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Safety behaviors toward innocuous stimuli can maintain or increase threat beliefs

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ABSTRACT

Safety behaviors can prevent or minimize a feared outcome. However, in relatively safe situations, they may be less adaptive, presumably because people will misattribute safety to these behaviors. This research aimed to investigate whether safety behaviors in safe situations can lead to increased threat beliefs. In Study 1, we aimed to replicate a fear conditioning study ($N = 68$ students) in which the experimental, but not the control group, received the opportunity to perform safety behavior to an innocuous stimulus. From before to after the availability of the safety behavior, threat beliefs persisted in the experimental group, while they decreased in the control group. In Study 2, we examined whether threat beliefs had actually increased for some individuals in the experimental group, using a multi-dataset latent class analysis on data from Study 1 and two earlier studies ($N = 213$). Results showed that about a quarter of individuals who performed safety behavior toward the innocuous stimulus showed increased threat expectancy to this cue, while virtually nobody in the control group exhibited an increase. Taken together, safety behavior in relatively safe situations may have maladaptive effects as it generally maintains and sometimes even increases threat beliefs.

Safety behaviors involve precautions to prevent or minimize a feared outcome. Many people regularly engage in such behaviors, such as frequent hand washing and avoidance of contact with potential contaminants (Deacon & Maack, 2008), particularly during the current pandemic to slow the spreading of the coronavirus. Safety behaviors that reduce threat are obviously essential to survival. However, they may also be used in low threat situations. For example, consider people who knock on wood to avert bad outcomes or patients with a panic disorder who sit down when they feel dizzy because they are afraid to faint. Although safety behaviors in such situations may be considered benign ("better safe than sorry"), there may be costs to performing them. Specifically, in some situations, the behavior is not proportional to the threat and may, ironically, even increase threat perception (Sharpe et al., 2022). How could safety behavior lead to increased threat perception? On the one hand, people may accommodate their cognitions to their behavior to reduce cognitive dissonance (Festinger & Carlsmith, 1959; Harmon-Jones et al., 2015). On the other hand, safety behaviors are also thought to prevent the disconfirmation and updating of threat beliefs (akin to "protection from extinction"; see Clark, 1999; Lovibond et al., 2009). For example, when patients with a panic disorder sit down when they fear fainting, they will not learn that dizziness is not a harbinger of fainting (Telch & Zaizar, 2020). Therefore, studies have been conducted to find out whether safety behaviors actually enhance threat beliefs.

Laboratory experiments (e.g., Lovibond et al., 2009; van Uijen et al., 2018; Vervliet & Indekeu, 2015) using fear conditioning paradigms have demonstrated that safety behaviors can maintain or increase threat beliefs in "high-threat" situations. These experiments started with a fear learning phase in which participants were exposed to neutral stimuli, such as geometrical shapes, that were repeatedly paired with a threatening outcome (e.g., a mild electrical shock). Then, participants received the opportunity to perform safety behavior (e.g., a button press) toward cues that signaled impending threat. Less is known, however, about the causal relationship between safety behaviors and threat beliefs in "low-threat" or relatively safe situations, in which cues do *not* signal an impending threat. These situations are more typical for clinical anxiety than high-threat situations (e.g., Lissek et al., 2006). Several field studies found evidence for a causal relation between safety

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behavior and threat beliefs in low threat situations. For example, college students who were instructed to apply contamination-related safety behaviors for a week (e.g., washing and disinfecting hands repeatedly; Deacon & Maack, 2008; Olatunji et al., 2011) showed increased contamination concerns a week later. Likewise, applying checking behaviors for a week led to increased safety concerns (van Uijen & Toffolo, 2015). However, these studies did not manipulate actual threat or safety. Therefore, a controlled lab study was conducted to examine whether safety behavior toward a safe stimulus increases threat beliefs when that behavior is no longer available (Engelhard et al., 2015). This experiment started with a fear learning phase, in which one neutral cue (i.e., "danger cue") was followed by a mild electrical shock, whereas two other neutral cues were not (i.e., "safety cues"). In a subsequent safety behavior learning phase, participants could prevent the shock by pressing a button in response to the danger cue. Next, in the safety behavior *shift* phase, participants in the experimental group, but not in the control group, received the opportunity to perform safety behavior toward one of the two *safety* cues. Finally, in a *test* phase, the danger and safety cues were presented without the opportunity to perform safety behavior. The results of the test phase showed that participants in the experimental group, relative to the control group, exhibited higher threat expectancy to the safety cue to which they previously applied safety behavior. In other words, from before to after the safety behavior shift phase, threat expectancy to this safety cue persisted in the experimental group while it decreased in the control group (Engelhard et al., 2015). This suggests that safety behavior toward safe stimuli does not increase but maintains threat beliefs. These findings were recently replicated (Xia et al., 2019).

Even though these two studies (Engelhard et al., 2015; Xia et al., 2019) provided evidence that safety behavior toward innocuous stimuli maintains threat beliefs, two problems remain. First, they excluded about 28% of participants in the experimental group who did not apply safety behavior toward the safety cue (Engelhard et al., 2015; Xia et al., 2019), potentially resulting in a selection bias. Second, they used statistical methods to analyze mean differences, which may neglect relevant heterogeneity in performance (Krypotos et al., 2018; T. B.; Lonsdorf & Merz, 2017). Advanced modeling techniques (see Bonanno et al., 2012; Galatzer-Levy et al., 2013) could elucidate whether and for whom safety behaviors to safe stimuli may also lead to increased threat beliefs, but such techniques require larger sample sizes.

The aim of the current research was twofold. First, in Study 1, we sought to replicate and extend Engelhard et al. (2015) by employing a design that would prevent the high exclusion rates in the experimental group. To reduce exclusion rates, we increased stimulus ambiguity (i.e., partial reinforcement and fewer safety cues presentations), which could motivate participants to apply safety behavior toward the safety cue (see Lissek et al., 2006). We also measured skin conductance in addition to self-report outcome measures to have a more comprehensive assessment of associative learning (Constantinou et al., 2021). Second, in Study 2, we performed a multi-dataset analysis on all three studies (Engelhard et al., 2015; Xia et al., 2019; current Study 1) using meaningful change scores and latent class analyses to examine heterogeneity in threat expectancy over time. We predicted that the experimental group would predominantly show a maintained or increased threat expectancy to the safety cue when the safety behavior is no longer available, while the control group would mainly show decreased threat expectancy to this safety cue.

1. Study 1

1.1. Method

1.1.1. Participants

One hundred Dutch-speaking undergraduate students were recruited and tested at Utrecht University. Of these, 32 were excluded (see below), resulting in a final sample size of 68 participants (14 males; 54 females; mean age = 20.85; *SD* = 2.02) who were randomly assigned to the experimental ($n = 34$) and control group ($n = 34$). The sample size ($N = 68$) was set before data collection using G*Power 3.1 (Faul et al., 2009; settings: repeated measures analysis of variance; within-between interaction, $\eta_p^2 = 0.025$, $\alpha = 0.05$, power = 0.80, 2 groups, 3 measures). We aimed to detect a small to medium effect (see Engelhard et al., 2015; Xia et al., 2019). The study adhered to the Dutch legal requirements and was approved by the Faculty of Social and Behavioral Sciences ethics committee at Utrecht University (FETC15-014).

1.1.2. Measures

Shock unpleasantness and threat expectancy. Shock unpleasantness was assessed with an 11-point scale, ranging from 0 (not unpleasant at all) to 10 (very unpleasant). Threat expectancy was rated on a visual analog scale ranging from 0 (certainly no shock) to 100 (certainly a shock); following Engelhard et al. (2015).

Neuroticism Scale of the Eysenck Personality Questionnaire (EPQ-N). Neuroticism was measured using the Dutch EPQ-N version (Eysenck & Eysenck, 1991; Sanderman et al., 2012), which may be relevant to explore individual differences in safety behavior (Lommen et al., 2010). It includes 22 self-report items (e.g., "Are you often troubled about feelings of guilt?") that are rated on a dichotomous scale (0 = no, 1 = yes). Cronbach's α was 0.88 in the current study.

1.1.3. Skin conductance

Skin conductance activity was recorded using two 8-mm passive Nihon Kohden electrodes that were placed on the index and middle fingers of the non-dominant hand. Two 4-mm Ag–AgCl reference electrodes were attached to the forehead. Skin conductance signals were amplified with a Biosemi system and were recorded with a separate computer running ActiView 7.06 at a 2048 Hz sampling rate.

1.1.4. Stimuli and apparatus

The conditioned stimuli (i.e., CSs: A+, B-, and C-) consisted of 6 × 6 cm blue, yellow, and pink squares (randomized for each participant) and were presented in the middle of the screen. The unconditioned stimulus (US) was a 0.5-s tone (95 dB) combined with a 0.5-s electrical shock (range 0.2–4.0 mA), which was delivered by a Coulbourn Transcutaneous Aversive Finger Stimulator [E13-22] through electrodes attached to the index and middle finger of the dominant hand. The combination of a shock with a tone may prevent US habituation (Lovibond et al., 2009). A serial response box (model 200A) with five lights and corresponding buttons was placed in front of the monitor. The experimental paradigm and response collection were controlled by Python 2.7.

1.1.5. Trial procedure

Trials started with an 8-s CS presentation, after which participants received 5 s to rate their threat expectancy using their dominant hand. Trials ended with a 0.5-s period during which the US could be presented. Inter-trial intervals (ITIs) randomly varied between 15 s and 36 s. In some phases, a response box light illuminated during a CS presentation, which provided participants with the opportunity to prevent the US (i.e., "if you press the button below the light, the shock will not occur"). Trials were presented in a pseudorandom order with a maximum of two identical trials in sequence. A maximum of 3 successive presentations of the same trials was allowed during the safety behavior acquisition phase.

The methodology differed from Engelhard et al. (2015) in three significant ways. First, we added skin conductance measures, a physiological measure of arousal, and, therefore, prolonged the CS presentations and ITIs. Second, we aimed to reduce the exclusion rate of participants who do not show a safety behavior shift by including fewer safety cue trials. Presumably, this would increase stimulus ambiguity, which may instigate fear (Lissek et al., 2006) and, thereby, safety behavior. Finally, we reduced the reinforcement rate to A+ from 100%

to 75%. This way, we did not have to exclude participants who applied safety behavior toward C- only three out of four times.

1.1.6. General procedure

Table 1 displays the general experimental procedure. After providing informed consent, participants were attached to skin conductance and shock electrodes. Participants selected a "certainly annoying, but not painful" shock level through a work-up procedure (Engelhard et al., 2015). Throughout the experiment, they wore headphones that played an 80-dB white noise to mask external sounds. Participants were instructed to learn the relationship between the blocks' color and shock occurrence. After six practice trials, they started with a Pavlovian acquisition phase in which A+ was followed by the US in 3 out of 4 trials (random reinforcement order), while B- and C- were never followed by the US. In the safety behavior acquisition phase, one of the response box lights illuminated during 6 out of 7 A+ trials (i.e., A+*). If participants pressed a button below the light, the US did not follow. In the safety behavior shift phase, response box lights illuminated during C- trials (i.e., C-*) in the experimental group, but not in the control group. In this phase, no safety behavior could be performed to A+. In the test phase, each stimulus was presented once without illuminated response box lights. C+ was always shown last. Finally, participants filled out the EPQ-N and were debriefed and reimbursed.

1.1.7. Data preparation

We based all our exclusion criteria on Engelhard et al. (2015). Participants were excluded if they did not show: CS-US contingency awareness (i.e., a higher threat expectancy rating to A+ than to B- in the test phase), safety behavior acquisition (i.e., at least four button presses during A+* trials), or safety behavior shift (i.e., in our study, at least three button presses during C-* trials). Data of 32 participants were excluded: 7 were unaware of the CS-US contingency, 16 showed no successful safety behavior acquisition, and 9 showed no safety behavior shift (i.e., 21% of the experimental group). Outliers were defined as more than 3 *SD* from the mean and were replaced with $M \pm 3$ *SD* (see Engelhard et al., 2015). We replaced 49 outliers (i.e., 2% of data) and also provided analyses without the replacement of outliers (see Tables S2 and S3 in the Supplemental Materials).

Similar to Lovibond et al. (2008, 2009), we computed the change in mean skin conductance level (SCL) by subtracting the mean SCL during the 10-s pre-CS baseline period from the mean SCL during the 5 s pre-US presentation. SCL data were mean-corrected following Lovibond (1992; Exp. 2). Analyses with *z*-transformed skin conductance responses yielded similar results as SCL and are not further reported.

1.1.8. Data-analysis

First, to inspect group differences in baseline variables, one-way ANOVAs were performed on age, neuroticism scores, shock level, and shock unpleasantness. A Chi-squared test assessed gender differences across groups. Second, to test whether Pavlovian acquisition was successful for threat expectancy and SCL, we used two 3 (Stimulus: A+, B-, C-) × 2 (Time: first, final acquisition trial) × 2 (Group: experimental, control) mixed ANOVAs. Third, to examine safety behavior acquisition effects on threat expectancy and SCL, we performed two 2 (Stimulus: A+, first A+* trial) × 2 (Group: experimental, control) mixed ANOVAs. To examine how threat responding to A+* developed over time, we used two 6 (Time: all A+* trials) × 2 (Group: experimental, control) mixed ANOVAs. Fourth, to test group differences to C- in the safety behavior shift phase, we conducted two 4 (Time: all C-/C-* trials) × 2 (Group: experimental, control) mixed ANOVAs with threat expectancy and SCL as dependent variables. Fifth, to test group differences in threat expectancy and SCL in the test phase, we performed two 3 (Stimulus: A+, B-, C-) × 2 (Group: experimental, control) mixed ANOVAs. To test whether threat expectancy and SCL to C- changed from the safety behavior acquisition phase to the test phase, we used two 2 (Time: single C- trial safety behavior acquisition, C- trial test) × 2 (Group: experimental, control) mixed ANOVAs.

All analyses were performed within a frequentist ($\alpha = 0.05$) and Bayesian hypothesis testing framework (using JASP Version 0.12.2.0; JASP Team, 2020). When the sphericity assumption was violated, we used Huynh-Feldt ($\varepsilon > 0.75$) or Greenhouse-Geisser ($\varepsilon < 0.75$) corrections. Holm–Bonferroni methods were used for all simple effects tests. Bayes factors (BFs) indicate that the data are BF times more likely under the alternative relative to the null hypothesis (Dienes, 2014). $BFs_{10} > 3$ indicate stronger evidence of data coming from the alternative than the null hypothesis, whereas $BFs_{10} < 0.33$ indicate the reverse. BFs_{10} between 0.33 and 3 can be interpreted as anecdotal or inconclusive evidence (Jeffreys, 1961).

Table 1
Design of Study 1.

Pavlovian acquisition	Safety behavior acquisition	Safety behavior shift	Test
A+ (4)	A*(+) (6)	A+ (4)	A+ (1)
B- (2) C- (2)	A+ (1) B- (1) C- (1)	B- (4) C$^{(*)}$- (4)[a]	B- (1) C- (1)

Note. A+, B-, and C- refer to visual stimuli; * refers to the availability of safety behavior; (+) indicates that shock only occurred if the participant failed to perform safety behavior; numbers in parentheses give the number of trials.

[a] The experimental, but not the control group, received the opportunity to perform safety behavior during this stimulus.

1.2. Results

1.2.1. Randomization checks

We found no evidence that the groups differed in gender distribution, $\chi^2(1) = 3.24, p = .072, BF_{10} = 1.69$, age, neuroticism scores, shock level, or shock unpleasantness, (all $Fs < 2.11$, all $ps > .151$, all $BFs_{10} < 0.61$), which suggests a successful randomization.

1.2.2. Pavlovian acquisition phase

Throughout this phase, participants had higher shock expectancy during A+ than B- or C- (Stimulus × Time), $F(1.60, 105.88) = 28.04, p < .001, \eta_p^2 = 0.30, BF_{10} > 1000$, see Fig. 1. Similarly, SCL was stronger for A+ than B- and C-, Stimulus × Time: $F(1.82, 120.09) = 2.68, p = .078, \eta_p^2 = 0.04, BF_{10} = 0.40$; Stimulus, $F(1.82, 120.36) = 12.68, p < .001, \eta_p^2 = 0.16, BF_{10} > 1000$. Groups did not differ in threat expectancy and SCL (interaction effects with group: $Fs < 2.81$, all $ps > .099$, $BFs_{10} < 0.20$), which indicates a successful acquisition on threat expectancy and SCL for both groups.

1.2.3. Safety behavior acquisition phase

Participants had lower threat expectancy ratings to the first response box trial (A*) than to A+, $F(1, 66) = 126.94, p < .001, \eta_p^2 = 0.66, BF_{10} > 1000$, which suggests that they learned that safety behavior canceled the shock. Throughout this phase, threat expectancy ratings and SCL during the safety behavior trials continued to decline ($Fs > 8.82$, $ps < .001$, $BFs_{10} > 842.30$). There were no interactions with group ($Fs < 1.61$, $ps > .168$, $BFs_{10} < 0.07$).

1.2.4. Safety behavior shift

Groups significantly differed in threat expectancy to C- across all trials (Time × Group), $F(2.13, 140.78) = 3.20, p = .041, \eta_p^2 = 0.05, BF_{10} = 1.66$, but there was no evidence that they differed on the first and last trial of C- (both $ts < 1.70$, $ps > .999$, $BFs_{10} < 0.70$). For SCL, there was no Time × Group interaction, $F(3.07, 202.50) < 1, p = .735, \eta_p^2 = 0.01, BF_{10} = 0.07$, nor a main effect of Time, $F(3.07, 202.50) < 1, p = .770, \eta_p^2 = 0.01, BF_{10} = 0.03$, but a main effect of Group, $F(1, 66) = 17.54, p < .001, \eta_p^2 = 0.21, BF_{10} = 217.74$.

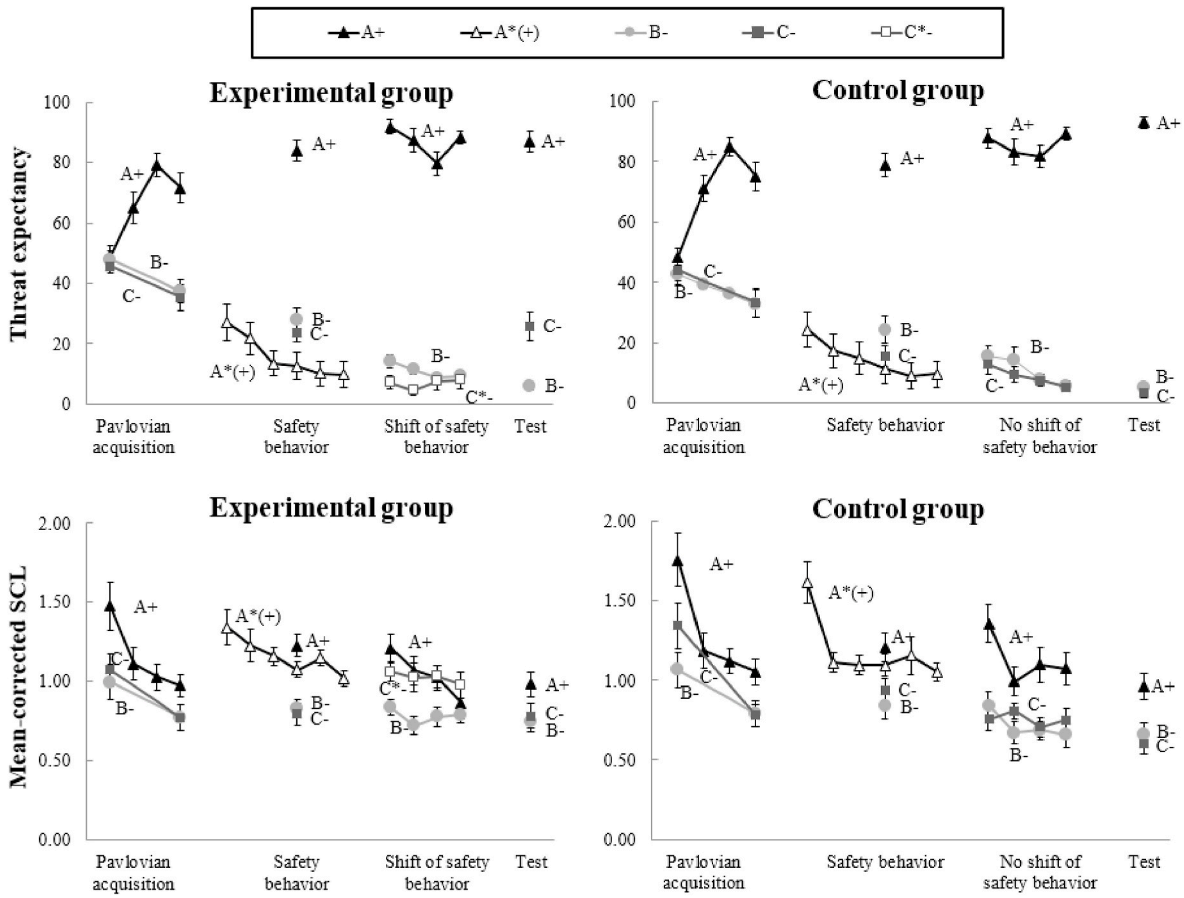

Fig. 1. Threat expectancy ratings and skin conductance level (SCL) during Study 1.

1.2.5. *Test Phase*

In this phase, groups differed in threat expectancy across stimuli, Stimulus × Group: $F(1.85, 121.83) = 15.53$, $p < .001$, $\eta_p^2 = 0.19$, $BF_{10} > 1000$. For both groups, threat expectancy ratings were higher for A+ than for B- and C-, $ts > 16.50$, $ps < .001$, $BFs_{10} > 1000$. Crucially, the experimental group showed higher ratings during C- compared to the control group, $t = 5.72$, $p < .001$, $BF_{10} = 446.37$. Also, ratings to C- were higher than B- in the experimental group, $t = 5.32$, $p < .001$, $BF_{10} = 190.21$, but not in the control group, $t < 1$, $BF_{10} = 0.22$. Furthermore, from the safety behavior acquisition phase to the test phase, ratings to C- did not change in the experimental group ($t < 1$, $BF_{10} = 0.20$), while they decreased in the control group ($t = 2.72$, $p = .034$, $BF_{10} = 9.55$), $F(1, 66) = 5.20$, $p = .026$, $\eta_p^2 = 0.07$, $BF_{10} = 2.64$ (Time × Group), suggesting that safety behavior maintained threat expectancy.

Given that a substantial number of participants were excluded based on a priori exclusion criteria, we decided to use sensitivity analyses to explore whether results are sensitive to exclusion of participants (i.e., those who did not acquire safety behavior and/or did not perform safety responses in 3 out of 4 trials and/or did not learn contingency). For the Stimulus × Group effect, sensitivity analyses using these two exclusion criteria yielded similar results (see Table S2 in the Supplemental Materials). However, the Time × Group effect disappears when the data of participants that did not meet the inclusion criteria were included (see Table S3 in the Supplemental Materials).

In contrast to the expectancy ratings, we found no evidences that the groups differed in SCL, Stimulus × Group: $F(1.84, 121.63) < 1$, $p = .512$, $\eta_p^2 = 0.01$, $BF_{10} = 0.15$; Group: $F(1, 66) = 2.35$, $p = .130$, $\eta_p^2 = 0.03$, $BF_{10} = 0.49$. SCL did differ across stimuli, $F(1.84, 121.63) = 9.87$, $p < .001$, $\eta_p^2 = 0.13$, $BF_{10} = 541.78$. Simple effects showed that SCL was higher to A+ than to B- and C- ($ts > 3.76$, $ps < .001$, $BFs_{10} > 22.55$), while B- and C- did not differ ($t < 1$, $BF_{10} = 0.14$). Thus, safety behavior did not result in stronger SCL to C- when the safety behavior was made unavailable.

1.3. *Discussion*

Study 1 demonstrated that safety behavior toward a safety cue maintains threat expectancy when the safety behavior becomes unavailable. This replicates previous studies (e.g., Engelhard et al., 2015; Xia et al., 2019). Our findings were not substantiated on a skin conductance level (in line with Xia et al., 2019), perhaps because this measure is not sensitive enough to detect differences between responses to two innocuous stimuli. Indeed, a previous study on safety behavior toward a *danger* cue did show group differences in skin conductance (Lovibond et al., 2009). Note that SCR analyses yielded similar results, hence the null findings are likely not related to the analytic methods. Potentially, the null finding on skin conductance could be explained by random variation. Many fear conditioning studies show a substantial variation in subjective and physiological responses (Mertens et al., 2018).

Although results demonstrated that safety behavior to C- maintained threat expectancy for this cue, this effect's Bayes factor was anecdotal (BF = 2.64), and reduced without correction for outliers (Table S3). In addition, the effect largely disappeared in sensitivity analyses. Therefore, in Study 2, we set out a multi-dataset analysis using meaningful change scores and latent class analyses to test heterogeneity in threat expectancy from before to after the performance of safety behavior to a safety cue. We hypothesized that the experimental group would predominantly show maintained or increased threat expectancy to the safety cue after removing the safety behavior and that the control group would mainly show decreased threat expectancy to this cue. In addition, we performed sensitivity analyses to explore whether our results would change when different exclusion criteria were applied.

2. Study 2

2.1. Methods

The Faculty of Social and Behavioral Sciences ethics committee at Utrecht University (FETC-20-347) approved this study. It was pre-registered on the Open Science Framework (https://osf.io/3vyrc/).

2.1.1. Study selection

We combined datasets from the three studies that used the same basic paradigm (i.e., Engelhard et al., 2015; Xia et al., 2019; current Study 1). There were minor variations in the number of stimulus presentations, stimuli nature, trial duration, and outcome measures (see Table S1 in the Supplemental Material).

2.1.2. Participants

We applied the same exclusion criteria as in Study 1 (i.e., no contingency awareness; no safety behavior acquisition; no safety behavior shift) and excluded 87 participants out of 311 (i.e., $n = 20$ of 101, Engelhard et al., 2015; $n = 35$ of 110, Xia et al., 2019; and $n = 32$ of 100, Study 1 of this paper). Data of 11 participants were missing (Engelhard et al., 2015: $n = 1$; Xia et al., 2019: $n = 10$). Complete case analyses are reported because the missingness might not be completely at random (van Buuren, 2012). The final sample included $N = 213$ students ($n = 99$ experimental; $n = 114$ control) with 57 males and 156 females.

2.1.3. Outcome measure

The outcome measure comprises a change score in threat expectancy to C- from the final trial of the safety behavior acquisition phase to the first trial of the test phase. For the Chi-Square and Bayesian Contingency Tables tests (see below), we computed meaningful change scores following Copay et al. (2007). Change scores ranging between 0 ± 0.5 *SD* were the *no-change* category, change scores smaller than 0–0.5 *SD* were the *decrease* category, and change scores larger than 0 + 0.5 *SD* were the *increase* category.

2.1.4. Data-analysis

First, to test whether we needed to control for between-study heterogeneity in our multi-dataset analysis, we calculated the Diamond Ratio (DR; see Cairns et al., 2020). Specifically, we calculated the DR for group effects on change scores in threat expectancy to C-. DR = 1 means no or little heterogeneity, DR = 1.40 indicates moderate heterogeneity, and DR of 2 and higher means large heterogeneity (Cairns et al., 2020). Second, to test group differences in the *no-change*, *decrease,* and *increase* categories, we performed a Chi-Square test and a Bayesian Contingency Tables test with Group (Experimental vs. Control) as an independent variable and the change score categories (i.e., *no-change*, *decrease*, and *increase*) as the dependent variable. These analyses were run in JASP Version 0.12.2.0 using the default settings (JASP Team, 2020). Follow-up analyses were run in MedCalc using the "N-1″ Chi-squared test, as suggested by Campbell (2007) and Richardson (2011). Third, to explore how individuals are categorized based on their change score (i.e., how many categories best fit the data) and whether these categories differ across groups, we performed a latent class analysis in Mplus (Version 8.4). We used the three-step procedure (Asparouhov & Muthén, 2014) with Group as a predictor. This method takes the uncertainty with respect to participants' class allocations into account in the subsequent multinomial regression analysis. The number of latent classes was determined by evaluating the combination of the Lo-Mendell-Rubin test (LMR), adjusted LMR, bootstrap likelihood ratio test (BLRT), the Bayesian information criterion (BIC; Tein et al., 2013), sample size, and interpretability. The LMR, adjusted LMR, and BLRT result in *p*-values, where *p*-values <.05 suggest that *k* classes are preferred over *k*-1 classes. The BIC can be compared between *k* and *k*-1 classes, where lower BIC values are preferred. The sample size criterion means that there cannot be many small categories in the final selection, as the third step of the analysis is a multinomial regression with group as a predictor and class as a dependent variable. The final criterion was interpretability (Geiser, 2013), which means that we prefer a *k*-class solution when we can also give meaning to it. Fourth, we performed multiverse analyses as sensitivity analyses to examine whether results would change when different exclusion criteria are used (see Lonsdorf et al., 2022). Fifth, we explored whether a change in threat expectancy to C- is related to anxiety-related personality trait measures.

2.2. Results

2.2.1. Meaningful change

We used a fixed-effects model for all analyses because the between-studies heterogeneity was small (DR = 1; 95% CI: 1.00, 4.49). As displayed in Fig. 2, groups significantly differed in meaningful change scores to C- from the safety behavior acquisition phase to the test phase, $\chi^2(2, N = 213) = 25.44$, $p < .001$; $BF_{10} > 1000$. Further examination showed that more participants in the experimental group (26/99; 26.26%) exhibited a meaningful *increase* in threat expectancy to C-, relative to the control group (3/114; 2.63%), while more participants in the control group showed *no change* in threat expectancy (experimental: 54/99; 54.55%; control: 78/114; 68.42%). Unexpectedly, groups did not significantly differ in the percentage of *decreased* threat expectancy (experimental: 19/99; 19.19%; control: 33/114; 28.95%). A sensitivity analysis using different exclusion criteria showed similar results (see Table S4 in the Supplemental Material).

2.2.2. Latent class analysis

Fig. S1 in the Supplemental Material displays the change score distributions across groups. The best solution with interpretable and analyzable classes was the three-class solution (see Table 2 and Fig. 3). The classes could be labeled as *decrease* (class 1; $n = 10$), *no-change* (class 2; $n = 183$), and *increase* (class 3; $n = 20$). Sensitivity analyses demonstrated that these results did not meaningfully change when different exclusion criteria were applied (see Table S5 in the Supplemental Material).

To compare group differences in classes, we calculated odds ratios (ORs). Participants in the experimental group, relative to the control group, were 14.63 times more likely to exhibit change scores that fell into the *increase* rather than the *decrease* class (95% CI = 1.93, 110.93; $p = .009$) and were 7.63 times more likely to have change scores in the *increase* rather than the *no-change* class (95% CI = 2.11, 27.60; $p = .002$). Groups did not significantly differ in OR of change scores in the *decrease* rather than the *no-change* class (OR = 1.91; 95% CI = 0.37, 9.93; $p = .438$). Thus, these results suggest that safety behavior increases the likelihood of an increased threat expectancy to safety cue C- when the safety behavior is unavailable.

2.2.3. Exploratory analyses

We executed Pearson correlation analyses to test a relation between change scores in threat expectancy to C- and *z*-transformed trait anxiety (Xia et al., 2019) and neuroticism scores (current Study 1). These analyses did not reveal a relationship between change scores in threat expectancy to C- and *z*-transformed anxious personality traits ($r = -0.17$, $p = .056$, $BF_{10} = 0.66$).

3. General discussion

We examined whether safety behavior toward a safety cue maintains or increases threat beliefs when the behavior becomes unavailable. In Study 1, we replicated and extended earlier fear conditioning studies on safety behavior to a safety cue (Engelhard et al., 2015; Xia et al., 2019). Our results showed that threat beliefs generally do not change when safety behavior is not available anymore, although skin conductance data did not corroborate this result (see also Xia et al., 2019). In Study 2, we performed a multi-dataset analysis (using meaningful change scores

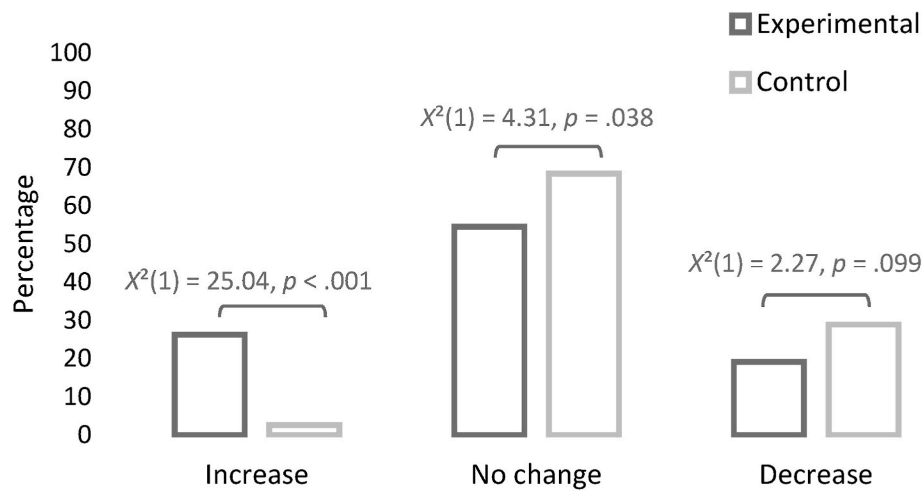

Fig. 2. Percentage of participants who showed an increase, no change or decrease in threat expectancy to C- from the safety behavior acquisition phase to the test phase (Study 2)
Note. The labels represent meaningful change categories.

Table 2
Latent class analyses on change scores in Study 2.

Classes	LMR	V-LMR	BLRT	BIC	Entropy	Min *n*	Max *n*
1	–	–	–	2025.55	–	213	213
2	.033	.027	<.001	1988.28	.96	20	193
3	.034	.026	<.001	1960.66	.95	10	183
4	.618	.605	<.001	1957.97	.95	4	180
5	.008	.006	<.001	1932.22	.95	4	149
6	.157	.136	<.001	1924.37	.96	4	145

Note. Change scores represent the difference in threat expectancy to C- from the safety behavior acquisition phase to the test phase.

and latent class analyses) to explore heterogeneity in threat expectancy to a safety cue before and after the performance of safety behavior. This revealed that the majority of individuals did not show an increase in threat expectancy. Nevertheless, about a quarter of individuals who performed safety behavior toward a safety cue showed increased threat expectancy to this cue, while virtually nobody in the control group exhibited an increase. Thus, the present research, together with prior clinical studies (e.g., Deacon & Maack, 2008; van Uijen & Toffolo, 2015), indicates that safety behavior in relatively safe situations may culminate in the increase or perseverance of threat beliefs.

Several findings warrant further discussion. First, in Study 2, participants strongly differed in their threat responses when the safety behavior was no longer available, which may indicate resilience or risk for clinical anxiety (Krypotos et al., 2018; Lonsdorf & Merz, 2017). Future research may elucidate whether these response patterns are related to specific traits (e.g., harm avoidance; Gazendam et al., 2020) or symptom profiles (e.g., obsessive-compulsive symptoms; Hunt et al., 2020) that are involved in clinical anxiety. If, for example, individuals who exhibit increased threat expectancy after safety behavior are more likely to develop anxiety symptoms, this paradigm can be used to identify such individuals to offer them preventive treatment (Paulus, 2015).

Another noteworthy finding in Study 2 was that a substantial number

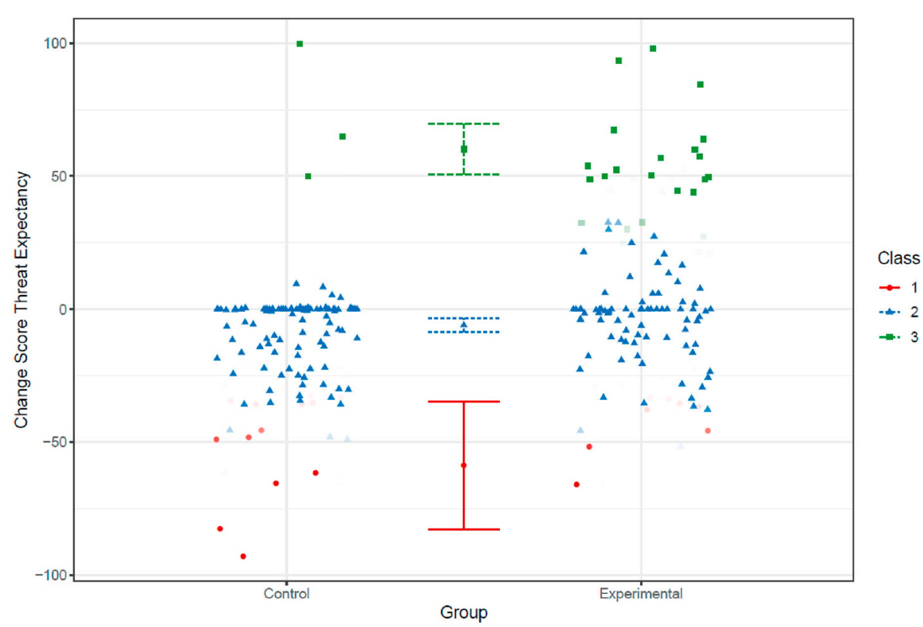

Fig. 3. Solution with three classes by group resulting from a three-step latent class analysis (Study 2)
Note. Change scores represent the difference in threat expectancy to C- from the safety behavior acquisition phase to the test phase.

of individuals showed *increased* threat expectancy to the safe stimulus (that was never paired with an unpleasant stimulus) when the safety behavior was no longer available. This is in line with previous field studies (e.g., Deacon & Maack, 2008) and with recent work showing that people who see police patrolling in safe situations may ironically feel less safe (van de Veer et al., 2012). How could these findings be explained? On the one hand, following the cognitive-dissonance theory, these individuals may have sought consistency in their attitudes and safety behaviors (Festinger & Carlsmith, 1959; Harmon-Jones et al., 2015; van Uijen et al., 2017). Indeed, previous work showed that patients with clinical anxiety rate objectively safe scenarios as more dangerous when the person in the scenario uses safety behavior (Gangemi et al., 2012; van den Hout et al., 2014). Future research could directly manipulate cognitive dissonance to test whether more cognitive dissonance is indeed related to increased threat perception. On the other hand, the increased threat beliefs could also result from higher-order conditioning (Seymour et al., 2004). Specifically, in the test phase, some individuals may have based their threat beliefs on the removal of the safety behavior rather than the safety cue itself (see Klein et al., 2021).

Our findings suggest that the availability or utilization of safety behaviors in relatively safe situations may potentially be detrimental for some individuals. The present studies could not disentangle whether this effect is more related to the availability or the utilization of safety behavior toward a safety cue. More research is needed to explore this further (see Kemp et al., 2019). Note that there is an ongoing debate about whether safety behaviors during exposure-based therapy are deleterious or beneficial. For example, a meta-analysis of experimental studies among fearful individuals demonstrated that self-reported fear at post-intervention did not differ between groups that did or did not use safety behaviors (Meulders et al., 2016). However, another systematic review reported that 15 out of 18 *clinical treatment studies* demonstrated that safety behaviors negatively affected treatment outcomes (Blakey & Abramowitz, 2016). Potentially, safety behaviors may occasionally be beneficial in lowering the threshold for starting with exposure (e.g., Rachman et al., 2008,2011; van den Hout et al., 2011), but they may be detrimental in the long term (Craske et al., 2008; Meulders et al., 2016). This is an empirical question that needs to be further investigated.

Several limitations of this research should be mentioned. First, in Study 1, 21% of participants in the experimental group were excluded because they did not apply safety behavior toward the safety cue. This may limit the generalizability of these findings (see Lonsdorf et al., 2017). However, sensitivity analyses in Study 2 showed that the results did not meaningfully change when we applied different exclusion criteria. A second limitation could be that our test phase only included one trial; hence our effects may be short-lived (see Xia et al., 2019). Therefore, future research should examine individual differences throughout an extended test phase. Third, we did not collect data on racial/ethnic identifications and culture/geographic background, which may limit the generalizability of our findings. Strengths of the present research include the well-controlled paradigm and advanced statistical analyses to explore individual heterogeneity.

To conclude, accumulated evidence suggests that safety behavior in relatively safe situations may have maladaptive effects: it generally maintains and sometimes even increases threat beliefs. Future research should test whether and for whom safety behavior in relatively safe situations culminates in clinical anxiety.

Author note

Angelica Tinga is now at the Dutch Institute for Road Safety Research (SWOV).

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CRediT authorship contribution statement

EAM van Dis: Conceptualization, Methodology, Software, Formal analysis, Investigation, Data curation, Writing – original draft, Visualization, Project administration. **A-M Krypotos:** Methodology (Study 2), Validation, Writing – review and editing. **MAJ Zondervan-Zwijnenburg:** Formal analysis (Study 2), Writing – review and editing, Visualization (Study 2). **AM Tinga:** Methodology (Study 1), Software (Study 1), Investigation (Study 1), Writing – review and editing. **IM Engelhard:** Conceptualization, Methodology, Writing – review and editing, Supervision, Funding acquisition.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.brat.2022.104142.

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