Eye Movements are Captured by a Perceptually Simple Conditioned Stimulus in the Absence of Explicit Contingency Knowledge

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Past reports suggest that threatening materials can impact the efficiency of goal-directed behavior. However, questions remain about whether a conditional stimulus (CS) can capture attention as previous results may have been influenced by voluntary prioritization of a to-be-ignored CS. In 2 experiments, eye tracking was used to evaluate whether neutral, perceptually simple materials capture attention when they take on aversive properties via probabilistic fear conditioning with strict methods in place to eliminate voluntary CS prioritization. During training, participants attempted to fixate search targets (i.e., horizontally or vertically oriented rectangles) as quickly as possible to avoid shock. In reality, shock administration was related to rectangle orientation so that 1 rectangle (CS+) predicted shock more often than the other (CS−). Subsequently rectangles became distractors and were to be ignored. At this point, participants were instructed to fixate a new target and incidences of CS capture were examined. Results showed that saccades were made more quickly to the CS+ than the CS− as training progressed, and that oculomotor capture by irrelevant rectangles occurred more often for the CS+ than the CS−. An independent physiological index (skin conductance response) confirmed that contingencies had been learned, as SCR magnitude was greater for CS+ than CS− trials early in the test phase. These effects were documented despite the absence of explicit contingency knowledge, assessed using a postexperimental questionnaire. Collectively, these outcomes indicate that a CS can capture attention despite being task-irrelevant, and that these effects do not depend on conscious awareness of learned contingencies.

Attention permits the selection of a small subset of information from a cluttered environment for detailed processing and evaluation. It is well-established that voluntary (or top-down) allocation of attention can improve the efficiency of goal-directed behavior (Posner, 1980; for review see Carrasco, 2011). However, attention can also be captured by physically salient objects with perceptual properties that run counter to current behavioral goals. For example, when a wide attentional spotlight is cast across elements in a search display, objects that suddenly appear (i.e., onsets) or are defined by a unique color can draw attention away from the task at hand, reducing the speed with which targets can be detected and/or identified (Theeuwes, 1994; Yantis & Jonides, 1984, 1990; for review see Theeuwes & Godijn, 2001). These capture effects are evident not only in behavioral response times, but also in the trajectory of eye movements to search display elements (e.g., Theeuwes, Kramer, Hahn, & Irwin, 1998; Theeuwes, Kramer, Hahn, Irwin, & Zelinsky, 1999), which tend to be tightly coupled with attention (Deubel & Schneider, 1996; Hoffman & Subramaniam, 1995).

Recent work indicates that perceptual (or physical) salience is not the only factor that can impact the efficiency of goal-directed behavior. As summarized in detail elsewhere (Awh, Belopolsky, & Theeuwes, 2012; Belopolsky, 2015), both recent selection history and the presence of previously rewarded items in a search display can impact the efficiency of attentional guidance. Generally speaking, the interfering effects of selection history are evident when an object that served previously as the search target (e.g., green circle) becomes a distractor, a manipulation that reduces the speed with which decisions about the current target (e.g., red circle) can be made (Theeuwes & Van der Burg, 2011). This priming effect has been attributed to a change in the representational status of these items in the attentional priority map, which effectively makes them more salient than others not by virtue of their physical properties, but based on their history as targets in previous trials. Several studies have also shown that attention and eye movements can be captured by items that were associated with reward during a training phase (e.g., Anderson, Laurent, & Yantis, 2011; Anderson & Yantis, 2012; Mine & Saiki, 2015; Theeuwes & Belopolsky, 2012). Subsequent to training, the presence of a previously re-
warded element in the search display captures attention despite instructions that explicitly indicate these items are task-irrelevant and should be ignored. Based on outcomes like these, Awh, Belopolsky, and Theeuwes (2012; see also Belopolsky, 2015) have proposed that the standard dichotomous view of attention is incomplete, and that recent experience (e.g., based on selection history, reward history, or associative learning) is a third factor that can impact attentional priority by facilitating or disrupting goal-directed behavior depending on task demands.

Consistent with the view that rapid detection of threat is likely to be evolutionarily advantageous (Ohman & Mineka, 2001), some studies suggest that aversive objects—those that signal threat—may capture attention as well. To date, most of this work has been conducted with “prepared” or ecologically relevant materials (e.g., dangerous animals, faces that convey expressions of fear or anger, downward-pointing V's; Bannerman, Milders, & Sahraie, 2010; Larson, Aronoff, & Stearns, 2007; Lipp & Derakshan, 2005; Ohman, Lundqvist, & Esteves, 2001; Yorozinski, Penkunas, Platt, & Coss, 2014) or with manmade objects that have come to symbolize threat (e.g., guns, syringes; Blachette, 2006; Brossch & Sharma, 2005). For example, in a classic demonstration of capture by threat, Ohman, Flykt, and Esteves (2001) had participants search for unique targets among a set of distractors. On a given trial, targets were either “fear-relevant” (e.g., snakes) or “irrelevent” (e.g., flowers), and distractors came from the other category (e.g., a snake among flowers, or a flower among snakes). Participants indicated as quickly as possible whether or not a target was present, and consistent with the view that attention is captured by threat, response times were faster, and unaffected by variability in set size (i.e., an index of preattentive pop-out; Treisman, 1982), when the search target was “fear relevant.” This basic outcome has been replicated in several studies using a variety of materials (for review see Quinlan, 2013), and the results are compelling, though not without criticism. For example, response time effects may be a consequence of categorical differences between threatening targets and associated distractors, perceptual factors (e.g., the presence, rather than absence, of distinctive perceptual features such as the pin in a hand grenade, which is missing from a ball, or the coiled shape of a snake), and other basic search display characteristics (see Quinlan, 2013). More generally, studies like these conflate bottom-up goals with bottom-up capture, as the capturing item is also the search target (see Theeuwes & Goodjin, 2001 for review).

An alternative approach that has potential value with respect to addressing some of these criticisms involves the combined use of simple materials (e.g., colored squares), a Pavlovian fear conditioning procedure (Pavlov, 1927; Schultz, Balderston, & Helmstetter, 2012), and the additional singleton paradigm (Theeuwes, 1992). During differential Pavlovian fear conditioning, a neutral stimulus (e.g., a blue square; the conditional stimulus or CS+) is paired with an aversive unconditioned stimulus (e.g., shock; US) that produces an unconditioned response (e.g., enhanced skin conductance response, or SCR). With repeated pairings, the CS+ comes to evoke a conditioned response (e.g., enhanced SCR) even when the US has not been presented. In the same session, a different stimulus (e.g., a red square; the CS−), never paired with the US, can serve as a control and does not elicit a conditioned response. Standard indices of associative learning include changes in physiological arousal (e.g., SCR, startle, heart rate) and reports of explicit contingency knowledge, but recent work from our lab has shown that eye movements can also be used to index learning during fear conditioning (Hopkins, Schultz, Hannula, & Helmstetter, 2015). In that experiment, disproportionate CS viewing was evident in advance of explicit contingency knowledge, an outcome consistent with the view that eye movements may represent an especially sensitive index of learning and memory (see Hannula et al., 2010 for review). Here, we take advantage of this sensitivity and address questions about whether or not eye movements are captured disproportionately by a task-irrelevant CS+ during the performance of a simple oculomotor search task using the additional singleton paradigm. Questions about whether or not capture occurs even when participants remain unaware of the imposed contingencies are also addressed, which is important as there is ongoing debate about the relationship between explicit contingency knowledge and the expression of a conditioned response (LeDoux, 2014; Lovibond & Shanks, 2002).

A handful of recent investigations have used similar methods to evaluate capture by a perceptually simple CS (Mulckhuyse, Crombez, & Van der Stigchel, 2013; Notebaert, Crombez, Van Damme, De Houwer, & Theeuwes, 2011; Schmidt, Belopolsky, & Theeuwes, 2015a, 2015b). Two of these studies (Mulckhuyse et al., 2013; Schmidt et al., 2015a) used eye movements to index capture, and in both cases it was reported that eye movements were made in error more often to CS+ than to CS− distractors. Consistent with this outcome, the presence of a CS+ also had an impact on saccade latency (Schmidt et al., 2015a) and saccade trajectory (Mulckhuyse et al., 2013) to the target stimulus. For example, relative to baseline trials, voluntary saccades were initiated more quickly to cued locations that had been occupied by the CS+ and were delayed when the to-be-fixedated location was on the side opposite the CS+.

The outcomes described above provide converging support for prioritization of threat and suggest that a perceptually simple CS can capture attention. However, it is also possible that these results were a consequence of methodological choices. In both of the eye tracking experiments, participants were aware of (i.e., had either been directly informed, or had been trained to know) the CS/US contingency, and in one experiment (Schmidt et al., 2015a) the CS+ could occupy the same location as the upcoming search target. Participants were told that if they were too slow to fixate the cued location when the CS+ was present in the display, they were likely to receive a shock. Based on these procedural details, it seems likely that in both experiments participants may have directed attention to the CS+ voluntarily, which calls interpretation of reported outcomes into question. One potential exception concerns findings from a “paired” stimulus that was used by Schmidt et al. (2015a). On a given trial, the paired stimulus was presented coincident with either the CS+ or the CS−, and although it was not aversive based on the instructional manipulation, it drew disproportionate attention (as indexed by saccade latency) in the context of the CS−. This suggests that the paired stimulus had acquired aversive value by virtue of its co-occurrence with the CS+ and may then have captured attention automatically in the presence of the CS−. However, careful consideration of data from a posttest questionnaire suggests that even in this case the complete absence of voluntary prioritization seems unlikely. When the questionnaire was administered, participants rated CS− displays, which contained the paired stimulus, more fearful than completely
neutral displays. As such, the possibility remains that participants assigned explicit aversive value to the paired item despite the absence of instructions to do so, which then renders any strong conclusions about the absence of voluntary prioritization tenuous.

Another potential shortcoming of the studies outlined above concerns US presentation during performance of the attention task. This approach was adopted to limit extinction, but may have artificially inflated estimates of capture. In other words, the CS+ may have captured attention because it continued to co-occur with a naturally aversive stimulus (i.e., the US). This concern was addressed in a subsequent behavioral investigation (Schmidt et al., 2015b), but in that study the reported measure was response time and eye movements were not recorded. The target was a green circle among green diamonds, and an irrelevant (to-be-ignored) singleton was presented on a subset of trials. The irrelevant singleton was also a diamond, but was either blue (e.g., the CS+ singleton was presented on a subset of trials. The irrelevant singleton among green diamonds, and an irrelevant (to-be-ignored) and eye movements were not recorded. The target was a green circle among green diamonds, and an irrelevant (to-be-ignored) singleton was presented on a subset of trials. The irrelevant singleton was also a diamond, but was either blue (e.g., the CS+; paired with shock during an earlier training phase) or orange (e.g., the CS−; never paired with shock). As reported in past work (Theeuwes & Belopolsky, 2012), the presence of either perceptually salient singleton slowed response times to the target, but this effect was most robust when displays contained the CS+. This outcome suggests that attention was captured more often by the CS+ than the CS−, but the results cannot speak to this issue definitively because response time difference between conditions may have been due to increased attentional disengagement difficulty following capture by the CS+, with instances (or amount) of capture well-matched across conditions. Furthermore, as in past studies, participants were aware of (i.e., could successfully articulate) the imposed contingencies and may therefore have directed attention voluntarily to the CS+ despite instructions not to do so. While we recognize that explicit knowledge does not necessarily imply voluntary (rather than rapid, involuntary) prioritization of the CS+, it seems prudent to reduce or eliminate awareness of the imposed contingencies. Such an approach would obviate any concern that instances of “capture” might be subject to some degree of voluntary (or top-down) influence. The studies reported here were designed with these limitations in mind.

The current experiments employed an adapted version of an oculomotor capture task that was used previously to evaluate the impact of probabilistic reward on attentional prioritization (Theeuwes & Belopolsky, 2012). However, training outcomes in the present study were aversive rather than appetitive. During training, participants were instructed to fixate a rectangle among distractors as quickly as possible to avoid shock. They were told that shock would be administered when saccade latency was too slow, but in fact, shock administration was probabilistically related to target orientation (vertical vs. horizontal rectangle). With repeated training, the rectangle associated with greater likelihood of shock (e.g., the vertical rectangle) was expected to acquire aversive value. Subsequent to training, rectangles became distractors (i.e., irrelevant singletons) and participants were instructed to disregard them and to fixate a new target (i.e., a gray circle).

Use of this paradigm meant that shock was only administered during training, that the CS was always task-irrelevant during test, and that it never occupied the same spatial location as the search target during test. In the absence of US presentation during the test phase, capture could be evaluated without ongoing presentation of the naturally aversive stimulus and changes in the likelihood of capture could be examined as testing progressed (i.e., to determine whether or not capture by the CS+ is extinguished quickly or remains stable over the course of testing). Furthermore, because eye movements were used to index capture, we could determine whether or not there were differences in the amount of capture by the CS+ and the CS−, which cannot be done with response time data alone. Collectively these methodological choices, along with precautions that were taken to limit explicit knowledge of the CS/US association, meant that it was unlikely participants would voluntarily prioritize the CS+ during the test phase as may have been the case in past work.

Consistent with outcomes reported in the reward-based version of this task (Theeuwes & Belopolsky, 2012), it was predicted that saccade latency would be sensitive to learning during the training phase (i.e., that saccades would be initiated more quickly to the CS+ than to the CS− as training progressed) and that capture would occur more often during the test phase when the CS+ served as the irrelevant distractor; these effects were expected despite a lack of explicit contingency knowledge. One final factor that distinguishes the current work (Experiment 1) from previous studies that have examined capture by conditioned stimuli was the collection and evaluation of a standard index of physiological arousal commonly used in fear conditioning tasks (i.e., SCR). Whether or not SCR would reliably index learned fear in this experiment was not clear, as results from past studies have been mixed (Schultz & Helmstetter, 2010; Sevenster, Beckers, & Kindt, 2014), and because SCR differences dissipate quickly in the absence of continued US administration (Knight, et al., 2004), which was avoided in the test phase here for reasons outlined above.

**Experiment 1**

**Method**

**Participants.** Twenty-seven undergraduate students (20 women, 18–29 years of age) completed this experiment and were compensated with extra credit in a psychology course. All of these participants provided written informed consent and experimental procedures were approved by the Institutional Review Board at the University of Wisconsin, Milwaukee. Data from one participant were dropped from analyses because of a failure to comply with instructions.

**Apparatus.**

**Eye tracking and stimulus delivery.** Eye movements were recorded with an EyeLink 1000 eye tracking system (SR Research LTD: Ontario, Canada). This system operates with a temporal resolution of 1000 Hz and has a head-supported spatial resolution of ±1°. Saccades were identified using an automated algorithm with minimum velocity and acceleration criteria set to 30°/s and 8000°/s², respectively. The control of stimulus delivery and data analyses were performed with Experiment Builder software (SR Research). Stimuli were displayed on a 22-inch View Sonic monitor with 1,680 × 1,050 pixel resolution and a refresh rate of 60 Hz.

**Electrical stimulus and skin conductance recordings.** The US was a 500 ms electrical stimulus delivered using a 60 Hz AC source (Contact Precision Instruments: Boston, MA) through two 8 mm silver/silver chloride surface cup electrodes (Biopac: Goleta, CA). Each electrode was filled with electrolytic gel and placed over the right tibial nerve above the medial malleolus.
Skin conductance was sampled at 1 kHz with a Contact Precision Instruments unit equipped with a SCS 24-bit digital amplifier. Data were collected via two surface cup electrodes filled with electrolytic gel which were attached to the sole of the left foot 2 cm apart. Data analysis was conducted offline using PSYLAB software (Contact Precision Instruments) in line with analyses performed in recent fear conditioning studies (e.g., Schultz, Balderston, Geiger, et al., 2013; Schultz & Helmstetter, 2010).

**Design and procedure.** After written informed consent was obtained, a US intensity work-up procedure was completed in which each participant determined the maximum level of US intensity that would be used in the experiment proper. The work-up procedure consisted of no more than five presentations of the electrical stimulus and the perceived intensity of each presentation was rated by the participant on a scale from 0 (no sensation) to 10 (painful but tolerable). The intensity of electrical stimulation was increased incrementally until a rating of 10 was made, at which point the work-up procedure was terminated. US intensity remained at this level for the duration of the experiment. Collapsed across participants, mean intensity of the electrical stimulus was 2.1 mA and the range was .5 to 5 mA.

Subsequent to the US intensity work-up, participants were seated 70 cm from the computer monitor and a chinrest was adjusted to comfortably fix head position. Once participants were situated a calibration procedure was performed using a 9-point automated display. Task instructions were provided after successful calibration had been achieved.

The experiment was subdivided into a training phase, a corresponding test phase, and a posttest assessment of awareness. Stimulus displays that were used in the training phase consisted of eight objects superimposed on a black background. These objects were situated on the circumference of an imaginary circle with a 9° radius surrounding the center of the screen. Individual objects were equidistant from their neighbors and from the screen center. Each object was red (CIE 1931: X = 41.24, Y = 21.26, Z = 1.93) and every display contained six circles (diameter = 2.7°), one triangle (width and height = 2.7°), and either a horizontally oriented rectangle (width = 2.7°, height = .9°) or a vertically oriented rectangle (width = .9°, height = 2.7°; see Figure 1a). Participants were instructed to find (i.e., fixate) the rectangle in a given display as quickly as possible after the search display was presented. They were also told that if their responses were too slow, they would receive a shock. In reality, shock administration was predetermined and not related to task performance. One of the rectangles (e.g., horizontal) was designated the CS+ and displays that contained this object coterminated with shock 80% of the time. The remaining rectangle (e.g., vertical) was designated the CS− and coterminated with shock 20% of the time. Counterbalancing ensured that rectangles of each orientation were assigned equally often to the CS+ and CS− conditions across participants, and that rectangles appeared with approximately equal frequency in every spatial location for a given participant (i.e., rectangles occupied each location either seven or eight times across trials). Collapsed across participants, each location in the training display was occupied by a rectangle equally often.

During the training phase, individual trials began with the presentation of a centrally located gray fixation point (diameter = 5°). Participants were instructed to advance the trial by pressing the space bar while fixating the center of the screen. Following the button press, the display remained unchanged for a short period of time (randomized from 800 ms–1300 ms across trials)—an approach meant to discourage anticipatory saccades—and then the search display was presented and remained in view for 6 s. Adjacent trials were separated by a variable duration 12-s to 16-s intertrial interval (intertrial interval [ITI]; see Figure 1a). The 6-s display duration and trial spacing were chosen to be compatible with typical SCR latency and duration.

Participants completed 10 practice trials without any shock and then 60 training trials subdivided into six blocks of 10 trials. The number of training phase trials was constrained by individual trial duration (M = 20s including the ITI; total training phase duration ~20 min). In addition, use of relatively few training trials (compared to standards in the attention literature) was meant to reduce the likelihood that awareness of the imposed contingencies would develop. Altogether, there were 30 CS+ trials (24 of which coterminated with shock) and 30 CS− trials (six of which coterminated with shock). Trial order was pseudorandomized with the constraint that there were no more than three consecutive trials of the same type.

**Figure 1.** Trial structure and event timing for the training phase (a) and the test phase (b), respectively. Note that event timing was modified for Experiment 2, but trial structure remained the same. See the online article for the color version of this figure.
The test phase, without shock, was initiated immediately after training. Individual trials began with the presentation of a centrally located gray fixation point, and participants could advance the trial by pressing the space bar while fixating the center of the screen. Following the button press and a variable 800 ms–1,300 ms delay, a placeholder consisting of eight red circles equidistant from each other and the center of the screen was presented. The place holder remained in view for 1 s, and participants continued to fixate the center of the screen. The fixation point was removed from view at a random interval between 50 ms and 300 ms prior to test display onset while the place holder remained on the screen, an approach that was meant to ease attentional disengagement when the test display was presented (Saslow, 1967). Test display onset was marked by one of the circles changing from red (CIE 1931: X = 41.24, Y = 21.26, Z = 1.93) to equiluminant gray (CIE 1931: X = 20.17, Y = 21.22, Z = 23.11). Participants were instructed to fixate this target as quickly as possible after the change was made and to maintain fixation at this location until the end of the trial. The test display remained in view for 5 s.

Three different display types were used in the test phase—namely, control trials, CS+ trials, and CS− trials. In contrast to control trials, which simply contained a gray circle (the target) among red circles (the distractors), the two remaining trial types also contained an irrelevant singleton (i.e., either the horizontally or vertically oriented rectangle from the training phase). CS+ trials contained the rectangle that had been presented more often with shock during training and CS− trials contained the rectangle that had been presented infrequently with shock during training. This irrelevant singleton appeared coincident with the appearance of the gray target circle in a location that was not occupied by one of the placeholders (i.e., as an onset), but it was never presented directly adjacent to the target. Across trials the irrelevant singleton appeared equally often in locations with an angular distance of ±67.5°, ±112.5°, or ±157.5° degrees from the target. As in training, adjacent test displays were separated by a 12- to 16-s variable duration ITI (see Figure 1b).

The test phase was preceded by nine practice trials. The irrelevant singleton was never presented during practice (i.e., these were control trials). During test, participants completed 54 trials (i.e., 18 control trials, 18 CS+ trials, and 18 CS− trials) subdivided into six blocks. As in training, the number of trials was fewer than the standard in oculomotor capture tasks. This number was constrained by the duration of individual trials, which were quite lengthy to permit reliable SCR data acquisition, and because it was expected that any fear response would extinguish rapidly in the absence of reinforcement (cf. Vansteenwegen, Francken, Vervliet, De Clercq, & Eelen, 2006). Trial order was pseudorandom with the constraint that no more than three consecutive trials of the same type could occur. Across trials, the target (i.e., the gray circle among red distractors) was presented with approximately equal frequency in every spatial location for a given participant (i.e., six or seven times per location across trials).

Following the test phase, participants were asked to complete a postexperimental questionnaire (PEQ) which required responses to three increasingly specific questions designed to assess explicit knowledge of the association between shock administration and rectangle orientation during training. The first question was opened and asked participants to speculate about the purpose of the experiment. The second question required participants to indicate, yes or no, whether they had noticed an association between rectangles presented at training and shock administration, and if so, to describe the pattern that they had noticed. Finally, participants were asked to guess whether they had been shocked more often when the rectangle was horizontal or vertical, or whether shock administration occurred equally often across orientations.

**Data analysis.** Two measures (i.e., eye movements and skin conductance responses) were used to evaluate the effectiveness of our differential fear conditioning manipulation and to address questions about whether or not attention was captured by the conditioned stimulus.

**Eye movements.** Trials were eliminated from analyses if saccades did not start within 1° of the fixation point, and if saccade initiation was faster than 80 ms or slower than 600 ms following display onset (Theeuwes & Belopolsky, 2012). This procedure resulted in an average loss of 6% of the trials. Additional trials were removed from analyses if the initial fixation was not directed to the target (training: rectangle; test: gray circle) or to the irrelevant singleton (test: rectangle) following display onset (resulting in an average loss of 3% of the trials). Ultimately, across participants, the range of excluded trials was 0%–11.4%. Saccades were defined as having landed on the target or the irrelevant singleton if they were within 15° of arc from the center of that stimulus.

Two dependent measures were used to assess the influence of differential fear conditioning on the distribution of attention to display elements: (a) saccade latency to targets following display onset (assessed during training and test); and (b) percentage of capture trials (test phase only). Saccade latency was defined by the amount of time (in milliseconds) required to initiate the first saccade to the target stimulus following display onset; test trials for which capture occurred were eliminated from this analysis.

**Skin conductance recordings (SCR).** Skin conductance recordings were evaluated 2 s prior to stimulus onset (i.e., baseline period) and for the entire duration of each training or test display (6 s). Mean baseline skin conductance level was subtracted from the peak SCR responses recorded during the stimulus presentation period (Pinelès, Orr, & Orr, 2009) and direct comparisons were performed to determine whether differences in peak SCR magnitude distinguished CS+ from CS− training trials, or CS+ from CS− and control test trials.

**Results.**

**PEQ:** Participants remained unaware of the experimental contingencies. Two participants (from 26) indicated on the PEQ that a fear conditioning procedure had been used in the experiment. Based on their responses, these individuals seemed to have some awareness of the purpose of the investigation, although neither participant guessed that we were evaluating whether or not conditioned stimuli can capture attention. Thirteen participants gave a “yes” response when they were asked to indicate whether or not they suspected an association between the rectangles and shock administration. However, none went on to successfully articulate
the imposed contingencies. Instead, they indicated that shock was tied to “how long you looked at [the rectangle],” to rectangles that appeared in a very specific spatial location (e.g., “upper-right hand position”), or to other behaviors (e.g., “not having fixated the white dot before finding the rectangle”). When the forced-choice test was administered seven participants correctly identified the white dot before finding the rectangle). When the forced-choice position”), or to other behaviors (e.g., “not having fixated the upper-right hand rectangle” or “not having fixated the upper-right hand rectangle”), or to other behaviors (e.g., “not having fixated the upper-right hand rectangle”).

Training phase: Saccade initiation is faster to the CS+ than the CS−. Participants successfully directed saccades to the target stimulus on 98% of the trials. As indicated above, the small subset of trials with initial saccades that were not directed to the target were eliminated from the saccade latency analysis.

To determine whether or not associative learning was evident in eye movement behavior, and to evaluate the progression of this effect over the course of training, data were subdivided into early (Blocks 1–3) and late (Blocks 4–6) training phase sets. Results from a repeated measures ANOVA confirmed that saccade latency was affected by our differential fear conditioning manipulation (main effect of trial type: $F(1, 25) = 4.21, p = .05, \eta_p^2 = .14$), an effect that became more robust as training progressed (Trial Type × Training Phase interaction: $F(1, 25) = 9.01, p = .006, \eta_p^2 = .27$), and Bonferroni corrected $t$ tests indicated that the difference in saccade latency, faster for CS+ than for CS− trials, was evident during the second, $t(25) = 3.65, p = .002$, Cohen’s $d = .73$, but not the first half of training, $t(25) = .42, p > .05$, Cohen’s $d = .08$; see Figure 2a. The main effect of training phase (early, late) was marginal, $F(1, 25) = 3.79, p = .06, \eta_p^2 = .13$.

A repeated measures ANOVA, calculated using the SCR data, showed a main effect of training phase, $F(1, 25) = 22.14, p < .001, \eta_p^2 = .48$. As training progressed, SCR magnitude was reduced for both CS+: $t(25) = 3.46, p = .004$, Cohen’s $d = .68$; and CS−: $t(25) = 2.98, p = .012$, Cohen’s $d = .58$; see Figure 3a. trials, an outcome that likely reflects SCR habituation with repeated exposure to the US (e.g., Katkin, Wiens, & Öhman, 2001; Marcos, 1998). Neither the main effect of trial type nor the training phase by trial type interaction were significant, $F_S \leq 1.56, ps > .05, \eta^2 < .06$. Further analyses confirmed that there were no differences in average latency of the peak skin conductance response across conditions. These differences might be expected if SCR responses during the 5-s trial period were affected by shock, which was delivered more often at the end of CS+ than CS− trials. The peak occurred within 4.4 s of stimulus onset for both the CS+ ($SD = 1.2$) and the CS− ($SD = 0.9$). Additional exploratory analyses limited to the first five training trials from each condition produced outcomes consistent with those reported above—that is, peak SCR magnitude did not distinguish CS+ from CS− trials, $t(25) = .52, p > .05$. This comparison was performed to determine whether or not there was evidence for any SCR differentiation across conditions prior to habituation. Finally, it is important to note that the absence of training phase differences was not a consequence of poor physiological response registration. Evaluation of the trial-averaged SCR time-courses (available in the Supplementary Materials) does reveal sensitivity to shock delivery during the ITI.

Test phase: Eye movements are captured disproportionately by the CS+. On average, the presence of an irrelevant singleton in the test display gave rise to oculomotor capture on 13% of all trials (range: 0%–33%, with one participant showing no capture). A repeated measures ANOVA was calculated to address questions about whether capture occurred disproportionately for CS+ (as compared with CS−) trials, and to evaluate the progression of this effect during early (Blocks 1–3) and late (Blocks 4–6) halve of
the test phase. Consistent with predictions, capture occurred more often when the CS+ was present in the display, $F(1, 25) = 11.78$, $p = .002$, $\eta^2_p = .32$, an effect that was especially strong during the first half of testing (Trial Type × Test Phase interaction: $F(1, 25) = 6.79$, $p = .02$, $\eta^2_p = .21$). Notably, the overall likelihood of oculomotor capture did not decline during the second half of testing (main effect of test phase: $F(1, 25) = .35$, $p > .05$, $\eta^2_p = .01$), and evaluation of Figure 2b shows that this is because capture by the CS− increased with progression of the test phase. Bonferroni corrected post hoc comparisons indicated that capture by the CS+ was greater than the CS− during the first half of the test phase, $t(25) = 3.83$, $p = .002$, Cohen’s $d = .79$, but that this difference was reduced and not significant late in testing, $t(25) = 1.04$, $p > .05$, Cohen’s $d = .21$.

Saccade latency was evaluated for the subset of trials that did not elicit overt oculomotor capture to evaluate whether or not the time required to initiate a saccade to the target stimulus was affected by the presence of an irrelevant singleton (as compared to control trials) or by training history (CS+ vs. CS−). A repeated measures ANOVA with the factors trial type (CS+, CS−, control) and test phase (first half, second half) was calculated. There were differences in saccade latency as a function of trial type, $F(2, 50) = 12.18$, $p < .001$, $\eta^2_p = .33$, and saccade latency was faster later in the test phase, $F(1, 25) = 4.83$, $p = .04$, $\eta^2_p = .16$, but the interaction was not significant, $F(2, 50) = .55$, $p > .05$, $\eta^2_p = .02$. Because the interaction was not significant, Bonferroni corrected post hoc comparisons were performed on data collapsed across blocks. Results indicated that the presence of an onset (i.e., the irrelevant singleton) in the test display slowed saccade deployment to the target stimulus (control vs. CS+: $t(25) = 4.01$, $p = .001$, Cohen’s $d = .83$; control vs. CS−: $t(25) = 5.70$, $p < .001$, Cohen’s $d = 1.12$), but there were no differences in saccade latency as a function of training history (i.e., CS+ vs. CS−: $t(25) = 1.45$, $p > .05$, Cohen’s $d = .30$; see Figure 2c).

Skin conductance responses were not sensitive to learned contingencies when comparisons were based on data subdivided into early and late testing periods (all $F$s ≤ 1.7, $p$s > .05, $\eta^2_p$s ≤ .13; see Figure 3b). However, it has been widely documented in the fear conditioning literature that when US administration is discontinued, SCR attenuates (i.e., is extinguished) rapidly. Therefore, it is common to use and/or evaluate far fewer trials (cf. Schiller et al., 2013; Schultz et al., 2013) than was done here. For these reasons, additional analyses, limited to the first five test trials of each type (CS+, CS−, and control trials), were performed. Collapsed across trials, results indicated that SCR magnitude varied as a function of trial type, $F(2, 50) = 5.10$, $p = .01$, $\eta^2_p = .17$. As predicted, SCR magnitude was greater to CS+ than to CS− trials, $t(25) = 2.53$, $p = .05$, Cohen’s $d = .7$ and control trials, $t(25) = 2.65$, $p = .04$, Cohen’s $d = .58$ shortly after testing was initiated; the SCR difference to CS− versus control trials was not significant, $t(25) = .03$, $p > .05$, Cohen’s $d = .006$ (see Figure 3c).

**Discussion**

As has been documented previously (e.g., Theeuwes et al., 1999), the appearance of an irrelevant singleton coincident with test display onset captured attention. Capture was evident in two measures—overt movement of the eyes to the location occupied by the irrelevant singleton (i.e., a vertically or horizontally oriented rectangle) and slower saccade initiation to targets in the presence of an irrelevant singleton when oculomotor capture did not occur. Importantly, and consistent with our predictions, oculomotor capture occurred disproportionately when the irrelevant singleton was the rectangle that was a reliable predictor of shock during the training phase (i.e., the CS+). Together with the finding that participants were significantly faster to fixate the CS+ during the second half of the training phase, this outcome indicates that participants had learned the association between rectangle orientation and shock administration even though PEQ performance suggests that they were not consciously aware of the contingency. Under these circumstances, it seems highly unlikely that participants deployed attention voluntarily to the CS+.

Successful acquisition of the programmed contingencies was also evident in the physiological data, as the peak skin conductance
response was greater for CS+ than for CS− and control trials early in testing. It may seem surprising that similar effects were absent from SCR data acquired during training, but we expect that this was a consequence of task complexity (cf. Hopkins et al., 2015) and relatively rapid SCR habituation (cf. Chen, Aksan, Anderson et al., 2014; Schultz, Balderston, Geiger, & Helmstetter, 2013). Standard fear conditioning experiments tend to use very simple stimulus materials and designs (e.g., Schultz & Helmstetter, 2010), and learning develops quickly after the onset of training. Therefore, differential responses to the CS+ and the CS− can be documented before habituation has taken place. In our case, learning, as indexed by saccade latency, was only evident during the second half of the training phase when there was also significant SCR habituation. This temporal coincidence of differential learning and habituation would likely reduce sensitivity to any potential SCR differences in our data. Subsequent to training, new instructions were provided and participants practiced the test phase task. This shift in experimental context together with the no-shock intensity workup procedure was as described for Experiment 1, but the initial fixation was not directed to the target or to the irrelevant singleton (i.e., either the CS+ or the CS−) on two thirds of the trials. The test display remained in view for 1,000 ms, and shock was never administered. As in training, adjacent trials were separated by a 2,000 ms ITI; instructions and trial numbers remained the same, and the PEQ was administered upon completion of the test phase.

While it was the case that trial duration was much shorter than Experiment 1, the number of training and test trials remained the same, which again represents a departure from standard practice in attention capture studies. The number of training and test trials were constrained for reasons outlined earlier—that is, to minimize (or eliminate) any explicit knowledge of the imposed contingencies during training and because extinction was expected to occur rapidly in the absence of reinforcement during test. More generally, we felt that it was important to keep the procedural details as similar as possible across experiments.

All of the analyses were performed as described for Experiment 1 with just one exception. Because the amount of capture increased slightly when timing parameters were modified, the amount of time required to disengage attention following capture, defined by dwell time on the CS+ or CS−, was evaluated for a subset of the participants.2 Results from this analysis would indicate whether or not learning history affected the speed of attentional disengagement subsequent to capture. As in Experiment 1, 4% of trials were dropped from the analysis because they fell outside of the criteria for saccade initiation speed while 5% of the trials were dropped because the initial fixation was not directed to the target or to the irrelevant singleton following display onset. Across participants the range of excluded trials was 0%–9.6%.

Results

PEQ: Participants remained unaware of the experimental contingencies. In contrast to Experiment 1, none of the participants guessed that they had participated in a fear conditioning investigation; instead participants stated that the purpose of the experiment was to investigate “how shocks affect sequence memory” or “how threat of shock affects stress level” among other ideas. Like Experiment 1, while 10 of the 20 participants answered

2 Participants who showed capture by one type of CS (e.g., the CS+), but not the other (e.g., the CS−), and therefore had an empty bin in a condition of interest were dropped from this analysis. Altogether, data from five individuals were excluded.
“yes” when asked they were asked whether there was an association between the rectangles and the shock, none of them went on to accurately articulate the contingencies. Instead, they indicated that specific spatial locations or the speed of their eye movements determined whether or not shock would be administered. In general, participants seemed to have remained completely unaware of the experimental objective. In response to Question 3, four participants correctly identified the CS+ as the rectangle most often associated with shock, four participants identified the other rectangle in error, and 12 participants indicated that both rectangles were associated with shock equally often.

**Training phase: Saccade initiation is faster to the CS+ than the CS−.** Consistent with instructions, participants made a direct saccade to the search target (i.e., the horizontally or vertically oriented rectangle) 94% of the time. As in Experiment 1, effects of learning were evident in saccade latency to the target, faster for the CS+ than for the CS− (main effect of trial type: $F(1, 19) = 5.65$, $p = .03$, $\eta^2_p = .23$). The trial type by training phase (i.e., first half of training, second half of training) interaction was not significant, nor was the main effect of training phase, $F$s $(1, 19) \leq 1.17$, $p > .05$, $\eta^2_p \leq .06$. Despite the absence of a significant interaction, Bonferroni corrected post hoc comparisons showed that the effect of trial type was significant in the second, $t(19) = 2.43$, $p = .05$, Cohen’s $d = .55$, but not the first half, $t(19) = .60$, $p > .05$, Cohen’s $d = .15$ (see Figure 4a of the training phase). This outcome replicates results reported for Experiment 1, and indicates that associative learning developed gradually over the course of training.

**Test phase: Eye movements and attention are captured disproportionately by the CS+.** On average, oculomotor capture was evident in 21% of the test trials (range: 3%–53%), and as reported for Experiment 1, a repeated measures ANOVA indicated that capture occurred more often when the irrelevant singleton was the CS+ (main effect of trial type: $F(1, 19) = 16.00$, $p = .001$, $\eta^2_p = .46$). The main effect of test phase (i.e., first half vs. second half) was not significant, nor was the trial type by test phase interaction, $F$s $(1, 19) \leq 1.26$, $p > .05$, $\eta^2_p \leq .06$. Bonferroni corrected post hoc comparisons showed that capture by the CS+ was more robust than capture by the CS− early, $t(19) = 3.24$, $p = .009$, Cohen’s $d = .80$ (see Figure 4b) but not late, $t(19) = 1.52$, $p > .05$, Cohen’s $d = .34$ in testing.

To address questions about whether or not it is more difficult to disengage attention from the CS+ than the CS− following capture, dwell time on the irrelevant singleton was calculated for the subset of trials that elicited overt oculomotor capture. In this case, analyses were collapsed across all of the blocks to avoid empty cells and were limited to the subset of participants for whom there was capture by both the CS+ and the CS− ($n = 15$). A direct comparison of CS+ to CS− capture trials indicated that dwell time was not influenced by the fear conditioning manipulation (CS+: mean dwell time $= 70.79$ ms, $SD = 36.46$; CS−: mean dwell time $= 68.34$ ms, $SD = 29.10$; $t(14) = .20$, $p > .05$).

Finally, as in Experiment 1, saccade latency was evaluated for the subset of trials that did not elicit oculomotor capture. A repeated measures ANOVA with the factors trial type (control, CS+, CS−) and test phase (first half, second half), confirmed that there were differences in saccade latency as a function of trial type, $F(2, 38) = 35.68$, $p < .001$, $\eta^2_p = .65$. The main effect of test phase was not significant, nor was the trial type by test phase interaction, $F(1, 41) > .05$, $\eta^2_p \leq .058$. Because the interaction was not significant, Bonferroni-corrected post hoc comparisons were performed on data collapsed across blocks. Consistent with results from Experiment 1, the presence of an onset (i.e., the irrelevant singleton) in the test display slowed saccade deployment to the target stimulus (control vs. CS+: $t(19) = 6.78$, $p < .001$, Cohen’s $d = 1.52$; control vs. CS−: $t(19) = 6.19$, $p < .001$, Cohen’s $d = 1.40$), but now there was a marginal difference in saccade latency as a function of training history as well. Saccade deployment was numerically slower to targets in the presence of the CS+ than the CS−, $t(19) = 2.48$, $p = .07$, Cohen’s $d = .59$, which hints at the possibility of disproportionate covert attentional capture by the CS+ even when evidence for overt movement of the eyes to that location was absent (see Figure 4c).

**Figure 4.** Eye tracking results from Experiment 2. (a) Saccade latency to targets during the training phase, with data subdivided into early and late halves. Saccades were initiated more quickly to the CS+ than the CS− late in training. (b) Percentage of oculomotor capture by the irrelevant singleton (the CS+ or the CS−) during the test phase, with data subdivided into early and late halves. Disproportionate capture by the CS+ was evident during the first half of the test phase. (c) Saccade latency to targets during the test phase, with data collapsed across blocks. Saccades were initiated more slowly in the presence of an irrelevant singleton (CS+ or CS−), and were marginally slower in the presence of the CS+ than the CS−. Error bars show SEM.
Current Discussion

Consistent with the view that the rapid detection of threat may be evolutionarily advantageous (Ohman & Mineka, 2001), several studies have reported that the detection of a search target is more efficient when targets are “threat-relevant” (e.g., Larson et al., 2007; Lipp & Derakshan, 2005; Ohman, Flykt, & Esteves, 2001; Ohman et al., 2001). Based on outcomes like these, it has been proposed that attention is captured by threat (Vuilleumier, 2005) although this conclusion is not universally accepted. For example, skeptics have argued that reported outcomes may have been due to categorical or low-level perceptual differences that distinguish threat-relevant from neutral materials (see Quinlan, 2013). More generally, because threat-relevant objects in most tasks also served as the targets of voluntary search, influences of top-down and bottom-up attention were working cooperatively in these studies, which undermines any strong conclusions about whether or not threatening objects enjoy attentional priority or capture attention when they are not task-relevant.

Recent investigations have addressed these concerns by using perceptually simple materials in combination with differential fear conditioning procedures and, in general, the results provide support for the view that attention can be captured by threat (Mulckhuyse et al., 2013; Schmidt et al., 2015a, 2015b; but see Notebaert et al., 2011). Eye movements were attracted to the location of a task-irrelevant CS+ more often than a CS− (Mulckhuyse et al., 2013; Schmidt et al., 2015a) and response times to a target stimulus were slower in the presence of a CS+ than a CS− (Schmidt et al., 2015b). As indicated in the introduction, however, this work is not immune to alternative interpretation. This is because targets could occupy the same spatial location as the CS+ (Schmidt et al., 2015a), shock continued to co-occur with the CS+ throughout testing (Mulckhuyse et al., 2013; Schmidt et al., 2015a), and participants were aware of (i.e., could articulate) the CS/US contingency (Mulckhuyse et al., 2013; Schmidt et al., 2015a, 2015b). It is possible then, that despite instructions to ignore distractors, participants directed attention voluntarily to known threat (i.e., the CS+).

The current experiments, which used a probabilistic fear conditioning procedure, address these potential criticisms and provide evidence for disproportionate capture by a perceptually simple CS+ that was task-irrelevant, never occupied the same location as the search target, and was presented in the absence of aversive outcomes. Eye movements provided evidence for gradual associative learning during the training phase and disproportionate CS+ capture during test despite the absence of explicit contingency knowledge. These results are consistent with past reports that show eye movements are sensitive to memory for items and item-context relationships even when explicit recognition is not diagnostic of past experience (e.g., Hannula, Baym, Warren, & Cohen, 2012; Ryan, Althoff, Whitlow, & Cohen, 2000; see Hannula et al., 2010 for review), and with reported claims that the presence of a threatening object can capture attention (e.g., Koster, Crombez, Van Damme, Verschueren, & De Houwer, 2004; Mulckhuyse et al., 2013; Schmidt et al., 2015a, 2015b). More generally, these results align with recent theoretical perspectives on emotion-attention interactions. For example, as proposed in the arousal-biased competition model, it seems likely that top-down attentional allocation to rectangles during training combined with differential physiological arousal consequent to shock delivery, rendered the CS+ more salient, and thus more likely to capture attention at test, than the CS− (Mather & Sutherland, 2011).

Overt and Covert Eye-Movement-Based Indices of Attentional Allocation to Threat

Attention capture was evaluated using two measures—one that indexed overt movement of the eyes to the location occupied by the irrelevant singleton (i.e., oculomotor capture), and another that indexed covert attention to irrelevant singletons when initial saccades were directed to targets as instructed. In the latter case, the dependent measure was saccade latency, and the expectation was that saccades to targets would be initiated more slowly in the presence of the CS+ than the CS−. While evidence for overt oculomotor capture was unambiguous, having been documented in both experiments, evidence for covert attention capture, absent a saccade in the direction of the irrelevant singleton, was merely suggestive. As has been reported in past work (cf. Godijn & Theeuwes, 2002; Theeuwes & Belopolsky, 2012), participants were generally slower to initiate saccades to targets in the presence of irrelevant distractors (i.e., onsets in the reported experiments), but differences in saccade initiation time as a function distractor type (i.e., CS+ vs. CS−) were not significant in Experiment 1 and were marginal in Experiment 2 with a trend for slower saccade deployment in the presence of the CS+, as expected.

Outcomes complementary to those summarized above were also reported by Theeuwes and Belopolsky (2012) who examined the influence of probabilistic reward history on attention capture. In their experiment, as in Experiment 1 here, differences in the amount of time required to initiate saccades to targets in the presence of high-versus low-reward distractors were not forthcoming. What exactly accounts for the absence of reliable differences in saccade latency to targets in the presence of distractors that can be distinguished based on their learned value is unclear. One interpretation of these results is that this particular measure is simply not a sensitive index of learning in these paradigms. However, another, admittedly speculative explanation for the differences in reported outcomes across our experiments concerns global saccade initiation time. Comparison of Figure 2c and Figure 4c reveals that saccades were made to targets more slowly in Experiment 1 than Experiment 2, an outcome that hints at the possibility that any differential competition associated with the presence of the CS+, as compared with the CS−, in the search display was resolved by the time saccades were made in the first experiment. In other words, if saccade initiation to the target stimulus is sufficiently slow, then any differential evidence for covert attention capture as a function of conditioning history may be lost. Whether or not this explanation is tenable could be tested directly in future studies by adopting procedures that influence the speed of saccade deployment systematically across blocks or trials. Finally, it is important to note that the relative absence of saccade latency differences across conditions was not likely to have been a consequence of poor resolution of peripheral detail, as observations of overt oculomotor capture depend on participants having successfully processed the same information (i.e., rectangle orientation).

While the primary objective in this investigation was to document capture after having controlled for a number of variables that
may have affected past outcomes, we had also planned to address questions about whether or not it is more difficult to disengage attention from the CS+ than the CS− when overt capture had occurred. Unfortunately, the amount of oculomotor capture by the CS− in Experiment 1 was quite low, which meant that differences in dwell time across conditions could not be examined. While this was also problematic in Experiment 2, preliminary analyses were performed based on data from a subset of participants \((n = 15)\) who showed oculomotor capture to both the CS+ and the CS−. Dwell time differences across conditions were negligible, which hints at the possibility that it was no more difficult to disengage attention from the CS+ than the CS−. This finding is consistent with interpretations of past work where response times, rather than eye movements, served as the dependent measure (Schmidt et al., 2015b). In that study, response times to targets were reliably slower in the presence of CS+ as compared with CS− distractors, a result said to reflect disproportionate capture by the CS+.

However, an equally plausible explanation for response time differences is that attentional disengagement was more difficult following capture by the CS+ than the CS−, with instances of capture well-matched across conditions. If this is correct, then it may suggest that difficulty with attentional disengagement will only occur (or is more robust) when participants are aware of the imposed contingencies. In other words, differences in the ease with which attention can be disengaged following capture may depend upon conscious assessment, or appreciation, of threat, which in our case had been purposely minimized. Such an outcome would be consistent with the view that qualitative differences distinguish implicit from explicit processing as has been proposed in the implicit perception literature (cf. Dehner & Jacoby, 1994; Jacoby, 1991; Simons, Hanula, Warren, & Day, 2007). Future studies could address this issue by systematically manipulating awareness (perhaps via explicit instruction, cf. Tabbert et al., 2011), and evaluating the impact of this manipulation on oculomotor capture and subsequent disengagement difficulty; of course, the number of CS− trials eliciting capture would have to be increased to permit meaningful analysis.

**SCR Sensitivity to Fear Conditioning Absent Explicit Contingency Knowledge**

In a departure from past work that has examined capture by a conditioned stimulus (Koster et al., 2004; Mulckhuyse et al., 2013; Schmidt et al., 2015a, 2015b), a standard index of arousal—the skin conductance response—was also evaluated here in Experiment 1. Results indicated that SCR magnitude declined over the course of training (i.e., a standard habituation effect) and did not differentiate CS+ from CS− trials during training or test when data were binned into early and late halves. However, SCR magnitude was greater for CS+ than for CS− and control trials when analyses were limited to the first five trials of each type during the test phase. This latter outcome is consistent with past work that indicates physiological sensitivity to fear conditioning need not be accompanied by explicit awareness and is notable because SCR is not always a reliable index of learning (cf. Ahmed & Lovibond, 2015; Lovibond & Shanks, 2002), particularly when corresponding knowledge of the imposed contingencies is absent (e.g., Hamm & Vaitl, 1996; Tabbert et al., 2011; Weike, Schupp, & Hamm, 2007). As such, our findings join a small, but growing number of studies that have reported greater skin conductance responses to CS+ than to CS− trials in the absence of explicit contingency knowledge (e.g., Balderston & Helmstetter, 2010; Bechara et al., 1995; Knight, Nguyen, & Bandettini, 2006; Schultz & Helmstetter, 2010). As described in the Discussion section following Experiment 1, the lack of global SCR differences in this experiment (i.e., during training and late in the test phase) was likely a consequence of several factors that distinguish our study from others (e.g., complex task design, habituation during training, rapid extinction during test). In sum, and consistent with past work, our data suggest that the expression of SCR differences may be short-lived when contingency knowledge is limited or absent (Raio, Carmel, Carrasco, & Phelps, 2012). Future studies might incorporate pupil diameter as another, potentially more sensitive index of physiological arousal (Reinhard & Lachnit, 2002); we chose not to evaluate pupil size here because changes in eye position might affect the reliability of this measure.

**Future Directions and Other Considerations**

Having documented capture, it is worth considering the neural mechanisms that might be contributing to reported outcomes, along with some potential limitations of the current work. It is well known that the amygdala is critically involved in the acquisition of a conditioned fear response (e.g., Bechara et al., 1995; LaBar, LeDoux, Spencer, & Phelps, 1995), even when explicit knowledge of the imposed contingencies cannot be reported (e.g., Balderston, Schultz, Bailleul, & Helmstetter, 2014; Knight, Waters, & Bandettini, 2009; Morris, Ohman, & Dolan, 1998). In addition, several neuroimaging investigations suggest that the sensory representations of conditioned stimuli are strengthened in specialized cortical processing sites (e.g., face-specific regions of the fusiform gyrus, extrastriate sites sensitive to object form) and can be accessed more efficiently, perhaps via direct feedback signals from the amygdala, than affectively neutral control materials (Pourtois, Schettino, & Vuilleumier, 2013; Vuilleumier, 2015). Preferential processing of these emotionally salient items may then be realized in attentional priority maps said to operate in the frontal eye fields and the superior colliculus, which will ultimately drive the eyes toward the most salient information in a search display (see Bisley, 2011 for review). By virtue of the conditioning manipulation, a task-irrelevant CS+ can then compete with top-down objectives to draw the eyes and attention to its location. Studies that combine eye tracking and neuroimaging methods could evaluate the viability of these claims.

There are just a few limitations of the current work that should be considered. First, concerns might be raised about the use of onsets during the test phase to evaluate differential capture by a CS+ and a CS−. This is because it is well-established that onsets reliably capture attention despite being task-irrelevant (see Theeuwes & Godijn, 2001). It is possible then, that a stronger test of oculomotor capture by conditioned stimuli would employ search displays absent the use of onsets. This is something that we plan to do in future investigations, but the important outcome here is that while the CS− and the CS+, both onsets, captured attention, they did not do so equally often. Based on this outcome, it seems reasonable to conclude that capture was not driven solely by the use of onsets. More generally, future studies could be developed to better distinguish the impact of threat from any influence of top-down prioritization allocated to rectangles during the training phase. For example, it is not clear based on our
data whether the CS−, which was paired with shock infrequently, would capture attention to a greater or lesser extent than a training phase target never paired with shock that was also rendered task-irrelevant at test. Direct comparisons of outcomes reported here to those from past investigations that have not used a fear conditioning manipulation are complicated by differences in procedural details (e.g., the number of items presented in search displays, distance between irrelevant distractors and targets, number of trials used in the test phase, identity and/or perceptual properties of irrelevant items) and therefore do not permit us to address this question without further experimentation.

A second concern has to do with how awareness was evaluated in this experiment. There is a great deal of debate about how best to evaluate awareness in experiments like the one reported here (cf. Lovibond & Shanks, 2002), and it is possible that contingencies were learned explicitly and then forgotten over the course of testing. An alternative approach requires participants to make online predictions about the likelihood of shock administration, and is considered more sensitive, but it is also the case that this approach encourages participants to strategize and deliberately attempt to discover the imposed contingencies. This change in processing strategy will arguably influence reported outcomes. In our case, it was important to minimize contingency awareness so that participants would not voluntarily direct attention to the CS+ during training or test. Based on this objective, it was determined that use of a postexperimental questionnaire was ideal. Our approach was limited because the index most sensitive to awareness was a single three-alternative forced choice question about shock delivery and rectangle orientation. Future experiments could have participants indicate on a posttest whether each of several displays was likely to have been associated with shock delivery during training or not; average performance could then be evaluated statistically for individuals and for the group to determine whether or not participants successfully distinguish displays that contain the CS+ from those that contain the CS−. In short, alternative, and increasingly sensitive methods for assessing awareness could be used in future work, which might then permit the comparison of data from participants who develop awareness of the imposed contingencies with data from those who remain unaware.

Third, as was indicated in the Method sections for both experiments, the number of trials that were used during training and test in this experiment were fewer in number than is the norm (e.g., Schmidt et al., 2015a, 2015b). In both cases this was a conscious decision. The number of training phase trials was reduced to minimize awareness of imposed contingencies and the number of test phase trials was reduced because fear conditioned responses were expected to extinguish rapidly in the absence of reinforcement (e.g., Vansteenkoven et al., 2006). That said, it is important to note that use of just 18 trials per condition at test meant that the total number of capture trials was comparatively low (range, Experiment 1: 0–12 capture trials from 36; Experiment 2: 1–19 capture trials from 36). It is possible that reinforcement would drive capture up, perhaps selectively for the CS+, but reinforcement might also lead to contingency awareness, which is something we were trying to avoid here. Future studies could evaluate effects of reinforcement on the longevity and magnitude of fear conditioned capture and might also evaluate differences in susceptibility to capture as a function of individual differences (e.g., in self-reported anxiety), as results here indicate that there is a fairly wide range of capture across participants.

Finally, it is worth considering alternative explanations for our results. It has been proposed, for instance, in the context of an affect-biased attention model that history of motivational relevance in a particular context may lead to predispositions in search that appear to be reactive or bottom-up, when they are in fact better characterized as a special kind of top-down control mediated by amygdala-cortical rather than frontoparietal-cortical interactions (Todd, Cunningham, Anderson, & Thompson, 2012). Notably, awareness of these predispositions (i.e., affectively tuned top-down templates) and their subsequent effect on attentional prioritization is not required by the model. Eliminating the search component of our training phase, and making the link between training and attention task performance less obvious, might permit some disambiguation of competing claims about the attentional mechanisms of affect-based prioritization.

Conclusions

In two investigations, capture by an aversively conditioned stimulus was documented despite the use of methods meant to minimize any voluntary prioritization of threat. Capture was strongest early in testing, which suggests that in the absence of reinforcement, a conditioned stimulus may only draw attention and the eyes for a short period of time after the training phase has been completed. This outcome is consistent with other demonstrations of relatively rapid extinction in the fear conditioning literature, particularly when awareness of contingencies has been minimized or eliminated (e.g., Raio et al., 2012). In this experiment, as has been reported elsewhere, eye movements seem to be a more sensitive index of learning than SCR, a standard, and frequently employed, index of arousal. Collectively, these outcomes complement past studies that indicate eye movements are an especially sensitive index of learning and memory and converge with past reports that have shown attention can be captured automatically by threat. Several potential avenues for future work were discussed. Perhaps most interesting would be the evaluation of qualitative differences in instances of capture and ease of attentional disengagement when contingency knowledge has been systematically manipulated as described in more detail above.3 In sum, it seems that much like selection history and reward (cf. Awh et al., 2012), attentional priority is sensitive to the presence of conditioned threat (Belopolsky, 2015).

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