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No experimental evidence for visual prior entry of angry faces, even when feeling afraid

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Abstract

Threatening stimuli prevent attentional disengagement. Less clear is whether threat captures attention in addition to holding it. One way to measure attentional capture is to examine visual prior entry. Visual prior entry occurs when one stimulus is consciously recognized as appearing prior to other stimuli. Using a temporal order judgments paradigm we examined whether threatening, angry faces would experience visual prior entry. Such a finding would provide evidence for attentional capture by threat. We further examined whether such attentional capture by threat was contingent on feeling afraid. Using Bayesian analyses, we found moderate support for the null hypothesis in two experiments ($Ns = 44, 63$). Angry faces did not capture attention, and there was no effect of feeling afraid due to watching a horror movie (Exp. 1), or anticipatory fear about giving a speech in front of an expert panel (Exp. 2). These studies were supplemented with a meta-analysis that suggests the visual prior entry effect is very small, if indeed it exists. Thus, the visual prior entry effect for threatening faces is likely a much smaller effect than the extant literature suggests.

No experimental evidence for visual prior entry of angry faces, even when feeling afraid

The ability to detect threatening stimuli rapidly is important for survival (Mathews & Mackintosh, 1998; Mogg & Bradley, 1998). Accordingly, threatening social and non-social stimuli do tend to attract attention (Compton, 2003; Öhman, Flykt, & Esteves, 2001). In the social domain, one common finding is that angry faces quickly attract attention (Hansen & Hansen, 1988; Öhman, Lundqvist, & Esteves, 2001; Pinkham, Griffin, Baron, Sasson, & Gur, 2010). Furthermore, this attentional bias toward angry faces is most pronounced in individuals with anxiety disorders (Gilboa-Schechtman, Foa, & Amir, 1999; Mogg, Philippot, & Bradley, 2004). A meta-analysis corroborated this notion, but also highlighted that the attentional bias toward threat is usually only present in anxious individuals (Bar-Haim, Lamy, Pergamin, Bakermans-Kranenburg, & Van Ijzendoorn, 2007). Others have suggested that anxiety plays a role in the initial capture of attention by threat (e.g., Fox, Russo, Bowles, & Dutton, 2001), the maintenance of attention due to threat (e.g., Williams, Watts, MacLeod, & Mathews, 1988), and the inability to disengage attention (Koster, Crombez, Verschuere, & De Houwer, 2004). In the present research, we investigated the effects of experimentally induced fear on the capture of attention by threatening faces (i.e., angry faces) in healthy individuals.

We examined a phenomenon known as *visual prior entry*. Visual prior entry occurs when “the object of attention comes to consciousness more quickly than the objects which we are not attending to” (Titchener, 1908, p. 251). The implication is that any stimulus that truly captures attention automatically will be experienced as having entered awareness prior to other stimuli even if the stimuli appear in very close temporal proximity. One method of examining visual prior entry is to present two stimuli varying in one dimension (e.g., emotionality) nearly simultaneously. Participants are then asked to report which stimulus appeared first. This type of task is known as a temporal order judgments (TOJ) task, and has

been used to assess threat-related attentional processes. However, the literature contains mixed results.

Using schematic faces, Fecica and Stolz (2008) reported ($N = 48$) that both angry and happy schematic faces were more likely to be judged as having appeared before neutral faces when they were in fact presented simultaneously. This finding is consistent with the occurrence of visual prior entry for threatening faces (although the authors did not measure visual prior entry). A subsequent set of five TOJ experiments ($Ns = 12-16$) found that both real and schematic angry faces were prioritized by the visual system such that they displayed visual prior entry (West, Anderson, & Pratt, 2009). Four of these experiments compared angry and non-angry faces. In a deviation from best practice TOJ procedures (see Spence, Shore, & Klein, 2001), participants were asked to name which stimulus appeared first (or second) (West et al., 2009). Typically, in TOJ tasks, participants are asked to indicate the location of the first stimulus (i.e., whether the left or right stimulus appeared first or second). This method keeps responding orthogonal to the manipulated dimension of interest, such as emotionality (Spence et al., 2001). Despite this concern, however, in two subsequent studies ($Ns = 15$), West and colleagues observed visual prior entry for fearful faces (except when the magnocellular pathway was suppressed) using the orthogonal response method (West, Anderson, Bedwell, & Pratt, 2010). Most recently, however, Schettino, Loeys, and Pourtois (2013) failed to find visual prior entry for angry or fearful faces relative to neutral faces across five experiments that employed the orthogonal response method ($Ns = 16-38$).

In light of meta-analytic evidence that attentional biases toward threatening stimuli are more pronounced in anxious than non-anxious participants (Bar-Haim et al., 2007), and findings that anxious and fearful states typically also promote attentional biases to threat (Edwards, Burt, & Lipp, 2010; Nelson, Purdon, Quigley, Carriere, & Smilek, 2015; Quigley, Nelson, Carriere, Smilek, & Purdon, 2012; Rutherford, MacLeod, & Campbell, 2004), visual

prior entry of threatening faces may be amplified among individuals high in anxiety or currently feeling fear. However, the one study that included a measure of trait anxiety failed to find any association of this measure with visual prior entry (Schettino et al., 2013). No prior work has investigated the effect of inducing a state of anxiety or fear on the visual prior entry of threatening faces; however, it seems plausible that induced fear may amplify the extent to which threatening faces capture attention. We tested this possibility.

People that are in fearful, self-protective states perceive greater anger in targets typically considered threatening (Maner et al., 2005). In another experiment, fear was induced by telling participants they would have to engage in public speaking (Wieser, Pauli, Reicherts, & Mühlberger, 2010). Participants then viewed angry, happy, and neutral faces. Compared to the control group, those induced to feel fear showed exacerbated attention-related electrocortical reactivity in response to angry faces. The authors interpreted this finding as reflecting enhanced motivational attention to threatening stimuli when people are afraid. In order to further investigate the attentional capture by threatening faces in states of fear, we induced fear in participants via a horror movie clip (Study 1) or the threat of public speaking (Study 2). We then compared the magnitude of the visual prior entry they experienced to those in a control condition. We hypothesised that we would find visual prior entry for threatening faces, but possibly only in the fear conditions.

Materials, data, and derived values for the two experiments and the subsequent meta-analysis are available via the Open Science Framework https://osf.io/ngt6y/?view_only=00c2520feb6b439687ec38e241e155a8.

EXPERIMENT 1

Method

Participants

Forty-four first-year students at UNSW Australia participated in Experiment 1 (25 female; $M_{age} = 19.02$ years, $SD = 1.30$) during the first semester of 2014. For participants to be able to sign up to the study, they needed scores indicating no probable anxiety or depression as indicated by a pre-screening questionnaire for the subject pool. We decided to stop data collection at the end of this semester¹. All participants were given course credit for their participation.

Apparatus

Experiments were run in a dark testing room. Observers were seated approximately 65 cm from the 22" CRT monitor (100 Hz refresh-rate). The experiment was delivered via Inquisit v4.0 (Seattle, WA: Millisecond Software).

Procedure, Stimuli, and Design

After participants provided informed consent, the experimenter explained the TOJ task. Participants then completed 20 easy practice trials where the delays between the to-be-judged stimuli (stimulus onset asynchrony; SOA) stimulus was 10 times larger than in the test phase, i.e., 0ms to 1000ms. A long interval was used because it allowed the experimenter to clearly talk the participant through practice trials if they did not initially understand the task. To mitigate an unexpected change in task difficulty, participants were explicitly informed that "the amount of time between the two faces appearing will be larger than in the test phase". After confirming understanding of the task, the experimenter left the room, and participants were randomly allocated to either watch a fear-inducing or neutral video by the computer software. On completion of the video, participants were randomly selected by the computer to self-report their emotional state (fear, tension, happiness, interest) either immediately or after the TOJ task. Emotional states were rated on a 7-point scale with

¹ Experiment 1 formed part of a one-semester undergraduate research internship course undertaken by the second author, and the aim was to have the study from design and material development to analysis completed by the end of the semester.

anchors of 1="not at all" and 7="very much" in response to "To what extent do you currently feel: [emotional state]". Counterbalancing of timing was conducted to rule out the effects of drawing awareness to participants' emotional state on emotional stimulus processing (Everaert, Spruyt, & De Houwer, 2013; Everaert, Spruyt, Rossi, Pourtois, & De Houwer, 2014; Spruyt, De Houwer, Hermans, & Eelen, 2007). The experimenter was blind to condition allocation, having left the room before the video induction was administered by the computer.

Fear induction. Participants in the fear condition watched a 6 minute 42 second clip from the movie *Scream* (starting with the line "Can you see me?"), while those in the control condition watched a clip of the same length from the episode "Mountains" from the David Attenborough series *Planet Earth* (starting with the line "This huge mountain chain..."). Horror movies such as *Scream* have been previously shown to induce fear and make participants feel self-protective (Gross & Levenson, 1995; Maner et al., 2005). Participants were randomly allocated to the fear condition ($n = 22$) or the control condition ($n = 22$).

Temporal order judgments. After presentation of a blank screen for 500 ms, each trial began with the presentation of a fixation cross in the centre of the screen, and square placeholders centred on locations 2.7° to the left and right of central fixation. Placeholders marked where the schematic face stimuli would appear and were $4.3^\circ \times 4.3^\circ$ in size. After 1000ms, one face (3.4° diameter) appeared in either the left or right placeholder (counterbalanced). If the first face was angry, the second face was always neutral, and vice versa. These stimuli were used in Experiment 2 of West et al., 2009 and consisted of one angry face and one neutral face and are visible in Figure 1. On 7.7% of trials the second face appeared simultaneously in the other placeholder. On the remaining trials, the second face followed after 10, 20, 30, 50, 80, or 100 ms with equal probability. These SOAs were selected to approximate the range and spread of those used in West et al. (2009) and are

reported as negative when the neutral face appears first, and positive when the angry face appears first. Thus, the pool of stimuli consisted of SOAs of -100, -80, -50, -30, -20, -10, 0, 10, 20, 30, 50, 80, 100 ms each presented 8 times per block. Participants completed 4 blocks of 104 trials, meaning that trials at each SOA was presented 32 times. Sixty ms after the second face appeared, both faces were masked and participants could indicate whether the left or right face appeared first by pressing the “A” or “L” keys (labelled “L” and “R” respectively). On response, the stimuli disappeared, and a blank screen was presented for 500 ms. Trial structures, excluding blank screens, are depicted in Figure 1. Responses faster than 150ms and slower than 3000ms were dropped from all analyses.

Figure 1. Stimuli and structure used for both experiments, with timings from Experiment 1. The duration for which faces co-occurred in Experiment 2 was 62.5ms. Stimuli in this figure are derived from West, G. L., Anderson, A. A., & Pratt, J. (2009). Motivationally significant stimuli show visual prior entry: evidence for attentional capture. *Journal of Experimental Psychology: Human Perception and Performance*, 35(4), Figure 1, published by APA, and adapted with permission.

Estimating the point of subjective simultaneity. The point of subjective simultaneity (PSS) refers to the SOA at which the threatening and neutral face would be equally likely to be judged as occurring first, and serves as the measure of visual prior entry. It was estimated separately for each respondent by fitting a two parameter cumulative Gaussian function to each participant's data (see Theeuwes & Van der Burg, 2013):

$$P(response|SOA) = \frac{1}{1 + e^{-slope(SOA-PSS)}}$$

Solutions were determined by adjusting the *slope* and *PSS* parameters so as to minimize the root mean square error across SOAs. Adjustment was performed using Solver in Microsoft Excel 2013.

Analysis procedure. Bayesian analyses were performed using JASP (v0.7.1.12) to determine whether there was evidence for visual prior entry of threatening faces (H_1) or against visual prior entry (H_0). A prior width of 1 was specified. A number of sets of analyses were conducted. First, Bayesian one-sample *t*-tests against a PSS value of 0 were performed. Next, using Bayesian independent sample *t*-tests, we examined whether being in a fearful state affected the PSS. Finally, using a Bayesian correlation, we examined whether self-reported fear states affected the PSS. In these analyses, and using published guidelines (Jeffreys, 1961), a Bayes Factor (BF_{10}) greater than 1 indicates evidence against the null hypothesis, with $BF_{10} > 3$ indicating moderate evidence against the null, and $BF_{10} > 10$ indicating strong evidence against the null. By contrast, a $BF_{10} < 1$ indicates evidence for the null hypothesis, with $BF_{10} < 0.3$ indicating moderate evidence for the null, and $BF_{10} < 0.1$ indicating strong evidence for the null.

Results

Manipulation check. Independent sample *t*-tests were used to compare the differences in emotional states between the two conditions within the two measurement time points. Fear and tension were successfully induced by the film clip from *Scream* when compared to the

nature documentary, and these effects persisted until after completion of the TOJ task (Table 1). There was also evidence for elevated interest in the fear condition after completing the TOJ task, and some evidence of reduced happiness prior to completing the TOJ task.

Table 1. Self-reported emotional state manipulation checks in Experiment 1

	<u>Fear</u>		<u>Tension</u>	
	Pre-TOJ	Post-TOJ	Pre-TOJ	Post-TOJ
Scream video (fear)	4.27 (1.79)	2.83 (1.99)	4.27 (1.68)	3.17 (1.74)
Nature video (control)	1.76 (1.17)	1.25 (0.46)	1.92 (1.26)	2.00 (1.07)
<i>t</i> -value	4.98 ***	3.35 **	4.71 ***	2.21 *
	<u>Interest</u>		<u>Happiness</u>	
	Pre-TOJ	Post-TOJ	Pre-TOJ	Post-TOJ
Scream video (fear)	4.55 (1.92)	3.58 (1.78)	2.45 (1.69)	3.33 (1.15)
Nature video (control)	4.46 (1.76)	2.50 (1.07)	4.23 (1.17)	3.25 (0.89)
<i>t</i> -value	0.15	2.04 *	-3.60 **	0.18

Note: pre refers to self-report after the film but prior to the TOJ task; post refers to after the TOJ task. Means (and *SDs*) are presented. Across conditions and measurement times, the correlations between each emotion state were: $r_{f,t} = 0.84$; $r_{f,i} = 0.28$; $r_{f,h} = -0.17$; $r_{t,i} = 0.27$; $r_{t,h} = -0.34$; $r_{i,h} = 0.37$.

*** $p < .001$, ** $p < .01$, * $p < .05$

Visual prior entry. No participants showed PSS values outside the range of possible values (range: -7.5 ms – 7.6 ms). Experiment 1 provided moderate evidence that the PSS did not differ from 0 for angry faces when collapsed across conditions, $BF_{10} = 0.120$, $t(43) = 0.22$, $p = .83$; in the fear condition, $BF_{10} = 0.172$, $t(21) = 0.32$, $p = .75$; or in the neutral condition, $BF_{10} = 0.163$, $t(21) = -0.05$, $p = .96$. There was also moderate evidence that being in the fear rather than neutral condition did not affect the PSS, $BF_{10} = 0.231$, $t(42) = 0.28$, $p = .78$ and moderate evidence that self-reported fear was not associated with the PSS, $BF_{10} = 0.206$, $r = -0.07$, $p = .66$ ². There was also no evidence of a self-reported fear-PSS relationship in either the fear, $BF_{10} = 0.381$, $r = -0.198$, $p = .378$, or neutral, $BF_{10} = 0.266$, $r = 0.025$, $p = .913$, condition examined separately. Completing the self-report emotion ratings before rather than

² Self-reported interest ($BF_{10} = 0.242$, $r = -0.11$, $p = .47$), tension ($BF_{10} = 0.369$, $r = 0.02$, $p = .47$), and happiness ($BF_{10} = 0.300$, $r = 0.02$, $p = .89$) were also not significantly associated with PSS values.

after the TOJ was unrelated to size of the PSS either when collapsed across conditions, $BF_{10} = 0.271$, $t(42) = -0.66$, $p = .515$; in the fear condition, $BF_{10} = 0.426$, $t(20) = -0.92$, $p = .367$; or in the neutral condition, $BF_{10} = 0.298$, $t(20) = 0.03$, $p = .976$. The fit of the two-parameter Gaussian function, indicated by RMSE (range 0.038, 0.107), did not differ across conditions, $BF_{10} = 0.295$, $t(42) = -0.80$, $p = .43$. Figure 2 depicts the predicted values after fitting the two-parameter Gaussian separately for each condition, as well as the mean PSS in each condition.

Figure 2. Experiment 1 raw data, curve fit, and PSS. Black represents the neutral condition, and mid-grey the fear condition. Circles represent mean proportion correct across participants, and their respective standard errors. Dotted lines connect these raw points. Solid lines representing the average predicted values from the two-parameter predicted Gaussian are overlayed. Mean PSS values for participants in each condition and their 95% confidence intervals are presented as vertical lines through the figure. In between these two lines is a light grey line representing zero. PSS confidence intervals in both conditions include zero.

Discussion

Experiment 1 failed to find evidence that being exposed to a fear inducing video, or having elevated levels of fear increased visual prior entry of threatening faces, and in fact found moderate evidence for no increase in visual prior entry. This finding was unexpected, as fear increases anger processing in facial cues (Maner et al., 2005; Wieser et al., 2010) and priming can promote visual prior entry in TOJ tasks (Theeuwes & Van der Burg, 2013). Indeed, one possibility is that the fear induction did not have the intended effect, and so replication with a different fear induction was desired. Another possibility, and one driven by evidence that there was no visual prior entry for threatening faces across conditions, or within either condition, is that threatening faces simply do not experience visual prior entry even under “optimal” emotional circumstances. Indeed, replicating these effects in fearful states would build on the null results in non-manipulated emotion states (Schettino et al., 2013).

EXPERIMENT 2

In Experiment 2 we changed the type of fear induction. Specifically, we had concerns that the horror movie might have induced a state of fear that was not self-relevant. Therefore, the fear induction in Experiment 2 aimed to leave participants not just feeling fear, but leave participants feeling fearful in a manner that made emotional expressions self-relevant. To achieve this goal, we led participants to believe that they would be performing a speech in front of a panel of evaluators after completing the TOJ task (i.e., instructions and preparation for the Trier social stress test; Kirschbaum, Pirke, & Hellhammer, 1993). We also changed the SOAs in Experiment 2. If the visual prior entry effect for threatening faces exists, but is small, then the temporal order judgment task should be more sensitive at smaller SOAs.

Method

Participants

Sixty-three first-year students at UNSW Australia participated in Experiment 2 (43 female, $M_{age} = 19.54$ years, $SD = 4.58$). In addition to the pre-screen for anxiety and

depression, as part of an ethical requirement, all participants who reported feeling anxious or depressed prior to being told what the experiment was about were not allowed to participate. Counterbalanced assignment to conditions occurred, with alternation between fear and control conditions. Three participants assigned to the fear condition declined to participate on hearing that they would need to perform a speech in front of a panel of evaluators during the process of seeking consent. In each of these cases, the next participant was assigned to the same condition. Thus, 31 participants completed the fear condition and 32 the control condition. Data collection ceased at the end of the second semester of 2014.

Apparatus

The apparatus was identical to Experiment 1, except the monitor was set to operate at 160Hz.

Procedure, Stimuli, and Design

Procedures are identical to those from Experiment 1, except that all participants now self-reported emotional state prior to the TOJ task.

Fear induction. Participants in the fear condition were led to believe they would give a five-minute speech in front of a panel of evaluators, detailing why they would be a suitable candidate for an administrative assistant position being offered in the psychology building.

All participants first watched the nature video from Experiment 1³. Then, for those in the fear condition the experimenter allowed ten minutes preparation time for the individual to brainstorm ideas they could discuss with the panel. Those in the control condition proceeded without any mention of a speech task. For the fear condition, after the experimenter returned, the participant was informed one judge was running late, and to time constraints the speech

³ The nature video was intended to equate emotional states across conditions, its efficacy was not verified, and it is possible that it did not ameliorate the fear in participants who knew that they would have to give a speech due to the consent form.

task would be moved to the final task of the experiment. Participants in the control condition were not given any information about completing a speech task.

TOJs. The task was similar to that used in Experiment 1 except: now on 11.1% of trials the second face appeared simultaneously in the other placeholder. On the remaining trials, the second face followed after 6.25, 18.75, 31.25, or 50 ms with equal probability. The faces were presented together for 62.5ms before masking. Moreover, the number of trials was reduced to 3 blocks of 108 trials each due to the additional time requirements of the speech task preparation ⁴. Again, responses faster than 150ms and slower than 3000ms were dropped from all analyses.

Results

Manipulation check. Fear and tension were successfully induced by the manipulation, while interest and happiness were unaffected (Table 2).

Table 2. Self-reported emotional state manipulation checks in Experiment 2

	Fear	Tension
Speech task (fear)	3.40 (1.65)	4.40 (1.78)
No task (control)	1.30 (0.48)	1.50 (0.71)
<i>t</i> -value	4.98 ***	6.38 ***
	Interest	Happiness
Speech task (fear)	4.10 (1.60)	4.50 (0.97)
No task (control)	4.80 (0.63)	4.60 (0.97)
<i>t</i> -value	1.62	0.24

Note: means are presented, with the standard deviation in parentheses. Across conditions, the correlations between each emotion state were: $r_{f,t} = 0.89$; $r_{f,i} = -0.03$; $r_{f,h} = -0.28$; $r_{t,i} = 0.06$; $r_{t,h} = -0.21$; $r_{i,h} = 0.59$. *** denotes $p < .001$, ** $p < .01$, * $p < .05$

Visual prior entry. One participant with a negative slope (-0.08) to their fitted function was dropped from analyses as this indicates that they tended to judge the late-occurring stimulus

⁴ Four additional participants were run with 4 blocks of 108 trials before it was determined that this made the total session duration too long. Including the functions calculated from these respondents does not change the reported results.

as occurring first. No analysed participants showed PSS values outside the range of possible values (range: -35.8 ms – 14.4 ms). Experiment 2 provided moderate evidence that the PSS did not differ from zero for angry faces when collapsed across conditions, $BF_{10} = 0.104$, $t(61) = 0.30$, $p = .763$; in the fear condition, $BF_{10} = 0.139$, $t(30) = 0.07$, $p = .942$; or in the neutral condition, $BF_{10} = 0.161$, $t(30) = 0.55$, $p = .588$. There was also moderate evidence that being in the fear rather than neutral condition did not affect the PSS, $BF_{10} = 0.194$, $t(60) = -0.17$, $p = .869$, and moderate evidence that self-reported fear was not associated with the PSS, $BF_{10} = 0.177$, $t(61) = 0.06$, $p = .638$ ⁵. There was also no evidence of a self-reported fear-PSS relationship in either the fear, $BF_{10} = 0.102$, $r = 0.102$, $p = .584$, or neutral, $BF_{10} = 0.436$, $r = 0.219$, $p = .237$, condition examined separately. Although the fit of the two-parameter Gaussian function, indicated by RMSE (range 0.031, 0.120), did not differ across, $BF_{10} = 0.211$, the data were re-analysed with one outlying case excluded (PSS > |3| SDs from mean, PSS range now: -10.0ms – 14.4ms): the findings were replicated with this exclusion; $BF_{10} = 0.283$, 0.259, 0.180, 0.238, and 0.436 respectively. Figure 3 depicts the predicted values after fitting the two-parameter Gaussian function after this exclusion separately for each condition and the outlying case, as well as the mean PSS in each condition.

⁵ Self-reported interest ($BF_{10} = 0.366$, $r = 0.17$, $p = .191$), tension ($BF_{10} = 0.234$, $r = -0.12$, $p = .372$), and happiness ($BF_{10} = 0.180$, $r = 0.07$, $p = .611$) were also not significantly associated with PSS values.

Figure 3. Experiment 2 raw data, curve fit, and PSS. Black represents the neutral condition, and mid-grey the fear condition. Circles represent mean proportion correct across participants, and have standard errors attached. Dotted lines connect these raw points. Solid lines representing the average predicted values from the two-parameter predicted Gaussian are overlaid. Mean PSS values for participants in each condition and their 95% confidence intervals are presented as vertical lines through the figure. To the left of these two lines is a light grey line representing zero. PSS confidence intervals in both conditions include zero. The one outlying case noted in text does not contribute data toward this plot.

Exploratory analyses.

In Experiment 1, the timing of probes into self-reported emotional states were manipulated in an attempt to rule out that the findings were due to drawing participant's attention to the emotion manipulation. Two further possibilities were examined here: first, whether the emotion manipulation affected noticing of the emotional cues in the critical task; and second, whether recognising that the intrinsic emotional cues of the dependent measure had any effect on visual prior entry. During verbal debriefing, 69% of participants identified that some faces were emotionally negative when asked if they had 'noticed any categories that the faces fell

in to'. Although emotion recognition was not perfect, exploratory investigation⁶ indicated that there was moderate evidence of no difference between conditions in the rate of recognisers and non-recognisers ($M_{fear} = 74.2\%$ vs $M_{ctrl} = 64.5\%$), $BF_{10} = 0.321$, nor was there any relationship between recognising the emotional content and the magnitude of PSS (Recognisers: $M = -0.05$, $SD = 1.10$; Non-recognisers: $M = 0.23$ $SD = 0.84$), $BF_{10} = 0.316$. Even under conditions where emotion was recognised as being involved in the task despite not being critical to the decision required, a condition normally expected to amplify the effects of emotional stimuli (Everaert, Spruyt, & De Houwer, 2011), visual prior entry for threatening faces was not present.

Discussion

In two experiments we failed to find support for our hypothesis that states of fear cause threatening faces to capture attention, in fact, we found moderate evidence for the null hypothesis. Even if the assumption is made that random allocation to conditions failed, these results were internally replicated in analyses of the correlation between self-reported fear states and visual prior entry for threatening faces both overall and within conditions. In Experiment 1, fear could be best considered as residual from the experience of watching a horror movie, while, in Experiment 2 the prospect of giving a speech would have been anticipatory. The absence of effects of these residual and anticipatory manipulations, the absence of self-reported fear effects within all conditions, and prior findings that trait anxiety (Schettino et al., 2013) does not modulate visual prior entry for threatening faces provide generalised evidence that emotion states (or the proclivity for them) do not impact visual prior entry for threatening faces.

Moreover, we found moderate evidence that there is no visual prior entry effect for angry faces relative to neutral faces, a finding that is both consistent and inconsistent with

⁶ Bayesian crosstabs using hypogeometric sampling.

prior findings (Schettino et al., 2013; West et al., 2009; West et al., 2010). Additional Bayesian analyses revealed moderate evidence for the null hypothesis. Although the Bayes factor is influenced by sample size, small sample sizes lead to a Bayes factor indicating neither evidence for or against the null hypothesis. Across all participants in both experiments, plotted in the order they were sampled (Figure 4), the moderate evidence for the null hypothesis continued to accumulate with increasing numbers of participants.

Figure 4. Progression of the Bayes factor across all sampled cases in Experiments 1 and 2.

The manipulation checks of self-reported emotional states indicated that our manipulations induced fear, but did not affect visual prior entry. One possibility is that the induction of fear actually prevented individuals from distinguishing threat cues from neutral ones. Indeed, such patterns have been observed in fear learning among anxious individuals (Lissek et al., 2005). However, we also could not replicate the basic visual prior entry effect in the control group. Indeed, in neither of the two fear or, critically, the two control

conditions was there any evidence of visual prior entry for threatening faces compared to neutral ones.

The pattern of results here suggests that in the strict sense proposed by Titchener (1908), threatening faces do not capture attention. These null findings have implications for future work on threat-related attentional processes. For researchers interesting in determining whether threatening faces capture attention, maintain attention, or influence attentional disengagement, our data suggest that threatening faces are unlikely to influence the initial orienting response.

SYSTEMATIC REVIEW AND META-ANALYSIS

The experimental results observed in the present work run contrary to some findings while replicating the null findings of others. In order to systematically consider possible boundary conditions of visual prior entry for threatening faces (e.g., photographic vs. schematic faces) we engaged in a systematic review of the literature. We consider six design choices that vary in the visual prior entry literature followed by a meta-analysis of the published literature.

Systematic Review

Four of the five studies identified via a search of the PubMed database⁷ were considered in addition to the present study. The other study did not test visual prior entry of threatening faces as indexed via either the reporting of choice data or a calculated PSS. No further studies could be identified from the references in these manuscripts, nor could any further papers be identified through citations of these manuscripts as examined in Google Scholar and PubMed.

Schematic faces. Our research used schematic faces rather than photographic faces. This decision is often made to better equate low level differences in how emotions are expressed

⁷ SEARCH: (“visual prior entry” OR “temporal order judgment”) AND (threat OR emotion OR anger OR fear). Last accessed 10/02/15.

(Purcell, Stewart, & Skov, 1996). However, one may argue that such faces lack ecological validity. The possibility that using schematic faces caused the absence of the effect seems unlikely, as all other studies using schematic faces reported significant results (i.e., Fecica & Stolz, 2008; West et al., 2009).

Angry faces. Angry (Fecica & Stolz, 2008; Schettino et al., 2013; West et al., 2009), fearful (Schettino et al., 2013; West et al., 2010), happy (Fecica & Stolz, 2008), and even neutral faces (compared to inverted neutral faces; West et al., 2009) have all been treated as the motivationally significant category of face, and been found to have statistically significant effects. Thus, it is unlikely that the choice of angry targets over fearful targets can account for the present results. Corroborating this assertion, among the null effects reported by Schettino et al. (2013) there was no divergence of effects for angry and fearful faces in the same study.

Single faces. In the present study, like the two studies using schematic faces noted above, we used only a single face per emotion condition. One issue with using a single face rather than an array of faces is that it may lead to habituation, such that the face loses its power to capture attention. However, given that significant visual prior entry and related effects have been observed under these circumstances in other studies (Fecica & Stolz, 2008; West et al., 2009), evidence for the habituation hypothesis seems weak.

Orthogonal response method. The orthogonal response method refers to asking participants to make TOJs on a dimension (e.g., left vs. right face) unrelated to the manipulated characteristic (e.g., angry vs. neutral). It is thought that response biases are more likely to emerge when individuals respond on the manipulated dimension (Spence et al., 2001), and for this reason the orthogonal response method was adopted. However, studies using both orthogonal (Fecica & Stolz, 2008; West et al., 2010) and non-orthogonal (West et al., 2009) response methods have previously found significant visual prior entry for threatening faces.

Masking of faces. One possibility is that additional processing of the face is required to achieve visual prior entry but this processing was prevented by the mask. An effect of additional processing time subtly moves the visual prior entry effect away from its origins in attentional capture to being more about the holding of attention, and for this reason a mask was selected in our experiments. Nonetheless, masking has been employed in both studies that have found (West et al., 2009), and failed to find (Schettino et al., 2013) visual prior entry for threatening faces. Moreover, the exploratory analyses of participants' ability to identify the affective nature of the faces during debriefing in Experiment 2 speak, at least somewhat, against any moderating effect of expression awareness.

Blocked designs. Blocked designs may be useful when multiple emotion comparisons (fear vs. neutral; angry vs. neutral) are being observed in the same study as the segmentation of stimuli may facilitate the processing of the specific emotional content (Schettino et al., 2013). Like the studies in which significant effects have been reported (Fecica & Stolz, 2008; West et al., 2009; West et al., 2010), this concern was not relevant to our design.

Judgment of first stimulus. Visual prior entry can be quantified by asking participants to judge which stimulus came first, came second, or whether both appeared simultaneously. The first of these, the method used in the two experiments reported here, typically leads to the larger estimate of visual prior entry (Spence & Parise, 2010). As such, it can be viewed as the most conservative tool for finding support for the null hypothesis, such as those observed here. Significant threat-related visual prior entry effects have been found when judgments are made for both which stimulus came first (Fecica & Stolz, 2008; West et al., 2009; West et al., 2010) and which came second (West et al., 2009).

Meta-analysis

Observations for the meta-analysis were defined as containing a measure of the PSS measure (in ms) of a shared between-subject condition, with discrete stimulus groupings from

the same subjects counting as separate observations (e.g., a fear-neutral and angry-neutral effect were both separately reported). In addition to the present two experiments, three of the articles that were identified in the review were included. These papers yielded a total of 20 observations in which the experimenters tested visual prior entry effects for threatening emotional faces by calculating the PSS (Schettino et al., 2013; West et al., 2009; West et al., 2010). Fecica and Stolz (2008) was not incorporated into the meta-analysis as the PSS was not calculated for each participant. Among the included studies, the PSS of each participant was calculated in a variety of ways, and the impact of this methodological variance is unknown. As it stands, every significant observation was observed by West et al. (2009; 2010), whereas every non-significant observation was observed in either the present study or those of Schettino et al. (2013). There was no attempt made to identify unpublished studies.

The first pass analysis of the average PSS, weighted by the standard error of the PSS, suggested some evidence of visual prior entry in the literature, but that this was confined to studies with smaller sample sizes (Figure 5). Examination of the funnel plot in Figure 6 makes clear that there are three distinct clusters of effects. For this reason, a random effects model was selected. Notably, consistent with the high exclusion rates due to problematic PSS estimates, Schettino et al.'s studies had much higher standard errors than the other studies. Effect sizes from five studies fell outside of the expected range after the application of a trim and fill procedure (Duval & Tweedie, 2000).

Figure 5. Plot of the average weighted PSS from a fixed effect meta-analysis, and Study PSSs, with studies arranged in ascending order based on sample size. Touching error bars indicate a shared sample size, which has been shifted slightly for presentational clarity. Error bars represent the 95% CI.

Figure 6. Funnel plot of the PSS after trim and fill via Duval and Tweedie's (2000) method. The solid line represents the PSS estimate from a DerSimonian-Laird random-effects model, and the dashed lines the pseudo 95% confidence interval around it.

Precluding the methodological accounts already explored above, it is reasonable to acknowledge that sample differences could account for some of this difference, but here we focus on data practices as an explanation. The data practices account we consider is one of segmentation based on small sample sizes. First, Egger's test rejected the null hypothesis that there were no small-study effects, $\beta = 1.83$ (95% CI: 0.73, 2.94), $p = .003$, suggesting significant bias. Because of their small sample sizes and low precision, we thus both excluded the studies of West et al. (2009; 2010), and sought to corroborate the estimates of the trim and fill procedure.

The random-effect meta-analytic estimate of the PSS based on only the work of Schettino et al. and ourselves was 0.88 ms (95% CI: -0.41, 2.17). We suspect this is an unbiased estimate given that (a) none of these individual effects was significant and it is unlikely there is a bias toward publishing null results, and (b) there was no relationship between sample size and effect size for these studies. Consistent with these studies measuring an equivalent phenomenon, there was no evidence of heterogeneity, $Q(12) = 4.71$, $p = .97$ ⁸. Although Fecica and Stolz (2008) did not report tests on the PSS calculated for each individual, it was possible to approximate it if it was assumed that all reported descriptive statistics came from a single participant. To test the validity of our conclusions, this was performed based on the reported aggregate descriptive statistics of decisions for the angry vs. neutral faces and angry vs. happy faces, via the methods we described for calculating the PSS in the experiments. This yielded values closely approximating the meta-analytic estimate, with PSS estimates of 1.34 ms (slope = 0.03, RMSE = 0.005) and 0.75 ms (slope = 0.04, RMSE = 0.007) respectively⁹.

⁸ For contrast, there was significant evidence of heterogeneity in the random effect model including the results of West et al., plotted in Figure 4, $Q(19) = 44.48$, $p < .001$.

⁹ This method does not generate a measure of PSS variability, precluding these estimates from inclusion in the meta-analysis or the provision of a confidence interval. If however the standard deviation derived in the subsequent paragraph (i.e., 7.54) is assumed for Fecica &

Our PSS estimates were more robust estimates than Schettino et al. (2013) as indicated by less variability (see Figure 4). Pooled across our studies the standard deviation is estimated at 7.54. Based on this information the effect size of visual prior entry for threatening faces is estimated as $d = 0.15$, and 350 participants would be required to detect a significant effect with .80 power (G*Power v3.1; Faul, Erdfelder, Buchner, & Lang, 2009). Thus, while our study was adequately powered to detect significant effects of the magnitude found in prior publications, it was not adequately powered enough to detect the post-hoc meta-analytic estimate. For context, the sample size required to detect such an effect is greater than the number of unique participants ($n = 329$) represented in the 4 papers in the meta-analysis.

Discussion

There may indeed be a very small visual prior entry effect for threatening faces, but based on the current published data there is no robust evidence for its existence (see footnote 5, confidence intervals around the mean weighted PSS in Figure 4, and random-effect estimate based on trimmed and filled data in Figure 5). The suspicion that there may be a very small effect, of unknown real-world implications, is additionally grounded in the fact that 11 of the 13 observations coming from manuscripts reporting only null effects have positive PSS values, which a one-sample binomial test suggests is unlikely to occur by chance if there was truly zero visual prior entry for threatening faces ($p = .022$).

Although moderator analyses of the six design features we reviewed would be informative, a random-effects meta-regression indicated that 100% of the between study variance was explained by lab group. Such a pattern of larger effects in the initial demonstration of an effect are typical in psychological research. Excluding the studies of

Stolz (2008) work, then the meta-analytic estimate is of an equivalent magnitude (0.95 ms) but the CI includes zero (95% CI: -0.03, 1.93). This reiterates that there is unlikely to be a noticeable visual prior effect for angry faces, and that if it does exist, it is very small.

West et al. (2009; 2010), there were insufficient observations not confounded by author group to conduct analysis of moderation by identified study characteristics.

Up until now, the temporal order judgement task has been presented on monitors for which the smallest intervals possible between stimuli have ranged from 6.25 ms (160Hz) to 16.67 ms (60Hz). This means that the effect that is being hunted may be 1/6th the magnitude of the hardware capacities. Power may be increased by adjusting how the stimuli are presented, perhaps to presentation via a tachistoscope, where submillisecond presentations are achievable and have been shown to evoke neural response (Sperdin, Spierer, Becker, Michel, & Landis, 2015). Alternatively, emphasis may be placed on the cases where the stimuli are presented simultaneously, to the near exclusion of trials of SOA except to make sure that participants are still attempting to make a decision. That being said, if visual prior entry for threatening faces is to be revisited, these questions should be re-examined with much larger samples, consistent with the meta-analytic effect size estimates.

CONCLUSION

Using Bayesian analyses, we found moderate evidence that threatening faces do not produce visual prior entry. This pattern was corroborated by a meta-analysis that suggested the apparent effect in the literature is driven entirely by the subset of initial studies with very small cell sizes ($ns \leq 16$) and unusually large effect sizes. Moreover, Experiments 1 and 2 demonstrated that feeling afraid does not induce visual prior entry for threatening faces, suggesting that they do not capture attention. These null findings are important for gaining an accurate understanding of when emotional expressions do or do not capture attention. In the case of attentional capture indexed via visual prior entry we found compelling evidence. Although visual prior entry for threatening faces may be a real phenomenon, it is probably a much smaller effect than implied in the literature.

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Author Note

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