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Failure to replicate the deleterious effects of safety behaviors in exposure therapy

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ABSTRACT

The current study attempted to replicate the finding obtained by Powers, Smits, and Telch (2004; *Journal of Consulting and Clinical Psychology, 72*, 448–545) that both the availability and utilization of safety behaviors interfere with the efficacy of exposure therapy. An additional goal of the study was to evaluate which explanatory theories about the detrimental effects of safety behaviors best account for this phenomenon. Undergraduate students ($N = 58$) with high claustrophobic fear were assigned to one of three treatment conditions: (a) exposure only, (b) exposure with safety behavior availability, and (c) exposure with safety behavior utilization. Participants in each condition improved substantially, and there were no significant between-group differences in fear reduction. Unexpectedly, exposure with safety behavior utilization led to significantly greater improvement in self-efficacy and claustrophobic cognitions than exposure only. The extent to which participants inferred danger from the presence of safety aids during treatment was associated with significantly less improvement on all outcome measures. The findings call into question the hypothesized deleterious effects of safety behaviors on the outcome of exposure therapy and highlight a possible mechanism through which the mere presence of safety cues during exposure trials might affect treatment outcomes depending on participants' perceptions of the dangerousness of exposure stimuli.

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Introduction

Phobias and other anxiety disorders are characterized by irrational beliefs about potential threat in one's environment which often persist despite the non-occurrence of feared outcomes. A key phenomenon posited to contribute to the maintenance of these beliefs is *safety behaviors* (Salkovskis, 1991). Safety behaviors are actions that are performed to prevent feared outcomes from occurring. Safety behaviors are theorized to play an important role in the maintenance of anxiety disorders by preventing disconfirmation of core dysfunctional beliefs; in other words, by preventing opportunities to acquire evidence that would disprove predictions of harm. To illustrate, a person with social anxiety may avoid eye contact because he or she is afraid of negative evaluation by others. Lack of eye contact, however, deprives the individual of opportunities to learn that the feared negative evaluation is not actually taking place. Avoidance of eye contact may even be construed by others as social ineptitude, thus bringing about the originally feared outcome.

A number of studies have shown that exposure therapy without the use of safety behaviors is more effective than exposure therapy in which patients are permitted to use safety behaviors. One investigation comparing variants of exposure therapy for panic disorder with agoraphobia found that patients who dropped safety behaviors during treatment showed a greater decrease in anxiety and panic-related cognitions than patients who did not receive instructions to drop safety behaviors (Salkovskis, Clark, Hackman, Wells, & Gelder, 1999). Wells et al. (1995) found that exposure treatment for social phobia was more effective when augmented with a decrease in safety behavior usage. Patients with social phobia who received group cognitive-behavioral therapy (CBT) with additional instructions to drop safety behaviors experienced greater treatment gains than patients who received standard CBT alone (Morgan & Raffle, 1999). Lastly, a meta-analysis of treatment trials for OCD showed that exposure treatment with total ritual prevention was associated with greater treatment gains than exposure treatment with partial or no response prevention (Abramowitz, 1996). In support of the notion that safety behaviors maintain pathological anxiety, these studies indicate that safety behaviors dilute the potency of exposure therapy.

A number of hypotheses and possible mechanisms have been proposed to explain how safety behaviors interfere with exposure

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therapy, but little research has been conducted to test their explanatory power. One explanation is the misattribution of safety hypothesis, which posits that safety behaviors prevent disconfirmatory learning because individuals attribute their safety not to the harmlessness of the situation, but to their use of safety behaviors (Salkovskis, 1991). Powers, Smits, Whitley, Bystritsky, and Telch (2008) gave placebo pills to participants during a claustrophobia exposure trial with one of three instructions: 1) the pill would make exposure harder, 2) the pill would make exposure easier, or 3) the pill would have no effect on treatment. Participants who were informed that the pill would make treatment easier exhibited more return of fear and reduced self-efficacy than those in the other two conditions. This finding implies that participants who received these instructions attributed their reduction of fear to the pill and not to the inherent lack of dangerousness of the situation, which would explain why fear returned when subjected to the situation again without the "helping" pill.

Additionally, the distraction hypothesis advanced by Sloan and Telch (2002) suggests that safety behaviors may impede fear reduction by drawing attention away from the exposure situation, which prevents people from fully examining their surroundings and acquiring sufficient information to determine that the situation that they fear is in fact not dangerous. Kamphuis and Telch (2000) assigned participants to one of four conditions during exposure treatment in a claustrophobia chamber: 1) guided threat reappraisal, in which participants were instructed to concentrate on their most feared prediction while in the chamber; 2) a cognitive load condition, in which participants performed several number-related tasks in the chamber; 3) the first two conditions combined; and 4) the exposure task without any of the above instructions. Participants who were given distraction tasks during exposure exhibited less fear reduction and treatment gains than participants who were instructed to concentrate on the task. These findings provide evidence that distraction, as defined by lack of attention to the perceived source of threat, is detrimental to threat disconfirmation.

In contrast to the findings of Kamphuis and Telch (2000), other studies have found that distraction *improves* fear reduction by increasing one's perceived control/self-efficacy over reactions and thoughts in anxiety-provoking situations. For example, Johnstone and Page (2004) reported that participants engaging in distracting conversation with an experimenter during a spider phobia exposure task showed greater fear reduction and greater self-efficacy for the task than participants who engaged in stimulus-relevant conversation. Similarly, Oliver and Page (2003) found that the addition of distracting conversation to exposure therapy for blood-injection-injury fear resulted in greater fear reduction as well as increased levels of perceived control when compared to exposure alone. In a subsequent investigation, Oliver and Page (2008) replicated the beneficial effects of distracting conversation during exposure therapy with the additional finding that distracting conversation about stimulus-irrelevant features of the environment more effectively enhanced exposure therapy than distracting conversation about the participant's non-anxiety related internal sensations. Although these findings appear to directly contradict that of Kamphuis and Telch (2000) and the distraction hypothesis in general, a potentially important distinction between the two sets of findings is the nature of the distraction. In the Kamphuis and Telch study, the distracter was a cognitive load-inducing number processing task whereas participants in the Johnstone and Page study engaged in stimulus-irrelevant conversation. It is possible that the cognitive load resulting from stimulus-irrelevant conversation was low enough that participants were able to evaluate the dangerousness of the spider at the same time, but high enough to distract them from feelings of anxiety. Parrish, Radomsky, and Dugas's (2008) review of the distraction, safety behavior, and neutralization literatures suggests that while anxiety-control strategies may be maladaptive in many contexts, certain strategies may actually assist with completion of exposure tasks in some situations depending on factors including cognitive load, levels of attentional focus, and self-efficacy.

Finally, there is some evidence suggesting that people infer danger from the presence of safety aids in their environment. For example, the sight of hand sanitizer dispensers, face masks, and rubber gloves implies the presence of harmful bacteria. Although these safety aids ostensibly prevent danger, their presence may increase fear instead of reducing it. Telch et al. (2010) found that the presence of a defibrillator during a panic challenge task increased participants' anxiety and perceptions of dangerousness. Research is needed to determine whether an increased perception of danger would hinder exposure therapy. If inferences of danger result in higher anticipatory anxiety prior to exposure, the exposure stimulus may be more potent as a result. Moreover, to the extent that an inference of danger decreases perceived self-efficacy specific to the exposure task, inferences of danger may interfere with exposure since lower self-efficacy at pretreatment often relates to poorer treatment compliance and outcomes (Taal et al., 1993).

The aforementioned explanations for the deleterious effects of safety behaviors in exposure therapy generally do not differentiate between the *availability* of safety aids and the *utilization* of safety behaviors. However, this distinction is important in both clinical and research settings. If safety behavior utilization has detrimental effects but safety behavior availability does not, then having safety aids around "just in case" during exposure therapy (e.g., high-potency benzodiazepine medication for a patient with panic disorder) could be a source of comfort and subsequently greater treatment compliance. However, if the mere presence of safety aids in the environment interferes with exposure regardless of their actual use, clinicians should take care to remove access to anything that could be construed as a safety aid during treatment sessions.

Powers, Smits, and Telch (2004) separately examined the effects of the availability and utilization of safety behaviors by comparing variants of exposure treatment of claustrophobic fear. In the safety behavior utilization (SBU) condition, participants were instructed to use three coping aids to assist them in each treatment trial. The three coping aids, selected to address common feared outcomes of people with claustrophobia, were as follows: (a) opening a door in the side of the chamber to let in air blowing from a fan, (b) having the chamber door unlocked, and (c) communicating with the experimenter via two-way radio. In the safety behavior availability (SBA) condition, participants were informed of the availability of these three coping aids but were instructed to use them only if they must. No coping aids were offered in the exposure only (EO) condition. As the authors hypothesized, the EO condition showed greater fear reduction and changes in catastrophic cognitions than the safety behavior conditions. Surprisingly, the SBU and SBA conditions showed equally impaired fear reduction from pre to posttreatment despite the fact that no participants in the SBA condition actually used the available coping aids.

If the *utilization* of safety behaviors is required to interfere with exposure, then one would expect Powers et al.'s (2004) SBA condition to resemble the EO condition rather than the SBU condition in terms of fear reduction and cognitive change. These authors interpreted the lack of difference between the SBU and SBA conditions as evidence that it is the perceived availability of safety behaviors that interferes with learning during exposure. If safety behavior utilization has no additive effect beyond availability, it may be necessary to re-evaluate the safety behavior theories mentioned earlier. Powers et al. interpreted the misattribution of safety hypothesis as the attribution of safety "to the availability or

use of the aid, thus keeping their false perception of threat intact" (p. 453). The revision of safety behavior theories to include safety behavior availability has significant clinical implications for the optimal implementation of exposure therapy. For instance, if the mere presence of a safety aid impedes threat disconfirmation or distracts the client from exposure-based learning, then clinicians should be especially vigilant for subtle safety objects or cues during exposure therapy.

The current study attempted to build on Powers et al. (2004) by replicating their findings regarding the harmful effects of safety behavior availability, and exploring the mechanisms through which the presence of available safety aids interferes with fear reduction. We expected to replicate the findings of Powers et al. (2004) with respect to differential reduction of fear and catastrophic cognitions in the EO, SBA, and SBU conditions. Specifically, it was hypothesized that participants in the EO condition would show greater fear reduction and cognitive change than those in both the SBA and SBU conditions, and that treatment outcomes would be equivalent in the latter two conditions.

An additional goal of the study was to explore hypothesized mediating variables to determine the extent to which the misattribution of safety, distraction, and inference of danger hypotheses accounted for differences in fear reduction between the conditions. Given the misattribution of safety hypothesis, it was predicted that misattribution of safety to actions participants performed (or knew they could perform) in the chamber would be significantly higher in the SBA and SBU conditions than in the EO condition, and that this difference would account for the advantage of the EO condition over the safety behavior conditions. Similarly, based on the distraction hypothesis it was predicted that cognitive avoidance would be would be significantly higher in the safety behavior conditions than the EO condition, and that this difference would account for the advantage of the EO condition over the safety behavior conditions. Lastly, given the inference of danger hypothesis, it was predicted that the extent to which participants inferred danger from the presence of coping aids in the environment would result in less fear reduction and worse treatment outcomes among participants assigned to the safety behavior conditions. Because no coping aids were offered to participants in the EO condition, the inference of danger from the presence of coping aids was assessed only in the SBA and SBU conditions. Although a multi-session design is required to demonstrate that change in a potential mediator temporally precedes treatment outcome, it was necessary for the current study to use a single-session design in order to fully replicate the methodology of Powers et al. (2004). Thus, as the cognitive process measures and treatment outcome variables were assessed simultaneously, our analyses of potential mediating variables do not constitute true tests of mediation and cannot be used to establish causal relationships.

Method

Participants

Study participants ($N = 58$) were undergraduate students at a Western university. Participants were recruited from a large pool of introductory psychology students ($N = 996$) and were selected via a two-stage screening process (see Fig. 1). Students received partial course credit for their participation. The sample was comprised primarily of women (77.6%) and ranged in age from 18 to 26 years ($M = 19.28$; $SD = 1.54$). Nearly all participants (98.3%) described themselves as Caucasian. Full *Diagnostic and Statistical Manual of Mental Disorders* (4th ed.; *DSM-IV*; American Psychiatric Association, 1994) criteria for claustrophobia were met by 46.6% of participants; an additional 20.7% met all *DSM-IV* criteria except the functional impairment/marked distress criterion.

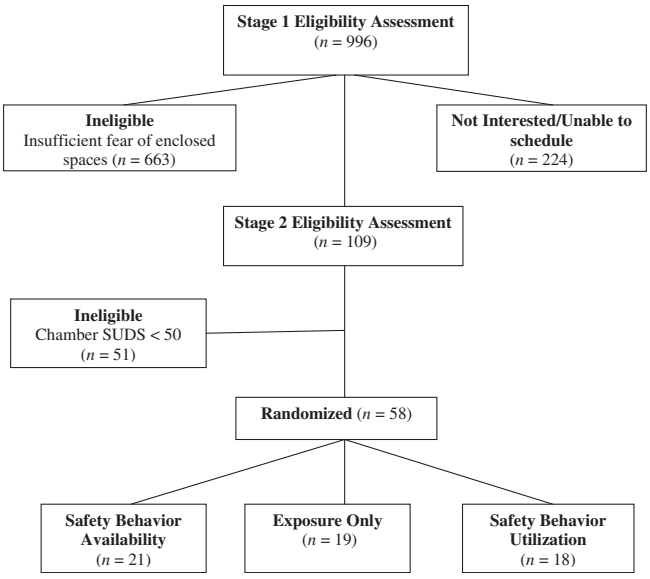

Fig. 1. Participant flow.

Stage 1 screener

Participants were recruited from the undergraduate psychology research pool based on their responses to a single item questionnaire on the introductory psychology mass prescreen used by Powers et al. (2004). The single item screener reads: "To what extent do you fear enclosed spaces? (0 = Not at all, 1 = A little, 2 = Moderate, 3 = Severe, 4 = Extreme)." Students who reported having at least a moderate fear of enclosed spaces received an e-mail inviting them to participate.

Stage 2 screener

After obtaining informed consent, potential participants were asked to complete a brief behavioral task to determine their eligibility for the study. The task involved lying in a "claustrophobia chamber," a 183 cm (length) × 61 cm (width) × 51 cm (height) wooden structure, for 2 min. The chamber's specifications matched those used by Telch and colleagues in their series of claustrophobia studies (Kamphuis & Telch, 2000; Powers et al., 2004, 2008; Sloan & Telch, 2002; Telch, Valentiner, Ilai, Petruzzi, & Hehmsoth, 2000; Telch et al., 2004). Potential participants were told to remain in the chamber as long as possible, but that the door of the chamber would remain unlocked in the event that they wanted to get out. They were then asked to remove their shoes and lie in the chamber with their head on the pillow. After they did so, the experimenter closed the door. Although the behavioral task lasted for a maximum of 2 min, this time limit was not divulged. Individuals who either a) exited the chamber in less than 2 min or b) rated their peak fear in the chamber as 50 or higher on a 100-point scale were invited to participate in the treatment phase of the study. Those who did not meet the above criteria ($n = 51$; 46.8%) were considered insufficiently phobic and excluded from the study.

Experimental design

Participants meeting eligibility criteria ($N = 58$) were randomly assigned to one of three conditions: a) exposure only (EO; $n = 19$), b) exposure with safety behavior utilization (SBU; $n = 18$), or c) exposure with safety behavior availability (SBA; $n = 21$). Self-report

measures of claustrophobic fear and self-efficacy were collected at pretreatment. These measures, along with items assessing within-trial cognitive avoidance, misattributions of safety, and inferences of danger, were collected after each of the six treatment trials. Finally, self-report measures of claustrophobic fear and self-efficacy were collected again at posttreatment. The behavioral approach task was administered at pretreatment and posttreatment. As Powers et al. (2004) did not find any between-group differences from posttreatment to follow-up assessment, a follow-up assessment was not included in the current study. Similarly, a control condition was not used given that all three exposure conditions were significantly more effective than a credible placebo treatment in the Powers et al. study. All study procedures were approved by the university's Institutional Review Board.

Measures

Structured Clinical Interview for DSM-IV (SCID)

The specific phobia section of the Structured Clinical Interview for DSM-IV-TR Axis I Disorders, non-patient version (SCID; First, Spitzer, Gibbon, & Williams, 2002) was used to determine whether participants met *Diagnostic and Statistical Manual of Mental Disorders* (4th ed; DSM-IV; American Psychiatric Association, 1994) criteria for claustrophobia. The SCID has demonstrated high discriminant validity and inter-rater reliability for DSM-IV anxiety disorder diagnoses (Carlbring et al., 2002). The SCID was administered to all participants meeting eligibility criteria for the study before assessing pretreatment measures. Although Powers et al. (2004) used the World Health Organization Composite International Diagnostic Interview (CIDI-Auto; World Health Organization, 1997) to determine whether participants met diagnostic criteria for claustrophobia, the SCID was chosen instead for the current study as it was thought to reduce the high rate of false positives inherent in a self-report diagnostic tool. Graduate and upper level undergraduate students who had received training in the specific phobia section of the SCID administered this interview to participants. Training consisted of instruction regarding the purpose and structure of the SCID, explanation of each specific phobia criterion using examples, and assessing experimenters' competency by having them administer the specific phobia module to mock participants with feedback.

The Credibility/Expectancy Questionnaire (CEQ)

The CEQ, developed by Devilly and Borkovec (2000), is a well-established measure of treatment expectancy and acceptance of treatment rationale with both good test–retest reliability and internal consistency. The CEQ is divided into two parts, asking participants to report how much improvement they *think* will occur as well as how much improvement they *feel* will occur on a 100-point scale. Composite scores were derived for both credibility and expectancy by summing the items corresponding to each factor. Given that treatment expectancy is often considered a major predictor of treatment outcome, it is important that responses to the treatment rationale are roughly equivalent at baseline. The CEQ was administered immediately after the presentation of the treatment rationale before the first treatment trial.

The Claustrophobia Questionnaire (CLQ)

The CLQ, developed by Rachman and Taylor (1993) and revised by Radomsky, Rachman, Thordarson, McIsaac, and Teachman (2001), consists of 26 items assessing claustrophobic fears and is divided into two subscales: Restriction Fear (CLQ-RS) and Suffocation Fear (CLQ-SS). Participants were instructed to report how anxious they would feel in a number of situations involving potential restriction or suffocation on a 1 ("Not at all") to 5 ("Extremely") scale. The CLQ has good predictive and discriminant validity, internal consistency, and test–retest reliability (Rachman & Taylor, 1993; Radomsky et al., 2001). The CLQ was administered at pretreatment and posttreatment.

The Claustrophobic Concerns Questionnaire (CCQ)

The CCQ assesses catastrophic cognitions specific to the chamber exposures (e.g., "I might be trapped," "I might run out of air"). Participants are asked to endorse how relevant each concern is on a 0–100 scale (0 = No concern; 100 = Extreme concern). The CCQ has demonstrated high internal consistency and predictive validity (Valentiner, Telch, Petruzzi, & Bolte, 1996). The CCQ was administered at baseline, after presentation of the treatment rationale before treatment, after each of the six treatment trials, and at posttreatment.

The Claustrophobia Coping Self-Efficacy Scale (CCSES)

The CCSES assesses one's perceived control over personal reactions specific to the chamber exposures on a 0–100 scale (0 = No confidence; 100 = Extreme confidence). Sample items include, "Estimate your confidence in being able to reduce your fear to a manageable level while in the chamber," and "Estimate your confidence in being able to control fearful thoughts or images while in the chamber." The CCSES has demonstrated good incremental predictive and discriminant validity as well as high internal consistency (Valentiner et al., 1996). The CCSES was administered at baseline, after presentation of the treatment rationale before treatment, after each of the six treatment trials, and at posttreatment.

Behavioral approach task (BAT)

Participants were invited, but not required, to complete up to six behavioral tasks assessing current fears of various types of enclosed spaces. The BAT took place in a small ($2.44 \times 3.05 \times 2.39$ m), windowless room with the door closed. The six steps were additive and included: (1) lying in a "mummy-style" sleeping bag on the floor, zipped up to waist-level; (2) zipping the sleeping bag up to neck-level; (3) while in the sleeping bag, having one's entire body (including face) covered by a medium-weight polyester blanket (165×225 cm); (4), being covered with a second blanket; (5) being covered by a third blanket; and (6) placing one's wrists in a pair of handcuffs. For each step, the experimenter described the task and asked the participant, "Are you willing to attempt this step?" Each step lasted for 60 seconds in order to ensure that participants spent a sufficient amount of time in each claustrophobia-relevant context. After each successfully completed step, participants verbally rated their peak fear during the step on a 0–100 scale (0 = No fear; 100 = Extreme fear or panic). An index of peak fear for each participant was determined by the fear rating for the highest completed step. All six BAT steps were completed by 48 participants (82.8%) at pretreatment and 52 (89.7%) at posttreatment. At pretreatment, 89.5% of the EO group, 94.1% of the SBA group, and 93.8% of the SBU group completed all six steps. A chi-square test of independence revealed no baseline differences between groups, $\chi^2 = .34$, $p = .84$.

Misattribution of safety scale

Two misattribution of safety items were constructed for the present study to assess the extent to which participants attributed the non-occurrence of their feared prediction (assessed prior to beginning the treatment trials) to actions they took in the chamber. On a 100-point scale, participants rated the extent to which their feared prediction failed to occur "because I took specific actions to prevent this from happening" and "because I knew I could take specific actions to prevent this from happening." Participants were not asked to specify their relevant actions, and as such safety may

have been attributed to the availability or utilization of the three coping aids provided by the experimenter or additional actions such as attempts to distract or reassure themselves. Previous research suggests that the use of covert safety behaviors in the chamber is common and does not differ in frequency based on whether participants are provided with the option to use the three coping aids (Deacon, Sy, Lickel, & Nelson, 2010). The misattribution of safety items were administered immediately after each of the six chamber exposure trials. Trial 6 scores on each item were summed to form a total score that served as a posttreatment index of the extent to which participants continued to misattribute the non-occurrence of their feared prediction to safety behaviors used in the chamber. Trial 1 scores were also summed and compared to Trial 6 sum scores in order to assess whether statistically significant changes in the misattribution of safety occurred during treatment. The misattribution of safety scale demonstrated good internal consistency ($\alpha = .81$).

Cognitive avoidance scale

Three cognitive avoidance items were constructed for the present study based on Johnstone and Page's (2004) one-item measure assessing the amount of attentional resources devoted toward or away from a given stimulus. Participants were asked to rate on a 100-point scale the percentage of time spent thinking about "actions you could take to prevent your feared prediction from occurring," "actions you could take to reduce your anxiety," and "distracting yourself (thinking about other things)." These items were administered immediately after each of the six chamber exposure trials. Trial 6 scores were summed to form an index of the extent to which participants were engaging in cognitive avoidance at posttreatment. Trial 1 scores were also summed and compared to Trial 6 sum scores in order to assess whether statistically significant changes in cognitive avoidance occurred during treatment. The scale had adequate internal consistency ($\alpha = .73$).

Inference of danger scale

Two inference of danger items were constructed for the present study to measure the extent to which participants inferred that the chamber was dangerous from the availability of the three coping aids. These items were adapted from Telch et al.'s (2010) Defibrillator Questionnaire. Participants rated their agreement on a 100-point scale with the following items: (a) "Because of the three coping aids, I felt that I was in a potentially harmful or dangerous situation," and (b) "The presence of the coping aids made me question the safety of the chamber." These items were administered to participants in both safety behavior conditions after each exposure trial. Since participants in the EO condition were never made aware of the coping aids, these items were not applicable and thus not administered. Trial 1 scores were also summed and compared to Trial 6 sum scores in order to assess whether statistically significant changes on the measure occurred during treatment. Trial 6 scores were summed to determine posttreatment levels of inference of danger. Internal consistency was excellent ($\alpha = .99$).

Procedure

Procedures common to each condition

Participants were randomly assigned to one of three treatment conditions described below and completed up to 30 min of in vivo exposure in the claustrophobia chamber. Each participant received brief psychoeducation about claustrophobia and exposure therapy. The experimenter explained that claustrophobic fear is fueled by avoidance of enclosed spaces and specific beliefs of harm, and that the best way to reduce such fears is to be confronted with the feared situation until the fear decreases. After the experimenter introduced the treatment rationale and procedures to the participant, ratings of treatment credibility and expectancy were assessed. Finally, participants were asked to identify their most feared negative outcome (e.g., running out of air, losing control, being unable to escape) from a list of common predictions associated with claustrophobia. Participants then conducted six exposure trials, which involved lying in the claustrophobia chamber with the door closed for at least 5 min. After the participant entered the chamber, the experimenter locked the door and stepped just outside of the room. At the end of each trial, participants were given approximately 10 min during which they were asked to rate their peak fear, catastrophic cognitions and self-efficacy during the exposure and to complete the cognitive avoidance, misattribution of safety, and inference of danger scales before beginning the next trial. All assessments and study-related tasks were completed within the same 90-min session.

Safety behavior utilization (SBU) condition

Prior to each trial, the experimenter informed each participant, "You will have three coping aids to assist you with the exposures," then provided instructions to the participant to access each coping aid. The coping aids were as follows: 1) opening a small door on the side of the chamber to let in air blowing from a fan, 2) having the chamber door unlocked, and 3) communicating with the experimenter via two-way radio.

Following Powers et al., participants were told to listen carefully to instructions from the radio and specifically to say, "Please unlock the chamber now," upon hearing a signal that 2 min had passed. They were also told to open the small window at the side of the chamber at the beginning of each trial to let in fresh air blowing from a fan. When the participant entered the chamber, the experimenter closed and locked the door, turned off the lights, and left the room, entering an adjacent room and closing the door. Immediately after stepping outside the room, the experimenter asked, "Are you okay?" on the radio. The experimenter then asked, "Everything okay?" on the two-way radio at thirty-second intervals for the entire duration of each exposure trial. After 2 min, the participant heard the signal for them to request that the door be unlocked. The experimenter then entered the room and unlocked the doors of the chamber. Participants were given a signal in both the SBU and SBA conditions to ensure that both conditions were equally aware that 2 min have passed (M.B. Powers, personal communication, April 23, 2008).

Safety behavior availability (SBA) condition

Prior to each trial, participants were given the option to use one of the three coping aids while in the chamber as listed above. Specifically, the experimenter informed them, "In order to assist you in coping with your fear while in the chamber, three coping aids will be available to you. However, please only use these aids if you feel you must." As in the SBU condition, participants heard a signal after 2 min, at which point participants had the option to ask the experimenter to unlock the door if they chose.

Exposure only (EO) condition

Participants completed six, 5-min in vivo exposure trials in the claustrophobia chamber without the coping aids given in the SBA and SBU conditions.

Results

Preliminary analyses

Baseline equivalence of conditions

Participants in the three conditions did not differ significantly with respect to age, $F(55) = .69$, $p = .50$, or sex, $\chi^2(1) = 3.37$, $p = .16$.

To confirm that the three experimental groups were comparable regarding baseline levels of claustrophobic concerns, a one-way ANOVA was conducted examining between-group differences on pretreatment BAT peak fear ratings and scores on the CLQ-SS, CLQ-RS, CLQ-total, CCSES, and CCQ. None of the tests were significant (all p's $> .05$), indicating that the randomization procedure resulted in comparable conditions (see Table 1).

Treatment credibility and expectancy

Mean credibility ratings for participants in the EO condition ($M = 5.32$, $SD = 1.42$), the SBA condition ($M = 5.96$, $SD = 1.36$), and the SBU condition ($M = 5.96$, $SD = .99$) did not significantly differ, $F(55) = 1.57$, $p = .22$, partial $\eta^2 = .05$. Similarly, mean expectancy ratings for the EO condition ($M = 41.31$, $SD = 18.09$), the SBA condition ($M = 41.19$, $SD = 18.70$), and the SBU condition ($M = 48.89$, $SD = 17.11$) were not significantly different, $F(55) = 1.11$, $p = .34$, partial $\eta^2 = .04$.

Duration of chamber exposures

The mean duration of time spent in the chamber during the six trials did not significantly differ among participants in the three conditions, $F(55) = 1.02$, $p = .37$, partial $\eta^2 = .04$. Participants spent an average of 297.9 ($SD = 16.0$) out of a possible 300 seconds in the chamber during each exposure trial, and 96. 6% of participants (94.7% in the EO condition, 100.0% in the SBA condition, and 94.4% in the SBU condition) remained in the chamber for the full duration of all six trials.

Safety aid utilization and treatment outcome

In the SBA condition, 19.0% ($n = 4$) of participants used at least one coping aid during the exposure trials. Of the three coping aids offered, only the window was used. Use of this coping aid was not associated with higher pretreatment severity on any of the measures (all p's $> .05$). Due to the small sample size, results of inferential statistical tests should be interpreted with caution.

Table 1
Descriptive statistics on measures of claustrophobic concerns at pretreatment and posttreatment for each treatment condition.

Measure	Treatment condition					
	Exposure only ($n = 19$)		Safety behavior availability ($n = 21$)		Safety behavior utilization ($n = 18$)	
	Pre	Post	Pre	Post	Pre	Post
CLQ-RS						
M	19.74	11.05	31.62	18.67	28.00	12.06
SD	6.59	5.60	7.28	10.35	10.91	7.38
CLQ-SS						
M	22.53	12.16	27.90	17.52	24.44	11.28
SD	7.54	6.27	8.51	9.39	10.16	6.55
CLQ-total						
M	47.06	23.21	59.52	36.19	52.44	23.33
SD	14.15	11.83	14.70	19.25	20.18	13.18
CCQ						
M	482.11	171.05	546.19	192.86	548.89	77.83
SD	155.91	154.23	123.19	182.43	149.39	94.06
CCSES						
M	226.32	348.68	187.86	334.52	194.17	414.72
SD	59.20	59.11	52.26	72.33	48.42	144.07
BAT peak fear						
M	69.05	32.21	79.29	47.62	74.44	35.11
SD	21.96	23.04	13.35	24.48	21.62	24.69

Note. BAT = behavioral approach task; CLQ-RS = Claustrophobia Questionnaire – Restriction Subscale; CLQ-SS = Claustrophobia Questionnaire – Suffocation Subscale; CLQ-total = Claustrophobia Questionnaire Total score; CCSES = Claustrophobia Coping Self-Efficacy Scale; CCQ = Claustrophobic Concerns Questionnaire.

However, effect sizes for pretreatment measures of severity for participants who did or did not use safety behaviors were mixed (BAT peak fear: $d = -.27$; CLQ: $d = .47$; ASI-3: $d = .55$; CCQ: $d = 1.07$), calling into question whether statistically significant differences would emerge with a larger sample size.

Treatment outcomes

Mean scores and standard deviations for the three conditions on treatment outcome measures at pretreatment and posttreatment are presented in Table 1. A series of 3×2 (condition by time) mixed ANOVAs was used to compare between-group changes in claustrophobic severity measures (CCQ, CCSES, CLQ-RS, CLQ-SS, CLQ-total, and peak BAT fear) as well as task-relevant self-efficacy (CCSES). We predicted a significant time by condition interaction on each treatment outcome variable, and that post-hoc tests would reveal significantly more improvement for the exposure condition vs. the SB conditions.

In the first model, a significant main effect of time was detected for CCQ scores, $F(1, 55) = 255.60$, $p < .001$, partial $\eta^2 = .82$, indicating improvement from pretreatment to posttreatment. A significant time by condition interaction was obtained, $F(2, 55) = 3.92$, $p = .03$, partial $\eta^2 = .13$. Follow-up t-tests of CCQ change scores revealed no significant difference in improvement between the EO and SBA conditions, $t(38) = .76$, $p = .45$, $d = -.23$. In contrast, participants in the SBU condition had significantly greater pre–post change scores on the CCQ than those in the EO condition, $t(35) = 2.63$, $p = .01$, $d = .85$, and the SBA condition, $t(37) = 2.05$, $p = .05$, $d = .66$.

In the second model, a significant main effect of time was detected for CCSES scores, $F(1, 55) = 324.78$, $p < .001$, partial $\eta^2 = .86$, indicating improvement from pretreatment to posttreatment. A significant interaction was found between time and condition for CCSES scores, $F(2, 55) = 4.08$, $p = .02$, partial $\eta^2 = .13$. Follow-up t-tests of CCSES change scores revealed no significant differences in improvement between the EO and SBA conditions, $t(38) = 1.18$, $p = .24$, $d = -.32$, and between the SBA and SBU conditions, $t(37) = 1.68$, $p = .10$, $d = -.64$. As with CCQ scores, significantly greater change on the CCSES was evident in the SBU condition than the EO condition, $t(35) = 2.99$, $p = .001$, $d = .84$.

Significant main effects of time were found for the CLQ-RS, $F(1, 55) = 106.16$, $p < .001$, partial $\eta^2 = .66$, CLQ-SS, $F(1, 55) = 118.87$, $p < .001$, partial $\eta^2 = .68$, CLQ-total, $F(1, 55) = 139.646$, $p < .001$, partial $\eta^2 = .73$, and peak BAT fear scores, $F(1, 55) = 163.78$, $p < .001$, partial $\eta^2 = .75$, indicating that participants' scores improved significantly between the pretreatment and posttreatment assessments. Condition by time interactions were not significant for the CLQ-RS ($F[2, 55] = .59$, $p = .56$, partial $\eta^2 = .02$), CLQ-SS ($F[2, 55] = .78$, $p = .46$, partial $\eta^2 = .03$), CLQ-total ($F[2, 55] = 1.14$, $p = .33$, partial $\eta^2 = .04$), and peak BAT fear ($F[2, 55] = 1.95$, $p = .15$, partial $\eta^2 = .07$).

Treatment process measures

Means and standard deviations on the cognitive process scales, assessed following the final claustrophobia chamber trial, are summarized in Table 2. Baseline severity of claustrophobic fear, as measured by the CLQ-total, was not significantly correlated with Trial 6 scores on any of the cognitive process measures (all p's $> .32$). A series of one-way ANOVAs showed that none of the conditions differed significantly in ratings on any of the scales measuring misattribution of safety, inferences of danger, and cognitive avoidance following Trial 6 (all p's $> .05$). Each treatment process scale was subjected to a series of 3×2 (condition by time) ANOVAs to examine the pattern of between-group changes in scores from Trial 1 to Trial 6. A significant main effect of time

Table 2
Descriptive statistics on cognitive process measures at Trial 1 and Trial 6.

Measure	Treatment condition							
	Exposure only ($n = 19$)		Safety behavior availability ($n = 21$)		Safety behavior utilization ($n = 18$)		All conditions ($n = 58$)	
	Trial 1	Trial 6	Trial 1	Trial 6	Trial 1	Trial 6	Trial 1	Trial 6
Misattribution of safety								
M	120.00	109.74	124.52	117.14	146.67	143.89	129.91	123.02
SD	58.93	66.41	49.24	72.82	31.90	70.60	48.81	70.35
Cognitive avoidance/distraction								
M	146.58	141.05	173.57	133.81	144.44	76.94	155.69	118.53
SD	77.80	78.59	44.73	85.42	24.75	68.22	61.90	81.85
Inference of danger								
M	–	–	46.00	13.25	51.44	2.78	48.58	8.29
SD	–	–	40.05	23.86	49.31	9.58	44.14	19.04

indicating a decrease in scores from Trial 1 to Trial 6 was found for the misattribution of safety scale, $F(1, 55) = 413.34$, $p < .001$, partial $\eta^2 = .88$, the cognitive avoidance scale, $F(1,55) = 374.06$, $p < .001$, partial $\eta^2 = .87$.and the inference of danger scale, $F(1,36) = 39.36$, $p < .001$, partial $\eta^2 = .52$. Follow-up *t*-tests of average cognitive avoidance scores revealed no significant differences between the EO and SBA conditions, $t(38) = .58$, $p = .57$, $d = -.08$, and between the EO and SBU conditions, $t(35) = 188$, $p = .07$, $d = .71$. Cognitive avoidance scores were significantly higher in the SBA condition than the SBU condition, $t(37) = 2.55$, $p = .02$, $d = .82$. No significant interactions were found for any of the three scales (all *p*'s > .10).

We had intended to examine whether scores on the cognitive process measures accounted for the hypothesized differences in treatment outcome between the EO condition and the two safety behavior conditions. However, such analyses were not possible because scores on process measures did not vary according to treatment condition. Instead, exploratory correlational analyses were conducted to examine the association between posttreatment scores on the cognitive process measures and treatment outcomes. Because there were no between-condition differences on these scales, scores were combined across all participants. These findings are summarized in Table 3.

Trial 6 misattribution of safety scale scores were not significantly correlated with improvement on any treatment outcome measure from pretreatment to posttreatment, all *p*'s > .05. Higher trial 6 cognitive avoidance scale scores showed a significant, negative association with pre–post reductions on peak BAT fear ($r = -.33$, $p = .01$) and the CLQ ($r = -.34$, $p = .01$). Because the inference of danger scale was not administered in the EO condition, only participants in the SBU and SBA conditions were included in the analyses. Trial 6 inference of danger scores were significantly and negatively correlated with changes in CCQ ($r = -.58$, $p < .001$), peak BAT fear ($r = -.51$, $p = .001$), CLQ ($r = -.43$, $p = .01$), and CCSES scores ($r = -.41$, $p = .01$). These findings indicate that participants in the safety behavior conditions who reported greater inferences of danger following the exposure task evidenced less improvement.

Table 3
Associations between Trial 6 cognitive process measures and pre–post outcome change scores.

Measure	Pre–post outcome change score					
	CLQ-SS	CLQ-RS	CLQ-total	CCQ	CCSES	BAT
Misattribution of safety	–.05	–.09	–.12	.14	.15	–.03
Cognitive avoidance	–.22	–.21	–.34*	–.19	–.15	–.33*
Inference of danger	–.38 *	–.37*	–.43**	–.58**	–.41*	–.51*

Note. *$p < .05$, **$p < .01$. CLQ-SS = Claustrophobia Questionnaire – Suffocation Subscale; CLQ-RS = Claustrophobia Questionnaire – Restriction Subscale; CLQ-total = Claustrophobia Questionnaire Total score; CCQ = Claustrophobic Concerns Questionnaire; CCSES = Claustrophobia Coping Self-Efficacy Scale (reverse scored); BAT = behavioral approach task.

Table 4
Benchmark comparisons of pretreatment characteristics and pre–post treatment effect sizes.

	Pretreatment characteristics	
	Powers et al. (2004)	Current study
Age		
M	21.06	19.28
SD	5.07	1.54
% Female	86%	78%
CLQ-total at pretreatment		
EO condition *M* (*SD*)	54.06 (15.99)	47.06 (14.15)
SBA condition *M* (*SD*)	47.36 (15.91)	59.52 (14.70)
SBU condition *M* (*SD*)	46.44 (13.71)	52.44 (20.18)

	Pre–post treatment effect sizes (within-group)					
	EO	SBA	SBU	EO	SBA	SBU
CLQ: suffocation (pre–post)	.58	.23	.18	.60	.50	.61
CLQ: restriction (pre–post)	.55	.32	.30	.58	.49	.59
CLQ:total	.58	.29	.29	.59	.50	.60

Note. CLQ = Claustrophobia Questionnaire; EO = exposure only; SBA = safety behavior availability; SBU = safety behavior utilization.

Benchmarking

In order to compare the current study's results to those obtained by Powers et al. (2004), the benchmarking strategy described by Wade, Treat, and Stuart (1998) was used. Since the current study is a replication and extension of the Powers et al. study, benchmarking is useful in determining the extent of the replication's success and detecting any notable deviations. Descriptive statistics of sample demographics (e.g., gender, age, baseline CLQ scores, diagnostic status) and pre–post effect sizes on the CLQ (the one measure common to both studies) for each study are summarized in Table 4. To assess the magnitude of improvement in CLQ scores within the treatment conditions, we used the procedures for calculating effect sizes recommended by Cohen (1988) for independent groups pretest–posttest designs. Uncontrolled within-group effect sizes, computed to characterize the magnitude of change within each treatment condition, were calculated as the difference between pre- and posttreatment means divided by the pooled standard deviation.

Qualitative comparisons of demographics showed that the current study's sample is highly similar to that of Powers et al. (2004). Furthermore, comparison of treatment effect sizes as measured by the CLQ revealed that although the within-group treatment effect sizes in the EO conditions were virtually identical in both studies, the current study's SBA and SBU conditions performed approximately twice as well as those in Powers et al (see Table 4).

Discussion

The present study was conducted to replicate the findings of Powers et al. (2004) suggesting that the utilization or availability of safety behaviors can equally interfere with exposure therapy, and to elucidate the mechanisms underlying this interference. Considerable efforts were made to replicate the methodology as closely as possible, which included obtaining the treatment protocols and communicating with the first author of the original study about specific details of the procedure. The identical screening procedure

resulted in a study sample whose pretreatment characteristics closely matched those in the original study. Contrary to expectations, the present findings failed to replicate those of Powers et al. Although all three conditions evidenced significant improvement, there were no differences in fear reduction. Moreover, participants in the SBU condition experienced more improvement in task-specific self-efficacy and greater reduction in task-specific claustrophobic cognitions than those in the EO condition. Lastly, participants did not significantly differ by condition in their endorsement of any of the between-trial cognitive process items.

One of the few ways that the current study deviated from the Powers et al. (2004) methodology was the nature of the behavioral approach tasks (BATs). In the original study, the BATs both involved entering the same claustrophobia chamber used for the treatment trials (without any coping aids offered). It is possible that the large advantage of the exposure only condition over the other conditions in their study were due to practice effects; that is, participants in the EO condition had already habituated to a nearly identical context whereas those in the SB conditions found themselves in a fairly novel context owing to the absence of safety aids. However, this explanation does not account for the finding that the SB conditions also evidenced less improvement than the EO condition on the CLQ in Powers et al. (2004). The BAT used in the current study eliminated any treatment-induced practice effects, instead assessing the extent that *generalized* fear reduction and learning took place thus constituting an arguably stronger test of treatment outcome. Thus, it was not possible to directly compare changes in peak BAT fear in the two studies. However, benchmarking of the CLQ scores revealed that the effect sizes of all three conditions in the current study are comparable to that of the EO condition in the Powers et al. study.

The reason why the safety behavior conditions in the current study do not seem to show the same disadvantages as those in the Powers et al. study is unclear. It is possible that the slightly longer BAT in the current study (6 min compared to 4 min in the original study) may have served as additional exposure treatment time and partially accounted for the difference in effect sizes. Perhaps additional replications of the paradigm in the treatment of different fears can shed light as to the actual robustness of the Powers et al. findings. Additionally, eliminating the pretreatment BAT may reduce the impact of BAT-related learning on treatment outcome. Future research may clarify the extent to which the deleterious effects of safety behaviors on exposure therapy demonstrated by Powers are sufficiently robust to survive minor variations in experimental methods.

The findings of the current study are inconsistent with Salkovskis' misattribution of safety hypothesis. Neither the use nor the availability of safety behaviors was associated with greater misattributions of safety, and misattributions of safety following the treatment were not associated with worse outcomes. Due to the broad wording of the safety behavior scale items (e.g., "because I took specific actions to prevent this from happening," "because I knew I could take specific actions to prevent this from happening"), it is possible that participants who used less-interfering coping strategies also endorsed them. For instance, participants could believe that they were able to prevent suffocation by reminding themselves that they were not in danger, or by calmly breathing. Since they might then learn that they can always use self-reassurance or controlled breathing when in an enclosed space in the future, lasting fear reduction could still occur. In short, perhaps misattributing safety to behaviors that do not preclude threat disconfirmation does not interfere with fear reduction in some cases.

The results provide mixed support for some aspects of Sloan and Telch's (2002) distraction hypothesis. Although no differences in safety behavior-related cognitive avoidance were found among treatment conditions, the percentage of time spent thinking about actions to prevent feared outcomes from occurring or to reduce anxiety was associated with less fear reduction as measured by BAT peak fear and CLQ scores. Given the correlational nature of these findings, however, it is possible that participants with greater residual claustrophobic fear simply spent more distracting themselves with coping attempts during Trial 6. These potential confounds illustrate the problems associated with attempts to test cognitive explanations of behavioral phenomena. Despite a wealth of speculation as to which cognitive mechanisms affect the process and outcome of exposure therapy, few if any validated measures exist to assess these mechanisms. The lack of validated cognitive process measures is a limitation of the current study and of research in this area in general.

Finally, endorsements of inference of danger-related statements were significantly associated with less improvement on all of the treatment outcome measures. Given that both safety behavior conditions performed as well as the EO condition in terms of treatment outcome, but greater inference of danger in the safety behavior conditions was associated with worse outcomes, perhaps the minimization of inference of danger could have resulted in the superiority of the safety behavior conditions over the EO condition. For instance, introducing safety aids as temporary comfort objects that will eventually be unnecessary may help to lower inferences of danger associated with the presence of safety aids. The results of the current study suggest that inference of danger should be more prominently considered as a possible mechanism when considering the role of safety behaviors in exposure therapy.

The apparent equivalence of the three exposure conditions in the present study is consistent with Rachman, Radomsky, and Shafran's (2008) hypothesis that the use of safety behaviors may not necessarily interfere with fear reduction or disconfirmatory learning. The "judicious use" of safety behavior, as defined by Rachman et al., is the use of safety behaviors, anxiety-control strategies, and other within-situation behaviors with the intended purpose of making exposure appear less threatening and increasing approach behavior in the initial stages of treatment. These results are also consistent with the results of Deacon et al. (2010), who found no differences between a "judicious safety behavior" condition and an exposure only condition for claustrophobic fear. However, the SBU condition in the present study involved an excessive and forced use of safety behaviors that exceeds Rachman, Radomsky, and Shafran's definition of their "judicious use." Having such a condition perform as well as standard exposure in the present study is unexpected and stands in stark contrast to the findings of Powers et al. (2004) using a nearly identical methodology.

Several limitations of the current study deserve note. As in Powers et al. (2004), the study population was a non-treatment-seeking undergraduate sample, which may limit the generalizability of the findings to clinical, treatment-seeking populations. The present study had a relatively small sample size, which limited our ability to detect significant differences between the three treatment conditions. However, examination of effect sizes on treatment outcome and process measures indicates that a larger sample size would have little effect on the statistical significance of our primary findings. The per-condition sample size of the current study actually exceeded that of Powers et al. (2004), further decreasing the likelihood that the failure to replicate can be attributed to insufficient sample size. The between-trial cognitive process measures could not be assessed for mediator status as they did not correlate with treatment condition; thus, it is impossible to determine the causal direction of any relationships between these constructs and treatment outcome. As these items do not have established validity or reliability, further research is needed to

determine whether these are adequate measures of the cognitive phenomena in question. Additionally, as the inference of danger scale was only administered in the safety behavior conditions, there is a possibility that its repeated administration may have influenced participants' response to treatment, potentially giving an advantage to the safety behavior conditions in the current study and contributing to the differences in findings between the two studies.

It should be noted that all three conditions in both the current study and Powers et al. involved some degree of additional safety behavior availability, as the informed consent process necessitated informing participants that they could exit the chamber at any time. In other words, the presence of general experimental assurances of safety may have lessened the overall effect of safety cues, which may account for the lack of robustness of the original study's findings. However, due to necessity of informed consent, safety is inherent in any experimental study of exposure therapy. This context is similar to real-world practice in which participants are typically free to terminate therapeutic exposures at any time.

An additional limitation is that the single-session design of the current study precluded the establishment of causal relationships between cognitive process variables and treatment outcomes. Future research exploring cognitive mediators of change in exposure therapy should utilize a multi-session design to circumvent this limitation. Another possible limitation is the absence of a control condition in the current study. The decision to exclude a control condition was based on findings from each of the previous claustrophobia chamber exposure studies published by Telch and colleagues (Kamphuis & Telch, 2000; Powers et al., 2004, 2008; Sloan & Telch, 2002) that EO was significantly more effective than a credible placebo control condition. Despite these findings, the absence of a control condition in the present study does not rule out the possibility that improvement was attributable to nonspecific factors such as expectancies and regression to the mean. Similarly, the current study did not include a follow-up assessment due to a consistent pattern of non-significant differences between posttreatment and follow-up measures in prior studies using the claustrophobia chamber paradigm (Kamphuis & Telch, 2000; Powers et al., 2004, 2008; Sloan & Telch, 2002). It is possible that a follow-up assessment would have yielded results deviating from previous findings, particularly with the cognitive process measures included in this study. It will be important for future research to determine the short-term and longer-term effects of the availability and utilization of safety behaviors during exposure therapy. The value of studying exposure interventions across multiple treatment sessions has been demonstrated by recent studies (Baker et al., 2010; Plendl & Wotjak, 2010) suggesting that between-session fear reduction may better predict improvement than within-session habituation.

In summary, the current study failed to replicate the findings of Powers et al. (2004) despite virtually identical methodology, instead failing to demonstrate that safety behavior availability or utilization interferes with the effectiveness of exposure therapy. In fact, the SBU condition resulted in more improvement than the EO condition on measures of task-specific claustrophobic cognitions and self-efficacy. Additionally, greater coping aid-induced inference of danger were associated with less treatment improvement. These findings raise the possibility that indications of danger may be more pertinent to exposure therapy than attributions of safety. Similar investigations using different phobias or anxiety disorders may clarify the extent to which the interfering effects of safety behavior availability and utilization on exposure therapy as reported by Powers et al. (2004) are applicable beyond claustrophobic fear. It is possible that certain types of safety behaviors might interfere more with exposure therapy for some fears and less so with others depending on the nature of feared predictions. For example, the use of distraction may not interfere with threat disconfirmation in people who fear suffocation in enclosed spaces, but may create a misattribution of safety in people who are afraid of going crazy in the same situation. In other words, it may be less important to assess or manipulate the presence of safety behaviors themselves than assessing the individual's perception of the function and/or meaning of these behaviors. Clinicians should perform a careful functional analysis of a client's behaviors in feared situations and assess the extent to which they impede or aid in fear reduction. Additional treatment studies and the availability of reliable and valid cognitive process measures may ultimately lead to research findings that will better aid exposure therapists in making clinical decisions.

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