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State Anxiety Carried Over From Prior Threat Increases Late Positive Potential Amplitude During an Instructed Emotion Regulation Task

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Emotion regulation has important consequences for emotional and mental health (Saxena, Dubey & Pandey, 2011) and is dependent on executive function (Eisenberg, Smith & Spinrad, 2011). Because state anxiety disrupts executive function (Robinson, Vytal, Cornwell & Grillon, 2013), we tested whether state anxiety disrupts emotion regulation by having participants complete an instructed emotion regulation task, while under threat of unpredictable shock and while safe from shock. We used the late positive potential (LPP) component of the event related potential to measure emotion regulation success. We predicted that LPP responses to negatively valenced images would be modulated by participants' attempts to increase and decrease their emotions when safe from shock, but not while under threat of shock. Our manipulation check revealed an order effect such that for participants who completed the threat of shock condition first self-reported state anxiety carried over into the subsequent safe condition. Additionally, we found that although instructions to regulate affected participants' ratings of how unpleasant the images made them feel, instructions to regulate had no effect on LPP amplitude regardless of threat condition. Instead we found that participants who received the threat condition prior to safe had greater LPP responses to all images in the safe condition. We posit that the carryover of anxiety resulted in misattribution of arousal and potentiation of neural responses to the images in the safe condition. Thus, our results imply that physiological arousal and cognition combine to influence the basic neural response to emotional stimuli.

Keywords: emotion regulation, threat of shock, state anxiety, misattribution of arousal, emotion–cognition interaction

Most individuals report routine use of emotion regulation (Gross, Richards & John, 2006), and the ability to effectively regulate one's emotions has important consequences for emotional and mental health, as well as well-being more generally. Maladaptive patterns of emotion regulation are associated with depression, anxiety and negative affect (Dennis, 2007; Ehring, Tuschen-Caffier, Schnülle, Fischer, & Gross, 2010; Saxena, Dubey, & Pandey, 2011), as well as decreased positive affect and well-being (McRae, Jacobs, Ray, John, & Gross, 2012; Saxena et al., 2011). Thus, investigating the factors that determine an individual's ability to successfully regulate emotion is important in understanding not only risk factors for mental health problems, but also the factors that predict general well-being.

Successful emotion regulation requires cognitive resources associated with *executive function*, a term used to describe several interrelated cognitive mechanisms underpinning self-regulation. Emotion regulation has been conceptualized as requiring the overlapping executive function constructs of cognitive control (Ochsner & Gross, 2005), effortful control (Eisenberg, Smith, & Spinrad, 2011), and inhibitory control (Carlson & Wang, 2007). Additionally, measures of cognitive control have shown a positive correlation with emotion regulation ability in adults (Bardeen, Stevens, Murdock, & Lovejoy, 2013; McRae et al., 2012; Schmeichel, Volokhov, & Demaree, 2008) and children (Carlson & Wang, 2007). Furthermore, Ochsner and Gross (2005) have noted similarities in brain areas subserving cognitive reappraisal (an emotion regulation strategy involving reframing the context or meaning of an affective stimulus) and those subserving nonemotional cognitive control tasks.

Executive function deficits have been shown in those with high trait anxiety (Bishop, 2009; Derakshan & Eysenck, 1998; Stout, Shackman, & Larson, 2013), and research suggests that state anxiety also disrupts executive function (Robinson, Vytal, Cornwell, & Grillon, 2013). Lindström and Bohlin (2012) found that exposure to threat stimuli reduced working memory and inhibitory control. Additionally, threat of shock, which induces state anxiety (Petry & Desiderato, 1978; Lissek et al., 2007; Robinson, Letkiewicz, Overstreet, Ernst, & Grillon, 2011), impairs visual and verbal working memory (Vytal, Cornwell, Letkiewicz, Arkin, & Grillon, 2013), and alters decision making (Clark et al., 2012; Keinan, 1987).

Given that state anxiety can disrupt executive function, state anxiety may also be accompanied by difficulties in emotion regulation. In support of this hypothesis, reappraisal training reduces the fear response during a fear conditioning task, unless an acute stressor is given immediately before the task (Raio, Orederu, Palazzolo, Shurick, & Phelps, 2013). Thus, examining the effects of state anxiety on emotion regulation could lead to a better

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understanding of the situations under which emotion regulation fails. This is particularly important given that stressful, anxiety-inducing situations are often the situations wherein individuals need to regulate their emotions most. Furthermore, given the association between emotion regulation difficulties and disorders like generalized anxiety disorder and depression (Dennis, 2007; Ehring et al., 2010; Saxena et al., 2011), a breakdown in emotion regulation ability due to prolonged and frequent state anxiety may be one mechanism behind the relationship between chronic stress and these psychopathologies (Chen & Hong, 2010; Hammen, 2005; Pizzagalli, 2014).

To examine whether state anxiety disrupts emotion regulation, we asked participants to complete an instructed emotion regulation task while under threat of unpredictable shock, and while safe from shock. Threat of shock was chosen as the manipulation, because it has been shown to induce state anxiety (Grillon, Baas, Lissek, Smith, & Milstein, 2004; Lissek et al., 2007; Petry & Desiderato, 1978; Robinson et al., 2011) and has been used to disrupt performance on other executive function tasks (Robinson et al., 2013). To assess the impact of state anxiety on emotion regulation, we measured the late positive potential (LPP) component of the event-related potential (ERP). The LPP is a sustained positive slow wave that can last for several seconds following the P300 (Cacioppo, Crites, Gardner, & Bernston, 1994). The LPP is larger in response to emotionally salient stimuli (Gable, Adams, & Proudfit, 2015), and this effect is increased or decreased when participants are asked to, respectively, up- or down-regulate their emotional responses (Hajcak & Nieuwenhuis, 2006; Moser, Krompinger, Dietz, & Simons, 2009; Thiruchselvam, Blechert, Sheppes, Rydstrom, & Gross, 2011). Additionally, individual differences in traits relevant to emotion regulation correlate with LPP amplitude during instructed emotion regulation (Moser, Hartwig, Moran, Jendrusina, & Kross, 2014). Based on these findings, we reasoned that the LPP response during an instructed emotion regulation task would be a sensitive measure of emotion regulation success during threat and safety.

To test this, we instructed participants to use cognitive reappraisal to modulate their emotions while viewing negatively valenced images, both under threat of unpredictable shock and during shock-free "safe" conditions. Participants also viewed neutral images, to serve as a baseline. We chose to have participants use cognitive reappraisal because it has been shown to modulate LPP amplitude (Moser et al., 2009). Furthermore, because cognitive reappraisal ability correlates with measures of executive function (McRae et al., 2012), we reasoned that effective reappraisal may be dependent on executive functioning and, therefore, susceptible to disruption by state anxiety. Thus, during the safe condition we expected to replicate previous findings, with lower LPP responses when participants were asked to decrease their response (vs. responding naturally) toward negatively valenced images, and higher LPP responses when participants were asked to increase (vs. responding naturally) toward negatively valenced images (Hajcak & Nieuwenhuis, 2006; Moser et al., 2009; Thiruchselvam et al., 2011). However, during threat we expected induced state anxiety to disrupt emotion regulation. As a result, we predicted no LPP difference for participants attempting to increase their emotions (vs. responding naturally) toward negatively valenced images while under threat of shock. Similarly, we predicted no LPP difference for participants attempting to decrease their emotions (vs. responding naturally) toward negatively valenced images while under threat of shock. As will be evident from our results, despite using a standard threat induction combined with a well-replicated emotion regulation task our findings were somewhat unexpected, with the regulation effects being overshadowed by carryover effects from the threat of shock induction.

Method

Participants

Sixty-four undergraduate students (41 female) received course extra credit and a $5 gift card for participation in this study. Participants had a mean age of 22.4 years ($SD = 5.1$). Written informed consent was obtained before the experiment. Three participants were excluded from analysis because the study protocol was not followed. Seven participants were excluded from analysis due to excessive artifacts in the data. One participant withdrew from the study. The data from 53 participants (32 female) remained for analysis.

Emotion Regulation Task

Participants completed an instructed emotion regulation task, in which participants were cued as to how to modulate their affective response (either up or down) toward a subsequent affective image, modeled after Moser et al. (2009). Participants were also shown neutral images that they were asked to respond naturally toward, to serve as a baseline. On each trial participants were presented with a sequence of a 2–3-s jittered fixation period, a 1-s regulation cue, an image presented for 4.5 s, and a screen prompting them to rate how unpleasant they felt on a scale from 1 to 8, with one being *not at all* and 8 being *very unpleasant* (see Figure 1 for depiction of task). As has been done in prior studies (Gardener, Carr, MacGregor, & Felmingham, 2013; Ichikawa et al., 2011; Langeslag & Van Strien, 2010; Moser, Hajcak, Bukay, & Simons, 2006; Moser et al., 2009), the trials were separated into increase and decrease blocks to avoid task switching effects (Monsell, 2003; Moser, Most, & Simons, 2010). During increase blocks the instructions were either "increase," "view," or "watch." During decrease blocks instructions were either "decrease," "view," or "watch." These cues instructed participants to either up-regulate their emotional response (increase), down-regulate (decrease), or to respond naturally (view and watch trials). Images following increase and decrease cues were always negatively valenced. The difference between "view" and "watch" trials was that "view" trials were followed by neutral images and "watch" trials were followed by negatively valenced images. This allows these cues to inform participants about what type of image is next, just as the increase or decrease cues do (Thiruchselvam et al., 2011). For clarity, we will refer to "view" trials as view-neutral trials and "watch" trials as view-negative trials for the remainder of this article.

Images were taken from the International Affective Picture System (IAPS; Lang, Bradley, & Cuthbert, 2008) and the Nencki Affective Picture System (NAPS; Marchewka, Żurawski, Jednoróg, & Grabowska, 2014). One hundred twenty negatively valenced IAPS images were used, with mean IAPS normative valence of 2.03 ($SD = .56$) and arousal of 6.34 ($SD = .77$) on a 1–9

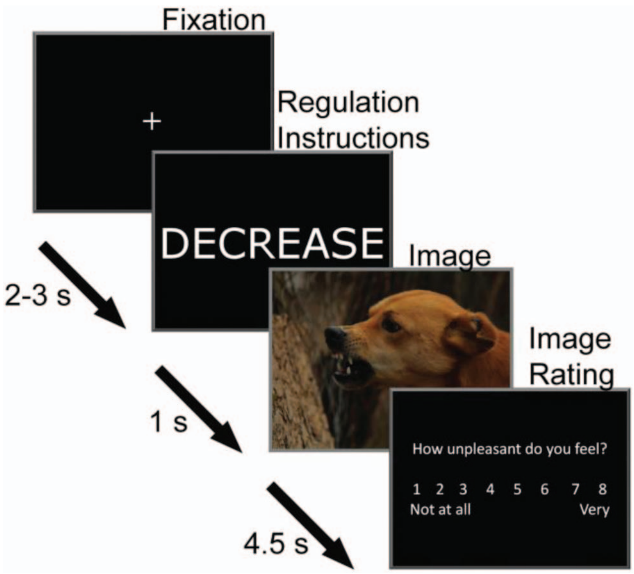

Figure 1. Emotion regulation task trial design. Each block was made up of either 25 increase or 25 decrease trials intermixed with 25 view-neutral and 25 view-negative trials. Due to the International Affective Picture System user agreement, the image in this figure is from the public domain, rather than one of the International Affective Picture System images used in the study. See the online article for the color version of this figure.

scale, along with 80 negatively valenced NAPS images, with a mean NAPS normative valence of 2.28 (*SD* = .36) and arousal of 7.05 (*SD* = .41; also rated on a 1–9 scale). Forty-five neutral IAPS images were used, with mean IAPS normative valence of 5.37 (*SD* = .68) and arousal of 3.68 (*SD* = .76), as well as 55 neutral NAPS images, with a mean NAPS normative valence of 5.71 (*SD* = .56) and arousal of 4.65 (*SD* = .38).

Before each block of the task, participants were given instructions taken from Moser et al. (2009), which were inspired by Ochsner et al. (2004). For increase trials, participants were asked to imagine that the pictured event was related to them or a loved one directly, or to "imagine that the pictured event gets worse" (Moser et al., 2009, p. 20). For decrease trials, participants were asked to "view the picture as a detached, third person," to "think of the picture as being fake or from a movie," or to "imagine that the pictured event gets better" (Moser et al., 2009, p. 20). Both increase and decrease instructions asked participants to not "think about something unrelated to the scene" (Moser et al., 2009, p. 20). Participants were asked to respond as they naturally would on view-neutral and view-negative trials, and were informed that images following a "view" cue would be neutral and those following a "watch" cue would be negative. After instructions, participants were asked what thoughts they could use to both increase and decrease their emotions if presented a picture of a large, angry dog, to check their understanding of the regulation instructions. If their answers indicated that they did not fully understand, further instruction was given, as necessary.

Participants completed two runs of paired increase and decrease blocks, one while under threat of shock and one with no accompanying threat of shock. The order of threat and safe runs was counterbalanced, as was the order of increase and decrease within the runs. Each block consisted of 75 trials, 25 of either increase or decrease (depending on block type), 25 view-neutral, and 25 view-negative. This resulted in a total of 300 trials. Images for each trial were taken randomly, without replacement, from either a pool of 200 negatively valence and 100 neutral images (depending on trial type).

Threat of Shock Manipulation

Before the run of threat of shock trials, electrodes attached to a PsychLab (Glastonbury, United Kingdom) shock stimulator were applied to the participant, and the level of shock was determined for each individual participant. To determine the level of shock, participants were given a series of shocks, with the magnitude of the current increasing each time, and asked to rate each one on a scale from 0 (*shock was not felt*) to 10 (*painful but tolerable shock*). The level of shock used in the experiment was the level that the participant rated a 10. For those who completed the threat of shock condition first (threat first), the shock electrodes were removed before they completed the safe run. Participants who completed the safe run first (safe first) had electrodes applied and completed the shock work-up after the safe run was over. Runs followed one another directly after these tasks, meaning that time between runs was not strictly controlled, but was typically less than 5 min. Before the threat of shock blocks participants were told,

> The electrical stimulation may occur during any trial type and the probability of the electrical stimulation occurring is equal for each trial type. The electrical stimulation will only occur during the presentation of a picture and may occur at any time while the picture is on the screen. The electrical stimulation will not occur every time a picture is presented, but it will occur on some presentations of the picture.

These instructions ensured that the shock was unpredictable (Grillon et al., 2004).

For each of the two threat of shock blocks (increase and decrease), participants were given three shocks. The timing of the shock was randomized, so that it could occur any time during image presentation. Trials where the shock was administered were removed from analysis, so the ERPs measured reflect the threat—not receipt—of shock. For safe blocks, three trials were randomly chosen and removed from analysis to maintain as much similarity as possible between threat and safe runs.

Manipulation Check

To examine whether our threat of shock manipulation successfully induced anxiety, participants were given the State–Trait Anxiety Inventory–State (STAI–State; Spielberger, Gorsuch, & Lushene, 1970) after each run (threat and safe), with the instructions modified to ask them to rate how they felt during the previous set of trials (rather than rating their current feelings). The STAI–State has 20 items, each of which asks participants to rate current feelings related to anxiety and has good internal consistency (α = .91; Barnes, Harp, & Jung, 2002).

EEG Acquisition and Analysis

EEG data was recorded using a direct-current amplifier and a 32-channel cap with shielded leads (Advanced Neuro Technology

B.V., the Netherlands) referenced to the left mastoid. Impedances were kept below 15 kΩ. Data were sampled at 512 Hz and subjected to an antialiasing low-pass filter (~138 Hz). Offline, data were filtered (Butterworth band-pass, .05–30 Hz) and an independent components analysis (as implemented by EEGLab v.12) was used to identify and remove artifacts due to eye blinks and eye movement. Data were then rereferenced to mean mastoid, epoched using the first 2 s following the onset of the image, and baseline corrected, using the 200 ms prior to image onset as a baseline. Trials with a change in voltage exceeding 100 μV were removed. Participants missing more than 25% of the trials of any given trial type were removed from analysis (N = 7). Average waveforms of activity at Pz for each trial type for each participant were created and each participant's data were averaged together to obtain a grand average waveform that was inspected to determine the LPP time window used for statistical analysis. Based on this inspection, mean voltage from 500 ms to 1,500 ms following image onset was extracted for each participant.

Results

Manipulation Check

To check whether the threat of shock had the expected effect of increasing anxiety we ran a two-way Condition × Order analysis of variance (ANOVA) using self-reported anxiety as the dependent variable, with condition referring to the threat of shock condition (safe and threat), and order referring to the order of the threat of shock condition (safe first and threat first). Participants reported more anxiety during the threat block than the safe block, $F(1, 49) = 9.13$, $p = .004$, $\eta_p^2 = .157$. There was no main effect of order, $F(1, 49) = .33$, $p = .57$, $\eta_p^2 = .007$. Contrary to our expectations, however, there was a significant interaction between condition and order, $F(1, 49) = 4.57$, $p = .037$, $\eta_p^2 = .085$. Safe-first participants reported higher mean anxiety during threat than safe, $t(26) = 3.63$, $p = .001$, 97.5% confidence interval (CI) [2.94, 14.1], but there was no mean anxiety difference between threat and safe runs for threat-first participants, $t(23) = .63$, $p = .53$, 97.5% CI [−4.06, 6.98] (Bonferroni corrected alpha of $p < .025$). This finding suggests that when the threat condition preceded the safe condition the anxiety induced by the threat condition carried over into the safe condition. Mean anxiety scores for safe and threat runs separated by order can be seen in Figure 2.

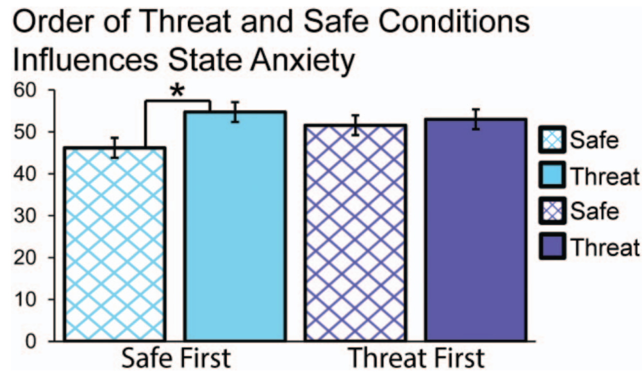

Figure 2. Mean self-reported anxiety (State-Trait Anxiety Inventory–State) following safe and threat runs for safe-first (blue) and threat-first (purple) participants. Safe-first participants reported less anxiety during safe (vs. threat) trials, with no mean difference for safe versus threat for threat-first participants. Error bars depict standard error. * Significant differences revealed by follow-up t tests, $p < .05$. See the online article for the color version of this figure.

ERP Findings

Mean LPP amplitudes were used in two Regulation × Condition × Order ANOVAs, one for increase blocks and one for decrease blocks (Moser, Most & Simons, 2010). In these ANOVAs, regulation refers to the three regulation instruction conditions (view-neutral, view negative, and either increase or decrease, depending on block), Condition refers to threat of shock condition (safe and threat), and order refers to the order of the threat blocks (safe first and threat first). Grand averaged waveforms at Pz for increase and decrease blocks are shown in Figure 3.

Increase condition. Within the increase blocks there was a significant main effect of regulation, $F(2, 102) = 11.78$, $p < .001$, $\eta_p^2 = .188$. Mean LPP amplitude was lower for view-neutral trials than for either increase, $t(52) = 3.74$, $p < .001$, 98.3% CI [.48, 2.33], or view-negative trials, $t(52) = 5.11$, $p < .001$, 98.3% CI [.82, 2.36]. However, there was no significant difference between increase and view-negative trials, $t(52) = .51$, $p = .62$, 98.3% CI [−1.11, .735] (Bonferroni corrected alpha of $p < .017$). Thus, the regulation main effect was driven by higher LPP amplitudes in both of the negative image conditions, rather than the instructions to regulate, meaning that participants were not successful at up-regulating their emotional response as indexed by the LPP. There was no main effect of condition $F(1, 51) = 1.69$, $p = .2$, $\eta_p^2 = .032$. Contrary to our hypothesis that participants would have more difficulty regulating while under threat than while safe, there was no interaction between condition and regulation, $F(2, 102) = .16$, $p = .85$, $\eta_p^2 = .003$. There was also no Order × Regulation interaction, $F(2, 102) = .46$, $p = .63$, $\eta_p^2 = .009$. There was, however, a significant Condition × Order interaction, $F(1, 51) = 13.1$, $p = .001$, $\eta_p^2 = .204$ (see Figure 4). Threat-first participants had a higher mean LPP amplitude for trials during the safe run than during the threat run, $t(25) = 3.47$, $p = .002$, CI 97.5% [.65, 3.51], but safe-first participants showed no LPP difference for safe and threat runs, $t(26) = 1.64$, $p = .11$, 97.5% CI [–.44, 2.4] (Bonferroni corrected alpha of $p < .025$). Thus, LPP amplitudes seemed to reflect a magnified version of the carryover effect seen in the self-reported anxiety scores, with the images eliciting a similar LPP for safe and threat when the safe condition was first, but a greater LPP response for safe than threat when safe followed threat. The lack of a Condition × Order × Regulation interaction, $F(2, 102) = 1.76$, $p = .18$, $\eta_p^2 = .033$, shows that this interaction affected each regulation condition similarly.

Decrease condition. Results for the decrease blocks were similar to increase, though not identical. Again, there was a significant main effect of regulation, $F(2, 102) = 22.77$, $p < .001$, $\eta_p^2 = .309$, that was driven by responses to negative images in general, as opposed to the instruction to decrease affective intensity. Mean LPP amplitude was smaller for view-neutral trials than for either view-negative, $t(52) = 7.72$, $p < .001$, 98.3% CI [1.55, 3], or decrease trials, $t(52) = 5.14$, $p < .001$, 98.3% CI [1.04, 2.95], indicating an overall larger LPP for negative than for neutral

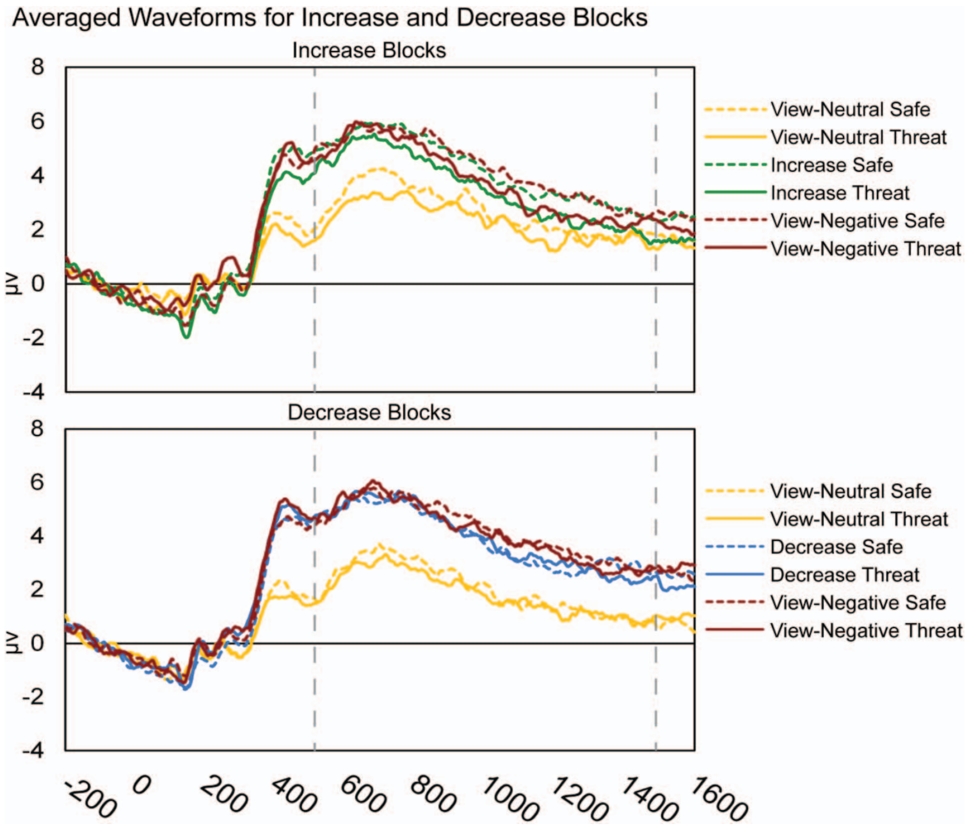

Figure 3. Averaged waveforms for increase, view-neutral, and view-negative trials during the increase blocks for both safe and threat conditions (top) as well as decrease, view-neutral, and view-negative blocks for both safe and threat conditions (bottom) at Pz. Vertical gray lines represent time window used for statistical analysis. See the online article for the color version of this figure.

images. However, there was no significant difference between decrease and view-negative trials, $t(52) = .7$, $p = .49$, 98.3% CI [−1.27, .71] (Bonferroni corrected alpha of $p < .017$), which implies that participants were not able to effectively down-regulate their response to negative images. Similar to results for the increase blocks, the threat condition did not differentially affect regulation, $F(2, 102) = .03$, $p = .97$, $\eta_p^2 = .001$. There was also no main effect of condition, $F(1, 51) = .095$, $p = .76$, $\eta_p^2 = .002$, and no Order × Regulation interaction, $F(2, 102) = .33$, $p = .72$, $\eta_p^2 = .006$. As in the increase blocks, there was a significant Condition × Order interaction, $F(1, 51) = 5.25$, $p = .026$, $\eta_p^2 = .093$ (see Figure 4). For safe-first participants, mean LPP amplitude of threat and safe trials did not significantly differ, $t(26) = 1.21$, $p = .24$, 97.5% CI [–.7, 2.16], while safe trials elicited a larger LPP than threat trials for threat-first participants, $t(25) = 2.32$, $p = .03$, 97.5% CI [–.03, 1.94] but this effect did not survive Bonferroni correction ($p < .025$), suggesting that caution is needed in interpreting this interaction. This Condition × Order interaction was similar across regulation conditions, with no Condition × Regulation × Order interaction, $F(2, 102) = 2$, $p = .14$, $\eta_p^2 = .038$. Although there were no LPP effects for regulation, the results for each regulation instruction are presented for each condition and order are presented in Figure 5.

Image Rating Findings

Ratings for how unpleasant participants felt immediately after each image were subjected to two Regulation × Condition × Order ANOVAs, one for increase blocks and one for decrease blocks. Figure 6 contains bar graphs with mean image ratings.

Increase condition. During increase blocks there was no significant main effect of threat, $F(1, 51) = .38$, $p = .54$, $\eta_p^2 = .007$, or order, $F(1, 51) = .65$, $p = .42$, $\eta_p^2 = .013$. There was a significant main effect of regulation, $F(2, 102) = 724.18$, $p < .001$, $\eta_p^2 = .93$. Unpleasantness ratings were higher for increase trials then for either view-neutral, $t(52) = 29.06$, $p < .001$, 98.3% CI [4.05, 4.8], or view-negative, $t(52) = 12.22$, $p < .001$, 98.3% CI [.73, 1.1], and images on view-negative trials were rated more unpleasant than view-neutral trials, $t(52) = 25.77$, $p < .001$, 98.3% CI [3.18, 3.85] (Bonferroni corrected alpha of $p < .017$). There was also a significant Regulation × Order interaction, $F(2, 102) = 3.62$, $p = .03$, $\eta_p^2 = .066$. Safe-first participants had a larger rating difference between increase and view-neutral trials than threat-first participants, $t(51) = 2.08$, $p = .043$, 98.3% CI [−1.34, .12], but this did not survive correction for multiple comparisons (Bonferroni corrected alpha of $p < .017$). There was also a trend toward a significantly larger difference between view-

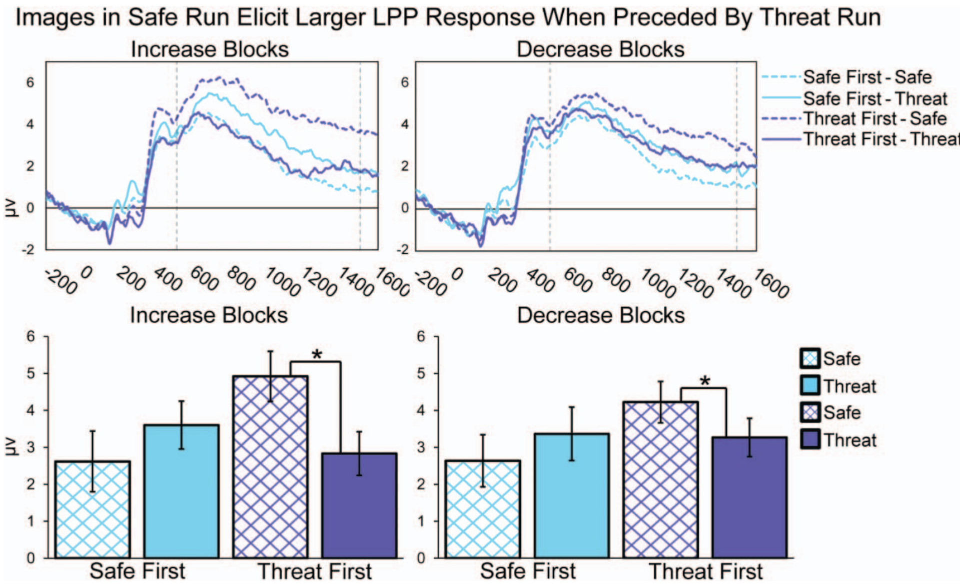

Figure 4. Averaged waveforms (top row) depicting Threat × Order interaction (regulation conditions collapsed) for increase (left) and decrease (right) blocks for both safe and threat conditions at Pz for safe-first (blue) and threat-first (purple) participants. Vertical gray lines represent time window used for statistical analysis. Bar graphs depict mean LPP amplitude for increase (left) and decrease (right) blocks for safe-first (blue) and threat-first (purple) participants with regulation conditions collapsed to depict Threat × Order interaction. Solid bars represent late positive potential responses during threat, and cross-hatched bars represent safe condition late positive potential responses. Error bars depict standard error. * Significant pairwise comparisons for follow-up *t* tests conducted within order condition, p < .05 (uncorrected). See the online article for the color version of this figure.

negative and view-neutral trials than those given threat first, $t(51) = 1.95$, $p = .06$, 98.3% CI [−1.17, .14]. There was no difference in increase minus view-negative trials between safe-first and threat-first participants, $t(51) = .64$, $p = .53$, 98.3% CI [–.47, .28]. There was no Condition × Order interaction, $F(1, 51) = 1.76$, $p = .19$, $\eta_p^2 = .033$, and no other interaction effects were significant ($ps > .05$).

Decrease condition. During decrease blocks there was no significant main effect of threat, $F(1, 52) = 3.8$, $p = .06$, $\eta_p^2 = .069$, or order, $F(1, 51) = 2.99$, $p = .09$, $\eta_p^2 = .055$. There was a significant main effect of regulation, $F(2, 102) = 331.1$, $p < .001$, $\eta_p^2 = .867$, with unpleasantness ratings being higher following view-negative trials than for either view-neutral, $t(52) = 23.84$, $p < .001$, 98.3% CI [2.97, 3.65], or decrease, $t(52) = 5.66$, $p < .001$, 98.3% CI [.38, .98] and images on decrease trials being rated more unpleasant than view-neutral trials, $t(52) = 17.3$, $p < .001$, 98.3% CI [2.26, 3.01] (Bonferroni corrected alpha of $p < .017$). There was also a significant Condition × Order interaction, $F(1, 51) = 6.1$, $p = .02$, $\eta_p^2 = .107$. In a pattern that mirrors the self-reported anxiety scores, safe-first participants rated images as being more unpleasant during the threat than safe set of trials, $t(26) = 2.69$, $p = .01$, 97.5% CI [.05, .76], while threat-first participants did not rate pictures during safe and threat trials differently, $t(25) = .47$, $p = .64$, 97.5% CI, [–.29, .19] (Bonferroni corrected alpha of $p < .025$). There was no Regulation × Order interaction, $F(2, 102) = 2.51$, $p = .09$, $\eta_p^2 = .047$, and no other interaction effects were significant ($ps > .05$).

Discussion

Because emotion regulation likely relies—at least in part—on executive function, which is disrupted by state anxiety (Clark et al., 2012; Lindström & Bohlin, 2012; Vytal et al., 2013), we predicted that state anxiety would impair emotion regulation. We used threat of shock, a common anxiety induction paradigm, to test this (Robinson et al., 2013). We predicted that the LPP response to negatively valenced images would be modulated by participants' attempts to increase and decrease their emotions when safe from shock, but would not be modulated by attempts to either increase or decrease their emotions while under threat of shock. However, our manipulation check revealed that our anxiety induction was qualified by an interaction with order such that safe-first participants reported less anxiety during safe compared to threat. In contrast, threat-first participants' anxiety levels during safe were equivalent to their anxiety during threat of shock. Thus, when the threat run was administered first, the anxiety induced by threat of shock appears to have carried over into the safe run. This anxiety carryover effect also seemed to influence LPP responses, with the primary LPP findings reflecting the order of safe and threat rather than regulation. There were no LPP amplitude differences between threat of shock and safe trials for safe-first participants. However, threat-first participants had greater LPP responses to all images during the safe than threat condition, regardless of valence or regulation instructions. This effect was more pronounced during increase blocks. Thus, although we used two well-validated para-

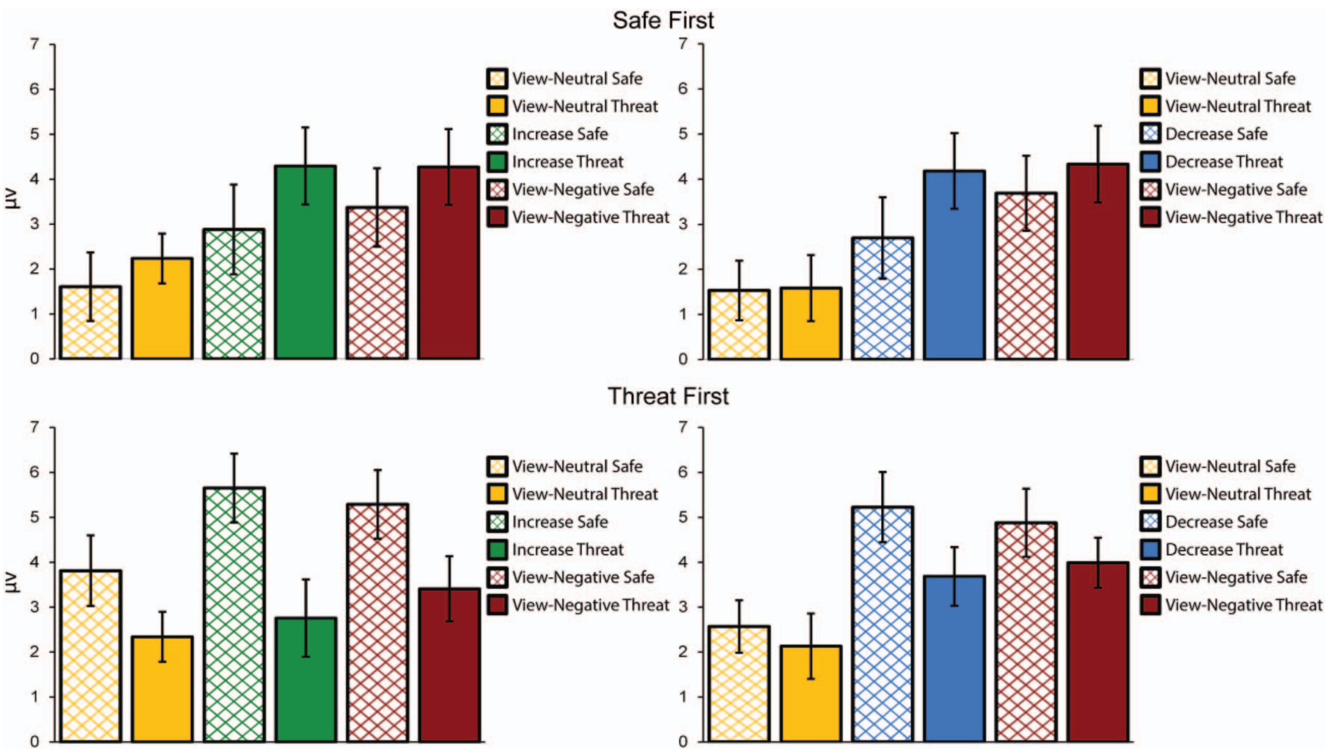

Figure 5. Average late positive potential amplitude for safe-first (top) and threat-first (bottom) participants for increase (left) and decrease (right) blocks. Error bars depict standard error. See the online article for the color version of this figure.

digms—instructed emotion regulation and threat of shock—the results were quite different than expected. The threat of shock manipulation eliminated previously reported regulation effects and instead resulted in carryover of state anxiety from threat to subsequent safe conditions that potentiated LPP responses to all images.

The surprising finding of greater LPP response during the safe condition (compared to threat) when it followed threat warrants consideration. One possible explanation is that participants misattributed the arousal caused by earlier threat of shock, once the threat of shock was no longer present. Literature on misattribution of arousal stems from the two-factor theory of emotion, which posits that emotional states are a function of physiological arousal and cognitive interpretations of that arousal (Schachter & Singer, 1962). Studies have supported the predictions of this theory that arousal induced by one task can increase the emotional response to a task that follows shortly after (Dutton & Aron, 1974; Messner & Wänke, 2011; Zillmann, Johnson, & Day, 1974). Critically, this misattribution effect seems only to occur when the source of arousal is not readily apparent (Schachter & Singer, 1962; Schwarz & Clore, 1983; Sinclair, Hoffman, Mark, Martin, & Pickering, 1994; Zillmann et al., 1974). For example, participants display less aggression when the opportunity to retaliate against an aggressor follows immediately after an exercise task than when there is a short delay between the exercise task and the opportunity to retaliate (Zillmann et al., 1974). Presumably, this is because the link between the physiological arousal and the exercise task is less apparent after a delay. A similar process may explain our results. Our manipulation check shows elevated anxiety during both threat and safe runs for participants who completed the threat run first. Arousal accompanying this anxiety may not have affected LPP responses to images during the threat run itself because the source of the arousal, the unpredictable shock, was readily apparent. However, when the safe run followed the threat run, residual arousal from the threat of shock, as evidenced by the sustained elevated anxiety ratings, may have been misattributed to the images, thereby causing greater LPP responses.

While the misattribution of arousal literature provides a possible explanation for our results, it also raises the question of why past studies using the threat of shock paradigm have not reported similar effects. The apparent misattribution effect in our results is based on the carryover of induced anxiety from safe to threat that we observed. However, most threat of shock studies do not elicit this carryover effect, at least not to the same degree (Bublatzky, Guerra, Pastor, Schupp, & Vila, 2013; Choi, Padmala, & Pessoa, 2012; Clark et al., 2012; Cornwell et al., 2011; Grillon & Charney, 2011; Robinson, Charney, Overstreet, Vytal, & Grillon, 2012; Vytal et al., 2013). Without anxiety induced via threat of shock carrying over into the safe trials, there is no residual arousal to be misattributed. Although studies using the threat of shock paradigm rarely test for order effects, most of these studies either have threat of shock trials intermixed with safe trials, or have short alternating blocks of threat and safety. Thus, if large carryover effects were at play in these studies, all of the safe blocks except for the first (assuming it precedes the threat block) would be affected. The fact that these studies report differences in anxiety between threat of

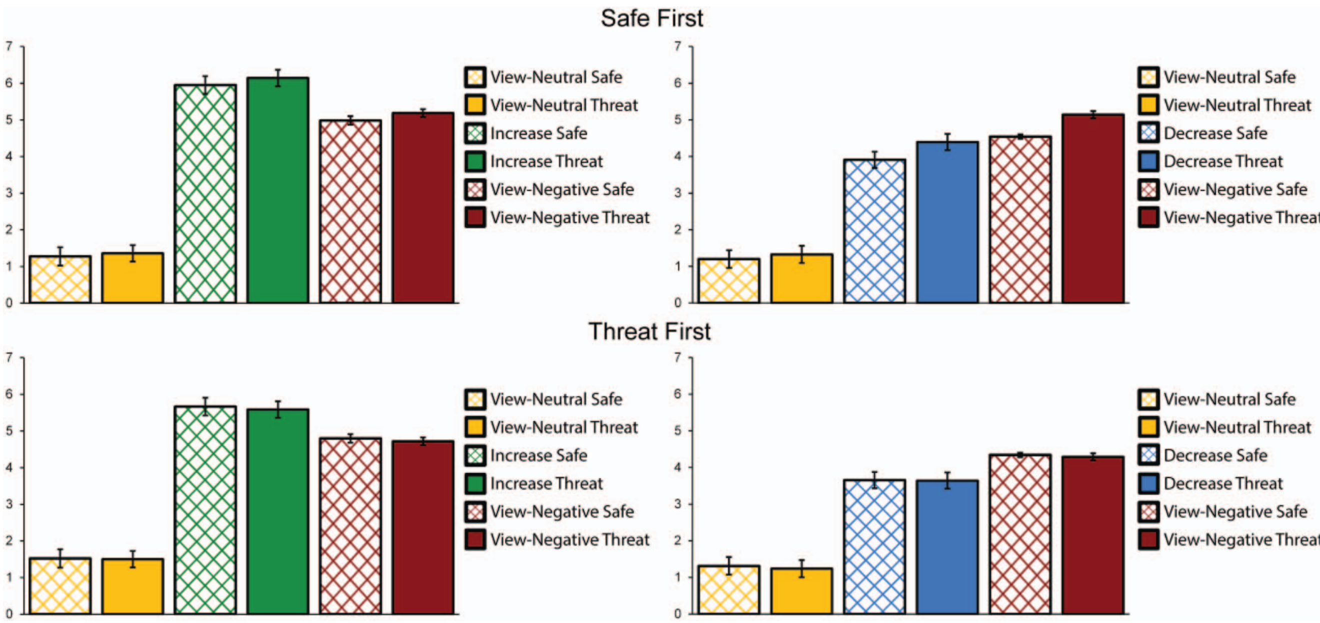

Figure 6. Ratings of image unpleasantness by regulation type for safe-first (top) and threat-first (bottom) participants for increase (left) and decrease (right) blocks. High numbers indicate that participants found the image more unpleasant. Error bars depict standard error. See the online article for the color version of this figure.

shock and safe trials, whether measured physiologically (Choi et al., 2012; Clark et al., 2012; Cornwell et al., 2011) or with self-reports (Bublatzky et al., 2013; Grillon & Charney, 2011; Robinson et al., 2012; Vytal et al., 2013), implies that carryover effects in these studies were either nonexistent, or smaller than those found in the current study. It may be that the emotion regulation task our participants completed led to a more sustained anxiety response because the task required them to be particularly aware of their emotions as they attempted to alter them. Additionally, our participants completed one long threat of shock run (made up of increase and decrease threat blocks), followed by a long safe from shock run, rather than having short alternating blocks of threat of shock and safety. This prolonged threat may have led to a sustained anxiety response that took longer to disengage, thus leading to carryover effects when the threat block was first. Future studies should investigate the variables that lead to carryover effects in the threat of shock paradigm. While published studies offer no indication that carryover effects are a problem when using the threat of shock paradigm, due to publication bias it is impossible to know how many, if any, threat of shock studies result in null results due to carryover effects.

If anxiety does disrupt emotion regulation, the carryover effects can explain why participants' attempts to regulate did not affect LPP responses in the safe run when the threat run was first. Because anxiety appears to have been elevated in the safe run when safe followed threat, regulation may have been disrupted in that condition, as well as when the threat was present. However, carryover effects do not explain why regulation instructions had no effect on LPP amplitude in the safe block, when the safe block was first. Except for the addition of the threat of shock manipulation, the instructed emotion regulation task we used was modeled on past studies (Moser et al., 2006; Moser et al., 2009). Although the effect of instructed regulation on LPP amplitude is well documented (Hajcak & Nieuwenhuis, 2006; Moser et al., 2006; Moser et al., 2014; Moser et al., 2009; Thiruchselvam et al., 2011), it is possible that we failed to replicate this effect due to the threat of shock induction, which could have produced some elevation in anxiety in the safe run, even when it was first, as participants anticipated receiving shocks in the following run. Alternatively, although the regulation effects in participant's self-reported emotions suggest that participants were engaged in the task, some other detail of how the instructed regulation procedure was implemented in the current study that remains unidentified may be responsible for our failure to replicate a regulation effect on LPP amplitude. For example, our within subjects design resulted in a relatively long experiment, with four blocks of about 13 min each. This may have fatigued participants, resulting in ineffective regulation. Whatever the cause, because the current study failed to find significant LPP regulation effects while participants were in the safe condition, no conclusions can be drawn concerning the effect of state anxiety on the ability to regulate emotions. Future research should further investigate this topic.

While participant's LPP amplitudes were not affected by instructions to regulate, participants did report less unpleasant emotions during decrease trials and more unpleasant emotions during increase trials (vs. view-negative trials). Additionally, the order effects seen in anxiety ratings and LPP amplitudes were replicated in participant's self-reported emotional response, but only during the decrease block. The reason for this discrepancy is unclear. Demand characteristics could be responsible for the significant regulation effects in the self-report data. However, it is possible that participant's self-reports were a more sensitive measure of emotion regulation. As previous researchers have discussed, different measures of emotion should not be considered interchangeable (Mauss & Robinson, 2009); therefore, some subtle difference in what is being measured via self-reports and LPP amplitude may also account for this discrepancy.

Additionally, while misattribution is a possible explanation of our LPP findings, it is not the only one. For example, it is possible that the pairing between the electrical shock and images that were presented resulted in a conditioning effect that caused participants to have more negative reactions to subsequent image presentations. However, because participants in the current study were subjected to the shock on a small percentage of trials (5%), any conditioning effects in our study were likely small. Another possible explanation for these results is that participant's LPP responses to images simply increased throughout the task, regardless of threat condition. This explanation arises from the observation that, while threat first participants had higher LPP responses during the safe block, safe first participants had higher LPP responses during the threat block, although this trend did not reach significance for either the increase ($p = .11$) or decrease blocks ($p = .24$). Although we believe that the misattribution hypothesis better explains why there was a larger difference between safe and threat for threat first participants, we cannot rule out this possible explanation. Thus, further investigation is needed to confirm that misattribution of arousal affects LPP amplitude. While the current study did not carefully control length of time between threat and safe blocks, a long interval between blocks would allow participant's physiological arousal to return to baseline, and should therefore abolish the misattribution effect. Thus, a study manipulating this variable could test the misattribution explanation we have proposed.

The current study sought to understand the influences of state anxiety on ability to voluntarily regulate emotions. Instead we found that the introduction of uncertain threat resulted in much broader impacts on emotional processing - elimination of regulation effects, and carryover of anxiety from the threat to safe conditions which resulted in exaggerated LPPs to visual images during safety. We posit that this carryover of anxiety resulted in misattribution of arousal and potentiation of neural responses to the images in the safe condition. The principle of misattribution of arousal is important, because it illustrates that individuals construct their emotional state by integrating their physiological arousal with contextual cues that provide information about the likely reason for that arousal. Our results imply that misattribution of arousal leads to an exaggerated affective neural response to the target of misattribution. Thus, our findings suggest that physiological arousal and cognition combine to influence the basic neural response to emotional stimuli, regardless of the veracity of assumptions regarding the source of arousal. Further research on the neural correlates of misattribution will lead to a better understanding of how the brain integrates physiological arousal and cognition in the production of emotion states.

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