *intervention*, $F(2, 261) = 0.38$, $p = .685$, $\eta_p^2 = 0.00$, 90% *CI* [0.00, 0.01] (See Fig. 2C).

Table 2
Means (M) and standard deviations (SD) of outcome variables for intervention effectiveness and memory accuracy.

	Clear (*n* = 49)	Unclear (*n* = 39)	Clear (*n* = 48)	Unclear (*n* = 41)	Clear (*n* = 45)	Unclear (*n* = 45)
Intrusions	*n(%)*	*n(%)*	*n(%)*	*n(%)*	*n(%)*	*n(%)*
Number of participants who reported at least one intrusion after Session 1	11(22.45)	8(20.51)	9(18.75)	10(24.39)	10(22.22)	4(8.89)
Number of participants who reported at least one intrusion after Session 2	5(10.20)	1(2.56)	4(8.33)	3(7.32)	9(20.00)	3(6.67)
	M(SD)	*M(SD)*	*M(SD)*	*M(SD)*	*M(SD)*	*M(SD)*
Number of intrusions after Session 1	0.45(0.94)	0.56(1.71)	0.48(1.11)	0.46(1.03)	0.76(1.68)	0.20(0.73)
Number of intrusions after Session 2	0.12(0.39)	0.03(0.16)	0.10(0.37)	0.17(0.80)	0.40(0.94)	0.11(0.44)
Memory reactivation at Session 3						
SUD pre reactivation	17.74(18.85)	19.28(17.65)	25.71(23.25)	20.85(21.04)	23.27(20.93)	17.84(19.12)
SUD post reactivation	32.63(24.01)	32.67(22.87)	44.04(27.37)	38.10(23.72)	46.89(26.85)	38.49(25.60)
SAM post reactivation	4.02(2.04)	3.74(1.98)	4.67(1.98)	4.12(1.99)	5.22(2.22)	4.73(1.92)
vividness pre reactivation	58.84(24.87)	48.15(26.68)	51.67(25.97)	42.61(22.73)	61.47(23.73)	50.87(26.02)
vividness post reactivation	68.06(23.03)	57.72(27.33)	64.33(21.68)	53.51(51.52)	65.98(22.50)	54.24(24.37)
Memory Recognition Task						
Right answers	22.98(4.54)	17.82(3.63)	22.12(4.72)	16.24(3.75)	21.58(4.36)	15.51(4.08)
Wrong answers	7.71(2.89)	9.38(4.25)	7.42(3.31)	8.37(3.40)	7.18(3.35)	9.04(4.12)
I don't know answers	7.31(4.18)	10.79(6.15)	8.46(5.46)	13.39(6.05)	9.24(5.09)	13.44(5.97)
Memory confidence	75.79(14.90)	66.48(15.00)	73.58(15.82)	63.31(14.48)	73.52(14.55)	61.52(19.92)

Note. *ImRs_Sensory* Imagery Rescripting with sensory-perceptual focus, *ImRs_NotSensory* Imagery Rescripting without sensory-perceptual focus, *NIC* no-intervention control group, *SUD* Subjective Stress, *SAM* Self-Assessment Manikins.

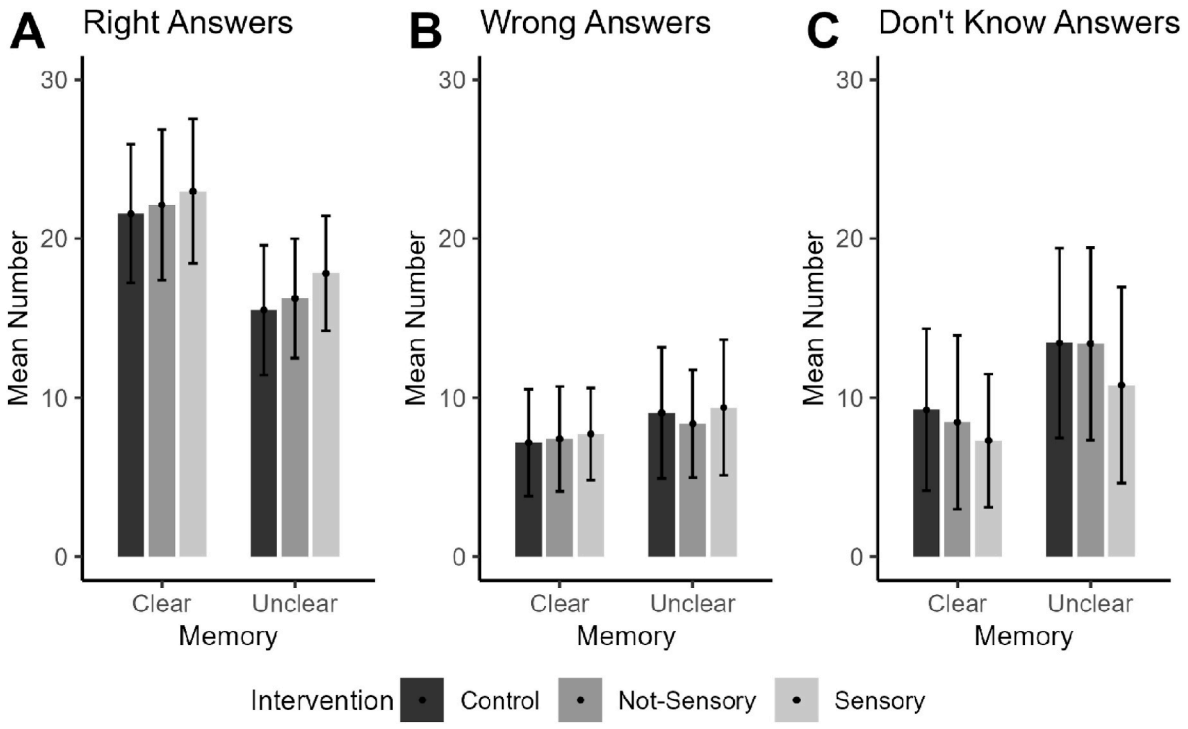

Fig. 2. Memory Recognition Task responses displayed separately for each intervention and film version.

3.3.2. Intrusions

For descriptive statistics, see Table 2. No differences were found between groups regarding the number of intrusions measured at t2 and t3 (See Table 3). $\chi 2$ test revealed that the number of participants who did not develop any intrusive memories within the first 24 h did not differ between film versions, $\chi 2(1) = 0.55$, $p = .46$.

Table 3
Multilevel Poisson Regression Model Predicting the Course of Intrusive Memories with the Predictors Time (Session 2, Session 3), Memory (memory_unclear vs. memory_clear), and Intervention (NIC, ImRs_NotSensory, ImRs_Sensory).

Predictor	Estimates (SE)	95% CI	z	p
Session 2 vs. session 3	−0.75(0.75)	[-2.22; 0.72]	−1.00	.316
Memory_unclear vs. memory_clear	1.05(0.58)	[-0.09; 2.19]	1.81	.071
NIC vs. ImRs_NotSensory	0.30(0.60)	[-0.87; 1.48]	0.51	.613
NIC vs. ImRs_Sensory	0.73(0.61)	[-0.46; 1.92]	1.20	.230
Session 3: memory_clear	0.10(0.84)	[-1.54; 1.74]	0.12	.903
Session 3: ImRs_NotSensory	0.32(0.97)	[-1.58; 2.23]	0.33	.741
Session 3: ImRs_Sensory	−2.28(1.32)	[-4.87; 0.32]	−1.72	.086
Memory_clear: ImRs_NotSensory	−0.69(0.72)	[-2.11; 0.72]	−0.96	.337
Memory_clear: ImRs_Sensory	−1.38(0.72)	[-2.79; 0.03]	−1.91	.056
Session 3: memory_clear: ImRs_NotSensory	−1.22(1.19)	[-3.56; 1.12]	−1.03	.306
Session 3: memory_clear: ImRs_Sensory	1.78(1.48)	[-1.12; 4.68]	1.20	.229

Note. *ImRs_Sensory* Imagery Rescripting with sensory-perceptual focus, *ImRs_NotSensory* Imagery Rescripting without sensory-perceptual focus, *NIC* no-intervention control group.

3.3.3. Subjective distress and arousal associated with memory reactivation at session 3

3.3.3.1. Subjective distress. Concerning SUD scores, a main effect of *time* was observed, $F(1, 261) = 3.62$, $p < .001$, $\eta_p^2 = 0.46$, 90% *CI* [0.38, 0.52]. Additionally, a significant interaction effect was found between *intervention* and *time*, $F(1, 261) = 3.6$, $p = .028$, $\eta_p^2 = 0.03$, 90% *CI* [0.00, 0.06]. There was a significant simple main effect of *intervention* at post-reactivation, $F(2, 264) = 4.12$, $p_{adj} = .034$, but not at pre-reactivation, $F(2, 264) = 1.38$, $p_{adj} = .504$. Simple pairwise comparison revealed a significantly lower post-reactivation SUD in the ImRs_Sensory condition than in the NIC, $p_{adj} = .025$. There was no significant difference between ImRs_Sensory and ImRs_NotSensory ($p_{adj} = .070$), nor between ImRs_NotSensory and NIC ($p_{adj} = 1.000$). There was no further significant main effect or interaction effect (all $Fs < 2.76$, $ps > .065$, all $\eta_p^2 < 0.02$); See Fig. 3A).

3.3.3.2. Subjective arousal. For SAM scores, a significant main effect of *intervention*, $F(2, 261) = 6.83$, $p = .001$, $\eta_p^2 = 0.05$, 90% *CI* [0.01, 0.09], and a significant increase over *time*, $F(1, 261) = 228.59$, $p < .001$, $\eta_p^2 = 0.47$, 90% *CI* [0.39, 0.52], were found. Bonferroni-corrected pairwise t-tests revealed significantly lower SAM scores in the ImRs_Sensory condition compared to the NIC $p_{adj} < .001$. There was no significant difference between ImRs_NotSensory and NIC, $p_{adj} = .078$, nor between ImRs_Sensory and ImRs_NotSensory, $p_{adj} = .081$. All other effects were not significant (all $Fs < 2.11$, $ps > .147$, all $\eta_p^2 < 0.01$; See Fig. 3B).

3.4. Exploratory analyses

3.4.1. Memory confidence rating

Due to missing data on confidence ratings, four participants were excluded from the MANOVA. There was no main effect of intervention, $F(4, 516) = 1.64$, $p = .163$, $\eta_p^2 = 0.01$, 90% *CI* [0.00, 0.03], Pillai's Trace = 0.03, nor an interaction effect between *memory* and *intervention*, $F(4, 516) = 1.17$, $p = .324$, $\eta_p^2 = 0.00$, 90% *CI* [0.00, 0.02], Pillai's Trace = 0.02, on the combined dependent variables. There was a significant main effect of *memory*, $F(2, 257) = 12.60$, $p < .001$, $\eta_p^2 = 0.09$, 90% *CI*

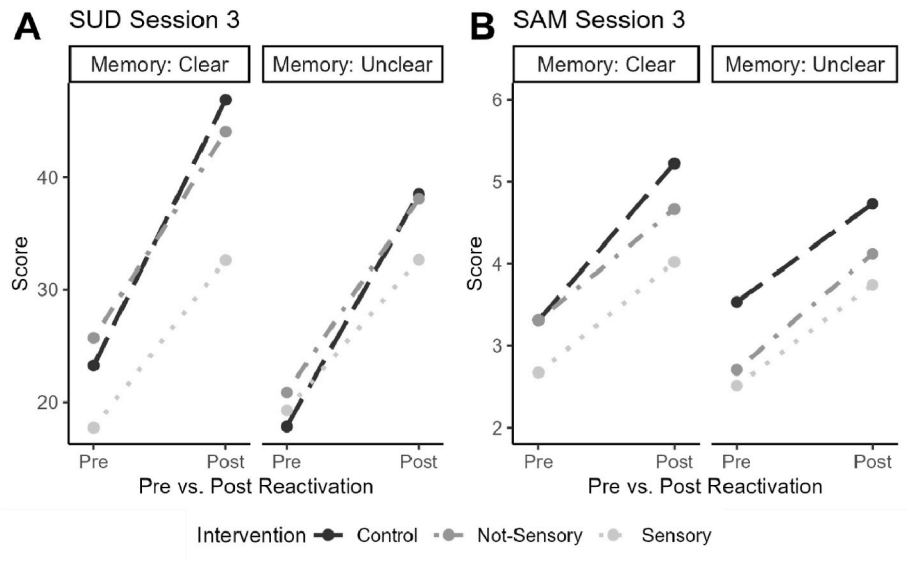

Fig. 3. Subjective distress (SUD) and arousal (SAM) before and after memory reactivation in Session 3.

[0.04, 0.14], Pillai's Trace = 0.09. Two post-hoc ANOVAs were conducted for mean confidence ratings for right answers and mean confidence ratings for wrong answers. Results showed significantly higher confidence ratings for right answers, $F(1, 258) = 24.13$, $p < .001$, $\eta_p^2 = 0.09$, 90% *CI* [0.04, 0.14], as well as significantly higher ratings for wrong answers, $F(1, 261) = 12.27$, $p = .001$, $\eta_p^2 = 0.05$, 90% *CI* [0.01, 0.09], in the memory_clear group than in the memory_unclear group.

4. Discussion

The aim of the present three-day online trauma film study was to investigate potential risk conditions under which ImRs could lead to memory distortions.

The main finding of the present study was, contrary to our expectations, that participants who received ImRs with a sensory-perceptual focus did *not* show impaired memory after the intervention as compared to ImRs without a sensory-perceptual focus and a NIC group. Instead, they even showed significantly *better* memory performance after the intervention than participants who had received no intervention. This was reflected in both a higher total number of correct memory recognition answers and a lower number of "I don't know answers" in the ImRs_Sensory group. Moreover, we did not find any group differences in the number of incorrect answers, nor did we find differences in the mean memory confidence ratings. Interestingly, this was true even for participants who had an incomplete and unclear original memory.

Although these results are not in line with our hypotheses, they align with previous research, which also did not find any adverse effects of ImRs on memory accuracy (Ganslmeier et al., 2022, 2023; Hagenaars & Arntz, 2012; Siegesleitner et al., 2019). Moreover, our results extend these earlier findings by suggesting that a sensory-perceptual focus during imaginative reactivation and subsequent rescripting of the memory does not increase the risk of memory distortion, not even in cases of incomplete and unclear original memories.

We based our predictions on findings in the false memory literature that highlight specific conditions under which the imagination of counterfactual content, as found in ImRs, is more likely to result in memory distortions. However, the unanticipated findings in the present study may stem from inherent dissimilarities between the ImRs utilized in our study and the experimental procedures employed in previous false memory literature. The latter were specifically designed to demonstrate the malleability of memories and differ in crucial aspects from ImRs as clinically applied. These methodological differences could potentially account for the observed variations in their impact on the recollection of events. For example, it is conceivable that the way in which the memory manipulation is introduced might play a crucial role. Based on the ImRs scripts used in clinical practice (e.g., Arntz & Weertman, 1999) and in earlier studies (Ganslmeier et al., 2022, 2023; Siegesleitner et al., 2019), participants in our study were explicitly informed prior to the intervention that they would be asked later to use imagery to modify their memory of the film. In contrast, in typical misinformation studies participants are usually unaware of the memory manipulation. In line with our finding, it has been shown that warning participants about the possibility of exposure to misinformation before the presentation of misinformation often reduces the misinformation effect (e.g., Greene et al., 1982; Karanian et al., 2020). The transparent and explicit introduction of the fact that imagined changes to the memory will be part of the intervention might produce similar warning effects in ImRs and thereby prevent participants from experiencing memory distortion.

Furthermore, our assumption that a sensory-perceptual instruction focus would increase the potential of ImRs to distort memory was based on earlier evidence suggesting that the more sensory-perceptual detail an imagined event contains, the greater the risk of memory distortions due to memory source confusions (i.e., actually experienced vs. imagined events; Johnson et al., 1993; Thomas et al., 2003). However, unlike memories of events that have actually been experienced, memories of imagined events usually also contain more information about cognitive processes involved in mentally creating the image (Goff & Roediger, 1998; Johnson et al., 1988). It has been shown that people determine the memory source depending on how many cognitive processes are associated with a memory (Johnson et al., 1988). In our study, participants in the ImRs_Sensory condition had to perform complex cognitive operations that not only involved imagining the course of events and how they change (as in the ImRs_NotSensory condition), but also involved mentally creating additional sensory details, such as the sound of the protagonists' voices. This might have facilitated correct source monitoring and, as a result, reduced participants' susceptibility to memory distortion in the ImRs_Sensory group as compared to participants in the ImRs_NotSensory group.

The finding that participants in the ImRs_Sensory group did not exhibit the expected memory distortion, and even performed better in the memory recognition task in terms of number of correct answers, might also be explained by rehearsal effects (Roediger & Butler, 2011). Both ImRs conditions required participants to rehearse (parts of) their memory, whereas participants in the NIC group did not have to reactivate their memory. Moreover, research on guided imagery as a retrieval technique has shown that imagery can work as a retrieval cue,

facilitating correct recall (Billings et al., 1995; Hyman & Pentland, 1996; Nori et al., 2014). Even though both ImRs conditions involved rehearsal of and imagery-based modification of the memory, the higher dose (i.e., rehearsal of more details) in ImRs_Sensory may explain why only participants in the ImRs_Sensory group remembered more correct details than participants in the NIC group. Moreover, the differing amount of instructions and, consequently, intervention durations in the two ImRs conditions alone could have caused different intervention dosages. This, in turn, may have contributed to the observed results.

If replicated, our findings might also have clinical implications. ImRs intervention protocols typically emphasize the importance of patients engaging in vivid imagery, including all sensory modalities, due to findings that imagery can act like an "emotional amplifier" (Holmes & Mathews, 2010). If vivid imagery increases the risk of side effects on memory accuracy, it would be recommendable to decrease vividness, which may in turn reduce the effectiveness of the intervention. This assumption is also supported by our data, which show that participants who received ImRs with a sensory-perceptual focus experienced the greatest reduction in subjective memory-related distress. Reassuringly, however, our findings do not suggest that instructions aimed at increasing vividness and perceptual focus will be problematic for memory retrieval.

In contrast to our hypotheses, the observed reduction of memory-related distress in our study was not paralleled by a reduction of memory-associated arousal or number of intrusions (although for memory-associated arousal, there was a descriptive trend indicating that participants in the ImRs_Sensory group showed the lowest memory-associated arousal at the end of study participation). As for intrusions, this might be due to a floor effect since the film used in our study induced only a small number of intrusions, leaving little room for improvements through the intervention (see Table 2). The film clip used in our study was not characterized primarily by images of physical or sexual violence, but rather achieved its aversive character due to the psychological violence against the victim. Moreover, the context in which the film scene takes place (a prison and violence by prison inmates) likely offered little to no associations with the participants' lives. It can therefore be assumed that during the study period participants were not frequently exposed to triggers for intrusions which might explain why we failed to measure intrusions.

Investigating the dissociation between effects of ImRs on voluntary vs. involuntary aversive memories was not the focus of the present study. However with regard to the generalizability of our findings, it appears crucial for future studies to establish whether memory accuracy would remain unimpaired, even when the intervention shows the intended reduction of intrusive memories. Future studies should therefore consider using a different film which might be better suited to induce intrusive memories (e.g., James et al., 2015; Lau-Zhu et al., 2019). Note however, that some earlier trauma film studies aiming to model treatment effects have experienced similar complications when examining intervention effects on intrusive memories, even after using different film clips, in that they either failed to produce a sufficient initial number of intrusions or found a rapid decline in intrusions, independently of any intervention (e.g., James et al., 2016; Siegesleitner et al., 2019, 2020). An alternative for future studies could therefore be to not only look at intrusion frequency, but to incorporate alternative/additional variables, such as intrusion load, reactivity to triggers, psychophysiological responses, etc.

In sum, our findings are in line with theoretical approaches proposing that ImRs might selectively change the meaning of and emotions associated with distressing memories *without* impairing memory of factual event details (e.g., Arntz, 2012). The findings also align with earlier studies showing a dissociation between the effects of ImRs on memory distress vs. memory accuracy (e.g., Hagenaars & Arntz, 2012; Siegesleitner et al., 2019). However, it remains unclear what underlying memory processes drive these effects. It has been proposed that ImRs might modify the original memory trace through memory reconsolidation interference (Arntz, 2012; Dibbets & Arntz, 2016), thereby removing the emotional component of the memory but leaving declarative memory components intact. It has been demonstrated that such selective memory modification is indeed possible through pharmacological (e.g., Beckers & Kindt, 2017; Nader et al., 2000; Sevenster et al., 2012; Soeter & Kindt, 2010) and behavioral manipulations (e.g., Golkar et al., 2017; Lau-Zhu et al., 2019; Monfils et al., 2009; Schiller et al., 2010). However, to date, it is not known whether these results can be translated to psychological interventions such as ImRs or whether other processes might account for the observed effects. For example, retrieval competition theory (Brewin, 2006) offers an alternative explanation according to which ImRs may create a new, more positively valenced memory trace that competes with the original aversive memory representation at retrieval. While this was not the primary question of the present study, investigating the potential for reconsolidation-based memory modification through ImRs is of high clinical relevance as it could mean more stable treatment effects (Beckers & Kindt, 2017). Future research is needed to address this topic.

4.1. Strengths and limitations

Our study has a number of important strengths. First, our study is the first to systematically investigate how the effects of ImRs on memory accuracy might be influenced both by specific therapeutic instructions and by the quality of memories typically found in clinical practice. Second, we designed our items to assess memory accuracy with a particular emphasis on information that holds practical relevance, especially within the legal context, including aspects such as identifying features of the perpetrators and the chronology of events. Third, using standardized intervention protocols allowed us high experimental control over the contents imagined during the interventions. Finally, it is important to note that when examining the memory effects of ImRs, even small effects towards an impairment of memory must be ruled out. Therefore, a strength of our study is that we powered it to detect small effects.

Despite these strengths, the results of the present study must also be interpreted in light of some limitations. First, it is worth mentioning that we did not collect information on the participants' ethnic identification or the cultural background. We therefore cannot say how representative our results are for people from different ethnic and cultural contexts.

Second, using an analogue design enabled us to experimentally control and manipulate memory content, which was crucial for the purpose of our study. However, our sample might therefore have differed from clinical samples in terms of important variables that have been found to influence susceptibility to memory distortions, thus reducing generalizability. For example, susceptibility has been found to be associated with depression (e.g., Brennen et al., 2007; Johnson et al., 1993), trait dissociation (e.g., Clancy et al., 2000), and level of arousal (Corson & Verrier, 2007), among other factors (see Loftus & Davis, 2006 for a review). Levels of distress and arousal elicited by the film clip used in our study are clearly not comparable to those elicited by real-life (traumatic) events. Furthermore, we know that susceptibility to memory distortions through misinformation increases as more time passes between the original event and the introduction of misinformation (e.g., Loftus et al., 1978). In our study, the ImRs intervention took place only one day after the memory of the film clip was induced. Although we attempted to take into account the influence of the strength of the original memory by manipulating it experimentally, we cannot exclude the possibility that different memory effects of ImRs would be observed if applied after a longer time interval. Furthermore, in order to avoid experimentally induced interference with memory, we did not perform a pre-intervention check on memory clarity for the film clip. This decision aligns with memory reconsolidation theory, which suggests that any memory reactivation could impact subsequent reconsolidation (Nader et al., 2000; Nader & Hardt, 2009; Kindt 2018). However, a pilot phase manipulation check confirmed our experimental manipulation's

success. Participants exposed to the unclear film clip exhibited lower memory confidence, provided fewer correct answers, and gave more incorrect and "I don't know" responses than those exposed to the clear film clip (detailed pilot data can be assessed on OSF under supplements). The main study results also support our intended manipulation, showing a significant memory clarity effect across all memory accuracy measures. However, future studies could additionally assess subjective memory clarity post-memory induction using a brief self-report measure to avoid triggering reconsolidation.

Third, we used an experimental version of ImRs that has been adjusted to the online study design in that the intervention was delivered via audiotape. The high vividness ratings indicate that participants could nevertheless imagine the script well (see Table S2 in the Supplemental Material). However, we do not know whether some participants would have executed the imagery task even better had it been delivered by an experimenter. Moreover, due to the sample and the memory induction, we used a standardized and stripped-down intervention protocol (e.g., no switch between adult and child perspectives). Although, with regard to the potential memory distortion (our main variable of interest) the supposed core aspects of ImRs (i.e., imagery-based memory reactivation and modification) were included, we can not rule out that we would have found different effects had we used a more naturalistic ImRs script. For example, in the clinical context, ImRs involves interactions between therapist and patient which might leave more room for suggestive processes that might affect memory accuracy. Note, however, that earlier studies that used personalized ImRs scripts delivered in a laboratory setting which involved interactions between participants and experimenter also did not find reduced memory accuracy after ImRs (Ganslmeier et al., 2022, 2023).

Fourth, while repeated retrieval typically enhances memory (Roediger & Butler, 2011), it has also been shown that it can enhance susceptibility to memory distortion in the context of misinformation (Heaps & Nash, 2001; Henkel, 2004). Moreover, repeated exposure to misinformation was found to increase the misinformation effect (Foster et al., 2012). As we only used one short ImRs session, we cannot draw conclusions about the memory effects of repeated ImRs (but see Ganslmeier et al., 2022 who found no memory deterioration, even when participants were instructed to repeatedly listen to recordings of the ImRs between sessions).

Fifth, like previous studies assessing the effects of ImRs on memory accuracy, we only examined whether the memory of the original event details was worse after ImRs than it had been before. During ImRs, the changes that are introduced to the memory are typically very salient and often involve major alterations to the course of events. For example, a new helpful figure might be introduced to the re-imagined scene and confront the perpetrator. However, less significant details from the original memory, such as what people were wearing, are generally considered clinically irrelevant and are thus not intentionally modified during ImRs. Nevertheless, it is conceivable that the memory of specific details of the original event might be affected by subtle processes that could occur during ImRs, even if those details are not intentionally altered. We were therefore particularly interested in determining whether simply instructing individuals to vividly imagine changes to their memory, as is typically done in clinical practice, would suffice to distort the original memory, especially for individuals with an unclear original memory. Future studies should additionally investigate whether counterfactual details introduced during the rescripting phase are later erroneously accepted or incorporated into memory reports (e.g., Reineck et al., 2023). Note, however, that future studies should keep in mind what type of changes introduced in ImRs are clinically relevant and are therefore worth testing for their potential to induce false memories.

Finally, we only used a memory recognition task to assess memory accuracy which might limit the generalizability of our findings. Given that trauma survivors who serve as eye witnesses in criminal proceedings are often asked to provide a detailed verbal report of the event as part of their testimony, using a free recall task in addition to a memory recognition task might improve external validity and generalizability. In addition, as free recall and memory recognition tasks involve different cognitive processes, results from memory recognition tasks and free recall tasks can differ (e.g., Malloggi et al., 2022), which stresses the importance of assessing the effects of ImRs on memory accuracy across different memory measures. Although earlier studies using a free recall task found consistent results in that participants who received ImRs reported more correct details in both the memory recognition task and the free recall task (Ganslmeier et al., 2022, 2023), we do not know if our findings would replicate across different memory measures.

4.2. Implications

When using ImRs to target aversive memories in clinical contexts, the main concern is reducing memory distress as well as symptoms of psychopathology. The effects of the intervention on memory accuracy only play a secondary role. However, memory accuracy can become critical in other contexts of a patient's life where correct recall of the historic facts is necessary. This is especially true for trauma survivors who may need to testify in legal cases. Concerns about the potential adverse impact on memory accuracy of interventions targeting aversive memories, such as ImRs, can then significantly affect the assessment of the victim's credibility in court (Gasch, 2018; Schemmel & Volbert, 2021). As a result, patients are often advised both by legal and psychological professionals to delay the beginning of trauma-focused psychological treatment until legal proceedings conclude (Bublitz, 2020; but see also different recommendations, for example the updated legal guidance on pre-trial therapy in the UK which explicitly states that therapy should not be delayed, The Crown Prosecution Service, 2022). Affected patients are therefore confronted with the dilemma of whether they should prioritize their health by seeking therapy or their credibility as witnesses. However, our results together with previous studies challenge these concerns and suggest that ImRs neither necessarily nor typically leads to memory distortion.

Nevertheless, more systematic research is needed to investigate any factors that could potentially increase the risk of memory distortion through imagery-based psychological interventions in order to minimize the risks of memory distortion through psychological interventions and of denying patients from receiving appropriate treatment. In addition, future studies should also assess the effects of ImRs on the accuracy of real-life autobiographical memories in clinical samples. Note, however, that for memories of naturalistic events that are beyond experimental control, only approximations for memory accuracy, such as consistency of memory recall, must be used, which in turn limits the validity of such studies with regard to memory accuracy.

5. Conclusion

In sum, our findings provide a valuable contribution to the current debate on potential adverse side effects of imagery-based psychological interventions like ImRs on memory accuracy. Our study expands earlier research that challenged concerns about potential memory distortions through ImRs by adopting a novel methodological approach which allowed us to specify the conditions under which ImRs may (or may not) lead to memory distortions. We could demonstrate that ImRs, does not distort memory – not even with a sensory-perceptual instruction focus, as typically provided in clinical practice. Further, by experimentally manipulating the quality of the original memory, we could account for (some of the) typical memory characteristics found in patients who receive psychological treatment (i.e., weak original memories due to forgetting with the passage of time or due to dysfunctional memory processes, e.g., dissociation). Our data indicate that even in cases of unclear and incomplete original memories, encouraging patients to form vivid images during the intervention does not pose a particular risk with regard to memory accuracy and may even improve memory accuracy. Although these findings are very gratifying from a clinical-therapeutic

point of view, future research is needed to systematically investigate potential risk factors that might lead ImRs to distort factual event memory.

Transparency

Conflict of interest

MA, AM, TE, and LW have no conflicts of interest to disclose.

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Preregistration

The hypotheses, study design and analysis plan of this study were preregistered at the Open Science Framework (https://osf.io/j9f85).

Data availability statement

Anonymized data, codes and study materials have been made publicly available via the Open Science Framework and can be accessed at https://osf.io/j9f85/. The design and analysis plan for the experiment were preregistered at OSF and can be accessed at https://osf.io/j9f85/.

Ethical approval

The study was carried out in accordance with the provisions of the World Medical Association Declaration of Helsinki and approved by the local Research Ethics Committee of the Department of Psychology at LMU Munich (17_Ehring_b). All participants provided written informed consent.

CRediT authorship contribution statement

Milena Aleksic: Conceptualization, Formal analysis, Investigation, Methodology, Project administration, Software, Writing – original draft, Writing – review & editing. **Alexander Reineck:** Formal analysis, Methodology, Software, Visualization, Writing – original draft, Writing – review & editing. **Thomas Ehring:** Conceptualization, Methodology, Supervision, Writing – original draft, Writing – review & editing. **Larissa Wolkenstein:** Conceptualization, Methodology, Supervision, Writing – original draft, Writing – review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper

Data availability

All data, codes and materials have been made publicly available via the Open Science Framework and can be accessed at https://osf.io/j9f85/. We referred to the link in the manuscript.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.brat.2024.104495.

Supplemental Material

The online version contains supplementary material.

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